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"EVERY MAN IS A VALUABLE MEMBER OF SOCIETY WHO, BY HIS OBSERVATIONS, RESEARCHES, AND EXPERIMENTS, PROCURES KNOWLEDGE FOR MEN"—SMITHSON

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CHAS. D. WALCOTT.
Secretary of the Smithsonian Institution

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SMITHSONIAN MISCELLANEOUS COLLECTIONS

PART OF VOLUME 54

Landmarks of Botanical History

A Study of Certain Epochs in the Development of the Science of Botany

Part I.—Prior to 1562 A.D.

BY

Edward Lee Greene

No. 1870

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The present paper by Dr. Edward Lee Greene, Associate in Botany in the U. S. National Museum, entitled "Landmarks of Botanical History," discusses certain epochs in the development of the science of botany. The subject is viewed from a philosophical rather than an industrial standpoint, and the author gives prominence to the biography of some of the early botanists, including Theophrastus (B.C. 370–286), and Brunfelsius, Fuchsius, Tragus, and Cordus of the fifteenth and sixteenth centuries. Doctor Greene has in preparation further contributions on this general subject.

CHARLES D. WALCOTT,
Secretary, Smithsonian Institution.

WASHINGTON,
November, 1909.

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PREFACE

Any discussion, or any indication even, of landmarks in the history of botany must needs be preceded by a somewhat careful enquiry into the nature and purposes of the science as such. Wherein does botany, as a science, essentially consist? With this question unanswered it were impracticable either to indicate the origin or trace the progress of it.

In the most extended use of the term, all information about the plant world or any part of it is botany. According to this view, all treatises upon agriculture, horticulture, floriculture, forestry, and pharmacy, in so far as they deal with plants and their products, are botanical.

What many will consider a better use of the term is more restricted. In this use of it there will be excluded from the category of the properly botanical whatever has no bearing on the philosophy of plant life and form. For example, that wheat, rice, and maize agree together as to that anatomical structure which is called endogenous would be a fact of purely botanical interest. Quite as clearly such would be the proposition that all three belong to the natural family of the grasses; or this, that each represents a genus; or that the roots in all these plants are fibrous, and of only annual duration. But if it be said that wheat, rice, and maize as food products are of supreme importance to mankind, the affirmation is as completely unbotanical as the several foregoing are perfectly botanical. It is strictly an economical consideration.

If such a distinction between botany and plant industry as I have sought thus to illustrate be received as legitimate, the province of botany is easily circumscribed and its scope clearly definable. In any event, for the purposes of the present work our definition of this science shall be that it occupies itself with the contemplation of plant as related to plant, and with the whole vegetable kingdom as viewed philosophically—not economically or commercially—in its relation to the mineral on the one hand, and to the animal on the other.

Such a distinguishing between the philosophical study of plants and the industrial does not dispute, but rather establishes, the existence of a wide border domain between science and industrial art where botanist and industralist work side by side upon plant subjects; it may be sympathizingly and intercommunicatively, or it may be ignoring each other's presence; a domain within which nevertheless each should be in touch with the other, because each may, and ought to be helpful to the other, as supplying some data useless for his own purposes but of significance in relation to the other's aims. The recognition of this border-land domain illustrates also, if it does not again directly argue, the distinctness of the two realms of botany and plant industry. Here one may observe that the distinction itself would seem less marked if he who is to set himself to the work of an economic or industrial botanist would first of all equip himself with a knowledge of the principles, and cultivate an interest in the aims, of philosophic and scientific botany; so that the industrial botanist as author might always have two reports to make upon any piece of research, one that should be of economic interest, the other one of interest botanical. It may be that this idea will be found to presuppose the conjunction of the philosophic bent of mind with the industrial; a combination of two qualities of mind as rare in the world as genius itself, and less desirable.

In quest, therefore, of a starting point—a first landmark— in the progress of botany, in my understanding of the science, one may pass those authors by who professedly treat of plants from the utilitarian point of view, whether they write of agriculture, horticulture, or of the materia medica. Passing these by, I say, though by no means as not meriting the botanist's attention; for all matters relating to the qualities of plants naturally interest him, unless he be of that school in power a century ago, but now declining in influence, according to whose teachings nothing but dry morphology was of any import. Moreover, to him who, like the farmer, the woodsman, and the primitive pharmacist, has much to do with plants industrially, philosophic ideas may occur about the vegetable kingdom as a whole or in part; and every such idea, though crude, perhaps even erroneous, is a concept essentially botanical. Quite as perfectly so is the distinguishing of different kinds of plants, and the practice of grouping like kinds together under one common (generic) name, which is not only universal, but even a necessity, with those who, like the farmer and gardener, have much to do with a considerable number of plants of different sorts. People following these occupations have actually a systematic botany, with a nomenclature, families, genera, and species, all their own. So then if, in the search for a possibly early botanical landmark, the writers upon farming, gardening, and medicine are to be passed by without serious consideration, it is not because no traces of genuine botany occur in them; it is because we are in search of him with whom the leading idea is that of a philosophy of plant life and form. The first botanist is the first man who undertakes research upon plants *as plants* rather than as things useful or deleterious to man and beast; and the first landmark in the history of botany is the earliest book in which plants and plant organs are discussed each in relation to others. If there is any attempt to distinguish and define plant organs, or any suggestions about the probable functions of any of them, any indications of how plants may be distinguished from minerals on the one hand, and from animals on the other, any attempts to correlate plants as like and unlike, and that upon some recognized principles—in any and all such endeavors, we recognize the activities of a philosophic mind in its attempts to solve problems not economic but scientific. In the author of any such treatise upon plants, however imperfect or even crude his notions may seem to us, we have nevertheless the author to whom belongs the name of botanist, as in the vocabulary of the sciences that name ought to be defined.

What is here undertaken is not a history of botany. There is no purpose of presenting in chronological succession the long line of the contributors to the upbuilding of this science, with an account of the best contributions each has made. That would be the work of a lifetime; indeed, of two lifetimes; for the history of no science can be made out, and presented in its perspective, but by him who first of all has mastered that science itself, in its completeness; and the domain of botany however philosophically restricted remains vast, insomuch that one lifetime seems requisite to the mastery of it in its several departments. A second lifetime should, then, be given to him who should be required to write its history. And still the presentation of a complete and accurate history of botany would remain impossible. Important data are wanting, and hopelessly so. For one example, more than two millenniums ago a highly philosophic and very extensive treatise upon plants was indited which alone among books of its kind has survived the passing of all the centuries. The author of it cites other authors on the same topic whose books, then extant, are long since lost. This writer had also in early life a very illustrious teacher who instructed him orally in botany among other subjects, and who also wrote two volumes of botany both of which passed into oblivion more than two thousand years since. How much, then, of the Theophrastan botany is that author's own? What of its principles are his only as having been imparted to him by his great friend and tutor Aristotle? What passages of the work are but compiled from writings of a more remote antiquity, with which Theophrastus may have been familiar, of which even the authors' names have perished? Questions like these serve but to admonish one of this, that the earliest beginnings of the science do not admit of discovery. The same is in a measure true of comparatively recent periods. The annals of our science, as gathered in hitherto, reveal no more thrilling epoch than that of the sixteenth century. Some of the best known authors of that period, Brunfels, Tragus, Fuchsius, studied, besides the not so very many printed books about plants that were then extant, numbers of old mediæval manuscripts from which they brought forth and quoted many a botanical idea, several of them well advanced beyond the ideas of the ancients as we know them. No annalist of a later age seems to have had time or disposition to ascertain how much of the assumed new and original botany of those German fathers—so they style them—was taken out of old mediæval manuscripts which, although they may still be extant, later historians have neither consulted nor troubled themselves to enquire after.

Contemporarily with those German herbalists there flourished in Italy a learned professor, first at the University of Padua, then at Bologna, afterwards at Pisa, whom people regarded as the one peerless botanist of the time. His university lectures were received as oracular, and students came to him from almost everywhere in Europe; yet Professor Luca Ghini published nothing. His supremacy as botanist of the first half of the sixteenth century is attested by tradition only. In the very next generation after him, several of the chief luminaries of the science were men whom he had trained, and to one of them, Cesalpino, there is now everywhere accorded the praise of having created the epoch of modern botany. To what extent is Cesalpino's great work, *De Plantis*, a product of the mind of Ghini? The question is one that forces itself upon us and is perhaps the more interesting because hopeless of ever being answered.

Such are a few examples of what the annalist who would be just and truthful will often find himself in need of knowing, yet can never ascertain; and they intimate but too pointedly the impossibility of any such thing as a complete and faithful history of any period when once that period is past. It was as realizing this, and also as wishing to avoid presumptuousness, that the present writer declined to undertake a history of botany and chose the title of "Landmarks" as permitting him to evade the responsibilities of the consecutive historian, and leaving him free to bring into clearer light—and especially for study on the part of American botanists—the lives and teachings of those and those only among botanists of the past whose names are more familiar. This plan bears on its face the appearance of an easier task, and such it really is; though that it is a less responsible undertaking may be doubted; for in this case quite as in the other, one must everywhere investigate individual merit, which is less apt to exist in proportion to a man's great contemporaneous popularity than in the inverse ratio of it. It will indeed be found to have happened now and then that the genius who has discovered principles has also elucidated them, applied them to the construction of a system, and gained for himself and his principles the credit and the honor that were due; but perhaps rather more commonly the genius discovering principles has but quietly made the simple announcement of them, has died scarcely honored, and has been almost forgotten, when some other, just far enough above mediocrity to see the value of the principles, and possessing industry and ambition to bring them forth and build on them the system which the principles themselves suggest, gets the credit of the whole, is thought to have created the epoch, and enjoys the fame. But the annalist who leaves all these things as he finds them, reiterating popular laudation of the parasitic propagandist, and burying inventive genius yet more deeply in oblivion, deplorably falsifies history. Quite as little does he deserve the name of historian if his mistakes in this regard be those of ignorance; if they come of his having failed to discover merit because of its having lain under the pall of forgetfulness for a century or two.

The historian who is both conscientious and discreet will give small heed to popular opinion about any particular man or epoch. Neither the adulation of the multitude is of any profound import, nor its voiceless indifference. Its outspoken opposition and denunciation may even be the highest praise. Such being any writer's estimate of popular opinion regarding botanical eras past, his readers will be surprised neither by chapters that are iconoclastic, nor by such paragraphs as reveal immortal honors due to men whose names had almost faded from the roll of fame.

It has seemed to me desirable that, in the tracing of these outlines of botanical history some prominence ought to be given to biography. The reader or the student of a book can never take the deepest possible interest in it so long as its author is unknown to him, or, as it might be said, known by name only; though that is but an empty phrase; for to know a person by name only is not to know him at all. A fair knowledge of the whole career, early and late, of the author of a literary or scientific masterpiece not only intensifies, as I said, one's interest in the work, but is most helpful to the understanding of it, if not indispensable to the full comprehension of it. To this natural and reasonable demand on the part of those who would like to learn something of the history of botany, the historians have not well responded. In most cases they give in a single paragraph, or even in a short foot-note, the year of a man's birth, that of his demise, perhaps the name of the institution whence he had his degree, and of those in which he occupied a professor's chair, and so ends the biography of a man who may have been a genius and the creator of an epoch in science; mere epitaphic statements, which seem only to bury more deeply out of sight the once living and active personality, and to relegate his very name to a still remoter place in the region of myth and shadow. There are probably few botanists of this twentieth century who have the most vague conception of what a single one of the earlier master builders of our science was like in his personality and character. To most of us they are too nearly mythical, and mayhap less livingly pictured in our minds than are some of those really mythological personages that men believed in four thousand years ago. It will be seen that in these studies of the landmarks, I give some prominence to the biographic aspect of botanical history. This has been done at great expenditure of time and thought; but I have felt that the end was an extremely desirable one; and I have little or no doubt that these sketches of the lives of great promoters of our science who lived in other centuries will be received by many as among the most welcome and instructive of my paragraphs.

United States National Museum,
Washington, D.C.
2 July, 1907.

Landmarks of Botanical History

By EDWARD LEE GREENE

INTRODUCTORY

PHILOSOPHY OF BOTANICAL HISTORY

Any history, in order that it shall merit well the name and answer the requirements, must have its definite philosophy. History presupposes some end awaiting attainment, and in itself it would seem to be a well connected record of the thoughts, the words, and the deeds that have either furthered or hindered the attainment of that end. It does not, however, assume that the actual makers of history recognize the ultimate end. That is something which not even the wisest can foresee otherwise than dimly and with much uncertainty. The aim of the science of botany, for example, being the fullest and most perfectly systematized knowledge of the plant world philosophically considered, it still is true that not one in a hundred among the rank and file of real contributors toward this ultimate purpose has had it definitely in view. The great bulk of botanical work hitherto accomplished has been done in detached pieces, and by such as had only proximate ends before their mental vision. And for the very best of research work no more is needed. He who carefully investigates and puts on record the whole life history of a dandelion or of a violet; who gives the whole anatomy of a few mosses, reeds, or sedges, or indicates the morphologic distinctions between the pollen grains of hollyhock and those of thistles, or traces the development of either one; who brings out the philosophy of the twining of a morning-glory stem, or indicates the organogeny or the functions of the stipules of vetch and pea; or he who after years of critical field study catalogues, with original notes and observations, the flowering plants—or the flowerless ones—of a single county, or of the watershed of any lake or stream, every such laborer contributes to the stock of botanical knowledge, and this without reference to personal comprehensiveness of botanical view, or a looking to far off ultimate ends.

Upon the historian of botany, however, it seems to devolve that he shall have some forecast of what botany in its perfection as a science shall be like; for in practice he sits in judgment on each epoch and decides whether as an epoch its tendency was more to the advancement of the science than to its retardation; from which kind of procedure it becomes certain that some ideal of perfection is in his mind. Every writer on botanical history must have his philosophy of that history, unless he content himself and hope to satisfy his readers with disconnected historic fragments.

It may be useful to survey in this connection, though with the utmost brevity, the methods of several representative historians of botany.

Tournefort (1700), eminent among even the greatest promoters of botany, was also its historian. The first fifty pages of his *Institutiones*[1] are occupied with an abridged history of the science during two thousand years preceding his own date. The history is prefaced by a definition. There are two parts to botany: the knowledge of plants, and the knowledge of the uses (vertus) of them. It is a distinguishing between systematic botany and economic. He says the distinction must be carefully noted. He denies to the properties or uses of plants any part in, or influence upon, the systematizing of them. A systematized presentation of the known facts constitutes the first beginning of every science. There can be absolutely no botany at all without systematic botany. These are Tournefort's ground principles. From them we shall gather his philosophy of the advancement of botany. The plant world can never come to be well known until sounder principles of classification shall have been established, and the whole aggregate of known plants shall have been grouped over again upon those better principles. The long line of the most noted authors before him had classified plants in all kinds of ways, some according to characters of the roots, some by differences of stems and leaves, one by fruits alone, another by the qualities and uses of the plants; another grouping them according to their places of growth, or ecologically, as we now say. Seldom were the systems of any two

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[1] *Institutiones Rei Herbariæ*, Paris, 1700.

authors at even approximate agreement. Often that of an individual author was a compound of inconsistencies, utterly inharmonious within itself. As to that very first necessity of botany, rational system, confusion seemed to reign. The flower was an organ hitherto little studied, and scarcely yet appealed to in the art or science of plant grouping. Two or three botanists of a century earlier than Tournefort had suggested that, after all, not in roots or stems or leaves, but in the flower there might perchance be found the key to a more satisfactory method of plant classifying. He undertook now a new systematization of the world of plants, everywhere appealing to anthology in so far as by the presence of flowers and fruits the appeal was possible. Ceasing to take as criteria the qualities of plants, or even the characters of their vegetative organs, and by giving special and close study to both flowers and fruits instead, with judicious co-ordination and use of the characters of both, will botanical system henceforward obtain best furtherance.

With neither the strong points nor the weak ones in Tournefort's system, nor with its success or failure, are we here concerned. All that will engage us now is his conception of botany as a science in process of further development and improvement; in other words, what he would have taken to be the leading philosophic threads of botanical history. They would probably be two, at least as chiefly conspicuous; for during his career his mind had been much occupied with (1) the thought that better and more firmly established generic groups had been the most crying need of botany from the earliest times, and (2) that such more acceptable and more securely established genera would result from the defining of them according to morphology of flower and fruit, the consideration of vegetative organs being omitted as far as possible. So then, from his own outlook over the past of botany and from his best forecasting of its future, they have helped it forward most who have most contributed to a better anthology and carpology, and such obtain with him foremost places in his epitome of botanical history. The fullest credit is given to all botanical travellers to distant shores who have contributed to the enrichment of botanical gardens, and to the making of illustrated folios representing flowers and fruits of plants alien and rare. Meanwhile how small consideration Tournefort accorded to plant anatomy and physiology is evinced by this, that in his history he has not a line to spare for the names of Grew and Malpighi, great promoters though they were of the cause of plant organography in general, and well entitled to rank among creative botanists. We may chance to find historians of less comprehensiveness of view than Tournefort, and some with greater.

Two generations later a countryman of his, Michel Adanson, sketched less succinctly than Tournefort had done the history of botany. It forms the more important part of a voluminous preface to Adanson's *Familles des Plantes.*[1] Eighty-five years after its first publication this History was reprinted, with many augmentations which the author had left in manuscript at the time his death.[2]

A man whom all nature in her every phase attracted and engaged, but still first and last and always a botanist, Adanson's horizon was a broad one. He was also a botanist with a specialty, that of discovering how genera naturally stand together in larger groups that may be called families. On the whole, and if such distinction be allowed as legitimate, he was a systematic botanist; most pronouncedly such. But the sketch that he gives of the history of botany is neither partial nor one-sided. He reviews the science as having progressed along many lines, not one of them unimportant. But since it is families of plants that he is now to treat of at length, the foremost thought in his mind in the writing of a history of botany as a preface to the book is, that he may demonstrate the early rise and tardy progress of this very idea of plant families. It is not, however, the history of that one aspect of botany merely that he writes. Something a little too near the one-idea history was what Tournefort had presented; even as one may to-day say of the latest of all the historians of our science, that he came rather too near to excluding from very thoughtful consideration almost everything except the history of plant anatomy and physiology, and of the taxonomy of the cryptogams. Adanson appears to have realized that no one part of botany is alienable from any other part; that the history of a part of it can not be written as disconnected from that of the other parts; and therefore, connectedly with the presentation of whatever had been done before his time towards a natural correlating or grouping of genera, he brings into view not only that line, but others along which botany has made progress; paying due respect to every kind of effort that makes for a fuller knowledge of the plant world.

With the main purpose, then, of finding early traces of the recognition of something like natural families, Adanson analyzes

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[1] *Familles des Plantes, Paris,* 1763, Partie I. Preface pp. i–cliv.

[2] *Familles Naturelles des Plantes de Michel Adanson,* 2 ed. Par MM. Alexandre Adanson et J. Payer, Paris, 1847.

briefly and in chronological succession more than sixty leading authors, beginning with Theophrastus and ending with some who have been contemporary with himself in the middle of the eighteenth century. Assuming that these analyses are correct, one may read connectedly, with small sacrifice of time and as it were step by step the progress which, up to Adanson's time, had been made in the grouping of genera into families—or whatever else one may choose to call such groups; and, while it will be regarded an important one among several threads that the philosophical and impartial historian is bound to follow I know not who besides Adanson has ever attempted to trace this one except for a very short distance.[1]

And the next thread of botanical story which Adanson picks up and follows is one that lies close alongside the aforementioned. The earlier endeavors to indicate groups of genera were made anteriorly to the time when structure of flower and fruit had come to be accepted as the guide. By what marks did those pioneers of classification guide themselves in their attempted groupings? By way of answer I give a short selection from Adanson's own more detailed report of the matter. Lobel (1570), he says was guided by general resemblances, size, qualities, and uses; Porta (1588), by ecology, forms of roots, of leaves, and vegetative organs generally; J. Bauhin (1650), has 40 classes, by appeal to all organs, as well as to properties of plants and their ecology; Rivinus (1690), inflorescence, calyx, and corolla; Boerhaave (1710), general aspects, ecology, leaves, fruits; Haller (1742), cotyledons, calyx, corolla, stamens, seeds; Gleditsch (1749), flowers, insertion of stamens; and so on through a list of some sixty writers, each a celebrity in his day as the author of some new attempt at system in botany.[2] Of a situation like this, and one so necessary to be brought forward in any history of the science, Sachs knew nothing, neither even Sprengel.

There is another outlook upon the progress of botany that is almost peculiarly Adanson's. At the beginning of the analysis of each author's treatise he notifies us how many different kinds of plants each man knew, or had under discussion in his book—Theophrastus 500, Hermann 5600, Tournefort 10,146, Ray 18,655, as examples—thus recognizing at every step the important consideration that, other things being equal, the greater the number of plant

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[1] Linnæus in his *Classes Plantarum* accomplished this admirably for a very limited period, that is, for the time between 1583 and 1738; only a small fraction of the time during which the idea of classes, or families, had been in the minds of botanists and found more or less distinct expression.

[2] Adanson, *Familles*, vol. i, pp. lxxxix–xciii.

forms a man knows, the safer his conclusions as to the interrelations of all, or of the members of any group of them.

Of course the specialist in plant anatomy, little interested in the whole chain of plant relationships—he to whom 500 species were enough for his own purposes—may chance not to be in sympathy with these searchings of all corners of the earth for new plants.[1]

But to what comprehension of the whole of botany has such a mind attained? It would have something like its parallel in the astronomer, if such astronomer there had been, who had deprecated the labor involved in the discovery of the planets Uranus and Neptune upon the plea that there was already enough to do with the rings of Saturn and the canals of Mars. At least somewhat like that is the attitude of the historian who makes light of the work of plant discovery and plant description. To ascertain, as Adanson was at the pains of doing, what number of species a given systematist had known, was the only possible way of informing himself of the comprehensiveness of the man's view of things. And as to the ideal and ultimate perfection of knowledge of the vegetable kingdom, that is manifestly impossible of attainment, so long as a single type, either living or fossil, remains undiscovered and undescribed. It is a principle which not only justifies, but, in the interests of the science as viewed without partiality or prejudice and comprehensively, imperatively demands the most thorough exploration of every field, the equipment of the best possible botanic gardens and herbaria, and also the highest possible perfection of the art of phytography, that is, plant diagnosis or description.

Of incalculable usefulness to the student of systematization is phytography. Its purpose is that of enabling the botanist to measurably complete his knowledge of this and that group of plants only some proportion of the species of which he has been able to see, inspect, and study in the living state. All that a man may learn about plants in twenty years of field work, supplemented by all that gardens and herbaria have to show, will not amount to the knowledge of any more than a fractional part of the specific membership of as much as one of the many families or considerable genera of higher plants. For the rounding out of his knowledge—general, even superficial knowledge—of whatsoever plant alliance, one is always dependent on descriptions. It is one of the most important conditions of all general botany; one that was fully recognized at the beginning; also one that will forever remain. It has always been and it will always be, that a good plant description, placed before

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[1] Sachs, *Geschichte*, pp. 42, 43. English edition, pp. 39, 40.

one mentally equipped for the exact interpretation of it, is decidedly more satisfactory than the usual herbarium fragment of a plant.

Yet one word as to correct and incorrect phytography. One who has a new plant in hand, and who knows it thoroughly from root to seed, may use the whole of an octavo page and the half of another in what will be supposed to have been an attempt to picture this type in words. This same plant may be much more distinctly pictured to the mind of the trained and habituated phytographer in one-fourth that space or even less, by using the set terminology of descriptive botany. This was invented for the twofold purpose of saving space and increasing perspicuity in plant definition. In its most nearly perfected state it is quite modern; and the history of this terminology is a very significant part of botanical history. The discovery of each term was, in its day, a distinctly botanical discovery and an important one; yet the Sprengels and Sachses have given rarely a hint of the evolution of terminology. To have made out lucidly its history would have been a heavy tax on precious time. Adanson almost alone, it may be said, has not neglected it. It was seen by him that in a well devised scheme of botanical history an account of the development of descriptive terminology and the art of describing should find place. Accordingly in these mere outlines for such history he charges certain authors with having described plants poorly; others he remarks upon as having described them fairly, while to here and there he gives the praise of having described them well.

One must not pursue further the subject of Adanson's topical divisions. Those presented may suffice for what I wished to illustrate, namely his appreciation of what ought to enter into the making of a history of botany. Synoptically placed, those few of his topics of which I make mention are:

1. History of grouping of genera as classes or families.
2. History of accepted criteria of affinity.
3. Progress in discovery of new types.
4. Development of phytography and its terminology.

This mere beginning of Adanson's scheme of history will enable me to indicate the contrast that subsists between his and that of Sprengel, whose not unpretentious work in two volumes was given to the public one year after Adanson's death.[1] Out of the four Adansonian topics named above, only one, the third, obtains good treatment at the hands of Sprengel. The first and second are blank with him; while under the fourth one may gather little beyond

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[1] C. Sprengel, *Historia Rei Herbariae*, Amsterdam, 1808, 2 vols., 8vo.

some salient points in the history of anthology. Anatomy and physiology are so discussed as if not inseparably connected with botany proper. Indeed in his partitioning of the science into the two compartments of the Systematic and Structural he expresses his mind to the effect that while Botany proper is a part of Natural History, the consideration of the inner structure and physiology of plants belongs rather to Physiology.[1] His treatment of these, as developed in the course of the seventeenth century is nevertheless full and explicit. But it is progress in the discovery of new types, history of botanical exploration at home and abroad, and the enrichment of botanical gardens, which more particularly engage Sprengel; and, as Adanson had been more interested in the development of the idea of plant families, Sprengel, as a devoted Linnæan, gives himself to the investigation of the history of genera and species. All the long way from Theophrastus to Linnæus Sprengel lists new types generic and specific as discovered and published by prominent authors; so that a fair chronological history of at least the European Flora is furnished; and these lists of each man's discoveries form so large a part of the body of his work that its principal index is an idex of genera and species.

There is no need of pursuing beyond this brief initiative our examination into the somewhat diverse philosophies of botanical history that have hitherto found expression. Every one may be permitted to have his own. In the present treatise exception will be taken to one assumption made by all earlier historians, that for the earliest intimations of anything looking in the direction of the science of botany we must have recourse to those oldest pieces of literature in which plants are more or less freely mentioned. Adanson, for example, does not begin botanical history without naming Orpheus, Musa, Solomon, Hesiod, Homer, Metrodorus, and Hippocrates who as poets or as physicians wrote of plants. Sprengel has among his initial chapters one bearing the title "Flora Biblica" another "Flora Homerica," another, "Flora Hippocratica"; and these chapters of Sprengel are botany, even very interesting botany[2]; but this is not saying that there is botany in the Sacred Scriptures, or in the poems of Homer, or in the medical writings of Hippocrates. They are, however, classic texts upon which a man of Sprengel's rare accomplishments may write botany. And yet I seem to apprehend certain rudiments of a science of botany in those ancient pieces of literature, the real substance of which those

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[1] Sprengel, *Hist. Rei Herb.*, vol. i, p. 3.

[2] *Ibid.*, vol. i, pp. 6-49.

authors of botanical commentary on the Bible, on Homer, on Vergil, and the classics generally, have completely overlooked. Let me repeat it, that in several pieces of very old literature there are legible traces of a science of botany; traces of which even learned and botanically instructed commentators seem to have failed to take due note.

Here, let any reader who has supposed that certain sciences had their beginnings in the minds of men who wrote books, banish, if possible, that idea. No opinion ever held by a multitude of people was more groundless. If, according to the definitions given by authorities, science is classified knowledge and classification is the process of distinguishing and separating between things like and unlike, then there are certain of our sciences the earliest rudiments of which are almost among the very necessities of human speech. It will not be easy to imagine a tribe of wandering savages on any continent or in any age unused to the distinctions of plain, hill, mountain, or spring, brook, river, lake, and ocean. Their very languages will show that their mind had wrought out these perfectly solid and immovable foundations of the science of Geography. Long subsequently the man of enlightenment, he who knows how to commit thought to writing, takes this old and hitherto unwritten classification of the diversities of the earth's surface, gives it logical statement, dignifies it with the Greek name Geography, and then proceeds to build as on very old yet firm foundations his nobler edifice. He may or may not recognize it that those indispensable group names, plain and mountain, lake and river, are but a heritage to his scientific geography from a very far off antiquity; from an antiquity the history of which neither has been written, nor ever will be. It were well, however, that the geographer should perceive it that the real first beginnings of his science are not with the author of any book, but that they antedate all writing.

Botany, as certainly as geography, had its initiative in primal man's distinguishings and separatings between objects appertaining to the world of plants. The fact that in the rudest and simplest dialects of primitive peoples there exist group names for botanical entities establishes this. It is improbable that there ever was a primitive language, other than that of some arctic tribe, in which there did not occur words equivalent to tree, bush, grass, or to trunk, branch, leaf, fruit, root; and every one of these is the name, not of an individual object, but of a group of like objects. Each is a general—a generic—name, and each testifies most clearly to observation, comparison, reflection, generalization, and also either the invention of a new word, or else the more extended application of an old one, which, in as far as science is concerned amounts to the same thing. It is possible to trace to a time that lies well within the period of modern botany the first detection and first naming of that kind of organ which we call a stipule; but no one will have the hardihood to propose that we may trace to its first employment the term leaf. Yet this term, which one may never hope to trace to its origin, is as strictly botanical as the later term stipule, and more important. Furthermore, there was a time when the very term leaf—or at least its equivalent in some lost language of a primal race—first came into use. And still further, the mental processes by which a Malpighi arrives at the distinguishing between the stipule and the other parts of the leaf, and those by which the unknown primal investigator came to distinguish between leaf and the stem or branch that bears it, are the same. Neither was more nor less scientific than the other. Each equally with the other had done a piece of strictly botanical research. This is not affirming equality of intelligence for the two, or questioning that he of the later time was capable of solving many problems of plant life impossible of solution by him of the earlier era. Also the motives leading to examination and distinguishing may have been quite different: he of the more recent period was actuated it may have been by that scientific curiosity, that mere zeal for knowledge, which often fires the cultivated mind; he of the primeval time was impelled perhaps by sheer necessity. He is much dependent on the plant world for life's comforts, even for its necessities. One part of a tree is of great use to him for one purpose, another part for a very different purpose, a third being of no use. Therefore from his utilitarian point of view it becomes manifestly needful that the different parts of plants be distinguished and each different part named. Language demands the introduction of such terms. But the mental processes, I repeat it, are the same in either case, and without respect to the actuating motive. It is all work of examining, comparing, distinguishing, segregating, and naming the segregates. Every step in the procedure of either is scientific. If one is tributary to a science of botany, so is the other. And if these reflections seem to indicate that scientific botany may be, as to its first elements, older than all literature, what of it? There is but one point of view from which it will be disputed, namely that which regards man as having made his first appearance on earth in a condition of advanced intelligence, with a well-developed language, and also bearing a divine commission to assign names to all manner of natural objects at first sight.[1] When one notes the perfect silence of the historians as to the possible origin of the most common and universal botanic terms, one seems forced to conclude that they accepted this doctrine of the sudden and inspirational derivation of them; and that then, as if unwilling to say so, they evaded the subject by going about the completely different and really quite irrelevant task of cataloguing the trees, shrubs, and herbs mentioned in the Bible, giving them the appellations due them according to the nomenclature of Linnæus' *Species Plantarum*. This was all a mere matter of giving the Linnæan Latin names of certain plants in place of their more ancient Hebrew names. It was not approaching by so much as one step the origin of botany, but rather, as I have said, evading the search.

Assuming that the simplest and most universally employed botanic terms entered into human speech not all at once by sudden and supernatural illumination of one particular mind, but one after another as a part of the natural and gradual evolution of language, it will be conceded that they had been formed and in use during long ages of human existence that preceded the invention of writing. And the chief botanical interest attaching to very early writings will be, not in that they furnish a few score Hebrew or Greek names of plants which the well skilled botanist of a recent period may translate into the terms of modern nomenclature; it will be in this, first of all, that they incidentally record names of some plant organs. Such words as fruit, seed, branch, leaf, and root occur, and these seem to reveal it that plants in numbers have been looked into and studied organologically, and with such success that these names of different parts of trees and herbs are already an indispensable and a firmly settled part of every language. Moreover, the terms tree and herb, grass and grain tell as plainly another story, that of a prehistoric distributing of plants in groups according to resemblances. These two kinds, or at least two phases, of botany are in the writings of Moses and of Homer, and perhaps more valuable because there only incidentally, that is, without botanical thought or purpose in the minds of the writers themselves. They only happen to give us, as through a window accidentally left open, a view in which we see individual plants consisting of named parts or organs, and also assemblages of individual plants, some spoken of as grass, some as herbs, some as thorns, others as thistles, some as bushes, others as trees. Though it be no more than a passing glimpse that one has gained, it is enough to excite curiosity, and to suggest a number of

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[1] Genesis vol. ii, pp. 19, 20.

queries legitimately botanical as to just what, in so primitive a time, may have been the full meaning and acceptation of this or that morphologic or taxonomic term as thus early in general use. Such questionings may not necessarily be idle or useless. There being no room for doubt that as far back as the time of Homer, and even of Moses, there was at least here and there a person somewhat specially skilled in the knowledge of plants, how would such a one have applied, for example, the term root? How many things, in his mind, would have been included under that name? What, in a word, might have been his definition of a root? Possibly we shall never know. Neither is it wholly impossible that we may some day ascertain it, at least approximately; for not so very many centuries after Homer specialists in plant knowledge began to write books upon the subject. Some of those books are still extant, and in print; though they have been made too little use of thus far by our historians, some of whom appear to have been disposed to divide the honors of elementary plant organography between Adam and Linnæus; which was an easy way of evading an important though most difficult part of botanical history. In the writings, I say, of the earliest of professedly botanical authors there would be reasonable expectation of finding a clue to that primitive conception of the root which was theirs who introduced the word into speech; for always the first work of him who is ready to reform and rebuild a science is that of showing wherein the prevailing opinions are at fault. To him nothing is more necessary than this. Our appeal in this instance must be made to Theophrastus of Eresus, whose writings on the philosophy of plant life and form are the oldest that are extant. As a controversialist this philosopher is of the mildest type; more apt to suggest, urbanely, that an old opinion may be wrong than bluntly to pronounce it false. His whole treatment of the subject of the roots of plants reads as if he had gone to work stealthily to undermine an old and everywhere received opinion that roots are simply the underground parts of plants. He names two or three familiar species which, as he reminds his readers, produce roots that are aërial, or at least not subterranean. Then he cites, and very well describes, certain subterranean parts—bulbs and corms, we call them now—which he thinks hardly ought to be considered roots. That Theophrastus openly discredits the doctrine that a root is a root because of its being subterraneously located is proof enough that it was the doctrine commonly received in his time. We are also perfectly warranted in believing that the exceptions he takes to it are his own and new; because no writer in ancient times was more careful than he to attribute to their proper authors any new or remarkable opinions which met with his own approval. But, that the primeval understanding of the root was that which I have supposed is again attested by its universal prevalence in our own time among people who have not been initiated into, or influenced by, the botany taught in our schools. Such peoples, dwelling in all parts of the world, if engaged in farming or gardening hold to a certain classification of farm products, and are wont to speak of grain crops, root crops, etc., using the last named expression without ever a suspicion that a potato is a kind of branch, and an onion a kind of bud. Beyond doubt a very great majority of the inhabitants of the earth to-day, if questioned upon the matter, would answer promptly, and fearless of contradiction, that whatever part of a plant grows beneath the soil is its root; and if any remotely domiciled rustic between Nova Scotia and Patagonia should remark that a white potato is a tuber and that onions are not roots but bulbs, we should know without parley that his abandonment of the principles of primeval plant organography had been brought to pass under the influence of modern book or school.

The survival of these primitive notions about the subterranean organs is more interesting than the origin of those notions. The tardiness of their displacement by a more rational organology is to my mind one of the curiosities of botanical history. That most complicated and difficult of organs, the flower, began to be well understood as early as the dawn of the eighteenth century; but at a time when, by the aid of better microscopes, the important function of stamens had been brought to light, and the doctrine of the flower thereby revolutionized and nearly perfected, it still remained that the rhizomes of iris, the bulbs of lilies and tulips, and the corms of crocuses were called roots by all the botanists; this also some two thousand years after Theophrastus of Eresus had suggested that it might not be very good organology. And as for our historians, I have not found with one of them any intimation of who it was who at last solved for us the hard problem of an acceptable definition of a stem; the definition which at once compelled the recognition of subterranean stems as being stems, not roots. In my view this has always appeared to be one of the most signal triumphs of organographic research. Using the term understandingly and comprehensively, organography is more than half of botany. It is the whole foundation and framework of the science, and a good deal more than that. The progress of botany all along is largely identifiable with the advancement that has been made in the knowledge of plant organs; yet it is just this which one is able to learn least about from the historians. This statement must be qualified by the admission that, as regards that sudden leap forward which anthology made early in the eighteenth century, Sprengel is quite explicit; though he gives little indeed of its earlier history. It is also acknowledged that the story of the rise of microscopic organology, and its progress down to the middle of the nineteenth century was given by Sachs, and with such fulness as to make it occupy more than half his entire volume of the History of Botany. Still these are but separate and disconnected chapters in the real history of organology.

If I here indicate this incompleteness of the history of botany as hitherto presented, it is not because I dare hope in these landmark chapters to make good the deficiency, though earnest and laborious effort is made to show how I think it may be done.

That prehistorically and primevally there existed not only an organology of plants but also a classification of the familiar kinds has already been suggested; and the proposition may here receive clearer statement. Moreover, certain somewhat stilted and pedantic views rather widely prevalent respecting systematic botany as of recent origin, no less than the interests of a truer philosophy of botanical history, seem to call for a vindication of this thesis.

Owing to the profusion of plant individuals on the face of the earth everywhere, the bewildering diversity of their forms, and the usefulness of them to man, it was never possible for men, at whatever stage of mental development, to intercommunicate concerning plants without having group names for them. Words that should have application to particular assemblages or kinds of plants were among the earlier necessities of language; and to speak of plants under group names is nothing less than to speak of them as already classified. The classification has necessarily preceded the invention, and the adoption into language, of the collective name. By way of illustration I select out of a hundred or two of plant names which in our English speech are as old as the language itself, the word "clover." It tells its own story. It was applied to certain plants which were seen to have this common characteristic, that each leaf was made up of—cloven into—three separate equal and in every way consimilar leaves. I say leaves in order to avoid being anachronistic; because leaflet is a term of really very modern invention; one unknown in English, and without its equivalent in any other language, at least of Europe, until the middle of the seventeenth century. Now clover was from the first the name of an assemblage of individuals; if of individual plants of several somewhat different kinds all exhibiting the common leaf character then it was what it now long has been—a generic name. It ought to seem superfluous to say that clover is just as much a generic name as Trifolium, and that white clover, red clover, and alsike clover are as perfectly binary specific names as *Trifolium repens*, *T. pratense*, and *T. hybridum;* but, as I have intimated already, the curious notion is here and there prevalent that a genus is not a genus, nor a species a species, until it obtain a Latin name. I have thought desirable to indicate thus plainly the incontestable fact that to the most primitive and untaught of herdsmen and cultivators, in their close dependence upon many members of the plant world, generic names and specific are as much a necessity, and as certainly in every-day use, as they are with us their school-taught posterity who call ourselves botanists. The true philosophy of botanical history seems to call for special insistence on this fact; as also that the viewing of a number of related genera, and the speaking of them under a family name, is likewise of a very remote antiquity. The English collective plant name "pulse" is as old as the language itself, as covering under a monosyllable all the sorts of peas, beans, vetches, and lentils. It is nothing less than a family name, invented as a means of briefly designating the whole natural group of those cultivated plants of various genera which, in recent botany, are called Papilionaceæ. Ancient Latin writers, to whom many genera of umbelliferous plants were known familiarly, saw plainly their interrelationship and called the whole assemblage of them the Ferulaceæ, naming it after the well-known genus Ferula which, as a genus, is represented by several species in the Mediterranean flora. And all this the Latins had only borrowed from a still more ancient Greek botany; for the Greeks had known as well the genus Ferula under the name Narthex, and were used to speak of the whole line of related genera as the Narthecodes.

From these two or three lucid examples of the naturalness of plant classifying taken from the records of antiquity, let us proceed to make some enquiry into like usages as they obtain among the most untaught in our own time. It is improbable that there may not be found in every country of the Old World peasant peoples who, entirely uninfluenced by books or schools, have nevertheless each some rudimentary system of botany; some terms expressive of their own classifyings of plants, at least such kinds of them as they have much to do with, whether as herdsmen, as cultivators of the soil, or as woodsmen. An American student, however untravelled except in his own broad country, may have gathered even here illustrations enough of the principle now under consideration.

To the colonists and early settlers of a new country no native products of the soil are more important than the trees. Timber, lumber, wood for all kinds of building and fencing purposes, for the construction of bridges, vehicles, and household furniture, not to speak of fuel, bark for tanning purposes, and in autumn mast of nuts and acorns for the fattening of swine for the slaughter—these are among the reasons why early settlers always located their first domiciles along the edges of great forests. And now, if we remind ourselves of certain conditions of the first colonists who came to these shores from western Europe three centuries ago, we shall realize that, while they found themselves in the midst of a land bountifully supplied with timber, the particular kinds were new and strange to them. Nothing was quite the same as anything that they had known in the Old World; and no kind of information would have been more welcome to these colonists than that relating to the enduring and wearing qualities of the woods of these different kinds of strange trees. Every kind was untested, and there was no one to teach them. All had to be learned by trial and experience. Yet not quite all; for, to a band of colonists of three centuries ago, coming to these shores from England, there must be credited such knowledge of English trees and timber as was usual with Englishmen of that period; a knowledge that would be of some service to them as American colonists notwithstanding that American trees were of a much greater number of species, and none quite identical with and European kinds. They had brought with them across the sea a knowledge of oak, walnut, chestnut, beech, elm, linden, and some other trees. As for the chestnut, the beech, and the linden, they found but one kind of each here, and these not very notably unlike their congeneric European species. The settlers would naturally expect to find the American trees of these sorts available for the same economic purposes as their European allies. Neither as to the aspect of the trees nor the qualities of their wood was there so much difference; but with those very important timber trees, the oak and the walnut, the case was different. In place of the one European kind of walnut, the Virginian forests presented them with at least a half-dozen, each strikingly unlike the Old World type, both as to characteristics of foliage and fruit, and as to color and qualities of the wood. If one type of these peculiarly American walnuts bears to-day the name of White Walnut,[1] it is undoubledly because the first settlers of Virginia, taking it for a probable equivalent of the English Walnut for lumbering purposes, found its wood to be by comparison much lighter in color, and named the tree, after the usages of lumbermen, by the color of its wood. The Black Walnut[2] in like manner obtained its name from the almost blackish hue of its wood compared with that of the tree of Europe.[3] And both these names bear distinctly the marks of an early colonial origin; for no native American woodsman of however early a period would have known the wood of the European Walnut so as to have made the comparisons.

From this representation of colonists as practical woodsmen—beyond all cavil an essentially faithful representation—it appears that men without the least training in school botany, exploring the woodland resources of a new continent with none other than utilitarian ends in view, find systematic botany an indispensable necessity, create for themselves a serviceable system of woodland taxonomy, make themselves the pioneers of taxonomic research in the new field; this not, however, as using the terms taxonomy and classification; not even as necessarily knowing so much as the name of the science which they are practising. Let us distinguish mental processes. Nothing more is here needful. He who is occupied with testing wood or timber as to its economic usefulness is doing the part of the industrialist. He who compares one sort of living tree with another, noting by what marks of habit, of bark, of foliage or of fruit the two may be distinguished, and who determines to call one of them by one name and the other by some name that is different, is doing exactly the work of the botanical systematist. This man may never have learned a syllable of the terminology employed in schools of botany. He may not have heard the Latin name for oak, for maple, for poplar, or any other genus of trees, or even the word genus; but he is a botanical systematist none the less; and since his business obliges him to be this he proves the utility of botanical system. It is not possible for the occupations of the farmer, the herdsman, or the lumberman to be carried on without botanical classification and a fixed nomenclature of both genera and species. Therefore those thus engaged have never at any time in

[1] *Juglans alba*, Linn.

[2] *Juglans nigra*, Linn.

[3] *Juglans regia*, Linn.

history waited for the schools and the school-trained botanists. They have made their own botany, have established both system and nomenclature; and these, in so far as they had proceeded, the professionals when they came upon the scene adopted. The two, that of rustic, of mountaineer, of herdsman, and of woodsman, and that of the schools, are as essentially one botany, as certainly one in kind, as wild pears, wild apples, and wild grapes are respectively one in kind with their cultivated and improved offspring of the orchards and vineyards. If this be true, then the annals of botanical science have another beginning than that which our annalists have assigned it.

When once it is seen that group names for plants are as old as language, and that these very names establish it that men always in all ages classified the many plants with which they had to do, there is another matter which immediately calls for careful investigation, that is, the parts of the plants to which rude primeval botanists looked for the marks by which to range their plants in convenient groups. We have already seen that Adanson alone among historians perceived that attempts had been made down through all the centuries to group plants by other data than those of flower and fruit. In bringing this fact into view, and by citing a long line of early authors in attestation of it, he was fearlessly contradicting, and at the same time successfully controverting what his contemporary, Linnæus, had said when in the warmth of his zeal for the great Cesalpino he had pronounced him first in the order of time among real systematists.[1] The truth about Cesalpino was simply this, that he had been the first to attempt an orderly arrangement of the plant world by universal appeal to the fruit and seed; and that alone would still have been the superlative of praise, doubtless well merited. But that the Cesalpinian system seemed incomparably superior to every one that had preceded it could never become a warrant for saying that those systems antedating it might be left out of view altogether, as never having been systems at all. I can conceive of nothing which science more inflexibly exacts of every scientific man than truthfulness. She cannot permit an enthusiastic fancy to take the place of fact. But there have been successive generations of botanists since Linnæus who, as if swearing by his authority as if he had been infallible, have seemed to have no idea that any plant classifying ever was attempted upon any other than that anthocarpological basis which now for some three centuries has been

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[1] Linnæus, *Philosophia Botanica*, § 54.

steadily in use. As a mere prejudgment it is deeply seated in the botanical mind of to-day, that nothing can be done, or ever could have been done, in the direction of an orderly arranging of the world of plants but by appeal to characters of flower and fruit. And along with this prejudice there dwells another as deeply ingrained, namely, that the flower was the same thing to botanists of four hundred years ago, if not to those of three thousand years since, which it is to us; whereas not yet two centuries have passed since the flower began to be known. Our classifying by flower and fruit was fairly established while as yet the corolla was regarded as the principal part of the flower, and was in fact the synonym for flower, without even its name corolla.

Something may be done towards undermining these prejudices by giving a few examples of primitive systematizing as undertaken while as yet the flower, as to its essentials and its functions, remained an undiscovered organ.

For a good illustration of classifying by leaf characters alone, those of flower and fruit being quite ignored, we need go no farther back than the later years of the sixteenth century and the first quarter of the seventeenth. Let us limit ourselves to the forty years intervening between 1583 and 1623; also inspecting certain pages of two of the widely known and thoroughly approved professional botanists of the time, Rembert Dodonæus and Caspar Bauhin. The genus Clover, in ancient Latin Trifolium, in Greek Triphyllon, already referred to in this chapter, is an ample one with the authors named. Bauhin's book contains names and descriptions of some sixty species; and since both he and Dodonæus are almost as strict adherents of binary nomenclature as was Linnæus himself who came into this field of nomenclature a century later, it will be easy to show what they received into the genus Trifolium by presenting here two opposite columns of binary names. In as far as they admitted to their Clover genus genuine clovers as we now understand them, the reproduction of their names need not be made.

Dodonæus (1583), and Bauhin (1623)	Recent Names
Trifolium bituminosum	Psoralea bituminosa.
Trifolium odoratum	Melilotus officinalis.
Trifolium corniculatum	Lotus corniculatus.
Trifolium cochleatum	Medicago orbicularis.
Trifolium palustre	Menyanthes trifoliata.
Trifolium acetosum	Oxalis acetosella.
Trifolium hepaticum	Hepatica triloba.

It ought here to be noted that for the combining of Melilotus with Trifolium, Dodonaeus is reponsible but not Bauhin, who at this point saw fit to abandon the trifoliolate leaves as essentially and without exception conclusive of membership in Trifolium. He does not, however, as others had done before him, accept the melilot species as constituting a genus of their own, but places them all as members of the genus Lotus, where also some species are trifoliolate, others not so.

I observe also that if only the first four of the species of the above list had gained admission to Trifolium along with the clovers proper, one might have thought it probable that some dependence, after all, had been placed upon the floral structure; for in that case the authors would have had a Trifolium composed of papilionaceous plants exclusively. But neither in the defining of the genus nor in the description of a single one of the about sixty species of Bauhin's Trifolium is any mention made of the morphology of the flower. And by the admission of gentianaceous, oxalidaceous, and ranunculaceous types into that genus it is placed beyond question that in his mind the genus was limited by nothing else but the herbaceous nature of the plants, ternate foliage, and dry fruits. I say dry fruits, because in Bauhin's book the strawberries, as typically trifoliolate as the most genuine of clovers and as herbaceous, stand in closest juxtaposition to them, and it is manifest that their, juicy berry-like receptacles, with seeds all on the outside, saved Fragaria from being merged in the Trifolium of that author. And in this giving so much attention to the fruit where flowers were wholly ignored we see the influence of Cesalpino's great treatise; for Bauhin and Cesalpino were contemporaries, in a manner, the former younger by thirty years.

All through such books as have here been cited one reads the resoluteness of purpose and the hard perseverance with which men labored to improve botanical system by studying and comparing texture and duration of stems, and above all else the morphology of leaves; a very crude system at its best; but system of some sort there had to be; the flower was still virtually unknown; the fruit was barely beginning to be appreciated in its usefulness to taxonomy; therefore the vegetative organs, chiefly the leaves, were most commonly allowed to be decisive.

The appeal to leaves was not, however, first thought of in either the seventeenth century or the sixteenth. Even then it had been more or less in vogue for three or four thousand years that we know of.

As a good example of an antique genus of trees based on leaf characters alone, with no dependence on those of flower or fruit, one recalls the Daphne of the Greeks. The original Daphne with them was the sweet bay, *Laurus nobilis* Linn., the type-species of the genus *Laurus* as now received. Its foliage, being lance-shaped, of a somewhat leathery texture, with no marginal indentation, represents a rather common leaf type, and the tree is evergreen. Now there was a period of Greek history within which there was not only some travel to foreign lands, but even some written reports about the natural products of other countries; and there is the most convincing evidence that every new tree or shrub that came to light that was both evergreen and clothed with oblong or lanceolate entire leathery leaves was at once relegated to the genus Daphne; was named as another kind of laurel. Here is a list, possibly not a complete one, of trees with *Laurus* foliage which ancient Greeks, on that account alone, had referred to that genus: *Laurus nobilis, Nerium Oleander, Nerium odorum, Avicennia officinalis, Rhizophora mucronata, Ruscus Hypophyllum*[1]*;* six species of so-called *Daphne* or laurel, belonging to five different genera, and representing as many different families, all anciently accepted as of one genus, because the foliage in all was much the same, and for no other reason whatsoever. And this again reminds us that in eastern North America, where there is no laurel, we have a number of kinds of native shrubs that have always been called by that name, just as if they had been members of the genus *Laurus* to which they are in no wise related. If we ask ourselves how this false naming came to pass, the answer is, that at the time of the early colonization of this new continent the old Greek idea was still dominant, that whatever bush or tree had a laurel foliage was a good enough laurel. The colonists brought that idea hither, and naturally enough applied the name to our Kalmias and Rhododendrons one and all.

This classifying by foliage was never received as anything like a general principle everywhere to be applied. So far from that, the rude primitive groupings were accomplished here and there under the sole guidance of the fruit; though in the main only as to none but its most superficial characteristics. Among fruit-bearing trees the apple tree was perhaps the oldest and most familiar type, unless the pear be excepted; and as new kinds of fruit trees in the course of history became introduced into Europe from other lands, every kind, the fruit of which was of considerable size and of something

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[1] Bretzl, *Botanische Forschungen des Alexanderzuges*, p. 405.

like the sphericity of the apple, was called a kind of apple tree, a member of the genus *Malus*, as it was called in Latin speech and writing. Here is a partial list of the kinds of *Malus*, or apple tree, that find record and description with several early authors; excluding, however, the true apples, which were of many varieties all with binary names.

Malus Armeniaca	Armeniaca vulgaris.
Malus Persica	Amygdalus Persica.
Malus arantia	Citrus aurantium.
Malus limonia	Citrus limonium.
Malus medica	Citrus medica.
Malus cotonea	Cydonia vulgaris.
Malus cydonia	
Malus aurea	
Malus Punica	Punica granatum
Malus granata	
Malus Indica	Zizyphus jujuba.

In outline, the history of the development of such a genus Malus as the above is this. In primeval southern Europe they had the common apple tree, *Malus communis*, and, from the beginning of the historic period at least, they had it in many cultivated varieties. The fruit was *malum* with the Latins, the tree *malus*. Then, as other kinds of trees were introduced from the East having spherical or ovoid fruits not too small for apples, their fruits were also designated as kinds of apples, and the trees as species of malus. To us who, with also several generations of our botanical ancestry, have become accustomed to a greatly improved classification, such a piece of systematizing as the above list of apple-tree species exemplifies cannot but seem absurd; but the presentation of something of that kind was necessary, partly in order that we might realize from what small and simple beginnings our later and better systems of carpological classifying have been evolved; also partly as demonstrating the groundlessness of the Linnæan hypothesis that classification by fruit characters took its rise with Cesalpino, and as late as the end of the sixteenth century.

I am unwilling to dismiss the subject of early and practically pre-Cesalpinian classifying by fruit without having given one more illustration of it. For this purpose I shall again advert to the taxonomic procedures of the early Virginian colonists. I have cited the case of their having found there, in place of the English Walnut, two allies of that tree, and that they named these new kinds Black Walnut and White Walnut respectively; also that this naming of the kinds was made not in reference to any morphological characters of the trees, but to that of the colors of the wood; this having been done quite after the manner of self-taught woodmen, whereas school-taught botanists would have assigned names suggested by organographic marks. But this all relates to nothing else but the making of specific distinctions and the assigning of specific names. When we ask ourselves by what marks they were able to refer these new trees to the genus of the walnuts, we obtain but the answer that it was by those of their fruits; these in such degree resembling that of the one kind of walnut before known to them as to warrant the conclusion that the trees were of the walnut kind, as they would have expressed it, rather than of the oak or chestnut kind.

But our colonists' experiences with the native American oaks, if they had been more fully recorded than they were, would have been still more interesting. As English woodsmen only one kind of oak can have been well known to them. In Virginia they can not have failed to meet at once with about a half-dozen sorts, most of them in aspect exceedingly unlike the English Oak; so much so that they can not reasonably be supposed to have identified them with that genus of trees at all until after close inspection. One of the sorts displayed to them the foliage of the chestnut tree, another that of the laurel, still another the leaves of a willow. The chestnut-leaved kind had not at all the bark nor the wood of chestnut trees, but of oaks, rather; therefore these first observers of the tree would hardly have needed to appeal to the fruits in order to satisfy themselves that this new tree was but an oak, merely imitating the chestnut as to its foliage. But among the other kinds, such as had neither foliage nor bark nor wood in any way answering their idea of an oak tree, they can not have determined to be oaks by any other note in each but that of its fruit.

That which I have thus far hypothecated concerning early Virginian colonists in relation to native Virginian oaks is demonstrable as something more than even the most rational of hypotheses. There is documentary evidence of the historic truthfulness of all, and more than all, that I have here but intimated as probable. That these men, forced by circumstance to make trial of the timber of trees new to them, did early recognize as oaks certain kinds most unlike what they had known as oaks, in all except their fruits, is attested by a colonial list of names of new American oaks which was published when the colony was but two generations old. I refer to Banister's *Catalogue.*[1] This contains a list of binary names of Virginian oaks, such as *Quercus alba*, *Q. rubra*, *Q. Hispanica*, *Q. castaneæfolia*, *Q. salicifolia*. Now while a casual reader of the catalogue cited would, without a second thought about the matter, attribute those five names to Banister, it is extremely unlikely that any one of them was invented by him. It is next to certain that the whole five are his mere translations into Latin of the oak names that he found in use among the colonists. Perhaps the plainest proof of this is, in that by turning those five binary names back into English you get precisely the names by which five common oaks are known to dwellers in that same region now, two hundred and twenty-seven years after Banister's having written his list. It is really evidence that is incontestable. To dispute it would be to affirm that the names were made by Banister himself, in Latin, then turned into English for the use of the woodsmen settlers; that these had been waiting sixty years or more for the professional botanist to come and tell them by what names to call their several kinds of oak; each part of which proposition, like the whole of it, is absurd. Under pressure of necessity, and from the outset, they must have begun to learn the different qualities of the wood or timber of those strange new kinds of oak. One or two of them were found comparable with the familiar oak of the mother country as being hard, durable, subserving the purposes of the builder, the wheelwright, and the cabinet-maker; another, not subject to decay when set into the ground, useful for posts; still another durable only when used for bars, rails, and like purposes; and there may have been a fourth and fifth kind excellent for winter fuel, but nearly worthless otherwise. No man will pretend to believe that colonial woodmen and handicraftsmen, learning by degrees the different qualities and uses of our various American oaks, did not immediately assign a particular name to each particular kind. The important industries of house-building, boat-building, cabinet-making, the constructing of vehicles, the building of fences, and the providing of the winter's fuel, all demanded quite imperatively that there be a well ordered and generally accepted system of woodmen's nomenclature of oaks as well as of other genera of timber trees. So it came to pass that all important trees everywhere, in America quite as elsewhere, had their established names before the arrival of the writers of floras and silvas; and there is many a kind of tree the Latin name of which bears the

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[1] Banister, *Cat. Plant. Virg.*, transmitted from Virginia to John Ray in 1680; published by Ray, in *Hist.*, vol. ii, in 1688.

mark of its having been originally the woodmen's vernacular name for it.

Hitherto this fact of the universal existence of a crude primitive system of plant classification—one that antedates all botanical writing, a system that is in vogue to-day all over the world in out-of-the-way places, in complete isolation from the influence of colleges and universities—appears to have remained unnoticed by botanical writers. At least, I have met with no allusion to the fact. I therefore doubt that it has entered into the minds of botanical thinkers in recent times that such untutored yet effective and useful plant taxonomy exists, and must have existed primevally.

There will be readers enough to whom this thought will be new and somewhat startling. The fond conceit has long prevailed, that there was never anything in the world that could be called science until some three centuries ago, or four, at the farthest. Among several ideas about the botany of the past—ideas very widely, almost universally, entertained, though without the least warrant from history—I shall here mention but the following: that plant genera did not obtain fair recognition until Tournefort, nor species as distinguished from varieties until Linnæus, nor families before Adanson. Now if, according to the present thesis, the beginning of the receiving and naming of common plants in groups is ancient beyond all possibility of discovery, then no author can be credited with, or any date be assigned for, the beginning of the recognition and naming of either genera or species. What great men like those just named accomplished for the improvement of system in botany was, the better delimitation of several anciently accepted genera, and the laying down of certain rules and principles by which they thought all plants, known and unknown, might be arranged in groups more nearly according to their affinities. Assuming that the rules and principles were philosophic, all this was immensely to the advantage of classification; but when for the twofold purpose of emphasizing the principles and making the new system easy to learn, they caused each genus name to be printed in large type, in the middle of the page, occupying a line by itself, then close under that the formal statement of its characters as a genus, and after that and only less conspicuously the species names, each occupying a separate paragraph, they were by this rigid formalism inaugurating, though they knew it not, an era of didacticism which now after two centuries has degenerated into an almost gross pedantry which rules systematic botany at present well-nigh universally. To illustrate the supremacy of this pedantry let me suppose that some accomplished botanist of Italy or France or Germany, having a new genus to propose, ignores all the usual post-Tournefortian formalities in his naming and defining of it. This will mean only that, declining to follow established typographical usages, he does not place the new generic name in large letters in a conspicuous place above all that he has to say of it, but begins his paragraph with a statement that the type is new, thence proceeding without a break to name the marks by which the genus is distinguishable from all its allies, then directly adding, in the same type and without the formality of an initial capital, even in the middle of a line, if it so happen, the name by which he proposes that the genus shall be known; all this followed, still without breaking the paragraph, by whatever else he may have to say about the plant or plants of this new genus. Between such simple uncapitalized compact taxonomic paragraph as I have supposed, and the familiar stereotyped form of naming and defining a genus, there is at first glance the appearance of great dissimilarity. As to the meaning of the two, and the information that is conveyed, there is no shade of difference between them. The plant type, supposing it to be the same, is as fully described and as certainly named in the more simple paragraph as in the one that is ostentatious. All that the botanical scholar can learn from the one he may learn just as perfectly and just as promptly from the other. Really the differences between the two are hardly more than typographical; yet notwithstanding this, it is probable that forty-nine out of every fifty botanists of to-day, if not even a much larger proporton of them, would in part fail utterly to perceive that the simple unostentatious paragraph which I have supposed, with generic name in small type set in the midst, had been intended as the publication of a new genus; and it is as probable that those of the forty-nine who did perceive the author's intention would deliberately ignore the paragraph, under the plea that the name and characters of a genus printed in a style so very unconventional must not be admitted to answer the requirements of publication. The genus must be treated as unpublished. This, be it noted, will be the same as to order that a new scientific fact be, in as far as possible, suppressed for the reason that certain familiar usages as to type and paragraphing were not followed in the publication of that fact. It will be regarding form of expression as superior to the facts expressed; will be allowing individual whim or fancy to ignore important matter; will make for the establishment of shallow pedantry in place of solid information and the use of plain good sense. I have stated this case hypothetically; but there are provinces where the situation is to-day actual.

But the greater number of my hypothetic forty-nine, as I have said, will even fail to discover that a plant group has been named and defined on any page where the modern typographical conventionalities are not in evidence. I think that among such as are quite proficient in systematic botany there are many whose impression of the printed pages of Ruellius, of Dorsten, and of several other classics of sixteenth-century botany would be that they give no account of families, genera, or species; and this only because great authors had not then learned to make a separate paragraph for every group, and to print the names of genera and of species in type different from and more conspicuous than that used for the descriptive passages. There is no question of the superior convenience of our modern style of printing taxonomic matter; still, for the mistaking of mere incidentals for essentials by people professedly scientific, it is not easy to frame excuse. But the psychologic fact is well established that men do in this wise err, and that there are multitudes of biologic taxonomists whom familiar usage has completely deceived into thinking that no name is generic unless printed in large letters; multitudes of botanists who will have been startled by the proposition incontestable that clover, parsley, hazel, and birch, all as here printed, are names as perfectly generic as TRIFOLIUM, APIUM, CORYLUS, and BETULA. Moreover, there have been learned historians of botany in post-Tournefortian times whose minds appear to have been under the same delusion, and who thereby missed one of the fundamentals of the philosophy of botanical history.

It is impossible that men, even the most primeval and unlettered, manage their affairs with various denizens of the plant world without classifying them. Names of plants, generic and specific, and also other names more comprehensive, are a part of the vernacular of every tribe of the uncivilized, as well as of that of every rural province within the bounds of civilization to-day. The very names attest the fact of classification; for no name is that of an individual plant. It is that of a group of plants, always; a group specific, generic, or more comprehensive than either.

It may occur to some that the named groups recognized by the untaught do not in their delimitation correspond to those that obtain with the professional plant taxonomist; as if that, if it were true, would in the least alter the situation or affect the argument. It will be difficult to understand how the vernacular genera of the ruralists can possibly correspond to the Latin-named genera of the scientific botanists when the latter themselves are seldom at agreement among themselves as to the exact limits of any considerable genus, or of any polymorphous species. When we ourselves may have learned to agree as to where one of our Latin-named groups is to end and the next is to begin, may we with some propriety criticise the same kind of doing as accomplished by the unindoctrinated.

And now let me demonstrate it, that in the history of classification the unlettered vulgus now and again has been first to arrive at the satisfactory delimitation of a natural group, the learned doctors having arrived at this same judgment later by one or more generations, and so as to seem to have adopted it from the untutored laity. This point may perhaps be most easily made plain by returning to the contemplation of North American colonial botany and the colonial dendrologists. It was shown above, that all the several American trees of the walnut alliance with which they became acquainted, although all, in certain particulars, different enough from that one Old World walnut which they had known, they called walnuts; precisely the same as if they had denominated them species of JUGLANS, which would have been the case assuredly, had they but known and used the Latin terminology in place of the English. We, of three centuries later, dispose of these American trees differently, referring nearly all of them to another genus; but what is remarkably to the credit of that colonial and primitive taxonomy is, that so exalted an authority as Linnæus found no fault with it, but simply adopted it. With him all the different kinds figure as good enough species of JUGLANS, and bear with him even the same specific names which the colonists had assigned, but of course Latinized.

When, in a preceding paragraph, I gave early American colonists the credit of having recognized and named as oaks a considerable list of native acorn-bearing trees; even as having determined them to be oaks by their acorns alone, I felt that there might be demurrers to the opinion that these had not learned this mark of the genus *Quercus* from the schools in some more or less indirect way. I may well, therefore, here place it beyond dispute that in this case also the unlettered men of field and forest did arrive at the proper delimitation of a genus of trees quite in advance of the professional taxonomists, and these last virtually adopted the genus, as we now have it, from the ruralists. In the first decade of the seventeenth century, when the Virginian colonists were beginning to learn the industrial uses, and at the same time the characteristic superficial—as we should now say morphological—marks, of many new trees, there was yet no book of botany extant, in as far as I can learn, in which it was taught that any and every tree that has an acorn for a fruit is hereby known to be a *Quercus*. Neither Pliny nor Tournefort, nor any author of the fifteen centuries that intervene between those two, has fewer than three distinct genera of acorn-bearing trees. With each and all of them a tree bearing acorns, in order that it may be of the genus *Quercus*, must be deciduous, and its foliage sinuately lobed. In other words, none but deciduous white oaks are properly called oaks by these old authorities. Trees bearing acorns, but evergreen as to foliage, the leaf margins prickly, are of a separate genus, *Ilex;* while those oaks of southern Europe with peculiar foliage, along with a thick spongy bark,—cork oaks, we call them,—are of a third genus, *Suber*. At the time, therefore, when Banister was turning into Latin those English binary names which colonists had given to Virginian trees bearing acorns, there was not yet a book of botany extant in which it was taught that its yielding acorns was a sufficient warrant for naming a tree an oak. It was only the country people, the woodmen, who held that view as to the extent of the genus Oak. The learned John Ray, the same who received from Banister the manuscript catalogue of Virginian plants and caused it to be published, himself never swerved from the doctrine then time-honored and classic, that we have here three genera, *Quercus*, *Ilex*, and *Suber*, yet expressly states that "the common people so extend the name *Quercus* as to include under it all kinds of trees that bear acorns."[1] In as far as I have been able to trace the history of oak classifying on the part of professional botanists, Linnæus appears to have been the very first to repudiate what had been the opinion of all his predecessors, and to adopt as more true to nature the more comprehensive genus *Quercus* which the vulgus had invented. And so, if we of the present, following Linnæus as to the limits of *Quercus*, are in the right, then let us concede freely the fact, from which there is no escape, that during long centuries the unlettered vulgus was taxonomically correct, while all the learned botanists were wrong.

By means of the popular nomenclature of common ornamental plants, one is able to see how those uninstructed in botany readily classify things according to floral structure. Everywhere lovers of flowers have a group of plants which they call by the collective name of lily. This happens to be many times more comprehensive

[1] Ray, *Historia Plantarum*, vol. ii, 1385 (1683).

than the lily genus of the modern professionals; though not so widely different from that of the books of botany of some centuries ago. It embraces *Lilium*, *Fritillaria* (Checkered Lily), *Hemerocallis* (Day Lily), *Amaryllis* (Belladonna Lily), *Vallota* (Scarlet Lily), and many another genus of liliaceous and amaryllidaceous plants, besides *Convallaria* and the members of several iridaceous genera. All of these have showy flowers of the same morphological type as that of the true lily. And to this floral type even the white funnel-form spathes of certain araceous plants have been associated, as the name Calla Lily plainly betrays; though it is not to be doubted that the entire, narrow, veinless foliage of all these plants has helped in the making of this popular generic synthesis. And then, on the part of the botanists, the analyzing, assorting, and systematically arranging of these diverse elements of the primitive genus lily—the genus as even now, I say, accepted by a great multitude of mere flower lovers—has occupied a great number of taxonomic specialists during later centuries. The carefully gathered records of the gradual evolution of our present taxonomy of the lily-flowered plants would fill a thick volume; would most perfectly establish the fact that those botanically untaught sometimes classify by the flowers; would illustrate how different generations of professed taxonomists have made their various appeals to different organs, some to the flowers chiefly, others giving much weight to considerations of roots, bulbs, and corms, while others were more influenced by considerations of the pericarp and seed. And such a book, in its completeness, would form an instructive epitome of the whole history of botany.

It would be as easy to produce instances of a primitive classifying by characters of the root; or, at least, of those subterranean parts of plants which, until within a very recent period, were universally confused as roots. But it may be unnecessary to multiply proofs of the existence of an almost more than fragmentary, and really rather extensive system of what one may paradoxically denominate pre-botanical botany. Enough may have been said already for the accentuating of the opinion that there are beginnings of our science which the historians should not have overlooked. It has been out of those crude beginnings that learning and philosophy have developed what we now call the real systematic botany. They are even the prothallium from which at length there has arisen the frond of whatever strength and symmetry and grace there may be in the now accepted taxonomy of plants.

This condition of things being once seen and admitted, we shall for the first begin to be able to understand, to appreciate, and to interpret the earliest botanical authors; not those only of ancient Greece and Italy, but of the fathers of botany in England, in France, in Germany, and Switzerland; a worthy company whose true position seems to me never to have been half understood, and whose works have therefore more or less completely baffled the attempts of profound scholars and eminent botanists who in the capacity of historians, have sought to elucidate the texts of these forefathers and to show what they severally accomplished. Botany did not begin with the first books of botany, nor with the men who indited them; though every historian of the science whom I have read has assumed that it did. The most remote and primitive of botanical writers, of whatever country or language, found a more or less extensive vocabulary of elementary botany in the colloquial speech of all. The chief organs of plants—stem, trunk, branch, leaf, flower, fruit, pod, seed, root, tendril, thorn, and a multitude of others—had been discriminated and named; the organs even known by all who had acquaintance with plants and trees, and the names were everywhere in use. Even the functions of several of the organs had been correctly ascertained before ever a line of botany had been written; most probably even before letters had been invented. The improvement of wild things by cultivation, the propagating of the newly acquired sorts by cuttings, by division of perennial roots, and, in the case of trees, by grafting, are likewise arts that seem to antedate history; as do also the designating of different varieties or species that are evidently nearly akin, by twofold names, one generic, the other specific or varietal.

All these conditions being recognized, a new and peculiar difficulty will confront the critical student of a protobotanical author. It will in exceptional cases seem doubtful as to whether a given fact or generalization is the fruit of that author's own investigations, and therefore new with him, or whether it be something already long understood and accepted, which he is but placing upon written record. It is, however, a kind of difficulty that gives zest to the study of classic texts; and many such doubts may give place to certainty, or something near it, after persevering examination, and comparison with other passages that are not of doubtful import.

I am unwilling to conclude this introduction without repeating it, that the essence and substance of botany proper are organography and the logical deductions that we draw from organography. They may not be said to be the whole of the science; yet duly and comprehensively considered they will be found to come near it.

The line of the development of organography—organography as necessarily including terminology—is that along which a truly coherent and philosophic history of botany must needs be written. The condition is one that will entail the expenditure of incalculable time and unremitting toil; but the cost of time and energy must not be counted if anything beyond disconnected and fragmentary paragraphs of history are to be the outcome.

CHAPTER I

THE RHIZOTOMI

It is characteristic of all very early phytography that the root, that least obvious and most hardly accessible of plant organs, is as carefully described as are the stems, the leaves, and the fruits. This fact that the first of all describers of plants should have taken the root into account, and that so uniformly and so particularly, must seem strange enough to every thoughtful botanist of later centuries; it is in such marked contrast to the descriptive usages with which we of the present are better acquainted.

In the voluminous and carefully technical phytographic works of the eighteenth century and the nineteenth, one may chance upon successive pages filled with descriptions of scores of species, about the roots of which not a word is said. Nothing like this occurs in any book or chapter of Theophrastus, Dioscorides, or any other classic botanical writer. In the case of every species of herbaceous plant, and of many that are woody, they do not conclude a description without telling us what the subterranean parts are like, whether fibrous or fleshy or tuberous or bulbous, usually informing us as to the colors of these organs, as well as the properties of them when they are known to have any. And so carefully did the fathers who wrought a revival of botany in the sixteenth century follow those classic models that, in their illustrated folios, never a plant is figured the root of which is not as faithfully delineated as the foliage or the flower. Even in the letterpress accompanying the plates of Brunfels, Fuchs, Tragus, and others, the root is as well described as the foliage, and much better than the flower. All this for the simple reason that the great masters of remote antiquity had set them the example. But how did it come to pass that the ancient Greek botanists were so almost singularly familiar with the underground parts of plants, and that they so accentuated the importance of them to phytography? One would not have expected this, and it seems almost anomalous. No one who ever went forth to make philosophic conquest of the vegetable kingdom was confronted at the outset by the roots of plants. These parts are recondite. He who for any purpose would inspect a root must undergo the labor and inconvenience of digging about the plant, wresting that root from its hiding-place, and then cleansing it, so that its characteristics may become visible and tangible. When all is done, there is a great sameness about roots. They do not promise much aid to him who would find marks by which to distinguish between like and unlike. Stems present a much greater diversity, and either leaves or flowers or fruits a hundred times more differences by which to distinguish plants, than do these underground parts. It being granted that the function of the root as a vital organ was thoroughly understood, as it appears to have been from the earliest historic period, still, as regards early descriptive botany, there would have been less reason to be surprised if the early fathers had commonly ignored it; or at best had made as little account of it in their descriptions of species as most of the modern systematists have done. And there must be a philosophy of this very ancient and once universal appreciation of the root as a subject of phytographic notice equally with stem and leaf. The cause must, if possible, be ascertained.

If the ancestors, even somewhat remote, of the first botanical philosophers had been savages such as, in dearth of animal food, had found the subterranean parts of many a wild plant available in its stead, then would there have been some show of reason for that singular prestige which roots had obtained so almost primevally. In the transition from savagery to civilization such root-food plants would have come into cultivation, where they would have held their place and been well known to enlightened posterity. But at the time when writing about plants began, at least with the Greeks and Latins, roots and bulbs constituted but an inconsiderable part of their table fare. The bulk of their farm and garden products were the cereals, orchard fruits, pot-herbs, and salads. The ancestry of the philosophers for centuries, possibly for millenniums, had been highly civilized, perhaps to the degree of having lost the traditions of nomadic life and the feeding upon wild products of the plains and woodlands.

In this civilization, however, the art of medicine held an important place; and in this circumstance we have a clew to that predilection for describing so faithfully the roots of everything which is so almost peculiar to the phytography of the ancients and their sixteenth-century imitators.

Throughout the whole period of Greek antiquity there was a class of men who followed as a regular business the gathering, preparing, and selling of roots and herbs that were of repute in medicine. It was of course naturally inferred and easily ascertained that whatever properties perennial herbs possess are concentrated in their subterranean parts during the season of the plants' rest in autumn and winter; so that what were called the roots of plants formed the bulk of the materia medica. All this the very name of rhizotomi, root-gatherers, sufficiently declares.

The botanists of antiquity, that is to say, they who investigated the plant world as philosophers rather than as economists, inform us that the rhizotomi were mostly unlettered men, usually more superstitious than scientific, observing an extensive ritual in the digging as well as in the after preparation of their simples; evidently mixing medicine and magic after a manner almost universal in the early history of the healing art; as often attributing to their preparations magic virtues as medicinal. Concerning some of the ceremonies of the rhizotomi we have information.[1] There were various prayers and incantations to be said or sung. Some kinds of roots were to be dug in the daytime, some others by night only; the powerful plant hellebore, only after the observance of various precautions. The point of a sharp sword must be drawn three times around certain roots to make them more efficacious. The gatherer of some sorts must be careful to face the east while digging. In the case of others he must stand on the windward side of the plant. Some require to be collected by one newly anointed with oil. As preparatory to the culling of other kinds, the rhizotomos must eat garlic and drink wine. As modern and as learned an author as Kurt Sprengel relegates all those observances without discrimination to the category of foolish superstitions[2]; this, as it seems to me, inconsiderately. There are plants enough the exhalations of which are so deleterious that persons of delicate organism may be uncomfortably affected by the mere passing close to them on the leeward side, of a breezy day. Any discreet American botanist not immune against *Toxicodendron vulgare*, if tempted to gather specimens of it, would use among other precautions that of holding himself to the windward of the plant if there happened to be a breath of air stirring. His act would be adjudged sane and reasonable. Another such precaution might be that of using gloves while handling any parts of the plant; whereas an old-time oriental,

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[1] Theophrastus, *Hist.*, Book ix, ch. 9.

[2] Sprengel, *Hist. Rei Herb.*, vol. i, p. 63.

with whom the anointing of the body with scented oil was a common practice, might successfully use oil instead of gloves while gathering particularly acrid herbs and roots. Also when one is informed that the ancient drug gatherer never proceeded to dig certain roots but with breath laden with an odor as intolerable as that engendered by garlic and alcoholic drink, there is still no reason to charge that to superstition; any more than the surveyor or handicraftsman, whose work for the day is in the midst of a steaming and unwholesome marsh or fen, makes free use of tobacco smoke as, by the physician's counsel, tending in some degree toward immunity from malarial influence. With us who believe so much in the efficacy of malodorous disinfectants as bringing immunity from infectious and malarial disease it should seem natural to attribute similar precautions to Greeks of 4000 years ago, especially when assured, as we may assure ourselves, that even at that remote period one of the rhizotomi propounded the theory, now in our day revived, that myriads of germs, minute, invisible, permeate every atmosphere.

Such partial apology for some of the so-called ritual observances of the rhizotomi is no digression. The historians have usually referred to them as in large part a body of superstitious fakers. Such, to a degree, many of them may have become in the long course of centuries during which their profession flourished. Superstitious observance is often enough the end of that which in the beginning was a reasonable and sensible measure of precaution; and it is not a legitimate office of history to exaggerate the differences subsisting between an earlier and a later age or race. The age of superstition even as regards medicine and pharmacy, though passing it may be, is not yet quite past. If the scholarly Sprengel cites the ceremonies of the rhizotomi with impatience, it is because he is influenced—as many another passage in his work makes it evident—by an almost morbid abhorrence of everything that to him has the appearance of a superstition. If anything appeared to be an empty ceremony, he could not tolerate the thought of it long enough to examine into the possibility of its having had an origin that was scientific and utilitarian.

If the rhizotomi were mostly illiterate men and quacks, still there were exceptions. Here and there among them there seems to have been a man of letters; and a few investigated plants more or less scientifically, and wrote books. The names of several such have been handed down through history, together with some of the more original and remarkable of their sayings. THRASYAS MANTIN-

ENSIS is mentioned by Theophrastus as one of the worthiest among them, because as if ignoring the common beliefs about magical effects he gave himself to the investigation of the properties of plants. He seems to have been the original proponent of the doctrine that the good or bad effects of a medicine may depend upon the temperament of the individual patient; a proposition which has met with some acceptance, at least outside the profession, if one may judge by its having been long since crystallized into a proverb, that what is medicine to one, may be poison to another. The idea is revolutionary, though without yet having brought about much of a revolution. To this same Thrasyas is ascribed the compounding of that vegetable poison, so frequently in use with the ancients, which never failed to bring a speedy and absolutely painless death.[1] Theophrastus devotes two chapters to an account of the pharmacological researches of this Thrasyas, and those o his eminently successful disciple Alexius, and of those of a third of the same school of intelligent and really scientific rhizotomi, Eudemus of Chios. One and another of these men, living at periods so remote as barely to fall short of being prehistoric, tested in their own persons the adaptability of the human system to the harmless use of drastic and poisonous vegetable substances. Beginning with small doses and increasing them gradually, it was ascertained that one might after a time consume without bad results such a quantity of hellebore, for example, as under ordinary conditions might have proven fatal. Using at first earthen pots and pans in the probing of questions about possible or probable antidotes to certain poisons, they would proceed, under the light gained by such experiments, to the using of their own stomachs as the crucibles.[2] And the reports of these instructive and daring experiments, together with the names of the men who made them, were either written and subscribed to at the time or else handed down by tradition to the time of Theophrastus who gave them permanent record.

Among the earlier rhizotomi there was a famous one named CLEIDEMUS, who wrote upon the subject of electrical storms so as to have been quoted by Aristotle in his Meteorology.[3] He also investigated diseases of plants, especially of the fig-tree, olive-tree, and vine. Cleidemus is therefore the earliest of vegetable pathologists. And what may be more interestingly significant is this, that Theo-

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[1] Theophrastus, *Hist. Plant.*, Book ix, ch. 17.

[2] *Ibid.*, ch. 18.

[3] Aristotle, *Meteor.*, Book i, ch. 2.

phrastus credits him with having maintained that there is the closest analogy between the organs of plants and those of animals. Information like this can not fail to awaken regret that the writings of Cleidemus have not survived; it would now be so very interesting to know whether this genius of a forgotten time went so far beyond those of later periods as to have apprehended the existence of breathing organs and those of sex in plants.

Hippon was among the rhizotomi who philosophized about plants in general, and wrote books. His writings are quoted by both Aristotle[1] and Theophrastus,[2] and he appears to have been the earliest among students of plant life and form to venture the opinion that all cultivated trees, shrubs, and herbs have been derived from wild ones, and are susceptible of reversion to their pristine condition. It is the earliest hint—and a very early one, apparently unknown to the annalists of evolution—of what cultivation may accomplish in the way of transformation. But the doctrine must have had the sound of a heresy verging toward atheism in the ears of a populace that had never questioned the proposition that every cultivated plant and tree had been coeval with the human race, and had been so created at the first.

But it is not that small, better-educated, more reflective, and philosophizing contingent of the rhizotomi, or the possible influence of these few upon early botanical theory, that we are just now chiefly concerned with. It is rather that in this whole body of those who, for so many pre-Theophrastan centuries, followed the root-gatherers' calling, we have the men who securely established that precedent, from which the earliest philosophic students of and writers about plants did not break away, of taking full cognizance of those among plant organs which nature had most deeply concealed, as if they were perhaps the last and the least to be considered. It was the example of the rhizotomists, in their books of plant description extant in the times of Aristotle and Theophrastus, that impelled Theophrastus and others after him to give the form, texture, color, odor, flavor, as well as the active properties, when these were known, of the roots or underground parts of almost every plant. And when, as already noted, it is seen that from Dioscorides and Pliny down through the middle ages, and out to near the end of the seventeenth century, authors in general described and figured the roots of every weed and grass and bush and tree, it will be conceded to have been the lot of the half-illiterate

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[1] Aristotle, *De Anim.*, Book i, ch. 2.

[2] Theophrastus, *Hist Plant.*, Book i, ch. 6.

rhizotomi to make their peculiar impress upon the character of descriptive botany, an impress that should last for well nigh two thousand years [1]

The occupation of the root gatherers is by no means peculiar to Greek antiquity. In every part of the world it may be as old as, or older than, the beginnings of civilization. Nor is it probable that in Europe there was any interruption or cessation of the occupation during the two thousand years intervening between the time of Homer and Hesiod and that of the Renaissance. The botanical writers of the sixteenth century, particularly those of middle Europe, refer to the practices, and even to the opinions, of such as ransack the woods to gather roots and herbs, sell them to the druggists and to the peasantry whom they serve in the capacity of physicians, and from whom the educated and philosophic students of plants themselves sometimes gain valuable information. Nor would it be safe to say that the rhizotomi are even now everywhere obsolete. Their traces are very plainly legible in the popular nomenclature of North American plants. Every common name into which root enters as a component is one that had its origin with the "herb doctor," or "root doctor," as he was called; perhaps not a few of the names were borrowed, along with some information about the plant's virtues, from the aborigines.[2]

[1] It was Valerius Cordus, the greatest if not the only botanical genius of the first half of the sixteenth century, who first gave expression to the opinion that, from the morphologic and phytographic point of view, the importance of the root had always been overestimated. He set the example for a reform of descriptive botany in this particular; but, as usual with men of genius, he was a century in advance of the ideas of the multitude.

[2] The following are illustrative examples: Alum-root, Blood-root, Bowman's-root, Culver's-root, Cancer-root, Canker-root, Black Snakeroot, Button Snakeroot, Seneca Snakeroot, Indian-root, Musquash-root, Colic-root, Pappoose-root, Pepper-root, Pink-root, Red-root, Yellow-root, Sheep-root. It were easy to double the number of such names of American plants, not one of which was assigned either by a learned physician or a professional botanist.

CHAPTER II

THEOPHRASTUS OF ERESUS. B. C. 370–286 (or 262).

LINNÆUS, in the practice of his favorite art of systematizing, classified not only plants but the writers about them. The writers he distinguishes primarily as Botanists, and Plant Lovers; recognizing as Botanists only such as treat of plants from some philosophic or scientific point of view. Choosing his illustrations from the annals of remote antiquity, he names among the earliest of the Greeks who wrote of plants Hippocrates[1]; but because he wrote of plants only in the interests of medicine Linnæus styles him Father of Medicine; a title that had been conceded to that worthy ages before Linnæus, and will be accorded him until the end of time, no doubt. Similarly Aristotle, who is also known to have written upon plants, but whose volumes on that subject have been lost, is down in the Linnæan list of ancient celebrities as Prince of Philosophers. To Theophrastus, however, he accords the title Father of Botany. From this opinion, far from having been newly promulgated in Linnæus' time, there has been no dissenting voice. On the contrary, Albert Haller, one of the most learned men in Europe in his day, and a botanist of such renown that Linnæus held him in reverence, and also in some fear, denominates Theophrastus "the first of real botanists in point of time."[2] Curtius Sprengel in the nineteenth century, having rehearsed the names of a long line of ancient authors who had written more or less concerning plants, says: "But the most illustrious of them all, and the true father of botany, was Theophrastus Eresius."[3] If the author of the latest of nineteenth-century volumes of botanical history, Julius von Sachs, makes but passing mention of Theophrastus, along with the names of Galen, Dioscorides, and Pliny—as if he had not been otherwise a botanist than they—he may be more or less excusable upon the

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[1] Linnæus, *Philosophia Botanica*, § 9.

[2] "Primus verorum botanicorum." Haller, *Bibliotheca Botanica*, vol. i, p. 31.

[3] "Celeberrimus autem omnium, verus rei herbariæ parens, THEOPHRASTUS fuit Eresius." Sprengel, *Historia Rei Herbariae*, vol. i, p. 66.

ground that he was not engaged upon a general history of botany, but only dealing with the short period intervening between the years 1530 and 1860 of our era. However, that Sachs had no acquaintance with Theophrastus, or even of Dioscorides, is proven by this, that he credits his sixteenth-century German compatriots with having gone straight to nature and described plants originally, whereas the truth is that nearly all the plant descriptions occurring in Brunfels and Fuchs, are almost word for word translations of the ancient paragraphs of Theophrastus, Dioscorides, and others; sometimes with a few words of their own added, as often with none. And as regards that exact and intimate knowledge of plants which comes of the careful study of them alive and growing, it is safe to say that all which Sachs' sixteenth-century German fathers knew combined, would have amounted to but a fractional part of Theophrastus' knowledge, and that much of their own observing had been suggested to them in their reading of his books. An abstract of Theophrastus' work should enable the unbiased and impartial to judge for themselves whether scientific botany had its beginning with those good German herbalists of the sixteenth century, or with an immortal Athenian or two who had lived, studied plants long and carefully, lectured to thousands of students, and written down the substance of their botanical lectures seventeen centuries earlier than they.

Life. Mitylene, a large and rich island in the Ægean Sea close by the coast of Asia Minor, was famous millenniums ago as having given birth to many an illustrious personage. Arion and Terpander, ancient masters of the art of music, Alcæus and Sappho, unrivalled among lyric poets, as learned critics gather from the fragments of their masterpieces that remain—these names but head the list of celebrities that had been born on that island in the earlier half of the thousand years next preceding the beginning of the Christian era. How very famous this island was for the excellent quality of its products, material, artistic, and intellectual, is shown in the fact that Greeks and Romans of a somewhat later period, wishing to bestow the highest praise on anything, whether it were a piece of music, a verse of poetry, or a cask of wine, were accustomed to pronounce it Lesbian—that is, fit to have come from Lesbos, the name by which the modern Mitylene was known anciently. Such are a few of the available hints of the environment in which the protobotanist was born in the year B.C. 370. His birthplace was Eresos, the most important town of the island, whence he has been styled Eresius—the Eresian, perhaps to distinguish him from others of his time who bore the not uncommon name Theophrastus. His father's name, Melanthus, and that of his boyhood's teacher at Eresos, one Leucippus, have found a place in history only as associated with that of this child and youth whom they called Tyrtamus. We shall be warranted in inferring that the child was of unusual gifts and marked by nature for the intellectual life; also that Melanthus, the fuller, was in comfortable circumstances financially, for the educational advantages that were given the boy were then somewhat rare and costly. How well the child had improved his opportunities is attested by this, that while as yet but a youth, he was away beyond the farther shore of the Ægean Sea, at Athens, and there numbered among the disciples of Plato. All historians of the period credit him with having been under that philosopher's instruction before coming to Aristotle; and as Tyrtamus was only twenty-two years old when Plato died, it is plain that the enrolment among Plato's pupils must have been made when the subject of our sketch was but a youth—possibly a precocious, eager, ambitious boy only.

The histories all read as if Aristotle's marked friendship and efficient patronage had had very much to do with establishing the fame and directing the luminous career of Theophrastus. There must be a large measure of truthfulness in this representation, though it is more than possible that it is somewhat exaggerated in Aristotle's favor; and history should take cognizance of the universal and even necessary fact, that in great friendships the influences are mutual, just as when, in the heavens, two planets move to their conjunction each influences irresistibly the orbit of the other, draws it somewhat aside from what should have been its path. The story, as always rather too briefly told, leaves an impression, not intended to be made, of great disparity between the two both as to years and some other controlling influences; seeming to represent Tyrtamus as the brilliant young favorite, and Aristotle the elderly admiring teacher and foster-fatherly patron. That the youth, as if irresistibly obedient to an old and revered master's mandate, should have renounced the name Tyrtamus that he brought with him from the paternal home in Eresos, so that henceforward he should be Theophrastus, is something to create almost a conviction that the one was old and masterful, the other young and submissive, and not to be thought of as an influence upon the thought and action of the elder. Such impressions are wrong, and must vanish by a comparison of certain well authenticated dates, which show that Aristotle was Theophrastus' senior by only fifteen years; also that the two were fellow students under Plato. Aristotle from the age of seventeen to thirty-seven had been Plato's pupil. Tyrtamus, it may reasonably be assumed, entered Plato's discipleship at seventeen if not earlier. If so, he may have been Aristotle's student companion there for from five to seven years before the day when Aristotle left Plato and opened a Lyceum of his own. Shortly after this Plato died; and then, unless he had done so even earlier, Theophrastus became Aristotle's student. And as for this new name, it is not necessary to suppose that it was bestowed merely as the flattering compliment paid a highly promising young student by an old preceptor. The two were in truth much upon an equality. They were companions and much attached friends, with no signal disparity between them as to years; so that the change of name may well be thought to have been brought about by mutual agreement.

I can not but wonder at the boldness with which Meyer pronounces this change of name from Tyrtamus to Theophrastus to be a fable.[1] No one else has questioned the authenticity of this part of the biography; and he has not been able to adduce so much as one valid reason for his pronouncement against its truthfulness. One of his supposed reasons is, that Aristotle was no flatterer. To have rendered this an argument, Meyer should first have disputed the sincerity of Aristotle's friendship for Theophrastus; for between genuine and devoted friends flattery is impossible. But he says the name Theophrastus was not uncommon among Greeks of the period, which is equivalent to saying that, if the philosopher had been going to give his disciple a new name he would have selected some uncommon name, or else that the Eresian had always been Theophrastus and never Tyrtamus. That "such a changing of names was unknown" is quite as inane as the rest of this historian's argument upon the subject. Not one in ten thousand of the ancient Greeks has been known to us by any name at all. Even of that comparatively very small number whose names we have heard, who shall say that none besides Theophrastus ever underwent a change of name because the event if it happened was not recorded? There is not the least reason for thinking that with Greeks, in passing from one age or condition of life to another, the taking of a new name was uncommon. There were distinguished examples of it among some of their neighbors, the Hebrews, for example. Meyer concedes that this "fable" about Theophrastus was universally received as a fact by all the ancients, and we add that it

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[1] E. H. F. Meyer, *Geschichte der Botanik*, i, p. 147.

is as perfectly authenticated as any other fact or incident in Theophrastus' life. He who can make fiction of this part of it may upon similar grounds invalidate, piece by piece, the whole biography. No reason having been given for doubting about this incident, every one to whom the appellation is not meaningless will see that Tyrtamus was a grotesque name, if not a ridiculous one, to be borne by a scholar and orator of commanding presence and predestined great renown. The young man himself, as well as his master and friend, must have realized this; and it is hardly to be doubted that the displacement and rejection of the unsuitable name was the first object which the change had in view; and that what the new name should be was but a secondary consideration—a matter of less importance. Indeed, the biographer Laërtius relates that at first the Eresian began to be called Euphrastus, and then later Theophrastus. Posterity should be grateful that the change was made; and also grateful for that devoted attachment between the two philosophers by which it came to pass that the elder of them, dying in middle age, had his own work taken up, and carried forward with success during almost another half-century.[1] It was such a friendship as led Aristotle to give to Theophrastus his own library, said to have been the richest one then in existence, and to have included the manuscripts of his own works, a treasure which by means of Theophrastus' jealous care was almost singularly preserved, and handed down to posterity well-nigh complete. Also the botanic garden which Aristotle had established at Athens was made a gift to Theophrastus; by whom also it was newly equipped, variously improved and adapted to greater usefulness; this, too, on a scale so extensive, that a wealthy friend of Theophrastus and benefactor of science is named in history as having borne the expense of those improvements.[2] The fact of the existence of this Athenian botanic garden will explain how Theophrastus, occupied as he was with the management of, and also engaged in teaching in, a school of two thousand students, with no time or opportunity for travel, gained so intimate a knowledge of the life histories of many plants as he surprises us with in certain chapters of his books. He had studied in that garden at morning, noon, and

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[1] Aristotle died at the age of sixty-three years. Theophrastus was then forty-eight; and, according to his own statement in his preface to that book entitled *Characteres*, he had finished it in his ninety-ninth year. St. Jerome says that Theophrastus died at the age of 107.

[2] The name of this first wealthy patron of botanical science was Demetrius Phalereus, according to Laertius, vol. i, p. 350.

evening for perhaps sixty years or more when, almost a centenarian, he wrote such clauses as the following in his will. They should help us to a realization of the scientific zeal and activity of a forgotten time.

"I bequeath to my friends, who are specially named in this my Will,[1] and to those that will spend their time with them in learning and philosophy, my garden, walk, and houses adjoining; upon condition however that none of them shall claim any particular property therein, or alienate them from their proper use; but that they shall be enjoyed in common by them all, as a sacred place where they may familiarly visit one another and discourse together like good friends."[2] And further: "I desire to be buried in any part of the garden that they shall think most suitable; charging them not to be at any superfluous expense either upon my funeral or upon my tomb. Which being done, my will is that Pomphylus who lives in the house take care of everything, as he did before."[3]

That Pomphylus was an overseer, directing the labors of bondsmen gardeners owned by Theophrastus, comes out incidentally in another clause. "As for my boys, it is my will that Molo, Cymo, and Parmeno be forthwith set at liberty. As for Manes and Callias, I will not have them given their freedom until they shall have labored four years longer in the garden, so that there be no fault found with their labor and diligence; but after that, let them have their freedom." Besides these five, two others are mentioned. "I give Cano to Demotimus, and Donax to Neleus."[4]

By means of this testamentary document, the transcript of which has been fortunately preserved, one is able to realize something of the extent, and even of the perpetuation during perhaps three generations, of this pristine garden for biologic research. And this realization will be exceedingly helpful toward a comprehension of the magnitude of Theophrastus' work along these lines. The philosopher was never, like many of his class and in his time, a traveller. He did not devote any more than a fraction of his time to botany. His writings on this subject amount to perhaps not more than a twentieth part of all that he did in the line of written authorship. And there are chapters in the *Historia Plantarum* that

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[1] Hipparchus, Neleus, Strato, Callio, Demotimus, Callisthenes, and Cresarchus are their names, according to Laërtius, vol. i, 361.

[2] Laërtius, vol. i, 358. [3] *Ibid.*, 359.

[4] Laërtius, vol. i, 360. Note also the botanical names of the two young slaves Cymo and Donax.

are so crowded with facts about seeds, seeds in process of germination, young seedling plants and older ones, observations upon this plant and that shrub as they appear in spring, summer, autumn, and winter, that, all being considered, we should have wondered greatly how this most untravelled and sedentary of the great philosophers had gained all this minuteness of knowledge about the little things of plant life had we not been informed concerning this great garden in the midst of which he dwelt, taking his daily recreation along its paths, and among its seed beds, and within the bounds of which, obedient to his last request, they buried him.

Theophrastus was a voluminous author, having written upon a great diversity of topics. The biographer Laërtius gives the titles of 227 treatises. Not many of these have reached us; but among those preserved are the *Historia Plantarum*, in nine books, with the fragment of a tenth, and the *De Causis Plantarum*, originally in eight books according to ancient records, of which the last two have long been lost. The following paragraphs are the result of a prolonged and laborious study of the principal work, the *Historia*, the edition quoted being that of Stapelius, published at Amsterdam in 1644.

As we have already seen,[1] there existed in the old Greek literature that was before Theophrastus, many a trace of properly botanical observation and reflection, so that he is not in such wise the father of botanical science as that no one before him had recorded a philosophic thought or suggestion about the plant world separately considered. Yet he is, in most cases which he cites, the sole perpetuator of the name and fame of such as Menestor, Hippon, and Leophanes whose passages he quotes and in quoting has saved from oblivion. There is, then, no reason to suppose that in his philosophizings about plant life he had been helped by any predecessors beyond that for which he has given them full credit. It has been observed by historians and critics that a few passages in Theophrastus are also in Aristotle, unaltered, and uncredited to their real author. This hardly merits notice. It is undoubted that the enlistment of Theophrastus' great talents in the service of botany was secured by Aristotle; and it is as certain that the alliance between these two celebrities of antiquity was that of the most devoted friendship; that at Aristotle's demise all his manuscripts, published and unpublished, complete and fragmentary, were gladly bestowed on Theophrastus. They became his property.

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[1] See pp. 48-50 preceding.

He was free to dispose of them, to publish any parts of them, according to his own judgment. Modern science and erudition have no example to show us of like community of even intellectual property between two illustrious friends. Moreover, from the fact of Theophrastus' having lived and studied and written during half a century after Aristotle's demise, we are warranted in thinking of him as of one who had acquired a great store of knowledge about plants beyond all that to which Aristotle in his briefer day had attained.

As having been the author of the oldest distinctively botanical treatise that is extant, the place of Theophrastus is unique, and invites to special and careful consideration. He writes from the midst of an advanced civilization; a state of society in which there is much farming, extensive cultivation of the vine and olive, fruit growing, market gardening, and cultivating of medicinal, aromatic, and ornamentally flowering herbs, shrubs, and trees; a time when many improved varieties of all sorts of things have been derived through cultivation, and when it is already perfectly well known that such improved varieties can not be depended on to come true to seed, but may be preserved, and the stock of each increased by division of roots, by cuttings, and by grafting. It is also a time when the very masterpieces of literature—some of them even in Theophrastus' time ancient and classic—abound in facts and fancies and myths and fables about flowers and fruits, shrubs and trees. Of course all obvious and familiar parts of plants—their organs—have their names. These are a part of the common vocabulary of things. Also group names for growths that are alike are in as universal requisition. If a genus evidently consists of several different kinds, be they what the botanist of the present would denominate species, or be they notable varieties only, each such kind is designated in speech or writing by a cognomen; so that a binary nomenclature, precisely that which all farmers, gardeners, and foresters find needful and have always created and employed, is perfectly established.

Such, in brief outline, is what Greek civilization had attained to in the way of experimental knowledge of plants, independently of all philosophy, and without help from the philosophers. And if Theophrastus had been less than a botanical philosopher, and if as a mere annalist he had but recorded the untaught industrial and experimental botany of his period, together with that very considerable vocabulary of botanical terms which then formed a part of the Greek language, he would still have done us an inestimable service; though masters of the science in the seventeenth, eighteenth, and nineteenth centuries of our era would not have styled him Father of Botany.

In our study of this maker of the first Landmark in the History of Botany the main object must be that of discovering in what ways, under what limitations, and yet how well, he accomplished the placing of knowledge of plant life and form upon the list of the sciences.

Method. That a treatise be recognizable as scientific it must be methodical. It is even a necessary characteristic of it. There must be a principle, or a set of principles according to which the facts or propositions find an orderly arrangement. This does not, however, imply that the method or system be of some particular kind, as, for example, that in botany one should be required to arrange the matter of one's treatise according to what are conceived to be the natural affinities between plant and plant. The author who writes botany from the industrial point of view, if he so elect, may discuss his plants in the order of their relationship in families, genera, and species; or, ignoring taxonomy of that sort, he may arrange them according to the nature of their serviceability in household economy and what are called the useful arts; may discuss in successive chapters food plants, drug plants, textile plants, vegetable dye-stuffs; trees, as supplying timber, fuel, oils, gums, sugars, resins, nuts, etc. That botanical matter so arranged may be scientific can not successfully be controverted.

Theophrastus of Eresus might have adopted such a method as this last. He was abundantly capable of discussing the plant world from the economic and utilitarian standpoint; indeed, had he so arranged the substance of his work he would but have been following established precedent. Every treatise on plants which was extant in his day was of the nature of agricultural, horticultural, or medical botany. There was not yet any other method of arrangement for botanical writing but the economical. Furthermore, his own chapters everywhere abound in references to the qualities of plants, and their uses in the economy of human life; though such references are commonly supplementary to the statement of other and different considerations. The very title of his work, *History of Plants* —in more idiomatic English, *The Story of the Plants*—seems to look toward an investigation of this realm of nature for its own sake, the vegetable kingdom thought of philosophically rather than industrially. No work was yet in existence, unless one by Aristotle which has been lost, in which plant organ was discussed in relation to plant organ, and the kinds of plants in relation to other kinds. The prevailing attitude of mind respecting the plant world was not such as would tend to the encouragement of other than utilitarian views of it. That these objects were brought into existence only with reference to man, and for his use and benefit, was even a part of the religious belief. And so deeply seated and generally prevailing was this sentiment that it is said the students of Aristotle and of Theophrastus became objects of ridicule with some of the literaries, poets, satirists of the time, because of their going about the country picking up and curiously peering into the least little things of nature, such as were of no possible use. And one may not attribute to antiquity alone these prejudices against philosophic nature study; for they rule the mind of untold millions even now. Antiquity, in this phase of it, is with us still, in the ideas of the uncivilized races, and also in rural districts of the lands of the enlightened. Not many a botanical traveller and explorer along the frontiers and in the remoter country sides has failed to be accosted with friendly queries respecting plants of which he has been seen to be making specimens: What use has this plant? What is that kind good for? And what betrays in these good-naturedly inquisitive rustics their complete subjection to the pre-Theophrastan utilitarian botany is, that when the man of science answers frankly that he knows no use whatever for the plants in question, he is not believed, but is silently credited with wishing, for his own pecuniary advantage, to keep their use a secret. The adherents of this archaic philosophy of the vegetable kingdom are, I say, doubtless numbered by thousands on all the continents; people who have not heard of any other; and we have no proof that another had been any more than merely suggested before Theophrastus.

In the Second Chapter of the philosopher's First Book there is presented the following list of the external and obvious organs of a highly organized plant, *i.e.*, a tree: root, stem, branch, bud, leaf, flower, fruit.[1] I recall that upon first reading these initial chapters of Theophrastan botany I was quite startled to find here this complete and faultless statement of the external organs of a tree or shrub; to be confronted with it even here, and be made to realize that it is so very, very old; that our own masters and tutors of a few years ago did not invent it, neither their own immediate

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[1] *Hist.*, Book i, ch. 2.

botanical ancestry, but that all so-called modern botany has this fundamental of plant-morphology from old Theophrastus, and all unawares. But the surprise passes. The sentence is so simple, so natural, so logical, the connection between term and term so perfect, that one doubts that it could have been done otherwise; and may be disposed to say to himself that any botanist of whatever epoch might have gone out on a morning walk, looked at a number of different kinds of growths, come in and written down that methodical proposition offhand. It is extremely improbable that it was so done. It is next to certain that it cost its author a great deal of careful observation and prolonged study and reflection. The sentence is studiedly methodical, and no less a dictum of science than a work of art. It is easy to forget that, as in art, the production from which all evidence of the artist's anxious care and hard work has been eliminated is the masterpiece, so in science and philosophy the axiom or the aphorism which when finished reads as easily, smoothly, and convincingly as if every one always must have seen its transparent truthfulness, and as if almost any novice in that same science might have written it down in just those words—that this is the little sentence which may have cost its author the expenditure of time and mental energy with which he might have written a whole volume upon some topic that was not difficult.

If the origin of the simplest elements of universal botany is to be shown, this Theophrastan list of plant organs will have to be looked into rather particularly; will need to be studied with great care and caution. Those six or seven important terms, as our philosopher links them together, constitute the most classic piece of elementary botany in existence. The sentence has also much to reveal about the author's botanical method in general.

First of all, the terms of the sentence, root, stem, leaf, bud, etc., have not been created or invented by Theophrastus. As the names of those things they are part of the common vocabulary; botanical terms, assuredly, yet in their framing and use doubtless far antedating all written botany. That our first philosopher of plant life, he who first brought the terms together and placed them in line, altered the meaning of certain of them by giving them a more comprehensive or else a more restricted application, is easily possible; though that does not here concern us. We have but to note that this fine equipment of most fundamental botanic terms, the first botanist—as we must denominate him despite the suggestion of the paradoxical—found ready made. They and other such terms had been the legacy to human speech made by a primeval race of nature-ponderers, almost infinitely remote and prehistoric even in Theophrastus' time.

Observe now, that while root, stem, bud, leaves, flowers, etc., are familiar and obvious parts of a tree, they are far enough from being a complete list of such obvious parts. Viewed at close range the bark is very conspicuous; more so than either the buds or flowers in the majority of arboreal and arborescent growths; and since he has here left it out, one is obliged to think that this point was well inquired into by him; that the omission was deliberate, and the result of sound reasoning. We shall find proof by and by that he investigated the inner structure of trees; and there, among the anatomical parts of the tree as they disclose themselves in the cross-section of its trunk or stem, we shall find him cataloguing the bark. To separate organography into the two divisions, morphological and anatomical, is, then, also classic. It is another part of universally approved botanical method which originated with and was established by Theophrastus.

With that Theophrastan list of organs under consideration, modern botany at the very outset divides it into two parts, labeling one division of them the vegetative organs, the other the reproductive. The Greek has incidentally given us to know that he, too, pondered very seriously indeed the question of a natural division of his series, and that he effected one. It is as far as possible from corresponding to our modern classification of the same organs, and must needs have been so; because the only reproductive organs of plants known to Theophrastus were seeds and buds. Of the sexual organism of the flower he had no information. He was without a microscope. His dividing line between the two classes of organs is drawn, not as with us toward the upper end of the series, but near the middle of it. Root, stem, branch, bud, form the first division; and the perfect naturalness of it may be realized by observing that precisely those organs, and no more, are what one enumerates as constituting deciduous woody plants in their winter condition. No leaf, no flower, no fruit is there; yet the organism as it stands betrays no imperfection. From its deepest rootlets to its remotest twigs and scaly buds it is alive, in health, perfectly normal in every particular. What is more, every such tree and shrub on the face of the earth passes half the period of its life in just that condition, no difference whether that life period be fifteen years or fifteen hundred. It is a classifying of the external organs of a plant as permanent and transient; a very remarkable division of the series, as true to nature as any classification of organs that ever has been proposed anywhere by any man, and yet, as I suppose, entirely peculiar to Theophrastus' method. How thoroughly natural and scientific this proposition is, may be further shown; as also how studiously the learned Greek nature student reached his conclusion about it. It is not the deciduous tree only whose different organs fall into the two classes of the permanent and the transient. He has discovered that the evergreen trees, like the rest, acquire one set of new leaves each year, and as unvaryingly lose an old set[1]; so that their perpetual verdure is due to this only, that each set of leaves remains on the tree during two or more seasons. It is clear to his mind that if leaves, flowers, and fruits are to be catalogued as plant organs at all, the division of the whole series into constant organs and inconstant must be maintained. But he is even perplexed with a question of whether those inconstant and scarcely more than occasional parts are to be listed as plant organs at all[2]; a position which most twentieth-century readers will think very singular and strange. But must there not also have been with him a time of doubt as to the placement of certain other very common and external parts of plants? Such things as prickles, spines, thorns, tendrils, excrescences of several kinds which imitate fruits but are not—it was by no accidental oversight that these were omitted from his catalogue of plant organs. Since in the phytographic and taxonomic parts of his writing he evinces his perfect familiarity with them, it becomes certain that he surveyed with his wonted carefulness the ground of their possible right to enumeration among these other organs, and that he deliberately ruled them out. The ground of his doubt concerning leaf, flower, and fruit as admissible into the line he states fully. Being himself first a zoologist, then a botanist, and always interested in making comparisons between these two kingdoms of nature, he is aware that the fœtus of a gravid animal is no part or organ of that animal. The fruit of plants, being analogous to the animal fœtus, should be denied any place in the list of plant organs. To make this part of the Theophrastan argument quite clear to the reader, it will be needful to anticipate our study of his anthology in so far as to say that the conception of ovary and ovules as being parts of the flower is one that never entered into the mind of Theophrastus. With him those first small rudiments, as they appear still encircled by

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[1] *Hist.*, Book i, ch. 15.

[2] *Hist.*, Book i, ch. 1.

or concealed within their colored floral leaves, are the fruit. From this incipient stage, in which, as I say, we call them parts of the flower, the Greek held them to be the fruit, and so forward to their fuller development and final maturity, always the fruit. Whether, by the way, his doctrine of the fruit or ours is the more natural, the more logical, and the less forced and arbitrary, he may determine who may divest his mind of its every prejudgment about the case. In the light of this explanation it will be easy to see that, if the fruit be denied by Theophrastus its place in the list of plant organs, the flower, that is what we call corolla, is equally disqualified, partly on account of its intimate connection with the fruit, and partly on the score of its exceeding transiency in all cases. Furthermore, because in the thought of the Greek the flower itself was but a circle of leaves, different from the ordinary foliage only as to form and coloring, if the floral leaves must fall short of mention in the list of important organs, the green leaves must remain with them. So the catalogue would begin and end thus: root, stem, branch, bud. Indeed, his first presentation of it is in this abbreviated form. Now against his own argument for the exclusion of such parts from the list, he presents such reflections as the following. It is only when trees stand vested in their full foliage, flowers, and fruits, that they seem to have reached their fullness of beauty and perfection. That which makes for the perfection of an organism should apparently be accounted a part of that organism. And as for permanency, there are exceptional cases among animals in which certain parts are transitory. Fowls periodically shed their feathers, and stags their horns; and his last observation here is that animals and plants are in many ways so very different in their constitution that arguments from analogy must not be pressed too far. And so, after much observation and astute reasoning upon the subject, he convinces himself that leaf, flower, and fruit are entitled to places in the list of plant organs, where nevertheless they by their nature form themselves into a separate division. One can not but admire this piece of Theophrastan method, wrought out originally and laboriously by himself, and so unique; in its deepest meaning fairly amounting to a division of the organs as vegetative and reproductive, and drawing the line between bud and leaf—mistakenly, of course—instead of between leaf and flower.

There is another mark of deep study in the making of this list. It lies there, rather well concealed from our first glance, in that elegant sequence according to which the names of the organs are placed: root, stem, leaf, flower, fruit. Its mere orderliness pleases one, even captivates the mind to that degree that one readily believes that it is of both natural and logical necessity, and that any single modification of that arrangement would make the whole unmethodical and altogether bad. To begin at one pole of the plant axis and proceed thence without a break to the opposite pole is artistic, and therefore satisfies our æsthetic fancies. It may also be that such procession of names of plant organs is called for by scientific principle; and since our protobotanist was keenly in quest of principles and a little indifferent to matters of phraseology, we must inquire into his scientific reason for writing the root first on the list of plant organs, rather than the stem.

We have already seen that primevally the root received more attention morphologically than any other organ of the series[1]; that early descriptive botany is rather anomalous in this particular, and that all this came to pass through the influence of root-gatherers and their patrons the physicians and the pharmacists. Such a merely economic and commercial consideration as that which influenced men of the time in their descriptions of roots can not be supposed to have the least effect upon the mind of Theophrastus at this particular juncture, where he is engaged upon a study that is in nature purely scientific, biologic. No botanist has lived in any age of the world more capable of distinguishing between the economic and the biologic in nature study. Now in those parts of his work which are descriptive, written for the purpose of enabling the reader to identify the plant, his sequence of the organs is different. It is now stem, leaf, flower, fruit, root; another not unnoteworthy item of Theophrastan method; one sequence for the treatment of organs phytographically, and another sequence for the discussion of them biologically. The former is well suited to its purpose; for, to the great majority of observers, nothing is seen of any plant or tree but its stem, foliage, flower, and fruit; and any reader would be discouraged if not repelled by a description beginning with a full account of the root, about which part he neither knows nor particularly cares to know anything. As to the other and biological sequence, it is evident that the philosopher arrived at it only after careful and prolonged investigation. "In all plants the growth of the root precedes that of the superior parts."[2] The allusion is to young plants, whether growing from seeds or from cuttings; which latter means of propagation was

[1] Page 45 preceding.

[2] *Hist.*, Book i, ch. 11.

only less extensively resorted to by the ancient Greeks than it is by ourselves; and they knew as well as we that when a cutting set into the ground has remained for weeks and maybe months without having been increased by so much as one leaf, that only means that it is slow in forming its roots, and that until these are somewhat grown, other growth will not begin. It is improbable that this kind of fact was not of common knowledge with gardeners and orchardmen ages before Theophrastus; so that all he had to do was to verify it by experiment. But when he states that even within the seed it is the radicle which first begins to swell and grow, and that this is invariably the first part of the growing seed to appear outside the shell,[1] we feel assured that these are fruits of observation on the part of no gardener, but of a biologist.

In the year 1672 of our era Nehemiah Grew, aided by a microscope (!), had repeated those investigations of the seed as germinating, and published what was but a confirmation of the view that Theophrastus had presented some nineteen hundred years before, namely, that biologically considered the root is first among plant organs, then the stem, and after them, leaf, flower, and fruit.

One must now leave the Theophrastan method in merely elementary organography, and survey briefly the outlines of his dealing with plant as compared with plant.

The philosopher has several different viewpoints from each of which he perceives the vegetable kingdom as a whole to be divisible into two parts. The first of these divisions is according to texture and duration of stem and root; a chapter in botany which he wrote for all time; the distinction between woody plants and herbaceous. Trees and shrubs, alike as to their woodiness, are distinguished by him precisely as in the most recent botany, not neglecting the word of caution that no hard and fast line separates the two; that many, like the filbert and the pomegranate, are naturally shrubs, the stems growing in clumps, and are seen in the form and dimensions of trees only when under the cultivator's care and art. Even the pear tree, olive, and fig when left to themselves become many-stemmed and shrub-like. He also apprizes the reader that certain pot-herbs of the gardens have always the one-stemmed and arboreal mode of growth, even approaching trees in their dimensions, and all within the time of a few months; but that these have not the duration of trees or even of shrubs, and therefore prove that they are neither. He mentions certain mallow and cabbage

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[1] *Hist.*, Book viii, ch. 2.

kinds as of this category.[1] He denominates them δενδρολάχανα, which his Latin translators have rendered olerarbores, and would appear in English as tree-potherbs. All herbaceous plants he classifies as perennial, biennial, and annual; also carefully stating that the annual and biennial are hardly distinguishable, inasmuch as both die, root and branch, within the space of a year, and as soon as they have once perfected seeds.

While this important example of botanical method is in hand, it will be pertinent to take note of those two mental processes, analysis and synthesis, of which every piece of method is the outcome; this for the purpose of clearing our own mental vision for an inquiry into the question of how much of this classifying according to texture and duration was found by Theophrastus ready-made and in common use, and to what extent, if at all, he revised, augmented, and improved it. Problems of this kind are most difficult; even impossible of exact solution by the mere botanist. The erudition of the specialist in philology and the history of language is here called for, without the aid of which the early history of botany never can be written. Happily, however, great linguistic learning is not requisite to a few reasonable inferences respecting Theophrastus' part in this classic piece of method. The distinguishing between woody growths and herbaceous is doubtless older than history. It is also evident that with remote enlightened antiquity tree and shrub were distinguished. The words representing these ideas are very ancient; but the half-shrub, or suffrutescent growth as we of to-day speak of it, appears to have been set apart as a group, and assigned a distinctive name by Theophrastus himself. There are many old and classic names for plants distinctively herbaceous. There is weed, grass, herb, vegetable, even the word "plant" itself as originally used, every one implying the herbaceous as to texture and short-life period; and so much for very primitive analysis and synthesis; but the putting of all these things together, the synthesis of them under one comprehensive term, was, if I mistake not, a Theophrastan contribution to botanical method. Moreover, and what is of even more profound interest, it appears as if the synthesis of everything that vegetates —tree, shrub, half-shrub, and herbaceous plants, including even sea-weeds and fungi—into one vast comprehensive assemblage of living entities called plants, is also to be attributed, if I mistake not, to Theophrastus. Primeval and prehistoric observations

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[1] *Hist.*, Book i, ch. 5.

and reasonings, I say, doubtless established and named such lesser groups as tree, perhaps bush, certainly garden vegetable and field and garden weed, and grass and reed, rush and mushroom; but the synthesis of all as in some way alike and interrelated, and as spoken of comprehensively under one word—that would seem to have awaited the advent of a master mind like that of Aristotle. The written record of so significant a piece of method in nature study I find for the first time in Aristotle's greatest botanical disciple. The fuller investigation of this topic may be deferred until we come to Theophrastus as the founder of Botanical Terminology.

From another viewpoint Theophrastus beheld the world of plants as divisible into the cultivated and the wild[1]; and he formally approves this line of separation, though almost as compelled by circumstances; for he admits that it is not natural, and that the differences are in the main such as result from cultivation. It presents, nevertheless, a forcible example of rude primitive plant classifying. Untaught peoples of all countries, and many all around us, hold to such a division,—and that even superstitiously; firmly believing that the wild parsnip or wild carrot—differing from its parent plant only as growing spontaneously by the wayside, rather than within the garden wall under cultivation—is a poisonous thing, perilous to the life of him who would dare to eat it. Our present nomenclature of plants, the vernacular as well as the Latin, presents countless clear vestiges of the former popularity of this antique parting of all the plant world into these two divisions. Such specific adjectives as *agrestis*, *silvestris*, *trivialis*, *arvensis*, *pratensis*, *hortensis*, *sativus*, *urbanus*, and many more tell of a time past when about the first question concerning any plant was, whether it was wild or cultivated. There is no need of citing examples of those hundreds of vernacular plant names the first term of which is "wild"; but all of them, as relics of pristine botanical ages, attest the once universal prevalence of this partitioning of all things that grow out of the ground, into the two groups of the cultivated and the wild.

As for Theophrastus, out of the some 500 species and varieties of plants of which he treats, only an insignificant proportion are other than domesticated; and he says that the uncultivated things of wildwood and mountain are mostly still unknown and have no names. To have assigned space in his book for the consideration of many wild plants must have appeared like a marked innovation;

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[1] *Hist.*, Book i, ch. 6.

one that indeed looked in the direction of a widening of the field of botanical investigation, and was therefore of promise for the future of the science; but it must have given occasion for carping critics to ask how wild plants, such as have neither names nor history, are entitled to a place in the History of Plants. But while this newly suggested distinction had within itself one element that would eventually accomplish its obliteration, Theophrastus did not perceive this. Among wild trees and other growths which it had been sought to introduce into gardens, there were some which had baffled every effort to transfer them, whether by root or seed. From this he reasoned that there existed a line of natural demarcation, at least between plants that were susceptible of domestication and such as were not. But some trees which he named as apparently impossible of domestication are now successfully cultivated.

Having held the status of an acceptable part of botanical method for a millennium and a half, at the revival of botany in the first part of the sixteenth century this distinction began to decline in popular favor, and within two centuries more it became so nearly obsolete that, in books descriptive of the plants of particular regions or districts, those of field and garden were wholly omitted. Only wild plants were now taken note of; and so an extreme squarely opposite to that of Theophrastan times had been reached. And what lends deeper interest to these observations now, is the circumstance that of late years there has been awakened the keenest passion for the study of cultivated plants that history has known. I speak, of course, of that purely philosophic and scientific investigation of them which, either together with or apart from the industrial in motive, engages the attention of many botanists. And here, realizing that the very father of written botany was chiefly attentive to domesticated growths, and upon these as principal subjects wrought out his scientific system, one wonders whether or not in botany the first cycle of its history is being completed.

From yet another outlook over the vegetable kingdom as a whole, all the subjects thereof range themselves under the two assemblages of the "flowering" and the "flowerless". Theophrastus records this; but assuredly the invention of such a division cannot be ascribed to him. It must have formed a part of the universal prehistoric botany. Never since human intelligence came into the world, and lived in converse with nature, can people have failed to remark the presence here, and the absence there, of those differently shaped, differently colored, and singularly congested foliar parts that they call the flower. It must have been observed immemorially, and accepted as a nature-taught fact, that such and such trees, weeds, and other plants never display flowers. They are flowerless. What I have been careful to state, namely, that the primitive notion of a flower is that it consists of leaves, must be insisted on. The reader must not permit himself to think of the pristine idea of flowerless plants as embracing anything less than the cryptogams plus the whole body of the apetalous phanerogams. With this fixed in our minds, we are then ready to inquire whether or not Theophrastus made any improvements of his own upon this part of pristine botanic method; whether perchance he looked into the matter far enough to have discovered that there exist such things as flowers destitute of flower leaves, and thereupon enlarged the boundaries of the flowering plants and correspondingly restricted those of the flowerless group. The answer must await our study of Theophrastan anthology.

In close connection with his separating between the flowering and the flowerless, the Greek divides the whole plant world again into the two categories of the "fruit-bearing" and the "sterile".[1] Judged by certain criteria that are of comparatively recent adoption there would be no call for this last distinction; for the flowering and the fructiferous would exactly correspond to each other, as would also the flowerless and the sterile; so that this last seeming distinction would be but a different naming of two primary groups before indicated. But this is not true; for when we have learned his doctrine of the flower we shall perceive that he had in mind fructiferous plants, and trees even, the flowers of which he had been unable to detect and which therefore of logical necessity he must classify as flowerless. Also, since he knew nothing of such sexual distinctions as have their ground in floral structure, there presented themselves to him what we of to-day know as the males of certain diœcious plants, which flowered freely, and yet were sterile invariably. There was, then, no correspondence at all between those two items of his method as he saw and indicated them.

As regards the scientific merits of these two groupings our estimate must be formed according to their correspondence with the facts known at the time the groups were proposed, and not by bringing, for example, Theophrastus' groups of "flowering" and "flowerless" abruptly into contrast with those groups as they stand

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[1] I have inverted Theophrastus' order here. He places fruit-bearing and sterile before flowering and flowerless.

in recent botany under those same names. Such a mistake as this the historian Meyer[1] must have made when he pronounced the Theophrastan distinguishing between "flowering" and "flowerless" to be "of little value." Its worth or worthlessness should have been determined—in the judgment of a historian, at all events—by the measure of its answerability to the state of knowledge actual at the time of its promulgation, and also with reference to its usefulness as an incentive to further inquiry into the nature of plants as flowering and flowerless. Or, even as the historic starting point in the designation of two groups, in name at least equivalent to Phanerogams and Cryptogams, this Theophrastan and very suggestive expression is of deep interest, and of no small value.

These mere outlines of his general method must be concluded with the bare mention of some other aspects of the vegetable kingdom as it presented itself to the comprehensive and deeply thoughtful mind of the protobotanist. He discusses, and in much more than mere outline, meteorology and climatology in relation to plant life; has chapter after chapter upon ecology and geographic distribution, and even touches more than lightly the topics of plant pathology and the transmutation of species.

Vegetative Organography. Theophrastus begins his botany at the beginning. The remark is pertinent; for with recent writers it is no uncommon practice to begin somewhere toward the middle of the subject, leaving the foundations of the science out of sight. It is an easy way, avoiding as it does the difficulty and the responsibility of laying down first principles. To recognize three separate realms of nature seems necessary; yet to indicate clearly the boundary lines between them is confessedly anything but easy. Theophrastus at the outset acknowledged the difficulty of distinguishing universally between the vegetable and animal kingdoms, but he faced it. There was no evasion. He addresses himself at once to the task of defining the plant as differing from the animal. And first he presents what must have been the popularly accepted method of indicating the distinction, though only to show its insufficiency for the purposes of science. Animals of whatever description have at least a mouth and a stomach. But it can not be said that all plants have roots, stems, branches, buds, leaves, or fruits. To circumscribe the plant world by listing the common organs of plants may enforce the exclusion of many things which are neither of the animal kingdom nor of the mineral, and are there-

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[1] Meyer, *Geschichte der Botanik*, vol. i, 162.

fore plants. Here it should be remarked that well toward the beginning of the seventeenth century there were people—among these at least one very eminent botanical author—who did not allow the fungi to be classed as plants at all. Theophrastus knew all these lower forms of vegetable life, fungi both terrestrial and hypogeous, lichens and algæ in abundance, and proclaimed it unhesitatingly that they are plants; plants indeed without stem or leaf or root or seed. "The phases of plant life are so exceedingly diverse in nature and constitution that, to give a general (*i. e.*, morphological) definition of a plant, and that in few words, is not possible,"[1] he says. He ventures, however, one distinctive peculiarity of plants. "They are not, like animals, endowed with ethical susceptibilities and the power of voluntary action."[2] This is metaphysical, indeed, yet no whit more so than that which Linnæus gave in the middle of the eighteenth century when, pressed for one single mark of distinction between the genus Homo as separate from those anthropoid mammals next in rank, he was compelled to cite the "Nosce teipsum"; that is, man's consciousness of his own existence. Between animal and vegetable kingdoms the man of the eighteenth century could offer no better distinction than this: "Plants grow and live. Animals grow, live, and feel."[3] But he of twenty centuries earlier had been more circumspect and cautious. Such movements as heliotropism and nyctotropism, and others seemingly akin to nervous irritability, were not unrecognized by Theophrastus, and may well have seemed to indicate something too closely allied to feeling.

In defining the manifest organs of higher plants the philosopher proceeds with like caution, preferring physiological to morphological characteristics; in this, the first precursor of the modern biologist, who, if required to name one distinction between plant and animal, confines himself to a point in physiology.

The root, Theophrastus says, is that by which aliment is taken up[4]; not a satisfactory definition to us moderns who demand morphological distinctions. Yet very safe is Theophrastus in his reserve; for what he names as characteristic of roots, albeit a merely functional characteristic, is one that holds. It is also apparently a new definition, framed by himself, and intended to

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[1] *Hist.*, Book i, ch. 2.

[2] *Hist.*, Book i, ch. 1. I trust I may not be found to have rendered too freely the words ἤθη and πράξεις.

[3] *Syst. Nat.*, 4th ed., p. 3.

[4] *Hist.*, Book i, ch. 2.

displace another definition which, though time-honored, was fallacious. That antique fallacious definition of a root has not yet been universally displaced. Without going quite out of sight of some of our colleges one may elicit that same old characterization of the root, and from the voices of people who have a certain familiarity with this particular plant organ. Multitudes in every land under the sun to-day will answer promptly that the root is the underground part of a plant. The Lesbian boy, while yet untaught by either Plato or Aristotle, it is most likely would have given this same answer. In maturer years, after much careful questioning of nature, he has found that that popular definition of a root does not hold good. The shrub or tree which the Greeks call Helix, the common ivy, he has observed minutely and experimented with until he has established it that those threads by which it climbs rocks, walls, and tree-trunks have nothing of the nature of tendrils, but are perfect roots, exercising in some degree even the usual function of roots.[1] He knows quite as intimately the shrub Ixos, the mistletoe, and that its seeds refuse to so much as sprout elsewhere than on the bark of living trees, into which bark it strikes its roots.[2] He had not seen the banyan tree of the Indies; but there were Greeks, educated Greeks, who had both seen and described it, with its many lesser subsidiary trunks grouped around the large central and original one. In their descriptions the travellers had stated so definitely the origin of the accessory trunks as starting out from main branches and growing straight downwards, that Theophrastus without hesitating declared that such things, however trunk-like they may at last appear, are roots.[3] Thus did the first master of organology completely invalidate the ancient world's definition of the root, and at the same time indicate with clearness the two categories of roots, subterranean and aërial. He did not, however, name them. As nomenclator of even his own most brilliant discoveries he was usually delinquent. We seem to read it between his lines that there were in his mind some suggestions of root characteristics which, had he been less cautious than he was, he might have added to that very reserved and merely physiological definition. The downward growth from the branches, in case of the banyan, and also the statement that those pendents at first and while young and tender are of a light color and hairy(!)—both these

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[1] *Hist.*, Book iii, ch. 18.

[2] *De Causis Plantarum*, Book ii, ch. 23.

[3] *Hist.*, Book iv, ch. 5.

reported facts influenced his decision that these were roots. We shall also see presently that he had noted other morphologic marks, but such as he did not think universal.

The roots of herbaceous growths he in general classifies as ligneous, fibrous, and fleshy; and these include many things recognized in recent botany as subterranean stems, that is bulbs, corms, tubers, and the more thick and fleshy forms of root stock or rhizome. All really fleshy underground parts he distinguishes again as vertically elongated, spherical, depressed-globose, and what he calls nut-like; this last class embracing such incongruities as the small and solitary shell-covered corms of the crocus, and those tuberiform enlargements that appear, as if strung like nuts, on the roots of the asphodel. The rhizomes of arundo, and of what he names as arundinaceous plants in general, and which he remarks are sometimes partly above ground, he denominates jointed roots, but notices that these all have fibrous roots attached to them Others, like the bulbs of squill and onion, are composed of a multiplicity of scales or tunics which can be removed one by one; so that these differ from other fleshy roots in that they exhibit two different kinds. Their nature is so peculiar that one might be excused for doubting that they are roots at all; for if in that they are subterranean they would at first seem to be such, they are in other particulars of quite another nature; because roots properly so-called diminish in size toward their lower extremity and end there acutely, whereas these bulbs and their like are widest at base and grow smaller in the opposite direction; moreover those fibres which descend from the bases of some and from the sides of others are the real roots which take up aliment; but the extuberant part is more like a fœtus, or a fruit.[1] However, after still further discussion, he seems to rest in the conclusion that, as roots are of various kinds, and even bulbs and other fleshy roots are functionally much alike, all may well enough be continued under the category of roots. If to any botanists of the twentieth century it may seem a strange thing that the Greek, having distinguished between roots as subterranean and aerial, should have failed—and after all his study of them—to classify stems also as aerial and subterranean, let them recall to mind that philosophic conservatism which led Theophrastus to make more of the function of an organ than of its form; that he was sure that corm and rhizome and tunicated bulb attract nutriment and are by that token roots.

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[1] *Hist.*, Book i, ch. 10.

Let them also contemplate the fact that for twenty hundred years and more all botanists accepted the decision of Theophrastus, and that even with Linnæus, those organs which we have somewhat recently been learning to regard as subterranean stems were nothing but roots; and that Linnæus in this particular was even so far back of Theophrastus that he had no doubts about their being roots.

That in the most primitive phytography roots received almost singularly minute attention has been adverted to, and the probable reason assigned. Accordingly we shall find Theophrastus, when done with their morphology, anatomy, and physiology, giving a full account of their differences as to color, odor, flavor, and their qualities as wholesome or deleterious. In color, some are white, others black, not a few yellow, some tinged with red, and some quite intensely red. As to odors and flavors there is again much diversity; and some that are sweet and pleasant to the taste are deadly poisonous, while several kinds that are of disagreeable odor or bitter are harmless, and even of medicinal value.

At the correct definition of a stem as being that part of a plant which bears leaves, Theophrastus did not arrive. His imperfect conception of the leaf, for one thing, stood in the way. Those merely scale-like short leaves, upright and even appressed to the stem, such as those of asparagus and orobanche, were mere scales in his view of them, and the stems of such plants he considered leafless. Again, to his vision there was a horde of stemless plants the leaves of which arise not from any stem at all, but directly from the roots. Here I can not forbear remarking that we of to-day, despite our better characterization of the stem, and our recognition of it as present in all except the very lowest plants, yet contradict our own definition in our practice, and have fallen back upon that of Theophrastus whenever we speak or write, as we freely do, about acaulescent plants and radical leaves. The ancient author defines the stem primarlly as that part which is the main vehicle of aliment to the other parts; adding that it rises up singly from the ground[1]; which is of course to distinguish it from the branches and leaf-stalks, both of which he knew to be also channels for aliment. This definition, equally with that of the root, evinces his distrust of morphological characteristics as definitive, and his feeling that the physiological are safer. But, the stem once defined functionally, he proceeds with care and skill

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[1] *Hist.*, Book i, ch. 2.

to indicate its many morphological diversities; and here not only classifies but also names several of the classes. He acts as if he thought the language of science would be defective if such dissimilar things as an oak log, a mullein stalk, and a rye straw all were to bear the name of stem. In practice, therefore, he called the stem of a tree its trunk, *στέλεχος*, the light hollow or pithy stem of all grassy and reedy things was a culm, *κάλαμος*; thus the ordinary word stem or stalk, *καυλός*, was mostly limited to designate those of what we know as herbaceous exogens; and so our modern botany has these three kinds of stems as designated by Theophrastus. Culm is even very manifestly a modification of the Greek kalamos. Embracing as it does almost all endogenous stems, it is more comprehensive than the English words straw, reed, and rush all combined; and in our botany we were obliged to borrow and make over just the Greek term which Theophrastus invented—unless he, too, borrowed it; or, what amounts to the same, extended the use and gave a new and scientific meaning to an old and familiar term. As to their forms and modes of growth he distinguishes many kinds of stem among herbaceous plants. And, as woody growths are classified as trees and shrubs, according as their trunks are one or several from each root, so the herbaceous are distinguished as one-stemmed or many-stemmed. The numerous kinds of bulbous plants both wild and cultivated he understands as being invariably one-stemmed, and therefore does not speak of this in his descriptions of such; but of other herbaceous growths his custom is to mention, in his descriptions of them, whether the root sends up a single stem or many. It is a distinction of importance to phytography; and if the anthological extremists of one and two centuries ago thought it superfluous, and neglected it, its value is now again beginning to be clearly seen and freely admitted. Again, herbaceous stems are upright, or reclining, trailing, climbing, or twining.[1] He also has observed that one-stemmed herbs are apt to be erect, the many-stemmed otherwise; or at least that the reclining or trailing are always many-stemmed. Among upright stems he perceives how different those of the umbellifers are from most others in that they are fluted, or at least striate, and, giving these and all their like a name that really points to their anatomical structure rather than to their external appearance, he denominates all such plants nervose-stemmed. Through having missed the discovery of the

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[1] *Hist.*, Book vii, ch. 8.

good morphologic mark of stems, he is constrained to offer the category of what he calls smooth-stemmed plants. Such "stems" are nothing more or less than the flower-stalks of acaulescent perennials, called smooth stems because devoid of those unevennesses now designated as the nodes and internodes, and received as the most universal mark of real stems. His examples of the smooth stem are those of the onion and leek,[1] good illustrations of the scape, as named and defined in later organography.

The leaf is a thing so almost infinitely diversified that he does not attempt to characterize it morphologically, or even physiologically, for he can not with any degree of certainty name its chief function. So without vouchsafing any definition of it, he goes about the rehearsal of its many aspects. Although as an organ it heads the list of that division of them which he has distinguished as transitory, and therefore in a manner secondary, no part of a plant would seem more deeply to have interested him, or to have been more carefully observed. He is even somewhat diffuse in his writing upon it; more so than in the case of any other organ, unless the fruit and seed are to be excepted; and, since the opinion now prevails almost too widely that little was done in the direction of plant organography until within the last two centuries, the interests of truth can not at just this point be better subserved than by giving the substance of this ancient Greek's morphology of the leaf somewhat in detail.

"Leaves are commonly attached to the stem or branch or to whatever else supports them, by a stalklet; this either firm and holding the leaf steadily in a certain position, or else slender and feeble, allowing the leaf to hang downward and perhaps tremble with the passing breeze, or even to become inverted, turning the usually paler lower face upward. But there are also leaves with no stalklet, these adhering directly to the branch. Some leaves arrange themselves only in opposite pairs, with regular intervals between the pairs, while others are scattered singly and without order up and down the stem."[2]

It will be seen that these beginnings of Theophrastan teaching about the leaf are precisely what one finds in every primer of botany to-day. Every beginner has to be taught the importance of the distinctions between a petiolate leaf and one that is sessile, and between the opposite and the alternate in leaf arrangement.

[1] *Hist.*, Book i, ch. 16; also Book vii, ch. 7.

[2] Excerpts from *Hist.*, Book i, ch. 16.

We have improved the descriptive phraseology, and are able to say the same things in fewer words; but that is about the only difference. Some general differences in the configuration of leaves are also adduced. There are those of rounded periphery, or even somewhat elongated, all without angles, and there are the angular in outline, some of them like those of the fig deeply cleft, others like those of the oak sinuated all around, still others with saw-like teeth all around; and some are sharply pointed at the apex, the slender leaves of pine and others even ending in a prickle. In certain thistles he notes that spines take the place of foliage. He is convinced that certain leaf-like organs in a number of asparagus allies are not leaves, yet he gives them no name; nor had they obtained a name—that of cladodes—even as late as the time of Linnæus, who, as if he had been of a pre-Theophrastan age, still called them leaves. The hollow and fistulous foliage of the onion and some of its kindred elicited remark from Theophrastus as being very exceptionally curious. So did that of the sedges, as being conduplicate and keeled. The essential characteristics of the leaf that is pinnately compound he also seems first to have detected; for he is at the trouble to argue the case before those who, as he seems to acknowledge that he himself also once did, regard this as a leafy branch. He has observed the autumnal falling of the foliage in the ash tree, elder, and sorbus, and reports that the whole of that which seems a branch falls away piece by piece, thus establishing it beyond dispute that the whole is one leaf. He even speaks of it, afterwards, as the pinnate leaf.[1] And, as there are kinds of tree in which leafy branches, by being somewhat lengthened and having their leaves set closely in two opposite ranks, resemble the compound leaf, Theophrastus in one place that I have noted cautions the student against being deceived. The case is that of the elm, the pinnately leafy twigs of which might be mistaken by the unwary for compound leaves. The observer is warned that the elm has but a simple leaf, *φύλλον ἀσχιδές*.[2] The leaves of the rose bush I do not find described in Theophrastus. These shrubs were so universally familiar that description of their foliage was needless; but that he recognized this as a pinnate foliage is evident by one comparison that he makes. Wishing to convey some notion of the leaves of an interesting tree of the Orient (*Tamarindus Indica*, Linn.), he says it is "many-leaved, after the manner of the rose bush,"[3] meaning that

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[1] *Hist.*, Book iii, chs. 11, 12, 13, and 16.

[2] *Ibid.*, ch. 14.

[3] *Ibid.*, Book iv, ch. 9.

the individual leaf of the tamarind tree is of many leaflets, like that of the rose. The philosopher devised now and then a new botanical term, but did this with reserve; and the discovery of compound leaves does not appear to have called, in his thought, for any such distinction as that of leaf and leaflet. He applies the term leaf, *φύλλον*, to the compound leaf as a whole, and to the individual leaflet indiscriminately. As regards the differences of compound leaves and the classification of them he did nothing; nor, indeed, does anything appear to have been attempted in this direction, after Theophrastus, until the time of Jung, who in the middle of the seventeenth century strongly advanced the morphology of the leaf. But Theophrastus in his environment can not have met with anything like that diversity of compound leaves with which Jung was familiar. He must have known the bipinnate fronds of certain ferns, but did not essay any description of them; and when a certain bipinnate-leaved tree from Egypt (*Mimosa polyacantha*, *Willd.*) was in need of a description, he evaded the difficulty of the situation by saying that its leaves were like those of a fern.[1]

We shall be furnished later with some proofs that the venerable Greek could make significant discoveries in connection with such very little things of the plant world as small apetalous flowers, and even the inner structure of small seeds. Without microscope, hand lens, or spectacles, he seems to have been almost microscopic-eyed sometimes, as well as always alert for the detection of the exceptional or unusual in the grosser morphology of things. We therefore wonder that, after his having noted so carefully that some leaves, even small ones, are compound and even doubly compound, he should not have taken stipules into account; for he makes no mention of them. During it may have been fifty winters he had seen the branchlets of the plane trees under which lay his daily walks, encircled at intervals by their wheel-shaped stipules still persistent after the body of the leaf had fallen. During as many summers of his centenarian career he had observed the foliage of many garden leguminosæ, in some species of which everywhere in cultivation anciently, more than half the foliar area is stipule; and yet this organ is unmentioned by Theophrastus. As to its presence on the boughs of the most common Athenian wayside shade tree, and its more conspicuous showing amid the herbage of every sort of pea and vetch and lentil, this father of plant organography is as silent as if the organ had not existed. This

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[1] *Hist.*, Book iv, ch 3.

seems anomalous. To the average reader it will seem like a curious hiatus in the Theophrastan leaf-morphology. The situation seems nevertheless readily to explain itself. One has but to recall to mind the ancient botanist's strong inclination to regard function first and form last everywhere in his organography. The excessively enlarged and leaflet-like stipules of *Pisum* and some other leguminous herbs have not the least appearance of being functionally different from the other leaflets. They are larger and also located a little differently from the others, but that is all; and there is the best of evidence that a thorough training in modern organographic refinements is requisite to the determination of the enlarged basal leaflets of the pea-vine as stipules. The evidence is this: that the most original and logical of great organographers and terminologists, Joachim Jung, as late as the year 1662 of our era knew nothing of any such organ as a stipule. Tournefort in the year 1700 knew them not, and Linnæus claims them as among his own organographic discoveries, though unwarrantedly, as we shall see later.

There is, however, one particular kind of stipule which, unless I misunderstand Theophrastus, drew his attention and elicited his comment. It was a case in which there is about the strongest possible contrast in appearance between the leaf proper and the stipular appendage; that of certain umbellifers in which while the decompound blade is deep green and almost capillarily dissected, the large stipular development below it is pale, membranaceous, wholly uncut, sometimes cup-shaped and hollow. Here again it would seem to require our modern refinement in organography to perceive in such a thin bladdery stipule and green capillarily cut blade the different parts of one and the same organ. Theophrastus, at all events, knew some umbellifers of this description, and wrote concerning one species of the genus Ferula that it puts forth at the nodes of its stem leaves and *βλαστοί*.[1] The blastoi, one from each node of the stem, have perplexed some of the botanical commentators upon the text. I think they are the stipules.

If Theophrastus does not anywhere formally define the leaf, that may have been for the reason that, not at all comprehending its function, it was not possible for him to define the organ, as he had defined root and stem, physiologically. Nevertheless he did state, in a most informal way, its very best morphological mark; that by which it is almost always readily distinguishable from a

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[1] *Hist.*, Book vi, ch. 2.

stem or any part of it. He observes that most leaves have an upper face and a lower. The upper, exposed to the sun, he notes as being of a deeper green and smoother; the lower face is paler, roughened by the greater prominence of the veins, and apt to be pubescent.[1] From the fact that the lower face of leaves when pubescent is found to be moist he says it has been inferred that it absorbs nutriment and feeds the rest of the leaf. This he confidently declares to be erroneous, affirming that all parts of a leaf are fed by way of the veins and fibres which are carried to every part, and which he knows to be connected with the bark. This was the thing which he could easily demonstrate and prove. The other proposition was not in his day demonstrable; for he could not know either the structure of a plant hair, or the existence of stomata. Theophrastus, like his class in all ages, is likely to be correct in that which he affirms, and wrong as to that which he denies.

Anthology. Without any understanding of flowers as organs of sex, and quite in the dark as to their significance in the economy of plant life, Theophrastus applied himself assiduously to the study of their morphology, and that with a measure of success compelling the admission that he is the founder of anthology; for several of his distinctions remain fundamental in the anthology of to-day, notwithstanding that the theory of the flower has been completely revolutionized within the modern period.[2] And his success is the more remarkable because of his having made his researches at a time when, for mere lack of optical equipments, the discovery of the functions of the essential floral organs was impossible.

From ages antedating all history cultivators must have observed that in such trees, garden shrubs, and herbaceous field crops as flower conspicuously, no fruit or seed develops but as an aftergrowth from the flower; that a young tree never fruits until after having for the first time flowered, and that any mischance befalling the flowers of the tree in their season extinguishes, for that year, all hope of fruit. Upon a considerable array of facts of this kind, the first philosophic investigator who came along might naturally propound such a theory as this, that wherever there is now a fruit or seed, at some time there must have been a flower; a proposition which the cultivators at once and with one voice would have disputed; for in the husbandry of antiquity no tree was more esteemed, nor any more familiarly known than the fig

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[1] *Hist.* Book i, ch 16.

[2] Namely, by Sebastian Vaillant, *De Structura Florum*, Paris, 1717.

tree, and this, as all the world understood, produced its fruits two crops a year, without a trace of flower. And Theophrastus, after all his searching for and philosophizing about flowers, seems to have found no way of controverting the universal opinion. He thought that the fig produces its fruit without flower of any description.[1] But in his philosophic quest for flowers of some sort as the forerunners of all fruits and seeds, he appears to have discovered true flowers, though sometimes recondite, in other trees that had been supposed to be like the fig, flowerless. The flower, in prehistoric thought and speech, may most reasonably be assumed to have been a thing showy on account of its being made up of leaves colored differently from ordinary foliage, and differently arranged. It must have been essentially that which modern botany knows as a corolla. This inference as to what a flower was before either botany or history began to be written is confirmed by our experience with untaught rustics and mountaineers of to-day, as to their understanding of what trees and plants have flowers and what have none. They are the modern counterpart of those unlettered ruralists of remote antiquity whom Theophrastus cites as denying that oak and walnut trees, hazel bushes and chestnut trees and junipers have any flowers at all.[2] The philosopher, the man of science who is truly such, has this among other characteristics, that with him negations are apt to go for naught. Of the populace they are largely the mental stock in trade, so to speak, but himself negations do not satisfy. They say that neither oaks nor hazel bushes have flowers. They recognize it that oaks put forth clusters of loose pendulous tassels that they call oak-moss, and also globose bodies denominated galls; but oak-moss is not a flower, any more than oak-galls are acorns. These are specimens of the facts, and of the reasonings upon them, which confronted the protobotanist of so long ago. Stimulated by the thought that almost always where a fruit or seed now is, there was once a flower, from the very heart of which the fruit or seed took its origin, he enters upon his researches. Now this very idea that flower and fruit are related as antecedent and consequent so that where any manner of fruit or seed is found the essentials of a flower must be sought, is the germinal idea out of which the whole of systematic

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[1] *Hist.* Book iii, ch. 6. There is but one record of the discovery of the flower of the fig until after the invention of magnifying lenses, and some eighteen centuries after Theophrastus. Even Linnæus, still later by two centuries, had the genus Ficus under the Cryptogamia.

[2] *Hist.*, Book iii, ch. 6.

botany as we have it has been engendered; and the originator of that idea would have been the father of botany even if he had accomplished nothing further.

There is one thing which he who would know, even in outline only and superficially, the history of botany, must not do. When in Theophrastus he meets with the word ἄνθος, or in Pliny with the term "flos," he must use care not to read into that term the meaning which the word flower has in modern botany; for, if he assume that the word stood, with those authors as with us, for a congeries of four circles of different organs, beginning with the calyx and ending with the gynœcium, he will never correctly apprehend a word they say about the flower. That view of the comprehensiveness of the flower which we now take, extremely unlike the ancient idea, was really first presented for acceptance within somewhat less than two hundred years from the date at which I write. The flos of remote antiquity, the pre-Theophrastan anthos, appears to have been simply the corolla, as we have said before; and that without a special name as such. It was but a set of leaves, shaped and colored and arranged differently from ordinary foliage, and having for its function the protecting of the future fruit and seed while in their tender and rudimentary stages.

Now oaks, walnut trees, alders, and hazels have no corollas. They had been considered flowerless because they have none, and correctly enough so long as the flower was defined as a whorl or tuft of specially altered and colored leaves; and it was so defined in the minds of the majority of people in that time, as it is in the minds of untold thousands in every land to-day. And the very possibility of detecting upon oaks and filbert bushes some small thing that should mark the point of origin and presage the coming of each nut and acorn involved the possibility of a revolution in the idea and definition of a flower; an extension of the term, to make it embrace anything, no matter how colorless, shapeless, and obscure, which should be found in the place where a flower ought to be.

This earliest *Historia Plantarum*, intensely interesting though it be as we have it, would have been still more so had its author given some record of his own processes of research; his successes and his failures in attempting to find flowers on trees and herbs that had the reputation of being flowerless. But the traditions of the lyceum at Athens were against that. Men were taught that knowledge is best communicated in language concise and brief; and Theophrastus' three short chapters of anthology may vie with any other three chapters of scientific matter ever written, in respect to the terseness and brevity with which important propositions follow one another in close succession.[1] His first proposition as to the general morphology of the flower is this: "Some flowers are capillary, like those of the grape, mulberry, and ivy; others are composed of leaves, like those of the almond, apple, pear, and plum trees."[2] These are trees; but he proceeds to say that quite the same is true of the flowers of herbaceous and annual plants, some of which have foliaceous flowers, others capillary. It is evident as can be that by examining the earliest germs of fruit in plants that never show flower-leaves, he has found those fruit-germs at a certain early period encircled by hair-like or filamentose things quite as transient as flower-leaves, and which seem in some way to take the place of them, though they have not always the usual special coloring of flower-leaves. On the strength of what he has studiously observed, he has now virtually given to the term *ἄνθος*, flower, a new definition, a scientific one. The term must embrace whatever is intimately though transiently connected with a fruit-germ, whether laminal and colored or filamentose and greenish. This, in so far as written records show, is the earliest proposition ever laid down concerning the morphology of the flower; and it was a mighty contribution to scientific botany. It is in substance the distinction of petaliferous and apetalous flowers. It will therefore hold its place in the science of plant life and form as long as such a science shall exist.

The investigations of Theophrastus along the line of what we denominate apetalous flowers appear to have opened his eyes to the presence of the capillary organs in a large and showy petaliferous kind; for in this same chapter he states that many flowers are twofold, showing another flower inside the main one. He cites such familiar garden flowers as the rose, violet, and white lily as examples; and, as against any suspicion of ours that his twofold flower of rose and violet and lily might mean a double flower, as composed of multiplied petals within the main outer circle, there occurs the one word *διχροα*, two-colored, or differently colored. It is, of course, the stamens within the corolla of red rose, purple violet, and white lily that are colored differently from the corolla. This is the earliest recognition of the flower as other than a simple organ. It is the beginning of the classification of its parts; a small beginning, but highly significant. It is given out for the first

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[1] *Hist.*, Book i, chs. 20, 21, 22.

[2] *Ibid.*, ch. 21.

time, that many flowers have two circles of organs, a flower within a flower, the one within readily distinguishable from the broad, leafy one outside. He does not formally name this inside flower, but he has found so many flowers that lack the leafy outside circle altogether, displaying nothing but the inner, that he names this kind capillary or woolly flowers. It must here be affirmed that Theophrastus knows nothing of the calyx as being any part of a flower. The color and texture of floral organs were what distinguished them from ordinary foliage; and by their points of agreement with the latter any green leaf-like organ or circle, however near the "flower," would fail to be included as a part of it. Also the ovary and ovules were not indicated or received as organs of the flower. They were simply the fruit or seed, in whatever stage from that of the flowering to that of full maturity; and this neither through dullness nor indifference. The colored leaves, together with the colored threads, set in the midst of them, were all there was to the flower. One may fancy some brilliant Greek pupil asking the master if that protuberance in the middle of many flowers ought not to be regarded as a part of the flower, and called the fruit, ought not to be called by a name of its own while in the flowering stage. He who knows the keenly penetrating and severely logical mind of Theophrastus will infer without chance of a mistake, what the substance of his answer would have been. At what particular point in its development will that protuberance begin to be a fruit? I suppose that such logic might silence the ablest morphologist who has lived hitherto. Our modern term ovary is but an illogical convenience. It suitably abbreviates the following expression: "fruit at the budding or flowering stages and for an indefinite period thereafter." Our neighbors the industrial botanists even of to-day have no need of the term ovary and ignore it. When a hard unseasonable frost has sterilized the ovaries of their trees, whether in bud or in flower, it is the "fruit" that has been killed; and so the Theophrastan anthology still lives and is widely though unwittingly approved.

What are now known as the styles were not segregated from the other flocci, or capillamenta—that is, from the stamens—until ages after Theophrastus. He made no distinction between these two, at least when found together within the same flower; and his capillary flower might consist of stamens alone, or of styles only, or of both. What is more, there are certain arrangements of stamens under which they failed to gain recognition by him as being of the nature of floral parts, as in the aments of hazel, the walnut, the oaks, and many more. He gave them a name, *ἰούλοι*,[1] Latinized as juli, writing about them so minutely and describing them so well as to attest his perfect familiarity with them; but apparently the more he studied them the more enigmatic did they seem.[2] The filbert, *Corylus avellana*, not only grows wild in Greece, but it had been cultivated there doubtless for ages before Theophrastus' time. It would be irrational to question that it was among the shrubs of that botanic garden in the midst of which lay his daily walks for many years. At all events, his perfect familiarity with its tassels is attested by the following account which he gives of them. "In autumn after the nuts have fallen, there appear in bunches of several certain things that look like worms, inserted on a short thick stalk. These are called juli. Each is made up of countless scales arranged somewhat after the manner of those of the nut pine (*Pinus pinea*, Linn.), the whole longer in proportion to its thickness than that, and also of equal thickness throughout. Before the end of winter it begins to grow. In early spring the scales separate and stand apart, and are then become yellow, the whole then sometimes as much as three inches long. When the leaves begin to put forth, these things shrivel and fall. Then the cups that enclose the nuts develop; one cup for each flower, and one nut in a cup."[3]

The concluding sentence places it beyond doubt that the writer knew the crimson pistils of the shrub as well as he did the yellow aments. He does not stop to describe them. They are of his class of capillary flowers, and that is enough. In a later chapter,[4] in which he brings out the habit and vegetative characters of the filberts, indicating two species (*C. avellana*, Linn., and *C. tubulosa*, Willd.) by differences of fruit, he has no occasion to mention again the flowers, but can not forego renewed allusion to those perplexing aments. "To these shrubs belong that julus of compacted scales which we have elsewhere described." The sterile aments of the oaks, slender, lax, more tufted than those of the hazel as well as of short duration, must also have been known to Theophrastus; and so were the colorless and very inconspicuous pistillate flowers; for, while he quotes the popular opinion of his time that

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[1] *Hist.*, Book iii, ch. 7.

[2] Pliny, some three centuries after Theophrastus, refers to the juli of the filbert in terms that prove them still incomprehensible to nature students of the time. He says they are *ad nihil utiles;* which we remark is a negation, and therefore unscientific.

[3] *Hist.*, Book iii, ch. 7.

[4] *Ibid.*, ch. 15.

these and other like trees are flowerless, he tersely contradicts it. "Both oaks and alders flower"[1]; by which he must have meant the axillary and scattered pistils of the oak, and the cone-like clusters into which the alder pistils are congested.

Of the aments of fir and pine I observe no mention in Theophrastus. Even the young cone of pistils with their subtending scales in the conifers, from its form he denominates a julus; but it is not of the enigmatic class. Without hesitation he denominates that the flower. It is also plain that with him this must be received as a petaliferous or leafy flower, for it shows nothing that could be called a capillamentum. The rudiment of each fruit rests in the axil of an ample and highly colored leaf.[2] Nor does he here cite the opinion of any of his forerunners or contemporaries as having denied or questioned that these are flowering trees, as they had done in the case of oaks and alders and hazels. The flowers of the fig tree he could never discover. To him it was as flowerless as a fern or moss. He was loath to believe that junipers are not equally flowerless. He had investigated them; had observed that in summer their fructiferous branches bear one set of fruits full grown, and another set newly formed and not half grown; a proof that its fruits require a year and somewhat more for their growth and ripening. He can hardly have failed to see the staminate aments, small though they be, and of brief duration. They were nothing, in his view; at least nothing floral, and not worth mentioning after he had once described the like phenomenon as conspicuous and of long duration in other trees and shrubs. What he was looking for, he could never find, that is, what he would have accepted as a flower, a folium, or a capillamentum indicating the seat of the juniper berry that is to be. The pistillate or fertile juniper flower is as far away as it is possible to go from having the appearance of a flower at all. It shows even under a lens no trace of style or stigma or ovary. It is so little different from the minutest first rudiment of a merely vegetative twig, that an experienced botanist, even of these later times, may fail to recognize it, though he search with a lens. It is improbable that there is a man in the world to-day who, in the feeble botanical light of the Theophrastan age, and without the aid of magnifiers, would ever have found the pistillate flower of a juniper.

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[1] *Hist.*, Book iii, ch. 5.

[2] "The flower of the fir is yellowish red, and otherwise beautiful."—*Hist.*' Book iii, ch. 6.

But what is fully as interesting as Theophrastus' failure to find anything upon a juniper tree which he could demonstrate to be a flower, is his recording the opinion of those who think differently. "There are those who say that the juniper tree is of two kinds, one that flowers and bears no fruit, and another that is flowerless but fructiferous."[1] The pronouncement is interesting as being diametrically opposed to the Theophrastan doctrine that nothing not in immediate juxtaposition to a fruit rudiment is to be regarded as a flower. It is a virtual contradiction of the opinion that juli or aments can not be flowers. These people who held that male and fruitless juniper trees have flowers, and that fertile ones have none, were people who evidently regarded those small evanescent yellow, dusty male aments of the juniper as true flowers, even the only flowers that any juniper ever has. If evidence were elsewhere wanting to prove Theophrastus a true philosopher and scientific man, devoted to the truth whatever that may be, rather than to his own theories, it is not wanting here. He publishes this adverse opinion of his neighbors for the very reason that it may possibly turn out to be the right opinion, concluding the whole passage with the recommendation that investigation of the subject be continued. "The matter should be looked into further."[2]

Quite as briefly as he had indicated the distinction between leafy flowers and capillary does Theophrastus give the suggestion that the leafy flower in certain plants is made up of but a single leaf.[3] It is practically classifying corollas as choripetalous and sympetalous. He writes of what he calls the monophyllous flower as if within the field of his observation it had been somewhat exceptional; and he warns the reader that it is not always distinguishable at a glance from the other kind. Viewed as to their periphery they will seem to be made up of separate leaves, but at the center or base they are seen to be monophyllous. In the morning-glory (*Convolvulus sepium*, Linn.), however, the monophyllous character is readily apparent, only a certain angularity of the periphery remaining in place of the appearance of separate leaves. Even small flowers may be monophyllous. Such are those of the olive tree. Lying on the ground under the trees they are readily seen to be perforate. From his having cited the olive blossom, one is assured that he held a corolla to be monophyllous even if the leaves were united only at base. But one must guard against mentally

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[1] *Hist.*, Book iii, ch. 6. [2] *Ibid.* [3] *Hist.*, Book i, ch. 21.

attributing to him any general recognition of all sympetalous corollas as being such as that to which later generations have arrived; for evidently he had not proceeded far along this line of anthological research, but was only at the beginning of the subject. Having so perfectly settled the monophyllous structure of the small and quite choripetalous-looking olive blossom, we are disappointed that he has not done as much for that of the elder tree. It is a tree in which he has manifested a special interest, and with which he has grown very familiar. The thin wood of its shoots and branches, along with an extraordinary development of pith; then externally the remarkably long internodes, and the foliage, from watching the falling of which in autumn he seems to have learned the very important classification of leaves as simple and compound—all these aspects of *Sambucus* he has noted fully. Will he not perceive that its flowers, like those of the olive, are of that structure which he designates as monophyllous? They are too small; much smaller than those of the olive tree; even quite minute to one who is without a lens; and Theophrastus may not have examined them very carefully as individual flowers. Either that, or else, in condescension to popular usage, he permits the corymb or umbel of small flowers to pass for a flower. And so he describes the blossoming of the elder thus: "The flower is white, composed of many small ones all white, the whole with the appearance of a honeycomb, and attached to the summit of a shoot by a number of stalks."[1] There is another type of monophyllous flower not as small, which first and last remained to him an enigma. It was the ovate, hollow, and pitcher-shaped corolla of *Arbutus unedo*, a most common type among ericaceous plants. He says it is not a leafy flower; an expression equivalent to the modern phrase apetalous flower. He describes it as being in the form of an egg-shell with one end cut off, leaving an aperture.[2] He can not have detected the five obscure recurved teeth at the orifice; for they would have taught him that this, like the faintly five-angled morning-glory blossom, is of five almost completely united leaves.

One chapter of the *Historia* opens with a sentence like this: "Flowers differ in respect to their origin and insertion."[3] It is one out of a number of Theophrastus' brief statements of significant fact, any one of which would have rendered famous any herbarist of the sixteenth century or the seventeenth who had been privi-

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[1] *Hist.*, Book iii, ch. 13.

[2] *Ibid.*, ch. 16.

[3] *Hist.*, Book i, ch. 22.

leged to announce it as a discovery of his own. In the most recent and approved taxonomy of flowering plants, this point in anthology, first indicated by the ancient Greek, holds a most conspicuous place. Let the Greek himself explain what he means by the origin and position, or insertion, of the flower; always keeping it in mind that with him the leaves and the thread-like parts in their midst are all there is to a flower, the ovary being the fruit. "Some produce the flower around the [base of the] fruit, as do the grape vine and the olive tree. . . . In the greater proportion of plants the fruit thus occupies the center of the flower. But there are not wanting such as support the flower on the summit of the fruit, as do the pomegranate, apple, and rose, all of which have their seeds [ovules] underneath the flower. A few bear the flower on the summit of the seed itself, such as the thistles, and all that have their flowers in that manner crowded together." It is a clear distinguishing between the hypogynous, perigynous, and epigynous in floral structure; clear notwithstanding that the one example brought forward to illustrate the epigynous insertion, that of the flowers of the composites, was not from the modern point of view well chosen; because then what he understood to be the seed we regard as a fruit. If he had been accustomed to assign names to what have proven to be his great discoveries in anthology, he would have called this third mode of insertion the epispermatous. He learned this springing of the "flower" from the top of the "seed" to be characteristic of the whole family of the umbellifers, and of the few rubiaceous plants that he knew, as well as of the thistles and their kindred. It seems to me that what is more to be wondered at in Theophrastan anthology than his distinguishing of the hypogynous, perigynous, and epigynous modes of insertion, is the fact of his having made out so positively, that the head in the composites is not a flower, but that it is a dense cluster of separate and distinct individual flowers, each complete in itself. Less than three generations ago, eminent systematists were still writing up the scales of such involucres as "sepals," the whole involucre as a "calyx," and the circle of ray flowers as the "corolla." At this juncture the sublime old Greek will appear to have lived before his time by more than two thousand years.

In his study of flowers the arrangement of them was not unnoticed. He observes that in most trees they appear as scattered on all the branches, all appearing nearly simultaneously, so that the flowering period of such is but short. In many herbaceous and half-shrubby growths they are clustered together; and in describing such plants as to their flowering he makes frequent use of terms equivalent to spike, raceme, and umbel, though not with such definiteness of meaning as they convey in modern botany. In these clusterings it is observed that the flowering of the whole cluster at once seldom takes place; that usually the lowest flowers are first to expand, then those next above them, this succession continuing in some plants so long that the seeds from the basal flowers are ripe before the terminal flowers have opened. The aromatic garden herb ocymum is named as a case in point;[1] but he mentions this kind of inflorescence repeatedly. He also names one plant whose flowering begins at the top, the succession of later bloom following downwardly. Thus is Theophrastus again a botanical discoverer. He has distinguished between the centripetal and the centrifugal in inflorescences. The historian Meyer was surprised at this, remarking also that he knew of no other botanist's having noted this distinction again until the time of Link and Robert Brown.[2] It is evident that Meyer pondered the fine picture books of his compatriots of the sixteenth century, Brunfels, Fuchs, and Tragus, to the neglect of the one real botanist that there had been among them all, Valerius Cordus.

Fruit and Seed. Without fully appreciating the significance of fruit and seed as furnishing the best clew to plant affinities, Theophrastus nevertheless studied them assiduously. Even flowers in their beauty and fragrance, and by their multitudinous forms, engaged him chiefly as being heralds of the fruit and seed. The perfecting of fruit he alludes to here and there as being the culmination of the plant's existence. He notes it that even such vigorous and enduring things as trees and shrubs shorten their life period by excessive fruit-bearing; that myriad annuals live but the length of one summer season because they exhaust all their vitality in the yielding of their one crop of seeds. Seeds were of very special interest, in his view; and succeeding generations of botanists have been with him in that opinion.

The scientific examiner of even commonest objects finds more things in nature than there are names for in common language. The investigator of things is therefore obliged to be the inventor of new terms; and every science has therefore its vocabulary of special terms, every one of them necessary to the science and to the man of science, but to the world at large useless. In connection with his study of seeds, Theophrastus was obliged to invent ne

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[1] *Hist.*, Book vii, ch. 3.

[2] Meyer, *Geschichte der Botanik*, vol. i, p. 166.

terms, and to give new meanings to old ones. Most seeds were grown and perfected under some special covering formed to shelter, contain, and nourish them until mature. For particular kinds of such coverings particular names were in common use: pod, husk, chaff, shell, and for succulent or fleshy coverings of seeds pome, berry, acine, or more comprehensively, fruits. A general term which should include all these coverings was needed, and the word pericarp was coined.[1] This done he defines a fruit scientifically. It consists of a pericarp and the seed or seeds which it encloses. Henceforward, while in agriculture, gardening, domestic economy, and the world's commerce a fruit is what it always was, in botany the term has another meaning, a meaning at once more exact and more comprehensive; and it has this new meaning universally, and from Theophrastus forward; for modern botany reiterates it from him, unaltered by a syllable; and that of the future will do the same. In practice he did not always rightly distinguish between pericarp and seed. Lecturing upon the fruit, and having a mature sprig of sage or other labiate in hand, he would have taught that the four black nutlets are the seeds, and that the green calyx is their pericarp. Or with a handful of spikes of wheat or barley before him, he would have mistaken the grains for mere seeds, and the chaff for the pericarps. Errors like these in the mere application of his terms were inevitable. They could never have been corrected without microscopically aided vision; and it was indeed a long, long time after the invention of the microscope that botanists first learned the structure of sage nutlets and wheat grains to be that of fruits and not that of seeds.

About pericarps he seems to have observed everything that lay before him within his own limited field. He notes the extreme diversity of them, but, as usual with him, and doubtless for want of time to correlate and classify he gives to the most distinctive kinds little more than an informal mention. Only a single deduction does he venture concerning pericarps in general as unlike other organs, a deduction superficial, curious, geometrical: "No pericarp shows a rectilinear or angular circumscription."[2] Yet the cursory reader of the main chapter on fruits—perusing it in the Greek original—might well wonder with what justice or propriety it can be said that the philosopher did not carefully and effectively generalize about seed in relation to pericarp, when he finds him

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[1] *Hist.*, Book i, ch. 3. περικάρπιον was also in use with Aristotle. The invention of it lies between him and Theophrastus.

[2] *Ibid.*, end of ch. 18.

using over and over again such exceedingly important taxonomic terms as angiosperm and gymnosperm. The fact is, he employed neither in anything like that breadth of meaning which they convey as used in later botany; but both of them very restrictedly. He used angiosperm only to designate one particular circumscription of what are known with us as capsular fruits, namely, the subglobose or urn-shaped or vase-like kinds. The example given is "that of the poppy and those like it." It is corrrelative with λοβός, the Theophrastan name for all leguminous fruits. His gymnosperms include nothing really gymnospermous in our taxonomic use of that expression. By the examples cited, "coriander, anise, fennel, cyminum," and several others among kitchen-garden plants, the gymnosperms were the umbellifers. They were naked-seeded to him, because, as already noted, he had not recognized any such organ as a calyx; one nowhere in all the plant world more recondite than in the umbellifers. The only real gymnosperms—according to our application of the term—which he knew, were the conifers; but they do not enter, with the umbellifers, into his category of that name. He expresses distinctly though modestly the idea as his own that, as to fruit and seed these stand naturally aloof from all other groups, and thinks the view may be tenable that cones are not fruits at all. "No trees bear capsular fruits, unless you can call a cone a capsule; but it is possible to regard the cone as different from a fruit." Under the head of anthology it was seen that the cone-scale at its flowering stage was a flower-leaf in Theophrastus' understanding of it. Logically, therefore, he would have regarded it in its maturity as a sort of pericarp. But that he left the cones in a place apart, as unclassifiable with other seed vessels—the types of what were to be named gymnosperms twenty centuries afterwards—is yet another evidence of the profound sagacity of the protobotanist. After these things, it is no longer with any surprise that we read his accurate descriptions—descriptions of them from center to circumference—of such fruits as the drupe, the pome, the nut, fig, pomegranate, and other types, and citing as he always does familiar examples of each different kind. But that he should have been as near as he was to a systematizing of the placental attachment of seeds within the pericarp is again almost startling; for when he records it that in many the seeds are as it were promiscuously crowded within the pericarp, while in others they are ranged in regular lines, or at least in separate groups—of which latter he says the squash, cucumber, and apple are examples—we know that he has been carrying on research in this direction, and has been able to make this fair beginning at discriminating the different modes of placentation.

The foundations of the whole philosophy of higher plant life and form center in—are concentrated within, if one may so speak—the seed. In botany no less than in zoology is embryology indispensable to a right understanding of the interrelations of things. From the minuteness of his researches into the structure of seeds and the behavior of seedlings, it almost seems as if Theophrastus may have realized this fact. He records many observations on them in all, even their most familiar aspects, not neglecting the diversity of them as to form and coloring.[1] In a few terse sentences he gives the results of what may have been years of investigation in his botanic garden, upon the subject of the different periods of time required by the seeds of different plants for their germination. "Ocimum, blitum, eruca, and radish are most prompt of all, for they come up on about the third day after the sowing; lettuce on the fourth day; cucumber and squash on the fifth or sixth; anise on the fourth, pepper-grass and mustard on the fifth; beets sown in the spring, on the sixth, in the fall, on the tenth day, orache on the eighth, cabbage on the tenth." Leek and shallot are such close congeners that he evidently expected they would agree as to the time required for their germination, but he finds the seed of shallot coming up at the end of from ten to twelve days while that of the leek takes nineteen or twenty. More than thirty days must be allowed, he says, either satureia or origanum, and forty for celery. After long experience he finds it remarkable that the most favorable conditions as to the season of the year and the state of the atmosphere do not shorten the usual time required for the germination of any kind of seed, though a cold atmosphere, concurring with clouded skies retards it.[2]

These studies in seeds and seedling plants, though by chance interesting and instructive to the gardeners of his time, are essentially those of a great botanical philosopher, with whom not the smallest fact re'ating to plant life is held unworthy to be placed on record. And as he proceeds, the twentieth-century botanist will be apt to read with amazement a passage like the following as occurring in Theophrastus. "Some seeds in germinating put forth their primary root and leaf from one and the same point; others, the root from one end and the leaf from the other."[3] Pre-

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[1] *Hist* Book vii, ch. 3.

[2] *Ibid.*, ch. 1.

[3] *Hist.*, Book viii, ch. 2.

serving all the terseness and brevity of the original in this very literal rendering, the statement of the fact may seem a trifle exaggerated. The first roots issuing from a grain of wheat or barley appear not quite from the base, but from near it, and the first leaf appears from a point well toward the summit of the grain. The two do not, as in dicotyledonous plants, come forth from the same point. But thus early in the history of botany, even by this first forefather, was given in these words, the first hint of a fundamental distinction between flowering plants as dicotyledonous and monocotyledonous. By way of further elucidation he continues: "Wheat, barley, rye, and all the grains sprout from both ends; that is to say, the basal and thicker end of the grain puts forth the root, the upper and narrower end the green herbage. The two, however, are connected and continuous as one. But neither the bean nor any other seeds of leguminous plants have this way of sprouting. These put forth root and stem from the same point, namely, that at which the seed was linked to pod, as if under that point [the hilum of later terminology] lay the special seat of the growing principle. In the case of seeds of this kind the root at first appearing begins to show a downward tendency, the stem an upward. The seeds of the frumentaceous and the leguminous kinds are alike in this one particular of sprouting from the point of insertion; but of certain trees, the almond, walnut, the oaks and their allies, the seeds sprout from the opposite end." I have omitted here one of the most important clauses; that in which he indicates his having observed in the bean and lupine allies the two cotyledons, joined to the hypocotyle.[1] Later in the same chapter he states without simile or comparison the same characteristic. "The seeds of all the latter," that is, of the particular trees he has mentioned, and of the leguminous herbs, "are in a manner two-parted." And again: "Wheat and barley make their first appearance with but a single leaf, peas and beans with several"; from which it is manifest that he counts the cotyledons as leaves, along with the one or two that appear in the plumule. Still other facts and phenomena observed and recorded by him about germinating seeds, and young seedlings, must enkindle toward Theophrastus the wondering admiration of the most accomplished modern botanist. He says that he finds it uniformly true, whatever the kind or the structure of the seed, that the "root" is first to appear, after that the leaf or leaves; also that the cereals, while

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[1] In Latin the clause runs thus: "Id quod in quibusdam partis pudendæ refert formam, ceu in faba et cicere, sed maxime in lupino."

as yet showing but a solitary primal leaf, exhibit quite a tuft of roots, these all simple and equal, whereas the two-parted seeds in their germinating exhibit several leaves and but a solitary "root." Furthermore he notes that the grains with their multiple roots send up culms that never branch, while the merely tap-rooted leguminous herbs exhibit stems that branch freely and widely.

To the beautiful work of a Malpighi one gives somewhat more credit than is fairly due it, until one has read these chapters of the ancient Athenian master. Then it is clearly apprehended that the man of the seventeenth century may have received the suggestions of his own work directly from the Greek philosopher; and is almost ready to add that the beautiful drawings of sprouting grain adorning Malpighi's folio might almost have been done from the Theophrastan descriptions of the same. It must needs be conceded that the botanic garden at Athens, founded by Aristotle, and the earliest of which there is any record, was wonderfully prolific of new botanical facts of profoundest import. What later one has equalled it in supplying first principles to botany as a science? Or who since Theophrastus has used an opportunity of that kind so immensely to the advantage of succeeding generations?

Anatomy. Immediately after having enumerated the principal external organs of plants, and given the first outline of a system based on these, Theophrastus takes up the subject of internal structure. Two short chapters contain the simplest elements of plant anatomy, as he is able to make them out. If these chapters commend themselves to our most careful reading, it is partly because they are the earliest in which such matter was discussed, and partly for the reason that after the writing of them some eighteen centuries elapsed before another botanist resumed the topic.

Apparently having in mind all forms of plant life except the lowest and simplest, he opens the subject with the statement that "plants are made up of bark, wood, and pith when pith is present."[1] We of the present are accustomed to this as being the structure of stems. Let none be disquieted by the fact that Theophrastus does not limit bark, wood, and pith to stems; for we are learning that he never writes a line carelessly; never indites the simplest and most fundamental proposition without rigid investigation and profound forethought. Possibly we shall find that he thinks the substances of bark, of wood, and of pith all

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[1] *Hist.*, Book i, ch. 3.

occur in other parts besides stem and branches—in leaves, or even in fruits.

Now if the history of plant anatomy is to begin as near its true beginning is possible, those three Greek terms must be consulted which come into our language as bark, wood, and pith. The first two will not detain us. The bark and wood are, each in many different ways, too indispensably necessary to primal man, to have failed to be distinguished and named long before the advent of the most primitive philosophies. Theophrastus took the terms *φλοιός* and *ξύλον* as he found them, ready to hand and well suited to his use. It was otherwise with the term which he selected by which to indicate the pith of plants. This part was not well known. Primal man, in quest of only the obvious and the useful in nature, may have been unaware of the existence of it. Woody growths, in that mature condition in which they supply the savage and the half-civilized with timber, fuel, bast, and dye-stuffs, show no pith. But that enlightened and philosophic inquiry into nature's obscure things, which Greeks had begun to pursue before Theophrastus' time, had brought to notice this part of plants which was not bark, neither wood. The philosophic must have discussed the substance, perhaps had written about it, for they had attempted to name it. This we have from Theophrastus himself, who says that some called it the heart of plants, others called it the marrow.[1] In this connection he did what with him is most unusual, almost timorously conservative man that he was; he declined to accept either "heart" or "marrow" as a suitable name for this part of a plant. This can mean nothing else but that he himself had taken the pith under special investigation and thereby had reached a new conclusion; had found that it was in no important point analogous to marrow, and farther still from corresponding to the heart in animals. We know enough about Theophrastus' temperament to warrant the assertion that he would have been the last of men to reject two names already in use for a thing, until he was able to prove that both were utterly false to nature. The new name which eventually he offered is one which can not have suggested itself from any study of the substance in that dry, whitened, imponderous, and effete condition in which it is seen in mature stems and branches, in which condition alone we of to-day call it pith. As philosopher, and as one whom we, if he had lived in our time, should have

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[1] *Hist.*, Book i, ch. 4.

known as a "scientific botanist," what he wished to ascertain was the function of this substance in the economy of plant life. This he certainly did discover; but he never could have done so had he confined his investigations to the pith as no longer vitalized and operative; therefore he must have sought it in its young and growing state, when of all parts of the plant this is most tender, replete with sap, and a strong center of vital activity, really the matrix within which are generated both bark and wood. The new name which he gives it is *μήτρα*, the word mother, *μήτηρ*, somewhat altered, and in earlier Greek the womb, the mother of life.[1]

Theophrastus never records his processes of investigation; but when he selects the word metra, matrix, to be the appropriate name of the pith of plants, we see at once that it does not really apply to it in its white, imponderous, and devitalized condition, and that he must have named it in reference to its earlier state, that of living parenchyma. We may well divest ourselves of the prejudice that the beginnings of a scientific and sound plant anatomy were not made until after the invention of lenses and microscopes. This man of antiquity, he who had progressed so far in plant embryology as to have made out even the seed distinctions between exogens and endogens, could just as easily discover, in the cross-section of a large and very tender shoot of elder or of maple, the first traces of the several fibro-vascular bundles standing in a circle midway between where the pith was to be and the place of the bark. Successive cross-sections made at intervals would reveal to him the gradual hardening and widening of those bundles, their final apparent meeting and coalescing into the cylinder of hard wood intervening between the central pith and the now formative bark. It is undoubted that Theophrastus had thus seen, in young shoots, the early stage at which bark and pith are all one in form and substance, and that from it, even within it, both wood and bark are gradually generated; and that because of this he called the juicy soft living pith the metra, the matrix. In reality, while searching to find what pith would be like in its living state, and what in that state its function might seem to be, Theophrastus had discovered living parenchyma.

Such a conclusion, when we have been driven to it in order that the man's words may not be meaningless, needs no further support; and yet there is one other circumstance which would confirm it

[1] Just as in certain European languages still spoken there is no one word for womb, but the phrase "mother of life" instead.

were confirmation called for. Hitherto I have conformed to usage, very ancient as well as modern, of writing bark, wood, pith, in just that sequence. It seems natural. It is at least a geometrical succession of the terms, and therewithal not inelegant. Even the translators of Theophrastus from Greek into Latin have made him out as having named first the bark, then the wood, and lastly the pith. Unwittingly they committed an inaccuracy, and have misrepresented him; for Theophrastus wrote it thus: bark, pith, wood. This sequence is not geometric, but it is biologic. It would be illogical and awkward, were it not more exactly truthful than the other and merely geometric succession of the terms. As he placed them, the story is told again of how in early stages of development bark and pith are substantially identical. Therefore in their most widely differentiated condition they are next of kin, more nearly related each to the other than either one is to the wood.

This biologic investigation of the pith led to the detection and segregation of other elements in stem structure; discoveries that are all his own, for he expressly says of these elements that they have no names. He thinks that he can not do better than apply to each the name of some analogous part of the anatomy of animals. The whole fabric of stems he makes out as consisting of what he calls veins, nerves, and flesh. They are new uses of these terms and he defines the botanical use of each, premising first of all that vein and nerve are substantially one, differing scarcely otherwise than as to dimensions, nerves being smaller than veins; adding also that in plants these are simply elongated and do not branch. The universal mark of the fabric which is composed of them is, that it splits, and is not otherwise readily divisible. Flesh has the very different quality of being divisible with equal readiness in all directions, like a lump of earth.[1] Here, quite as we had been prepared to expect from his new naming of the pith, and his indicating its consanguinity with bark rather than with wood, we see plainly that Theophrastus discovered and distinguished well what in post-Malpighian times we have learned to know as parenchyma and prosenchyma. He is now able to add a new chapter to plant anatomy, for he can name the elemental constituents of bark, of wood, and of pith. Wood, he says, is composed of nerve and moisture; although some woods, such as of the palms and of ferula, are of flesh; such however in mature and dry condition show that the flesh has been partly converted into wood. When he cites

[1] *Hist.*, Book i, ch. 4.

the wood of palm and umbellifer, we realize the expression of the popular notion of wood as being that which the bark of any tree encloses, without regard to density or ponderability. His language here nevertheless evinces plainly his having taken full cognizance of the fundamental difference between the wood of palms and that of all exogenous trees. To pith he attributes flesh and sap only. Bark is composed of all three of these elementals in most cases, as in the oak, poplar, and pear tree, though in some, the grape-vine, for example, of nerve and sap only—that is to say, without flesh, or what we call parenchyma.

The structural elements of the leaves of exogens he apprehended as substantially identical with those of the bark. The stalklet, and therewith the fibrous framework of the blade, are of that which he designates as nerve, or sinew.[1] Next to this he names the epidermis, and after that, what he calls the flesh, known to us as the mesophyll. In the leaves of palms and reedy or grassy plants he finds no flesh at all, and thinks these are composed of fibre and epidermis only. The edible fruits of trees and shrubs are composed mostly of flesh, with little or no fibrous tissue; while, on the other hand, some pericarps consist of a rind or skin only. He has so clearly distinguished these two or three elementals of the plant fabric that he is able to trace and point them out in every plant organ from root to fruit.

Phytography. In descriptive botany there are two different methods by which it is undertaken to convey by means of language the image, so to speak, of some tree or other growth which the describer has seen, and the reader is supposed not to have seen. That two distinct methods in phytography exist is something of which I have seen no mention either with any botanical author, or with any historian of botany; but a suggestion of them has been made very recently, with also a good account of Theophrastus' phytographic method, in an excellent treatise on some parts of Theophrastan botany by Dr. Hugo Bretzl. Let me for convenience designate the methods as the natural and the artificial. They may, however, quite as fitly be named the comparative and the positive methods of plant description. Of the two, one is very ancient, the other strictly modern; and the artificial or positive is doubtless the more perfectly adapted to its purpose, though only for such writers and such readers as are competent to use it; for it requires the mastery, on the part of both, of a very extensive vocabulary of special terms; in reality, the learning of a new

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[1] *Hist.*, Book i, ch. 17.

language. What I have designated as the natural method of describing plants vindicates its right to that title by the fact of its primitive universality. If I speak of it otherwise as the method of comparisons, in allusion to what it chiefly exacts of him who would use it: that is, first, familiar acquaintance with certain specific types as standards of comparison, and second, the ability to construct a mental image of the unknown by means of the describer's telling in what several particulars it differs from the given type known to both. By way of illustration, an untaught woodman, familiar with junipers, reports to a botanist that in a new region to which he has wandered there are trees in all respects like red cedar, or juniper, except that instead of yielding berries they bear round cone-like bodies approaching the size and form of smallish walnuts. The botanist at once pictures in his own mind cypress trees, and assures his informant that his new trees are cypresses. Such is the method of comparison in phytography; and it may quite as aptly be called the natural one, for it is that invariably used by the primitive and untaught; not, however, always very well and successfully. Being the primitive method, it is therefore that of Theophrastus; not, however, to the complete exclusion of a number of absolutely definitive terms such as are now in use and always have been. The Greek observed that, taking the plant world as a whole, the leaf is the most endlessly diversified of organs, and also that within the limits of a species its form is constant; from which two conditions it follows that no other organ is so readily available in making distinctions between different plants. Now as to the art of plant-description by the method of comparison, it is very necessary that the types to be used in comparing be chosen considerately. There seem to be indications of his having thought upon this matter, though he has not explained, or even didactically set forth his scheme. It is only by a certain order and fitness in the scheme itself that one infers the author's having studied it out. The most common types of leaf outline are perhaps the lanceolate, ovate, oblong, and suborbicular. He knows nothing of any such terms; but when he wishes to say that which would express what modern botany expresses by the term lanceolate, he says the leaf is that of the laurel, *i. e.*, *Laurus nobilis*. A leaf that we should describe as oblong, unless it be much too large, is with him that of the olive tree. A leaf that is of rounded contour and nearly as broad as long is compared by him to that of the pear tree. For the ovate in outline his type is the leaf of the ivy, *Hedera Helix*, in respect to which one must not fail to note that it is the leaf peculiar to the mature and fruiting ivy bush, not that of the rooting and climbing young plant; for the leaves of this are too broad to represent the ovate, and are angularly lobed.

Note now certain points of agreement among these four leaf-outline types. (1) All are leaves of trees; for few herbaceous plants display such uniformity of foliage as to the individual specimen. There is apt to be one form and size of leaf near the base of such a stem, and another widely different description of leaf at the summit, with intermediate forms up and down between base and summit. With some notable exceptions—of which the Greek takes advantage here and there—leaves of herbs do not answer the purpose. (2) The trees are all selected from among such as are most universally and familiarly known; every one of them common in cultivation. Every civilized Greek of three thousand years since knew the sweet bay, the olive, the pear tree, the ivy. (3) The leaves of them all have a certain firmness of texture, either leathery, or approaching it. By this prevision any soft herbaceous plant having lanceolate entire foliage may be described as to its foliage by merely saying that its leaves are like those of the laurel, but thin. (4) The leaves of all four of the types are entire: whereby the leaf that is of lanceolate cut, coriaceous texture, and entire margin, no matter in what genus it may occur, may be described by simply saying that it is the leaf of the laurel. Supposing, however, that a tree is to be described the leaves of which are lanceolate, coriaceous, but with margin serrated, then its description as to leaf will be. that it is like the laurel leaf, but serrated. And this selecting of types that have entire leaves is manifestly better than it would have been to have selected serrate leaves for types. In such a climate as that of the Mediterranean, evergreen trees and shrubs predominate, all of them with coriaceous foliage, the kinds with entire leaves being very notably in excess of those having toothed or serrated leaf-margins; therefore the choosing of as many entire-leaved types as he could was natural to a man in Theophrastus' place and environment, as well as making for economy of time and space in describing things. It was by no means accidental that this descriptive botanist selected the olive, the sweet bay, the myrtle tree, and the box as patterns of leaf outline to be referred to in his phytographic work.[1]

How well such a system of morphologic types is adapted to the

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[1] See also Dr. Hugo Bretzl, *Botanische Forschungen des Alexanderzuges*, pp. 8–22.

purposes of description may best be shown by quoting a few examples. A wild elm tree that inhabits the mountain districts he says has "leaves like those of the pear tree, but longer in proportion, with serrated margin, and a rough rather than smooth surface."[1] This makes it plain that Theophrastus' first purpose in choosing leaf types was that of imparting ideas of general outline. To begin the account of an elm leaf by affirming it to be like that of a pear tree is awkward and even mischievous, upon any other supposition than that by such phrase he alludes to size and general circumscription only; which also the expressions immediately succeeding prove; for he who had pictured in his mind at first a pear leaf for an elm leaf must now proceed, under direction of the describer, to alter it by giving it a very distinctly saw-toothed margin, and after that a roughness of surface, of which there is no trace upon the pear leaf. What he now sees mentally as a leaf of the little known wild elm is like a pear leaf in nothing save its general contour. The elder tree, *Sambucus nigra*, Theophrastus seems to have taken pleasure in describing rather minutely, although the tree was no rarity, but rather familiarly known. But it seems to have been this which taught him the existence of such a thing as a compound leaf; and he gives a particular account of the species from root to fruit. When he comes to the lanceolate individual leaflet he says it is like the leaf of the sweet bay but larger, relatively wider in the middle and at base, more pointed at summit, serrated all around, and the whole more soft and pliable in texture.[2] To one acquainted with the sweet bay and the elder, I do not know where, in even the most recent botany, he will find a more complete description of the elder leaflet. The leaves of maples, mostly wildwood trees and less familiarly known, are compared with those of the omnipresent plane. The maple leaves are also ample, cleft somewhat after the same manner, but not to the middle as in the plane, *Platanus orientalis*, longer in proportion to their breadth, of a more delicate texture, and not rough to the touch.[3]

This system of leaf describing by comparison with types is both natural and not ill adapted to the purposes of phytography. Had it not been so it would not have remained in vogue for two thousand years after Theophrastus. Greek authors after him, as well as Pliny and other Latin writers, knew no other method of leaf

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[1] *Hist.*, Book iii, ch. 13.

[2] *Ibid.*, Book i, ch. 13.

[3] *Ibid.*, Book iii, ch. 11.

diagnosis. The number of the indicative types was gradually augmented, and the use of them was universal even with the fathers of sixteenth- and seventeenth-century botany; nor has any later generation wholly ceased from the usage; though it is perhaps chiefly conspicuous nowhere but in nomenclature, where such specific appellations as *salicifolia*, *alnifolia*, *betulifolia*, *delphinifolia*, and a hundred more, though ostensibly figuring but as names, may chance to be the best part of the diagnosis, at least in the estimation of any not well versed in the post-Linnæan descriptive terminology.

It is not to be inferred from anything here said, that the Greek knew nothing of any geometric terminology of leaf forms. In that chapter in which he treats of leaf forms in general he names the orbicular, the oblong, the angular, and some others[1]; but they lack definiteness of meaning, at least such definiteness as the exigencies of plant diagnosis call for; though terms that bear upon differences of apex, margin, and base as well as the superficies of the leaf are of more fixed and certain meaning.

It will be observed that even flowers are described by Theophrastus comparatively, the less known being brought into contrast with the well known; and the same rule applies, of course, in his diagnoses of fruits and seeds. He was not particularly given to describing plants. A great proportion of those which he discourses upon were well known to all who would become his readers. The common things of the gardens, of the cultivated fields, of orchard and vineyard and of academic grove, were so familiar that the mention of the name was sufficient. But when he undertakes the description of any herb or bush or tree, he is apt to give more than a rude outline of it; very often a good word picture of it; and he who does this is a master of phytography, without question about the age in which he has lived, or the method he has employed. In the case of a number of Asiatic and African trees unknown to Theophrastus except by report of travellers, he so carefully gleans all that others have said about them, and with such consummate art sums up the whole, and draws up his own description, that in reading it one finds it not easy to realize that the author of it never saw the tree. The books of botany that were composed by Greeks and by Latins within three or four centuries after Theophrastus show that the authors of them copied his descriptions whenever such were available, and in other cases made his the model of their own diagnoses. When we come to the

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[1] *Hist.*, Book i, ch. 16.

era of strongly renewed scientific activity in the sixteenth century, we shall find botanical authors of the time employing many of Theophrastus' descriptions of plant species without alteration of any kind; some, like Brunfels, Tragus, and Cesalpino and their class, formally crediting each such diagnosis to its ancient author, others without making such acknowledgment. But they of this period who ventured upon new descriptions of plants which Theophrastus had described of old seem to have exposed themselves to public censure, as needlessly, perhaps irreverently, supplanting or amending the excellent work of the father of phytography.

Taxonomy. To teach, as it has been taught and is still taught, that Tournefort (1694) first ranged the members of the plant world under genera, that Linnæus (1753) first clearly distinguished species and varieties, and that Adanson (1763) first proposed the grouping of genera into families—all this is to inculcate fable. It has been already suggested, and forcibly enough, that plant taxonomy was not invented in any school, or by any philosopher; that it is everywhere as old as language; that no plant name is the name of an individual plant, but is always the name of some group of individuals, and that all grouping is classifying. Botanical taxonomy began at whatever time far off and prehistoric men began to give names to plants; and it increased with the recognition and the naming of new groups—always groups, never single plants. Had some earlier Theophrastus appeared upon the scene some thousands of years earlier than this one did, in this particular at least he would have found himself in the midst of a like environment. He would have found some hundreds of kinds of cultivated plants familiarly known, spoken of always under group names. In other phrase, he would have found a certain taxonomy ready to hand, such as answered the needs of those who had to do with plants industrially.

The real Theophrastus, entering the field, not in the far-off age of Homer, whose poems are full of tree and plant lore, but many hundred years later, had much to do in the first place in acquainting himself with the vast sum of knowledge, of theory, of poetic fancy, and of superstitious fable that was then extant concerning plants. All this he accomplished, as his pages plainly show, and that with the occasional expression of something like the scientific man's impatience of the superstitious and the fabulous. As distinctively a nature student, however, exploiting the realms of nature from the philosopher's viewpoint rather than from that of the economist, it would not have been strange had he invented some new taxonomic scheme of his own, and then, thrusting aside all the commonly accepted plant groupings, had sought to install an entirely new system of taxonomy in place of the old.[1] He did nothing of the kind; and if he did not, it may have been for the excellent reason that that already in vogue, when duly examined, to a great degree commended itself to the philosophic judgment as having been deduced from nature, and that in as far as it had progressed, was often well enough done. Parts of it could not be amended, and, we may now say confidently, were destined to acceptance as sound taxonomy as long as the world of plants should endure, or a botanist remain to study it; at many points it might be amended or added to, and the whole must be extended and variously improved.

We may assure ourselves by a study of Theophrastus, that something very like this was the task to which he addressed himself as regards the classification of plant organs and the systematization of plants themselves; and the careful reader of his chapters will note often his great conservatism—his manifest aversion to startling the good public by pronouncements that are new, and that will openly antagonize them as assailing their old doctrines and their deeply ingrained prejudices.

All these things being true, one ascertains with difficulty, if at all, what the historian is most in need of knowing, namely where this writer of the first book of botany is recording points of taxonomy that are of prehistoric discovery and universal traditional acceptance, and where he is introducing some amendment or improvement of his own. For example, in a very early chapter of his work Theophrastus ranged all the plants that he had ever seen, or heard, or read about, under the four primary groups of tree, shrub, half-shrub, and herb—*δένδρον, θάμνος, φρύγανον, πόα.*[2] It is one of the most classic pieces of plant taxonomy; one that stood the test of all the ages and is immortal. Nothing that by any means could be elicited from out the hazy past would be of deeper botanical interest than information as to whether this fine piece of taxonomic work had been handed down in its completeness to Theophrastus, or whether as he gives it it represents much augmentation, or condensation, or finishing and perfecting accomplished for it by himself. We have met with like pro-

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[1] This is almost precisely what Tournefort undertook to do in the seventeenth century, and Linnæus again in the eighteenth.

[2] *Hist.*, Book i, ch. 5.

blems before[1]; to the full solution of this I am incompetent. For this, the botanist's best skill would need to be supplemented by the erudition of the specialist in Greek philology. Nevertheless the following hints may make it plain that δένδρον, θάμνος, φρύγανον, πόα, is a piece of classification that was studiously wrought out by Theophrastus himself. In the Greek of his time there were at least two words for tree, δένδρον, and ὕλη, the former more particularly designating such as were cultivated; the fruit-bearing, nut-bearing, and such as were planted for shade or ornament; the latter applying more specifically to wild trees used as timber; almost the equivalent of our English terms woodland, forest, timber-tree, etc. We have elsewhere remarked upon the Theophrastan classifying of all growths as tame and wild. The idea was deeply seated in the Greek mind; and for trees in general, wild as well as domesticated, he could not well have chosen any other term than δένδρον, though it was more properly the appellation of the civilized contingent of arboreal growths. Similarly θαμνος, at least etymologically, signified a densely compacted woody growth,—and not necessarily of low stature. The full-grown olive tree was sometimes called a θάμνος, on account of the bushy density of its head. Also as looking to the distinction between bushes of cultivation and a wildwood thicket, the latter was also ὕλη sometimes, if not even usually. Thus again as with δένδρον the botanist selects for extended use that which signifies the cultivated and better known.

Coming now to the class of suffrutescent plants—the half-shrubby, half-herbaceous kinds—it appears to me no less than certain that Theophrastus was first to discover, indicate, and name this particular category. There seems to have been no word for this kind of thing in the older Greek; for φρύγανον meant nothing more than a bundle of faggots, dead and dry branchlets and twigs of trees which, either as windfalls or as left behind by the wood-chopper, were laid lengthwise and tied into bundles for fuel. There was, however, the old word θάμνίον, which one almost wonders he did not adopt for his category of the half-shrubby. It is but a diminutive of θάμνος and means a little bush or small shrub. At second thought one perceives that it would not well answer the purpose. It gives no intimation of the true characteristic of the suffrutescent growths, which is this, that the lower and woody part represents a shrub, while the upper portion, that which bears more scattered foliage together with the flowers and fruits, is

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[1] See page 66 preceding.

herbaceous—that is, dies back every autumn after the fruiting and is renewed again in the summer. In late autumn and early winter, while the dead or half-dead upright and parallel summer branches are still present, surmounting the shorter tuft of truly woody lower branches, the bush would vividly enough recall a faggot bundle. Even where abounding, as such growths do on open plain or stony mountain slope throughout all half-arid regions of the world, they must have been used as faggots always. He who knows familiarly such ancient garden plants as the lavender and sage and rue, and the wild half-shrubby artemisias and other like composites of all dry climates, will perceive readily that *φρύγανον*, the faggot bundle, lent itself to Theophrastus' scientific purpose in this instance. He might have created a new term; but the conservative prefers to make new use and application of some old and familiar term. The public never takes kindly to new names.

In distinguishing the category of the suffrutescent, the Greek had proceeded analytically. In establishing upon all herbaceous plants one comprehensive group under one name, his procedure was synthetic. It was not indicating a single new group hitherto unrecognized and naming it. It was the synthesis of a number of groups long recognized and separately named; the putting together of such, to constitute a single more comprehensive assemblage, and under one name.

A glance at the actual situation in which we English-speaking people find ourselves as to our terminology of the herbaceous will help us to apprehend clearly the Theophrastan standpoint. We have no single word by which we venture commonly to designate the aggregate of things herbaceous. If in our fundamentals of botany we still follow Theophrastus in writing or speaking of tree, shrub, and herb, that is at once the beginning and the end of our using the term "herb" thus comprehensively. Thenceforward we ignore it and write or speak about "herbaceous plants"; this for the manifest reason that "herb" used by itself has almost universally a special meaning of which it seems impossible to divest it. An herb is something, neither tree nor shrub, which is either medicinal, aromatic, or culinary. The other terms in common use for subordinate groups of herbaceous plants are vegetable, weed, grass, and worst of all the word "plant" itself; for this, as first introduced into our English speech, and as almost universally employed down to our day, signifies only things herbaceous, yet not all; for neither weeds nor grasses are commonly called plants, in our tongue, except technically. Thus our category of the herbaceous includes the segregates herb, vegetable, weed, grass, plant. And the endeavors of our English scientific forefathers to make any one of those terms serve as the name for herbaceous growths in general have been unsuccessful. Theophrastus had been in the same predicament as that which became theirs. In his mother tongue there was *φυτόν*, the herbaceous plant of cultivation, at least as to its most primitive signification; there were *λάχανον*, the kitchen-garden vegetable, *βοτάνη*, the weed, and *πόα*, the grass, the forage herb. The first of these terms *φυτόν*, the herbaceous thing that men plant and transplant and cultivate, was a term that he himself appears to have rendered unavailable. He had made it to include the whole vegetable kingdom from oak and pine to seaweed and fungus. The name that to him seemed most reasonably available for designating the sum of all things green—herbaceous—was the common name of the grasses and fodder plants, *πόα*. When I say reasonably available, my thought is that the selecting of the term grass rather than the term herb for expressing the aggregate was most natural to any one who, like Theophrastus, had thought the matter over. The grasses form by far the greater proportion of that low-growing verdure which, outside of the forests and thickets, covers the whole earth in all temperate latitudes during half the year. It was therefore more true to nature —more scientific—to take up that term which came so near being synonymous with verdure. The term herb is comparatively unfit, as suggesting a much smaller aggregate, and that also of plants marked by odors, flavors, and other qualities which the eye can not detect. There are even modern languages in which the word for all green herbage is the correlative of our word "grass": languages in the botanical vernacular of which, what we call herbaceous plants are known only as "grass." This was strictly true of our English of only a few centuries ago.[1] Thus again how plain it is that the forefather of all botany produced this primal outline taxonomic only after the most careful weighing and considering of every point involved. Nor must our attention be called away from this first chapter of Theophrastus' classification without our having observed the sequence of the groups. Why does he begin with the Grand Division of the trees and proceed downward to that which

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[1] There is a familiar sacred verse that attests this: "All flesh is grass, and all the glory of man is as the flower of grass. The grass withereth, and the flower thereof falleth away."—I Pet. i, 24. At the date of this translation gramineous plants were regarded as flowerless. Therefore in the minds of the learned translators even showily flowering herbaceous plants were a part of "grass."

will include such phytologic ambiguities—for such they were in his day and for ages afterward—as lichens and fungi? There is no room for doubt that one consideration was the very rational one, that botany may not safely begin at any point where doubt may arise as to what realm of nature the subjects of study belong to. The title of trees in general to be regarded a part of the plant world is secure; that of no other growths more so; while judged by his own criteria of phytologic rank, the very highest place must be accorded to trees. In his view they were the most perfectly organized of all.

Inasmuch as the fourth and last Grand Division πόα, the Grass Plants, *i. e.*, the Herbaceous, is the largest of all as to numbers both of individuals and of species and as regards their almost universal prevalence, it will naturally be here that we shall look for further expression of taxonomic idea; suggestions of grouping subordinate to that of πόα, the herb.

The most comprehensive of his subordinate groups is that which he denominates καλαμώδης,[1] which comes into Latin as Arundinaceæ; its type being that superb grass *Arundo Donax*. In an author so primitive one does not look for any rigidly formal diagnosis of a group. Calamodes in itself is diagnostic. It will include all plants that recall calamus, that is, *Arundo Donax*. In one place he has written that the leaves of this, and other things which he cites, seem to be made up of nerves only, comparing them with those of the grape and fig, which he says have not only nerves but also flesh and epidermis in their make-up.[2] The interpretation of this is that he has become aware of the differences of anatomical structure subsisting between the leaves of endogens and exogens. His group Calamodes—better written, after the usage of his Latin translators, Arundinaceæ—embraces *Arundo* and the cereals *Triticum*, *Secale*, *Hordeum*, *Oryza*, and others; that is, he names these as types. By their leaf characters all other gramineous plants cultivated and wild; known or unknown, fall within the lines circumscribing his Arundinaceæ. And what else besides the true grasses? In one place he names as among the arundinaceous certain plants the leaves of which in so far depart from the typical as to present an angular cross-section, and these seem to him as if made up of two leaves joined by their edges to something like a keel. It is the conduplicate and keeled leaves of such things as *Cyperus* and *Sparganium*, as any botanist would know from the

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[1] *Hist.*, Book i, ch. 10.

[2] *Ibid.*, ch. 17.

description of them, even without Theophrastus' having named those particular genera, adding that other denizens of marshy grounds have such foliage. Even the palms—of which he knew only *Phœnix*, pinnate-fronded—he cites by name as being arundinaceous.[1] Such reeds and rushes as to him seemed quite leafless would still vindicate to themselves places within the group by virtue of their altogether pithy or else hollow stems or culms. We are sure, then, that this assemblage of the Calamodes embraced all true grasses, all sedges, besides the juncaceous, typhaceous, sparganiaceous, plants, and palms. It will be observed that all are monocotyledonous; that the group embraces by far the greater proportion of such; only the showily spathaceous and the really petaliferous genera being left out. Recalling here the circumstance that in all early taxonomy roots figure very conspicuously, it becomes interestingly significant that into this great class of the Calamodes,—a group which, as regards the number of species, comes near being the equivalent of our endogens—not a genus is admitted that has either bulb or corm. Every member of the vast assemblage has copious fibrous roots; these in moiety of the species supplemented, as Theophrastus might have worded it, by that which he had chosen to name the jointed root, *i. e.*, the slender rhizome; a single sedge, *Cyperus esculentus*, bearing nut-like protuberances as if at the ends of some few of the root fibres. There is not the shadow of a doubt that this pristine plant anatomist and systematist recognized the structure of leaf-stalk and flower-stalk of *Arum* and *Colocasia* as at full agreement with that of half his Calamodes; and the same must have been familiar to him in the case of the grassy-leaved crocus and its allies; and the "roots" of these must have excluded them from taxonomic consociation with the rest, in all probability, even had their flowers been leafless and less in contrast to those of grass and reed and sedge. The aggregate of bulbous and cormose plants, the araceous I think excepted, were known and spoken of by him as βολβώδης—the Bulbaceæ. As a group it contained, first of all and typically, the onion and its several congeners, even the leek, a plant that though alliaceous is not bulbous; after these the bulbous Liliaceæ, Amaryllidaceæ, and the cormose iris allies. With the types and several of the species of all these he was familiarly acquainted.

There are other natural families not a few of which Theophrastus apprehended with precision, even assigning names to several of them. Such are the Umbelliferæ, for an example, to which as a

[1] *Hist*., Book i, ch. 16.

family he gives the name ναρθηκώδης, which in the Latin versions had to be written Ferulaceæ, from ferula—in Greek ναρθηξ, a stalk of fennel or of ferula—commonest umbellifers of the Mediterranean region. Plainly the thistles and their natural allies were accepted by him as constituting another such family group; for he often refers to them under the collective name of ακανθώδης, and twice mentions that all of them are prickly-leaved herbs whose flower consists of a head of florets, each floret sitting on the summit of a seed.[1] In Latin versions these are the Acanaceæ, from *Cnicus Acarna*, Linn., one of the most common thistles of Greece and Italy. Into this family Theophrastus admitted *Dipsacus*, the leaves of which are not prickly, as he concedes, but on account of its answering to the thistles in this, that its florets crown each its seed. And *Eryngium* also, and not unnaturally, with its spinescent foliage and capitate inflorescence, finds place among the thistles rather than with the umbellifers. The numerous cichoriaceous genera, with their peculiar habit, milky juice, and sameness of character as to flower and fruit, formed also a family with our Greek, who called it κιχωριώδης,[2] which the Latins wrote Cichoraceæ. These for examples of his having given to groups of genera class names and family names. Others need not be cited; but it should be mentioned that the family relationship of small groups of genera is in many an instance clearly seen by him when no group name is used. The pines, fir, spruce, and larch are discussed in a place by themselves; the various poplars, together with alder and birch, occupy successively another place, and the same is true elsewhere in the dendrological chapters of the book. The intimate relationship between the poppies and the pond lilies was so clearly perceived by Theophrastus that, while in general he seems to like to group things ecologically, the aquatics in chapters apart from mesophytes, he nevertheless proceeds without a halt from the papaveraceous plants of the grain fields to their kindred of the lakes and rivers.[3] He perceived upon them all, the marks of one and the same family.

It is again very interesting to note here and there a question raised as to the extent of some family; whether such or such a genus is or is not of the same family with such another, for example: "Some affirm that cucumbers and squashes are as closely interrelated as radishes and turnips are; others deny it."[4] The

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[1] *Hist.*, Book i, ch. 22.

[2] *Ibid.*, vii, ch. 11.

[3] *Ibid.*, ix, ch. 13.

[4] *Ibid.*, vii, ch. 4.

passage reveals gardeners and botanists of remote antiquity in debate about the affinities of genera; and the man whose word of authority might or might not have ended the debate, diplomat that he is, as well as philosopher, expresses no opinion; though none who have studied him well can doubt that he had one, and the correct one.

This outlining of families of plants and giving them family names entails one extremely important logical sequence, which one must not fail to indicate. His Arundinaceæ, Bulbaceæ, Ferulaceæ, Acanaceæ, Cichoriaceæ, and all the rest, as established on certain organologic characters, are each and all logically and completely subversive of that distinction which he formally keeps up, between things cultivated and things wild; for each such family necessarily includes both. The few historians who have not shrunk away from the time-consuming task of studying the Theophrastan volumes, have been perplexed by his seeming approval of ancient Hippon's theory about the origin of cultivated plants, which seeming approval is at once followed by a feeble argument or two against the theory. Here is what Meyer says, referring to the primary divisions of tree, shrub, half-shrub, and herb: "Each of these four is subdivided into the groups of the Cultivated and the Wild. Hippon's pronouncement, that every plant is at first wild, and then by cultivation made tame, is thus in a general way approved, though Theophrastus immediately adds that certain wild plants are not at all amenable to cultivation, while others take to it readily, whence it will follow that such a distinction is not altogether untrue to nature."[1] This historian's difficulty arises through his having missed two items important to the understanding of the man Theophrastus. First, that the illustrious Greek was as successfully a student of human nature[2] as he was an investigator of the plant world; and that he studied to avoid opposing with needless directness the prejudices of the multitude. If he should pay no respect to those time-honored categories of the tame and the wild, but should jumble them all together, and openly, forty-nine out of fifty among his readers would adjudge him not only a bold innovator, but perhaps also a godless heretic; for, as elsewhere intimated, the staple plants of agriculture, even in ancient paganism, were viewed as special creations of the gods—their immediate gifts to men. Old Hippon

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[1] Meyer, *Geschichte der Botanik*, vol. i, p. 162.

[2] See his *Characters of Men*, a work completed, as he tells us, in his ninety-ninth year.

the rhizotomos had been an outspoken heretic of this stamp. Theophrastus quotes his bold theory. In his secret soul he believes it sound; yet for the sake of avoiding scandal to the forty and nine or the ninety and nine, he veils his belief by admitting that some wild plants refuse to be tamed; a fact which may innocently be construed as against Hippon's idea. The other fact which the historian failed to apprehend is that the Greek outlines, and gives names to, a half-dozen or more of large natural groups, every one of them embracing without discrimination plants domesticated and wild. He thus completely nullifies that distinction, yet it is all so quietly, and as regards the superficial reading so covertly, done as to escape the notice of the forty-nine out of fifty of his readers; even also of our latter-day botanical historians, learned men and able, yet with mental vision impaired by the strong light of those typographic pedantries—convenient and helpful, certainly—which the botany of the nineteenth century had as a legacy from that of the eighteenth; affected by a sort of botanico-literary dysopsia which is slow to perceive that such a name as ferulaceæ is as perfectly the name of a natural family of plants as when printed FERULACEÆ.

The recognition of genera—using the term in a modern sense—is as informal with Theophrastus as that of families. However, when we come to the word itself, γένος, genus, it is employed variously—that is, with several different degrees of comprehensiveness. Indeed every natural group is with him a "genus," whether it be of the whole assemblage of herbaceous growths, or a family group, or a genus in our sense, or a species, or a variety merely. It seems to be the exact equivalent of our English expression "a kind"; and because such use of the word "kind" is not yet obsolete, at least surviving in rural districts, it will not be difficult to make plain its meaning. If a gardener or farmer of the present day mention to a botanist that he has in cultivation a strange plant of the squash, cucumber, and gourd kind, the latter understands perfectly that this is something belonging to the family of the Cucurbitaceæ, though he can go no further. But if it now be said in rustic phrase that the plant is something in between the squash kind and the gourd kind, he has used the word in a different and much more limited sense; for now the skilled botanist at once puts out of mind seven out of the eight primary divisions—subfamilies—of the cucurbits, or, in other words, dismisses from his thought, we will say, sixty or more of the seventy genera of this family; for he clearly understands the farmer to have that in view which must lie close to the two nearly related genera *Cucurbita* and *Lagenaria*. Now for a third manner of using the word "kind," with a most distinct third meaning, the farmer shall say that he has a new kind of squash. The botanist now has not the least doubt that the genus *Cucurbita* is meant; whether a species or a variety he can not tell; but the expression "kind of squash" at once translates itself into the school-taught expression, "species or variety of *Cucurbita*."

These three distinct old-fashioned uses of the word "kind" illustrate well the different ways in which ancient Greeks and Latins employed their word "genus." It is not a usage that makes for that perspicuity which a science calls for. For three meanings, three words are better than one. Nevertheless there is seldom room for doubt in Theophrastus' writings as to whether by "genus" he means such a group of species as we receive under that name, or a more comprehensive, or a less comprehensive group; any more than one well read in English fails to get the meaning of each of the uses I have brought forward of the equivalent word "kind." But the modern botanist who innocently should read into the term genus of an ancient author always the meaning which it has in modern botany would soon reduce his own mind to a state of utter bewilderment as to the ancient author's meaning. I have therefore been at the pains of making this attempt at an explanation. Upon this and many another important matter of terminology the historians have been silent.

Employing now the word "genus" quite as used in modern botany the genera of Theophrastus are numerous; most of them obtaining acceptance and holding their places in the systematic botany of the present, most of them also bearing the same names under which they were written about by him. This will be best shown by a few examples, which I select from under the letters A and C of any Latin index to his work:

Abrotonum
Acanthus
Aconitum
Ægilops
Agrostis
Aira
Alopecurus
Althæa
Anchusa
Anemone

Calamagrostis
Calamintha
Cedrus
Celastrus
Cenchrus
Cerasus
Ceratonia
Cercis
Chelidonium
Cissus

Anethum
Anthericum
Antirrhinum
Aparine
Aristolochia
Arum
Asparagus
Colutea
Coriandrum
Coronopus
Cratægus
Cycas
Cydonia
Cyperus

There must be more than a hundred Theophrastan genera the names of which are as familiar as these. A very considerable proportion of them were then known only as consisting of a single species, and are therefore of the kind which we speak of as monotypical; others were made up of from two to several, and the species are mentioned by name at least, when not described. If he establishes no new genus, and all of them which he enumerates or describes were of common recognition, and under those same names, even before he had penned a line on botany, this fact of itself will demonstrate anew the truthfulness of the proposition that the perception of genera and the naming of them are older than history, and that plant names, generic and specific, are a part of human language always and everywhere.

As to the grouping of his genera, almost the whole story has been told, at least by implication. There were the genera of trees, the genera of shrubs, etc., in places apart; and there were ecological groupings of wild plants and particular assemblages of genera of things cultivated in field and garden; as to these last, the mere retention of antiquated popular groupings which, in deference to the cultivators, he was unwilling to ignore or displace.

There occur in Theophrastus a number of passages which seem like forecastings of a system based more particularly upon flowers and fruits; a system the development of which was of course impossible then, or even at any later period preceding the invention of the microscope. But the very impossibility of his having been able to develop such a system is something which makes his few and faint adumbrations of it interesting and remarkable. I shall cite but two or three.

Commenting on the cylindric spicate inflorescences of certain cereals and grasses, he recalls that those of the plantains are so like them and even the flowers so similar, and thinks it might not be presumptuous to regard them as being interrelated.[1] To the average botanist of to-day the idea of any consanguinity as sub-

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[1] *Hist.*, Book vii, ch. 10.

sisting between *Plantago major* and an *Alopecurus* or a *Phleum* will seem crude enough; and this partly because parallel-veined leaves do not indicate to a certainty that a plant is an endogen and therefore more or less allied to grasses, and partly because we with our hand lenses and microscopes perceive between the small apetalous flowers of *Plantago* and *Alopecurus* marked differences that were impossible of discovery by Theophrastus. But the one thing noteworthy is that the Greek thus makes flower and inflorescence the criterion of natural relationship. He does not positively affirm it. It was but a pointed suggestion; and the suggestion passed unheeded during seventeen centuries.

The true hellebore and the veratrum are not more closely allied than are alopecurus and plantago, but as to their flowers and more particularly as to their follicular fruits, there is a strong likeness between them. It may have been this circumstance which, along with their powerful medicinal qualities, led to their being named as of the same genus, *Helleborus*.[1]

More signally indicative of his regard for fruit characters as sometimes taxonomically outweighing the vegetative, is the fact of his having associated the yellow water lily with the poppies, rather than with nelumbo. Having given account of the wild poppies of the grain fields and stony uncultivated lands, he who is so apt to treat of plants in ecologic groups proceeds now to speak of the poppy "that is called nymphæa."[2] Evidently its milky juice, the generalities of its floral structure, and above all the external form and the inner structure of the capsule, as well as the seeds themselves, constrained him to think of this and the poppies as congeneric. Also when this same chapter ends with an account of Aristolochia, the capsules of which are so much like those of poppies, one can assign no other reason for it than that by their fruits he guessed that Papaver and Aristolochia were interrelated. Other like instances need not be cited; though it should not here be lost sight of that his families, the Umbelliferæ and the Carduaceæ, were in his mind characterized each by marks of flower and fruit. And so, when the antho-carpologic doctrine of affinities is traced to its beginning, one no longer may think of it as having originated with Cesalpino. The idea had been suggested to his mind, and most impressively, by Theophrastus.

Nomenclature. No such thought as that of botanical nomenclature finds expression with the Greek. When he wrote of any

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[1] *Hist.*, Book ix, ch. 11.

[2] *Ibid.*, ch. 13.

tree or shrub or herb he used that name by which it was known in the everyday speech of Greeks. It does not appear that it ever occurred to him that a living thing, or any group of living things, required to be named otherwise than as commonly designated in his mother tongue. When in reading his books one encounters batrachium, erigeron, lithospermum, leucoium, myriophyllum, myrrhis, narcissus, and a hundred more as familiar, it is because he knew no other names for them. Nevertheless Theophrastus has great part in what is now come to be received as the scientific nomenclature of the genera and species of plants; and if this has come to pass without forethought or purpose on his own part, it is still natural and was inevitable. During some two centuries next succeeding the writing of it, this was almost the only treatise on botany that was extant, and the names of plants therein written about obtained by that very fact great prestige. When at length the Latins began to study plants, and would write about them, they had to learn Greek in order to be able to read the works of Theophrastus, for that was the one supreme treatise on plants. All well-known plants were therefore known to Latins by their Theophrastan and Greek names, as well as by their Latin names when they had such. Pliny, the supreme Latin writer about plants, in translating Theophrastan texts by the hundred into Latin for Roman readers, made use of familiar Latin names in place of the Greek names when there were such, *e. g.*, instead of the Greek itea he wrote salix; in place of drys, quercus; Latin ulmus, sambucus, and ranunculus in place of Theophrastan ptelea, acte, and batrachium. There were still many scores of plant types which were known to Latins by no other names than those that had been assigned them in Greek; another evidence that Theophrastus by his books had been the one teacher and authority upon botany to Latins as well as Greeks. Platanus, cerasus, rhamnus, anemone, thalictrum, delphinium, helleborus, pæonia, and a host of other such remained the only names of the genera, whether one spoke or wrote botany in Latin or in Greek; and so during some seventeen centuries most of the plant names in use were quoted from Theophrastus. The popular fable about Linnæus as first nomenclator of botany is not yet a hundred years old, and will need to be perpetuated for sixteen centuries yet to come if the years of his nomenclatorial fame are to equal those during which Theophrastus held the prestige.

Early in the sixteenth century, when new impulses moved men everywhere to scientific research, Latin had now long been the universal language of the educated. Theophrastus had been translated into Latin for the convenience of those who had not learned Greek; but still, as to botanical nomenclature Greek held its prestige fully. When in the course of their herborizings the botanists found plants in no wise answering to any descriptions in the ancient books, and therefore adjudged new and nameless, under the conditions then prevailing it would have been the most natural thing in the world if all new plant names of the period had been made in Latin; and this indeed often happened, *e. g.*, *Pulicaria* and *Fragaria*, Brunfels (1531), *Digitalis*, Fuchs (1542), *Sanicula*, Tragus (1552), *Bidens*, Cesalpino (1593); yet Latin names for new genera are somewhat exceptional even for that period, Greek-made names being commonly preferred. The reason was simply this. The greater proportion of plant names then in use, even in Latin botany, was Greek, and that by unbroken tradition from the Greek father of all botany; and Greek-made names for new types were more in harmony with the general tone of botanical nomenclature than Latin names. Thus has it come to pass that even down to our twentieth century the favorite etymology for new generic names is Greek. Such very modern names as *Calliandra*, *Chimonanthus*, *Chionanthus*, *Chionophila*, *Chionogenes*, *Epigæa*, and hundreds like them, all very modern, attest the perpetual influence of Theophrastus upon botanical nomenclature.

In botany as elsewhere the genus presupposes species. A genus may consist of many species, of few, or of one only. Theophrastus had very many monotypic genera, at least as they were then known. The specific representative of a monotypic genus has with him but one name, commonly a one-worded name; that is, the one species constituting such genus lacks a specific name. It really has no need of any. Where there is but one thing of a kind, there is never in ordinary speech a second and qualifying name. If neither men nor things existed but in monotypes, language would not need adjectives, and there would be none. Had there been but one race of men on the earth, the name of that race would have been man simply, and the adjectives Caucasian, Mongolian, African, etc., would not have existed. The Theophrastan nomenclature of plants is as simply natural as can be imagined. Not only are monotypic genera called by a single name; where the species are known to be several, the type species of the genus—that is, that which is most historic—is without a specific name, at least very commonly, and only the others have each its specific adjective superadded to the generic appellation. The situation may best be shown by examples. In giving these it seems advisable to present the Theophrastan Greek names in Roman type.

Theophrastan	Modern
Clethra	Alnus oblongata.
Melampyron	Melampyrum arvense.
Dolichos	Phaseolus vulgaris.
Ostrya	Ostrya vulgaris.
Peuce	Pinus picea.
Peuce Idaia	Pinus maritima.
Peuce conophoros	Pinus pinea.
Peuce paralios	Pinus Halepensis.
Mespilos	Mespilus Cotoneaster.
Mespilos anthedon	Cratægus tominalis.
Oxyacantha.	Mespilus Pyracantha.
Syce Idaia	Mespilus Amelanchier.
Aria	Cratægus Aria.
Cydonion	Pyrus Cydonia.
Coccymeles	Prunus domestica.
Spodias	Prunus institia.
Cerasos	Prunus Cerasus.
Pados	Prunus Padus.
Oié	Sorbus domestica.

The first four names above are those of genera known to Theophrastus as consisting each of a single species. It is evident he saw no occasion for any second and qualifying name in any case of that kind. To have given such second names would very certainly have exposed him to the criticism of having abandoned the attitude of the philosopher, the man of literary taste and scientific brevity, and having assumed the rôle of the pedant. Why do botanists of a recent time invariably append the needless second name to every monotype? I ask the question but to emphasize this point in the history of biologic nomenclature. I recall no instances of the assigning of the useless specific adjective to a generic monotype until well toward the time of Linnæus; and despite the weight of his authority in favor of it, the nineteenth century was on the dawn when there were no longer eminent botanists standing out against the practice. The assigning a species name in these instances is, of course, previsional. The monotypic genus may cease to be such; but even then, according to Theophrastan usage, the generic name alone might stand as that of the original and typical specific member; but that is too provincial. Theophrastus' work, in as far as relates to taxonomy and nomenclature, is provincial; not a universal flora, but a somewhat local one. It was not a perfect pattern for the universal. There is also a certain lack of uniformity in a system of nomenclature which fails to provide every species with a distinctively specific name; and it was nothing more than the desirability of uniformity which brought about the modern usage. But, as we shall see, this question was long in controversy, and was settled late.

Matters of nomenclature and taxonomy are almost inseparably connected. The name itself is but the expression of a taxonomic idea. Excepting those rare instances in which an individual historic tree has received a proper name, every plant name that ever was, in any language, is the name of a group. Naming is classifying. The Theophrastan names for pomaceous and drupaceous genera have been above placed in close succession, and opposite them Linnæus' disposal of the same type. By comparing those several names it is readily seen that the eleven species are distributed to five genera by Linnæus, whereas by Theophrastus they had represented nine. In what may be called the average twentieth-century classification of them,—as far as the century has proceeded,—the same eleven species are ranged under about eight genera, namely Amelanchier, Cotoneaster, Cratægus, Cydonia, Sorbus, Cerasus, Padus, Prunus. If we of the present are correct, the mean between Theophrastus and Linnæus is the happy one; and this in any case must be admitted, that every revulsion against the Linnæan taxonomy of these fruit trees is a step in return toward the Theophrastan. The same sort of departing and then returning finds illustration in the naming of water lilies as that was done formerly, and has been done over again of late.

Theophrastan	Linnæan	Recent
Nymphaia	Nymphæa lutea	Nymphæa lutea.
Sida[1]	Nymphæa alba	Castalia alba.
Lotos	Nymphæa Lotus	Castalia Lotus.
Cyamos[2]	Nymphæa Nelumbo	Nelumbo speciosa.

The Greek, it is thus seen, received the four species as representing each a genus. With Linnæus the genus of them all was one; while recent systematists have well-nigh completely returned to the Theophrastan view, in all save the names of the genera; and the

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[1] Linnæus, suppressing the white water lily genus, daringly transferred its name to that of a genus of insignificant malvaceous weeds.

[2] Sir J. E. Smith, most ardent Linnæan though he was, restored *Cyamus* instead of *Nelumbo*, insisting on its right of priority.

restoration of even these will follow, under the law of priority.

The shortcomings of Theophrastan nomenclature as to uniformity have not all been indicated. There is one other. While something like half his plant species have but a single one-worded name and that the generic, there are not a few of his genera that are invested with a double—that is, a two-worded name. It is highly important that this be fully understood. This kind of name is frequent with every botanical author that I am acquainted with, between Theophrastus and Linnæus; and with this fact overlooked there is no understanding any single pre-Linnæan author's plant names either generic or specific. Nor have I found any writer of botanical history making so much as a passing reference to this. I subjoin a very few such Theophrastan genus names as samples; giving, as usual, their equivalents, with also the specific names as now in use.

Theophrastan	Modern.
Calamos Euosmos	Acorus Calamus.
Dios Anthos	Agrostemma Flos Jovis.
Dios Balanos	Castanea vesca.
Carya Persica	Juglans regia.
Syce Idaia	Amelanchier vulgaris.
Ampelos Idaia	Tamus communis.

Most of the names in the left-hand column have exactly the form and structure of ordinary generico-specific binaries, one term being a noun, the other a qualifying adjective. Their respective equivalents placed over against them demonstrate beyond cavil that these particular binaries are not of the usual meaning of such two-fold names, but are purely generic. To take up the first on the list: Theophrastus has a genus Calamos, the great reed-grass arundo its type, phragmites also being included in the genus. It is not imaginable that a botanist of Theophrastus' ripe experience and great attainments should think those large grass-plants and the sweet-flag to be of the same genus. Beyond doubt, however, that name Calamos euosmos did originate in the notion that arundo and acorus are next of kin; for, however unlike they are as to size, foliage, and other particulars, there is a remarkably close similarity in their rootstocks, these being of almost the same size, form, and color in the two. The gatherers of roots and herbs, as we know, looked first of all to the "roots" of things, and these were their first criteria of plant relationships. To these it would be perfectly natural to place the sweet-flag alongside arundo, the true *κάλαμος*, by its closely imitative "root," and then on account of the aromatic properties of that root to call the plant *κάλαμος ἔυοσμος*. It is equally incredible that Theophrastus should have adjudged the service tree which grew on Mount Ida to be a kind of fig tree. The country people who found the sweet fruits in some way suggestive of figs must have been the creators of that name *συκῆ ’Ιδαῖα*. But the author who desires to conciliate the public will use great reserve in the matter of suppressing, altering, or even amending established and familiar names; and Theophrastus left plant nomenclature as he found it. And what reasonable objection could have been raised against such binary generic names? It can hardly have entered his mind that it made any difference whether such name were one-worded or two-worded. Outside the domain of our Latin-worded technicalities it makes not the least difference to any of us to-day how many words go to the making of a generic name. Ivy is a generic name, and as certainly such are Ground Ivy and Poison Ivy. Pine, Ground Pine, and Princes' Pine are names of three genera in no wise interrelated. The same may be said of the five following, Pink, Moss Pink, Squaw Pink, Mullein Pink, and Pink Root. We having no fault to find with such generic names as Star of Bethlehem, Lily of the Valley, Grape Hyacinth, Jerusalem Artichoke, Indian Turnip, American Cowslip, and some scores of others like them. We are a living illustration of Theophrastus in this regard, except that we have two languages for our botany, whereas he had but one. We have two languages in which we use botanical names, with a separate set of rules for each. Into our Latin nomenclature we do not admit any of these two-worded generic names which we use so freely and so readily in our vernacular. In this we differ from a very long and illustrious line of our own botanical ancestry. It is less than two hundred years since what we know in English as Dogtooth Violet and in Latin as Erythronium was in all Latin botany the genus Dens Canis, Taraxacum was Dens Leonis, Convallaria was Lilium Convallium, Glechoma was Hedera Terrestris, Helianthus Flos Solis, Drosera Ros Solis, Centaurea was Centaurium Majus, and the little gentianceous genus Erythræa was Centaurium Minus. By the same token, Chelidonium was the genus Chelidonium Majus and Ficaria was the genus Chelidonium Minus. In a word, Latin botany for more than seventeen centuries admitted two-worded generic names as freely as the simpler kind; and all after the example of Theophrastus and the prehistoric nomenclators.

This is not the place in which to give the history of the elimination of binary generic names from Latin-written botany. But it was imperative to show that such names are common with Theophrastus, Dioscorides, Pliny, and thenceforward for well-nigh two thousand years. Without knowledge of this fact the ancient names can not be understood. Without understanding of names as applied during such period, its taxonomy is an enigma, and the setting forth of the history of taxonomy is impossible. To read into that ancient Greek name Aster Atticus, as has been done lately, the character of an ordinary binary plant name of the nineteenth century,[1]—to fail to recognize in that a mere two-worded generic name—seems to evince a condition of bewilderment as to the whole subject of botanical nomenclature with ancient Greek and later Latin authors. That the term Aster is generic and Atticus specific can not well be believed to have been in the mind of Dioscorides; for the genus was monotypic, and they did not then give specific names to monotypes.[2] Nevertheless, in genera of several species generico-specific binaries quite like Aster Atticus as to form were very frequent with Theophrastus; though he applied them to species and to varieties indiscriminately,[3] as the subjoined Latin versions of some of his binary names will show:

Salix alba
Salix helix
Salix nigra
Papaver rhœas
Papaver nigrum
Papaver corniculatum
Origanum album
Origanum nigrum
Origanum creticum
Origanum heracleoticum

Triticum agrigentinum
Triticum africum
Triticum assyricum
Triticum egyptium
Triticum siculum.
Triticum thracium.
Olea domestica.
Olea silvestris.
Phlomis alba.
Phlomis nigra.

Ecology. Among forecastings of method with Theophrastus, that of the natural associations of plants in particular places is very definitely presented. That which we have learned to designate as the ecologic he accepts as being a very natural kind of grouping. "All are distinguishable as either terrestrial or aquatic, just as we also primarily distinguish animals; for there are some

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[1] E. S. Burgess, *Memoirs of Torrey Club*, vol. x, (1902), pp. 57 *et seq.*

[2] Sixteenth-century botanical scholars knew this well. Cesalpino wrote the name always as one word, Asteratticus, or else Asteracticus.

[3] It was the usage of nearly all authors down to and including Tournefort in the year 1700. A binary name meant either a genus, a species or a variety.

plants which grow nowhere but in the sea; small ones in our sea (*i. e.*, the Mediterranean), larger ones in the Red Sea. Others affect only marshes or other very wet places. Some can not live in very wet ground, but restrict themselves to dry ground. Certain others are littoral only. A few trees thrive in either moist land or dry; such are the myrtle, alder, and willow."[1] In another place, having wild trees and the diversity of them under consideration, he affirms that they differ according to the different nature of the localities in which they grow. "There are lands that are overflowed by water, there are marshes, and there is dry ground; there are rocky places and smooth pasture lands and harder soils, besides other diversities. There are depressions in the landscape where all is tranquil, and there are elevated and wind-swept exposures; which varied conditions tend to the production of many different things."[2] Hereupon follows a considerable catalogue of trees which in Macedonia he says occur nowhere but in the mountain districts: fir, wild pine, spruce, holly, linden, hornbeam, beech, box, arbutus, juniper, yew, wild fig, alaternus, phillyrea, walnut, chestnut and holly-leaved oak. Then there is given a list of such as are common to mountains and lowlands: tamarix, elm, poplar, willow, cornel, alder, oak (*Q. robur*), wild pear, wild apple, privet, hop-hornbeam, ash, hawthorn. As to these denizens of both highland and plain he says that "in general they are of larger dimensions and more comely form on the plains, but of better timber and better fruit in the mountains. To this rule the wild pear and wild apple are exceptions, both being of better timber and better quality of fruit on the lowlands; for in the mountains the trees are gnarled and thorny. Even as to the peculiarly montane sorts of trees, those inhabiting the lower valleys are both the largest and the most copious; and on the highest summits everything is in its most stunted condition, excepting such as by nature require the cold."[3]

Reminding ourselves of this, that Theophrastus treats mainly of field and garden plants, giving much less space to the uncultivated, it becomes particularly noteworthy that in one place eight successive chapters are given up to locating and describing aquatic and other hydrophilous growths;[4] all of them, of course, wild plants. Without any formality of naming the distinctions, yet in practice

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[1] *Hist.*, Book i, ch. 7.

[2] *Ibid.*, iii, ch. 3.

[3] *Ibid.*, iii, ch. 4.

[4] *Ibid.*, iv, chs. 7–14.

he distinguishes (1) marine aquatics, (2) marine littoral plants, (3) plants that grow in deep fresh waters, (4) plants of shallow lake shores, (5) those affecting the wet banks of streams and rivers, (6) and those of marshes. His marine herbaceous plants are mostly algæ. The submerged trees, resembling in mode of growth and branching oaks, fig-trees, the palm, and the vine, and attributed to the Red Sea, are mostly corals; organisms that were still considered to be plants, denominated lithophytes, until within the last two hundred years. The author, in describing the marine oak, marine fig-tree, etc., is careful to inform his readers that their resemblances to the trees of the land are only those of mode branching; that they are smaller than their terrene analogues and have no leaves. In stagnant fresh waters several cubits deep thrive nelumbo, nymphæa and trapa. Along the shores, in shallow water are reeds, rushes, the papyrus, sparganium, and typha. The banks of running streams suit the poplar, alder, and willow, the roots of which only are laved by the flowing waters. In wet sandy soil not far from streams thrives the cyperus with nut-like edible roots.

That ecology should have formed a sort of taxonomic basis for Theophrastus in his treating of wild plants was most natural. Such pronouncedly hydrophilous growths as reeds and rushes, coarse sedges and the largest grasses, phragmites and arundo, besides typha and sparganium—all are at agreement not only ecologically but in many respects also morphologically. They all have upright, smooth, and simple stems, filled with pith when young, some of them hollow when mature, but none ever woody-solidified; their foliage long, narrow, entire, never with any trace of the network of veins. Moreover, every one of the group was what would have been called flowerless, by all save Theophrastus and his students; because they had no flower-leaves. Other aquatics, such as nelumbo and the water-lilies, colocasia and sagittaria, were like the rest structurally except as to foliage, the leaves being large and ample, rounded rather than narrow-elongated, with radiating veins if any, and some of them with large flowers made up of showy leaves. The Greek, I say, might have written such a diagnosis of his aquatics as a group viewed morphologically. That he saw these marks none will doubt who reads his descriptions of the species.

In the vegetation of the mountains he again lists the trees that grow on the exposed and sunward slopes, and those that flourish nowhere but upon cold northward declivities, and such as inhabit only the frigid summits.

Dendrology. Almost an undue proportion of Theophrastus' space is given to the consideration of trees. He appears to have been a great lover of them; and his knowledge of them is presented with so much system that at least a considerable dendrological paragraph becomes a necessity if one is to convey any adequate notion of his botanical work as a whole. He classifies them (1) as cultivated and wild. This is one of his general divisions of all kinds of plants; one that has already been sufficiently discussed in another place. Trees are (2) deciduous or evergreen. Their diversity as to tenure of foliage is so thoroughly discussed, and withal so judiciously, that the more than two millenniums that have passed seem to have recorded but few and unimportant additions or amendments to the principles of this chapter as he left it.[1] Adhering to his classification of all things as cultivated and wild, he gives two lists of trees that are evergreen; the olive, palm, sweet bay, myrtle, the cypress, and our pine among the domesticated; for the wild, the fir and spruce, wild pine, certain kinds of oak, holly, box, and the arbutus tree. The last, he says, sheds the foliage from its lowest branches, while the head of the tree remains evergreen. This distinction of evergreen and deciduous he regards upon the whole as quite natural and valid, despite reports he has heard, and readily accepts as probably true, that in the warmest climates grape vines and fig trees shed their leaves so tardily as to seem almost evergreen. He has observed that, among perfectly familiar species, some regularly divest themselves of all foliage in earliest autumn, others later, while a few habitually retain it until winter has begun. He can therefore credit those who assert the existence, in other lands, of kinds that do not cast their old leaves until near the time of the development of the new in spring. He has even found out that the most strictly evergreen develop one new set of leaves, and as invariably lose one old set, every year. Upon deciduous trees and shrubs he seems to have kept phenologic records. He relates that certain kinds come into leaf early, others late; also that such as are first in leaf in the spring are not the first to shed their foliage in autumn, and it is equally established, that those latest in leaf do not retain them longer than others. He has likewise learned that trees in a moist climate and soil retain their foliage for the longest period, while deciduous things of a dry soil and poor shed their leaves earliest of all; and finally, that a young tree keeps its foliage until a later date than does an old one. The foliage of evergreens is usually narrower

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[1] *Hist.*, Book i, ch. 15.

than that of deciduous trees, is of a firmer texture, and in a much greater proportion of species is fragrant or aromatic.

In respect to their modes of branching, trees are (3) regular or irregular. If at any time before having studied Theophrastus I had been asked who first taught the distinction between the excurrent and deliquescent in the forms of trees, I should have attributed it by guess to some dendrologist of about the middle of the nineteenth century whom I could not name. Therefore nothing that I have come upon in this author of antiquity has more surprised me than his lucid setting forth of these two modes of tree development. The differences seem to have forced themselves upon his notice while studying the fir tree. At least, he gives fir as the best type of what we call the excrescent; and the oak is his example of the other mode as to branching. The distinction (4) of flowering and flowerless in trees did not imply the recognition of such as in modern botany are called cryptogamous. It was but a matter of the author's success or failure to find what he would have allowed to pass for flowers. And the classifying them as (5) fructiferous and sterile is not at all the equivalent of the flowering and flowerless division. There is one kind of sterility that is manifestly accidental only; a consequence of something unfavorable in the environment. The palm is sterile in Greece; yet if transplanted to Babylon from Greece, it becomes as fruitful as the Babylonian.[1] Peach trees are sterile in Egypt. The wild sorbus of Greece, transplanted from its mountain habitat to the fervid low country, though flowering copiously in the new situation, never fruits there. The reason is plain to him. Its nature requires the cold climate of the mountains. But when he alludes to the black poplar of the island of Crete as sterile when introduced on the mainland, one may suspect that to have been owing to the possible circumstance of only male trees having been brought over. I do not think Theophrastus ever suspected the fact of diœcism in any plant or tree, however often we may find him speaking of them as male and female. Of this I have more to say in another place. Again individual trees according to their species have (6) certain of their branches fructiferous, certain others always sterile.[2] He has observed that some, like the vine and the fig tree yield fruit on no branches but the newest, those of the season; that almond, apple, pear trees, and many more fructify upon no other branches than those that are one year old;

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[1] *Hist.*, Book ii, ch. 3.

[2] *Ibid.*, i, ch. 23.

some like the carob (*Ceratonia Siliqua*, Linn.) produce their fruits both upon those of this season and the season before, with also a few at the same time upon old and thick branches. Some kinds are fruitful on none but their topmost branches; others are fruitless at summit, and only fruitful on lateral branches. Another distinction is that of (7) the location of fruits in respect to foliage. Some trees bear their fruits beneath the leaves; on others it is borne above them; while in some, like the sycamore fig tree (*Ficus Sycomorus*, Linn.), it grows down on the naked trunk.[1] From our point of view this is a useless distinction; but not so with Theophrastus, who seems to have been unable to attribute to the foliage of trees any more important function than that of a protection to the young and growing fruit or seed.

Trees are extensively treated of (8) as to their ecology and geographic distribution. There are trees peculiar to mountain districts, and others confined to lowlands and plains. As of the former habitat he names the fir, wild pine, spruce, holly, box, walnut, chestnut, and many more. A still greater number of different kinds are of the plains only; among them are one of the elms, the ash, maple, alder, willow, poplar. A few kinds are common to mountain and plain.[2] Among the montane some, like the wild pine, luxuriate on slopes that look southward, and will hardly grow at all in any other places, while the fir, on the contrary, attains perfection on the cool and shady sides, and if ever seen elsewhere, has an inferior growth and is unlike itself. The tallest and largest firs known occur in a deep valley in Arcadia where they say the sun never shines. He notes it as a general rule that the kinds of tree affecting shady and cool places are tall and straight, their trunks not forking or parting into subsidiary trunk-like branches; but that arboreal growths of this latter description are those of open and sunny places. Certain trees are wont to grow nowhere but along watercourses; and certain others belong exclusively to the highest elevations of the mountains near perpetual snow.

It is evident that in those coniferous and hardwood trees belonging to cold northward slopes of southern mountains Theophrastus sees a sort of fringe, so to speak, of the great almost unknown regions of Europe northward; for what reports have been brought from that direction indicate that there, even the lower lands are clad with forests of fir, pine, oak, chestnut, and others known at the South only on the mountains; and he thinks it may be reasonable

[1] *Hist.*, Book i, ch. 1.

[2] *Ibid.*, iii, ch. 4; also iv, ch. 1.

to infer that the whole North has no other silva than that thus indicated. He has not been able to learn that it has kinds of trees peculiarly its own.[1] Southward, however, across the Mediterranean, and away up the Nile, are very different kinds of trees; many that can not be successfully transplanted from that dry and heated climate into regions where there are rains and cold weather at the winter season. There, in some localities where it never rains, the palms attain their greatest dimensions and their best quality of fruit; not, however as indicating that they have no need of moisture. On the contrary, wherever a grove of wild palms occurs water is sure to be found at no great distance below the surface of the ground, though it is usually subsaline, a circumstance which, he says, has taught the cultivators to use a little salt with advantage in growing dates in other than their native soil.[2] In Phœnicia and in Syria there are various kinds of palm; because these like other trees differ according to differences of region and climate as well as according to the culture given them. The palms as a group interest him deeply; they are in many ways so very unlike other trees, in their best known type bearing every thing—leaves, flowers, fruits—in a single terminal tuft, the caudex being without a branch. Now, with Aristotle, father of biologic investigation, and with those of his school, there was much and serious inquiry into the question of a soul, and some particular seat of life in plants. The latter was hard to locate; so many are the trees which, as susceptible of propagation by mere cuttings, thereby proclaim it that their seat of life is everywhere, so to speak. But these palms were different. Cut off the leafy and fructiferous summit of the tree and the whole is killed, just as one kills an animal by decapitation.[3] He was near thinking that in this kind of tree that one terminal part is the seat of life; but he knows of a smaller palm, native to the islands of Sicily and Crete (*Chamærops humilis*), which, if the top be removed, or if even the whole tree be cut down to the ground, renews itself by shoots from the root. He has grown from seed the few kinds of palms available at Athens, knows all about their germination and early stages of development; finds no distinction of bark, wood, and pith in the structure of their trunks;[4] recognizes in them the only

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[1] *Hist.*, Book i, ch. 6.

[2] *Ibid.*, ii, ch. 8.

[3] Theophrastus had heard that at Babylon there were palms of some sort the top of which could be made to root and grow again. *Hist.*, Book ii, ch. 2.

[4] *Ibid.*, i, ch. 9.

plants of which he dares to say that they exist in the two sexes, a male that flowers and is fruitless, and a female that is flowerless but bears fruit[1]; but a special seat of the vegetal soul, or life, evades him even here. That he admits trees into the alliance of the palms on vegetative characters alone, when the fruits are not in the least date-like, is seen in two instances. One such is so described as to have convinced some authorities that what he had in view must have been the cocoa-nut palm of the farther Indies; but it is now no longer doubted that it is the *Hyphæne coriacea*, and not *Cocos nucifera*. He also knew, and well described the type of the Cycadaceæ, *Cycas circinalis*, as a kind of palm.

Theophrastus, like an ancient Humboldt, or Grisebach, takes pleasure in making comparisons between certain of those trees of arid northern Africa and certain others of southern Europe with which all his readers are well acquainted. There is the persea, as he calls it (*Cordia Myxa*, Linn.), which in some ways suggests to him the pear tree, a large and very handsome tree, in its mode of branching, its foliage, flower, and fruit externally resembling the pear; but it is evergreen and ripens fruit at all seasons, the fruit however possessing a nut at its core like that of a prune, etc.[2] There are other Egyptian trees so unlike any known to his untravelled countrymen that he can not contrast them with any familiar kinds; but the competent botanist of to-day will recognize the genera and species of some of them by his descriptions. About Memphis are trees frightfully armed with thorns in every part except the trunk. It is the arid subtropic region of several gum-bearing acacias and their allies. He attributes to all of them the leguminous fruit, καρπὸς ἔλλοβος, says the pods are gathered and employed as a substitute for galls in tanning leather, and also used medicinally. One kind he calls white thorn (*Acacia Senegal*, Willd.). To this he attributes flowers beautiful and fragrant, so that they make garlands of them. Another he denominates black thorn (*Acacia Arabica*, Linn.). Quantities of gum are gathered from this kind. It exudes from the trunk where incisions have been made, or even spontaneously without incision of the bark. In the vicinity of Thebes there are extensive forests of these trees, and that far away from the river, where they are never irrigated. Such pen pictures of foreign dendrologic scenes are not rare in Theophrastus; and they are always so vividly drawn that the reader inevitably thinks of him as writing from the very midst

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[1] *Hist.*, Book i, ch. 22; also Book ii, ch. 8.

[2] *Ibid.*, iv, ch. 2.

of the things he is describing, whereas it is quite certain he is only compiling from travellers and historians. To this picture of Egyptian dendrology he will add yet another member of this same alliance of trees, and this the most remarkable one. Forbiddingly spinescent or thorny like the others, it has a more delicate foliage, like that of a fern. Whenever a branch of this tree is disturbed by a touch, all the leaves upon it seem very suddenly to wither away and collapse; then after a little time they revive and return to their former condition. This quaint description of what has been called sensitive foliage is probably the oldest extant. The species that was described by Theophrastus is doubtless *Mimosa polyacantha*. These and as many more kinds of tree and shrub he mentions by name and short description as "peculiar to that region."

In succeeding chapters he dwells at some length upon the ligneous growths of the Arabian deserts, where it rains no oftener than once in four or five years, and trees are scarce and all of them spinescent[1]; and describes the varied and often luxuriant silva of the more distant Indies.[2] It is to be remembered here that Theophrastus was contemporary with Alexander the Great, whose expedition to the farther Orient was the first of its kind in all history to include among its officials learned men whose duty it was to write up the geography, climatology, and even the zoology and botany of the regions traversed; an enlightened thought of Alexander's, beyond doubt suggested by his boyhood's illustrious tutor, Aristotle, father of all nature study.[3] To the manuscripts brought back by this scientific staff of Alexander, Theophrastus was indebted for all that he knew of the farther Oriental plant ecology and geography; and all that remains of those reports is what the philosopher quoted from them. The originals were long since lost. In these chapters of Theophrastus we have the earliest, and very interesting and faithful accounts of the banyan tree (*Ficus Bengalensis*, Linn.), citron (*Citrus medica*, Risso), the cactus-like euphorbia (*E. antiquorum*, Linn.), the oleander (*Nerium Oleander*, Linn.), the tamarind (*Tamarindus Indica*, Linn.), a tree which they reported to possess the singular faculty of folding up closely its pinnated leaflets at nightfall and going to sleep for the night;

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[1] *Hist.*, Book iv, ch. 8.

[2] *Ibid.*. ch. 5.

[3] See Bretzl, *Botanische Forschung des Alexanderzuges;* also an excellent abstract of the same by Dr. F. Fedde in *Abhandl. des Bot. Verein Brandenburg* for 1903, pp. 97–109.

the earliest record in botany of the phenomenon now known as nyctotropism.

The *Historia Plantarum* is not without its chapters on the diseases of trees, and the influences of the seasons upon them[1]; the proper seasons of the year for felling timber of different kinds[2]; qualities and special uses of wood of different trees[3]; but space must not here be given to comment on these economic aspects of the subject. A more philosophic interest attaches to questions of longevity in trees.

Under the caption of duration (9) he records a number of curious facts, and discusses briefly a question that may arise in reckoning the age of certain trees. He certifies that it is common with orchardists and vine growers of his time to renew, as it were, an old and moribund tree by cutting it down near the ground, and then training up in its place one of the new shoots that are thrown up from the base of the stump. How, he asks, is the duration of the tree upon this spot to be estimated? Do the old and new tree constitute two individuals or only one? If the main trunk be essentially the tree, then the new trunk is that of a new individual; and he adds that the very roots of the original tree perish, eventually, and that the new one now has none other than its own.[4] Yet individual grape vines the continual growing and fruiting of which during two centuries is perfectly authenticated have in this way been renewed by the cultivator's art, several times over within the two centuries. He finds it a prevalent opinion in the rural districts that all wildwood trees are long-lived and all the domesticated of short duration. This the philosopher does not think well grounded. It is true only in a very general way, and with many exceptions. Some kinds of forest trees live very long, others do not; and the same may be said of the domesticated, though these, upon the whole, have a shorter period. And, universally, those that fruit copiously have a shorter time of life than the unprolific; also the kinds of wild trees that affect low and wet land are shorter-lived than those occupying dry and barren ground. Even sweet-fruited and aromatic trees live longer than the sour-fruited kinds, he has observed. On the reputed great ages of certain individual trees still living in his day as well as carefully preserved and religiously venerated—such as the olive tree at

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[1] *Hist.*, Book iv, chs. 16, 17.

[2] *Ibid.*, Book v, ch. 1.

[3] *Ibid.*, chs. 2, 3, 4, 5.

[4] *Ibid.*, Book iv, ch. 14; *De Causis*, Book ii, ch. 15.

Athens said to have been planted by Hercules, and the Caphian and the Delphic planes, both believed to have been planted by Agamemnon—he is somewhat incredulous. The traditions as to their origin have come down by some who also wrote fables. It might be well to investigate. He thinks it quite certain that olive trees and planes, as well as many other kinds, live a very long time. Beyond this platitude he will not go; but it is manifest that he is not in sympathy with the mind of the credulous multitude as to the extreme age of this or that individual and historic tree. He was rather skeptical on the subject, and probably would not have believed it possible that northward in Europe far beyond the Mediterranean oaks sometimes lived through ten or a dozen centuries; nor that on another and unknown side of the world there were conifers[1] of considerable dimensions then living which would be flourishing still, after the passing of twenty-two or three hundred years.

Transmutation. In this twentieth century of our era there are farmers in the world, and not unintelligent, who believe that to some seed of wheat or barley after it has been sown in the field something may happen by which it comes to sprout and grow up into a plant of what they call chess, or cheat; a plant known to botanists as *Bromus secalinus;* this name itself now apparently destined to perpetuate forever that old opinion—older than history, no doubt—that a grain of barley, *secale*, may become the parent of a plant of chess.

The seemingly indicative facts upon which this transmutation theory appears as if it might have established itself in the minds of prehistoric grain growers were several. Neither chess nor darnel grew commonly elsewhere than in the low wet parts of grain fields. In these spots only very few, depauperate, and almost infertile were the stalks of wheat or barley, though the seed of one or the other had been sown there copiously. The explanation which a very primeval and elementary philosophy could offer was, that the grains of wheat, debilitated to the verge of decay by unusual cold and dampness, became unable to generate a better plant than the small-grained and worthless chess, or cheat, as the farmers still call it. Even Nature herself had taught them with what ease she can, and how every year she actually does effect more marvellous transformations, at least in the animal kingdom. The fish-like creatures that swim about in pond and pool in the spring

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[1] The conifers were favorite subjects of study with Theophrastus; and if some of our Sequoias are rightly estimated to be twenty-five centuries old, they were not small trees in our philosopher's time.

months, they had seen come forth in summer, changed to such things as frog and toad. In their simple life, lived very much out of doors, the observing and intelligent had taken note of how underground grub and tree caterpillar by and by assume a larval state, rest in that for months together, and then suddenly are born again, the one as beetle, the other as butterfly. In his environment, the primeval nature student would not doubt the easy possibility, or the apparently strong probability, of some kind of sudden transformation in the world of plants; and the well-known frequent appearing of chess in wet ground instead of wheat where only wheat had been planted might be evidence enough of such transmutation.

That the supreme philosopher of antiquity, the father of animal biology, who knew so nearly everything about metamorphosis in lower animals, also must have investigated the case of the supposed transmutations of plants, appears most probable. For this department of botanical history it may be thought particularly unfortunate that Aristotle's botanical writings have not survived.

Theophrastus does not formally and didactically discuss this question, though he makes a number of references to this changing of one plant into another as something universally believed in his day. I shall reproduce a number of them.

Recording in one place the usages of his time as to the different seasons of the year and the several methods of sowing cereals, as well as giving a long list of leguminous plants that he names, he concludes the chapter with a remark like this: "None of the above are liable, on account of a bad condition of the seeds, to change into other plants except wheat and barley, which people say may change into darnel (lolium); more particularly wheat, and this being said to occur as the result of wet weather, and in muddy places of the fields."[1] In the same connection he records it that "Some think flax also changes into darnel."[2] Quotations like these read much as if the author had been unwilling to take the responsibility of either affirming or denying the proposition. "People say" that such metamorphoses occur. But in another paragraph, one relating to different kinds of wheat as imported into Greece from other parts, he affirms that from Pontus, from Egypt, and from the island of Sicily grain-growers of his time obtain seed wheat which matures crops free from lolium; though that from Sicily comes up infested by a different weed called melampyrum,

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[1] *Hist.*, Book viii, ch. 6.

[2] *Ibid.*, ch. 7.

i. e., black wheat.[1] In this report about certain of the imported seed wheat as immune from changeability into lolium, one almost reads between Theophrastus' lines that he regarded the absence from or presence of darnel in wheat fields to be due to the absence or presence of darnel seed in the seed wheat at its sowing. There is another passage in which the philosopher by implication seems to question the wheat-darnel metamorphosis. With the ancient husbandmen as with the modern it was usual to sow wheat either in autumn or in spring. Theophrastus records this, and also says that lolium always germinates in the autumn. He has investigated the case, and gives some points of diagnosis by which young plants of lolium may be distinguished from young plants of wheat.[2] This autumnal germination and winter growing of the lolium almost forces upon the thoughtful reader the inference that if lolium occur in a field of wheat that was sown in spring, it was already up and growing at the time the wheat was sown. But there is one phase of this popularly credited metamorphosis doctrine of which Theophrastus is so impatient that he openly denies it. "Some say that barley changes to wheat sometimes, and wheat to barley, and that in the same field. Such statements are to be received as fables. Changes of that kind would be without a cause. It is diversity of condition that induces change."[3]

However skeptical Theophrastus may have been about all such pretended metamorphoses, he had doubtless the usual prudential reason for declining to assail them openly at every mention of them. The belief in them was universal; and the time for the elimination of such belief from even thoughtful minds was yet far distant. We find it persisting with men of intellectual attainments as late as the seventeenth century; at which time Scaliger, a most learned commentator on Theophrastus, avers that he himself has witnessed the transformation of wheat into barley and intimates that the Greek might have done better than to discredit the phenomenon.[4]

If it was the metamorphosis attending the development of the individual reptile, and the insect, which helped to elevate to the dignity of a quasi-rational belief the superstition about the changeability of wheat into lolium, it must be allowed that the reasoning was not very cogent. The cases are not parallel. One is that of the changes in an individual between youth and maturity. In

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[1] *Melampyrum arvense*, acc. to Sprengel, *Hist. Rei Herb.*, vol. i, p. 96.

[2] *Hist.*, Book i, ch. 7.

[3] *Hist.*, Book ii, ch. 3.

[4] See Stapel's edition of Theophrastus (1644), p. 78.

the other it is the seed of one species which, between the sowing of it and the germination, mysteriously changes to that of another species.

There is, however, a phase of transformation in plant life that runs parallel to the metamorphoses of lower animals. This has become generally known only recently, and by means of the compound microscope as applied to the making out of the life histories of ferns, liverworts, and other flowerless plants of lower organization. As illustrated in these plants, this kind of individual metamorphosis could never have become known to the nature students of antiquity, or even to those of the earlier modern epoch, owing to their lack of the necessary optical aids. But somewhat analogous metamorphoses take place in the individual life histories of certain higher plants, even of trees; and this fact is not so commonly known as it ought to be. In the family of the Mimosaceæ there is a considerable list of trees which only in the state of seedlings of a few years old exhibit the usual delicate fern-like doubly pinnated foliage of their family. Before the trees are old enough to flower they have divested themselves of every trace of that kind of leaf and are clothed instead with very narrow, simple, entire, firm and almost leathery organs, in cut somewhat recalling willow leaves, or perhaps better compared to those of mistletoe. Now it will not be in the least to the discredit of a circle of experienced and quite skilful botanical amateurs of the Northern Hemisphere if, placing before them two branches of such an acacia, one from the ferny-leaved young tree, the other from the mature tree with its stiff phyllodes like mistletoe leaves, and stating that these two branches represent one and the same species of Australian acacia, the whole circle of them suspect at first that I may be jesting. Some of the Australian eucalyptus species undergo as complete a metamorphosis in the individual, with this difference that the adult tree, at least in the earlier stage of maturity, exhibits both phases of branch and foliage; the lower and flowerless portion of the head of the tree seeming to represent one genus, the middle and upper branches—those that have the flowers and fruits—seeming as if they must be those of another genus, or even of another family. It is quite as if the tree at a point just below midway of its axis, had become by grafting from that point upwards a tree of another genus.

Of these changeable acacias and eucalypts the ancient Greeks of course knew nothing; but they were familiar with a similar case, that of what is known in very modern botany as *Hedera Helix*, the English ivy. This, as they well understood, is most commonly seen as a trailing or climbing shrub, the stems rooting everywhere, all the foliage leathery, of angular outline and dark green. In this phase the plant may pass the whole of its existence and remain flowerless. Occasionally, when very old and having climbed by wall or tree trunk to sunlight and upper air, a new thing happens. Out of the summit of this dark-green creeping ivy mass an upright bush appears; its branches not rooting, firm, independent, bearing leaves not leathery in texture, not in the least degree angled, and even of a decidedly light green; and this bush up at the top of the ivy will in course of time bear flowers and fruits. The ancients before Theophrastus had no difficulty in explaining this phenomenon. Their firm belief in all sorts of transmutations as taking place in nature saved them any perplexity. They held these two phases of the ivy to be generically distinct, and had their fully established names for the two genera: Helix for the rooting and climbing plant of dark angular foliage, Cissus for the upright bush of the pale thin ovate leaves, that into which the Helix sometimes in old age transformed itself.

Again Theophrastus fails to be satisfied with the popular philosophy, and suggests one that he thinks more rational. If every plant of Helix under the right conditions and with fair opportunity would develop a Cissus bush at summit in maturity or old age, which he says some agree to as being probable, then he would be of the opinion that the distinction between the two is not a generic one, but only a matter of the age of the individual.[1] He who has taken note of this philosopher's way of advancing his most revolutionary propositions with urbane reserve will understand him as here pronouncing against the time-honored doctrine of a generic change from Helix to Cissus, and as averring that these are but the young and the old phases of one species. It was the inductive philosopher, the scientific botanist, undermining as it were by stealth an ancient botanical superstition, because he had a truly scientific proposition to put in place of it. In this instance Theophrastus was, as usual, far in advance of his own time. For centuries after him men still held to the bigeneric ivy. I should confidently expect to find this pre-Theophrastan view surviving still among the peasantry of some parts of Europe where *Hedera Helix* is common and well known.

Living in the midst of a time when a thousand superstitions prevailed everywhere concerning plants, their origin, magic powers,

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[1] *Hist.*, Book iii, ch. 18.

and their often grotesque metamorphoses, it is greatly to the credit of Theophrastus that he should have risked something of his own popularity as a teacher and author by expressing, even covertly, some of his own doubts and disbeliefs; even as to some, the truth of which seemed so probable that the belief in them is not yet in our time obsolete.

Recapitulation. Certain opinions that are completely groundless respecting Theophrastus' merits as a botanist, opinions quite opposite to any that had ever before been expressed, and such as no man who had read three chapters of that author could have entertained, have been widely disseminated during the last thirty years. One such statement of opinion is before me and reads thus: "Greek authors built their views of the philosophy of botany on very weak foundations; scarcely a plant was known to them exactly in all its parts; they derived much of their knowledge from the accounts of others, often from dealers in herbs. From this scanty material, and from various popular superstitions had Aristotle formed his views on the nature of plants; and if Theophrastus possessed more experimental knowledge, he still saw facts in the light of his master's philosophical doctrines."[1] Such reckless writing as that, betraying innocency not only of Theophrastus' work, but also of that high opinion of it which had been expressed by most accomplished botanists of the eighteenth century and the nineteenth, has been widely read by botanists of the present generation. In view of this, it seems more than desirable that there be presented briefly and synoptically something like an enumeration of those items, or elements, of universal botany of which Theophrastus appears to have been the discoverer and first promulgator.

In this recapitulation I shall employ a few modern terms, such as petal, corolla, and andrœcium, unknown to ancient Greek botany, that I may thereby both more clearly and more briefly express the fact of the Greek's having recognized, though under other names, the things themselves.

1. He distinguished the external organs of plants, naming and discussing them in regular sequence from root to fruit; the naturalness of which sequence was afterwards pointedly denied; but in modern botany it stands everywhere approved.

2. He classified such organs as (*a*) permanent, and (b) transient; a division of them which may yet be shown more scientific than the modern distinguishing of them as (*a*) vegetative, and (*b*) reproductive.

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[1] Julius von Sachs, *History of Botany*, English edition, p. 16.

3. The existence of aerial roots, as being of the nature of roots, and thus different from tendrils and other prehensile organs, was discovered by him and has never since been disputed.

4. He remarked upon the inconsistency of retaining in the category of roots certain enlarged, solidified, jointed, and otherwise peculiar underground parts; a suggestion which lay unheeded during two thousand years of botanical history, and has only recently led to the open recognition of the category of subterranean stems.

5. He recognized, by differences of size, solidity, and other particulars of structure, three classes of stems: the trunk, stalk, and culm.

6. By never speaking of calyx and corolla as peculiar and separate organs, but always referring to their parts as leaves merely, it is evident he regarded the flower but as a metamorphosed leafy branch; to which forgotten Theophrastan philosophy of the flower[1] neither Goethe nor Linnæus had but returned, when each supposed himself the discoverer of a new anthogeny.

7. He divided the plant world into the two subkingdoms of the flowering and the flowerless.

8. The subkingdom of the flowering he again saw to be made up of plants leafy-flowered and capillary-flowered; really the distinction between the petaliferous and the apetalous; one the deep import of which was first realized and taken advantage of by the systematists of some two centuries ago.

9. He indicated the still more important differences of the hypogynous, perigynous, and epigynous insertion of corolla and andrœcium.

10. He distinguished between the centripetal and centrifugal in inflorescences.

11. He was first to use the term fruit in the technical sense, as applying to every form and phase of seed encasement, seed included; and gave to carpology the term pericarp.

12. He classified all seed plants as (*a*) angiospermous and (*b*) gymnospermous.

13. Respecting the texture and duration of their parts he classified all plants as tree, shrub, half-shrub, and herb; also noted that herbs were of perennial, biennial, or annual duration.

14. He indicated with clearness several of those differences in the structure of stems, leaves, and seeds by which the botany of later times separates plants monocotyledonous and dicotyledonous.

15. He described the differences between the excrescent and deliquescent in tree development.

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[1] Reaffirmed and somewhat improved by Cesalpino in the year 1583.

16. He knew how the annual rings in the stems or trunks of certain woody growths were formed.

17. Theophrastus, with natural vision unaided by so much as the simplest lens, and without having seen a vegetable cell, yet distinguished clearly between parenchymatous and prosenchymatous tissues; even correctly relating the distribution of each to the fabrics of pith, bark, wood, leaves, flowers, and fruits.

This list of facts botanical which Theophrastus saw, and in the main discovered, is not complete, but it embraces well-nigh all the first rudiments of what even to-day is universal scientific botany. It illustrates superabundantly the fact that Theophrastus, and no man of any later time, is the father of the science as we now have and hold it. And in the light of the above partial recapitulation of his discoveries, what possible remark could be more inane than this? "If Theophrastus possessed more experimental knowledge [than Aristotle], he still saw facts in the light of his master's philosophical doctrines." When a man has firmly laid the foundations of a science, and then has added the suggestions of almost the whole superstructure, what faintest shade of pertinency can there be in asking what his philosophic doctrines were? As reasonably might one leave any scientific work, alive with new facts, quite unexamined because its author's philosophy was that of a school unpopular, or his creed unorthodox.

The most generous interpretation of the words quoted would seem to be, that their author, having no knowledge of Theophrastus, thought to absolve himself from the task of acquiring it by trusting that the Greek would never again be found worth studying.

To me it seems not improbable that historians of the future, learning to know this great founder's mind better than it is yet known, may agree in some judgment not unlike this: that all that has been added to the understanding of plant life and form—to morphology, anatomy, physiology, perhaps even to taxonomy—within the last three centuries has been due to the inventions of the opticians, and to the increased number of students and investigators, rather than to the appearing on the botanical horizon, within the modern period, of any one mind in powers of observation, penetration, and sagacity superior to Theophrastus of Eresus.

Plumier (1703) sought to commemorate Theophrastus in a newly discovered genus of West Indian shrubs, yet was so inconsiderate as to name the genus ERESIA. This Linnæus (1740) changed to THEOPHRASTA.

CHAPTER III

GREEKS AND ROMANS AFTER THEOPHRASTUS

LEAVING Theophrastus, and going forth in search of the next landmark in the progress of our science, we seem at once to enter an almost boundless pathless waste. Or, as the outlook has been described by another: "If history be a connected succession of events, botany from Theophrastus forward to the sixteenth century has no history. Only isolated pieces of information, like bits of wreck half buried up and down stretches of seaside sand are left. These are connected with certain names; but, beyond that, are hardly of historic import. Of written monuments of real botany in the Greek language after Theophrastus there remains not one. For the small volume of Nicolaus Damascenus, known to us only by a translation into barbarous Latin, perhaps might, and possibly might not, have been reckoned such a monument."[1]

What is here understood is, that a great multitude of scattered fragments, together with several completed pieces of writing about plants that, out of the literature of antiquity, have been preserved do not afford material for the history of botany for so much as one only of the ten centuries that next succeeded the times of Aristotle and Theophrastus; and that this is true partly for the reason that the pieces and the fragments that have reached us are from men who were not botanists after the order of Theophrastus, but writers on medical, agricultural, and horticultural botany.

That the whole number of Greeks who wrote of plants in one way or another was very great there is evidence enough. As many as three hundred and fifty years ago the learned botanist, zoologist, and bibliographer, Conrad Gesner, gave out a printed list of more than one hundred names of Greeks who, in his day, were known to have written more or less botany.[2] Of ancient Latin authors of botanical works similar to those of the Greeks in kind, the same

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[1] Meyer, *Geschichte der Botanik*, i, 202.

[2] This in one of the several valuable papers that are prefatory to Hieronymus Tragus' *De Stirpium Historia*, 1552.

distinguished bibliographer enumerates ten or twelve; but does not seem to have quite finished that catalogue. As considerable a number of Arabian medical botanists of a less ancient period is also given.

The best of those botanical fragments, gathered in as it were from the wreck of ages, were what really inspired the first beginnings of modern botany in the sixteenth century. From the time of the establishment of universities and better schools of medicine in the middle ages, the best text-books of pharmacy were those of the ancients, Hippocrates, Nicander, Dioscorides, Pliny, Galen, and as many more less celebrated than they. The remedies in use were almost wholly vegetable, as were also the poisons and their antidotes; and the old authors' books were the topics lectured on in every school, and their plant descriptions were trusted to for the correct identification of plants alimentary, medicinal, and poisonous. And so, not even from the simplest outline of botanical history may all mention of the old Greek, Roman, and Arabian agricultural, horticultural, and medical botanists be omitted. We are not indeed able to construct out of their literary remains a botanical history of their period; but we know that they became at last, and incidentally, the inspirers of a new epoch which dawned upon botany a thousand years or so after the last of their line was dead. Sketches of the life and work of a few of them, and only such as came after Theophrastus, will here find place.

NICANDER OF COLOPHON.—This Greek grammarian and poet flourished in the second century before the Christian era; was native of a small village, Claros, close by Colophon in Ionia, and was anciently known as the Colophonian Nicander by way of distinction from others of the name of Nicander. He was of great renown as a poet, and his topics were mostly such as invite to the consideration of the living things of field, forest, and wilderness. Evidently Nicander was a naturalist, also learned in pharmacy and toxicology, and chose to express himself in poetic measure. That which may have been his most elaborate work has been lost, that is, the *Georgica*, a versified treatise of agriculture praised by Cicero,[1] and extensively quoted by Athenæus,[2] whose quotations are all that remain of the poem. Among these remnants, there is a long passage on flowers and other ornamental plants, an account of the Egyptian nelumbo, a dissertation on poisonous fungi—the earliest on record—and even another on the cultivation of edible

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[1] Cicero, *De Oratore*, Book i.

[2] Athenæus flourished some three centuries after Nicander.

mushrooms, besides many things relating to various other food plants.[1]

The only works of Nicander that have reached our time are two poems, under the somewhat lugubrious titles *Alexipharmica*, a treatise on poisons in general and their antidotes, and *Theriaca*, on poisonous animals. The first of these, according to Meyer,[2] who appears to have read both of them carefully, is of 630 verses, and has under discussion 21 different poisons, of which 2 are mineral, 8 animal, and 11 vegetable products; and the remedies for them are with hardly an exception vegetable. The account of the symptoms of different poisonings is said to be both true to modern experience and vividly drawn, but the plants themselves, whether poisonous or antidotal, are hardly more than named, never described, and the book as a whole is devoid of matter properly botanical.

In the *Theriaca*, a more extensive work of 958 verses, botany, as well as zoology, fares somewhat better. After a preliminary statement of means of frightening away poisonous animals or keeping them aloof, together with certain precautions to be observed by such as sleep out of doors at night, there follow some descriptions of certain more common and dangerous kinds which are often drawn with remarkable exactitude and faithfulness to nature. And here again, the bites and stings of these have always their remedies in certain plants, of which also in most cases only the names are given, though sometimes a few hints are given as to how the plant may be identified. The three particular plants, centaurea, aristolochia, and trifolium, are together efficacious against every poisonous animal's bite or sting. The identity of Nicander's centaurion is uncertain. It may have been *Hypericum olympicum*, but that of Theophrastus, whom Nicander often quotes, is *Ferula opopanax* more probably. The aristolochia is that of modern botany, the species either *A. rotunda* or *A. longa* or both. The only trifolium known to the Greeks was our *Psoralea bituminosa*. In the two poems thus adverted to Meyer counted the names of 125 different plants.[3]

Sprengel gives a list of some thirty species of Nicandrian plants which, though not in all cases identifiable with certainty, seem to have been first mentioned by this writer.[4]

Adanson in 1763, resolving to dedicate a genus of plants to

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[1] Haller, *Bibliotheca Botanica*, vol. i, p. 54.

[2] Meyer, *Geschichte der Botanik*, vol. i, p. 247.

[3] *Ibid.*, 248.

[4] Sprengel, *Hist.*, vol. i, p. 129.

Nicander, doubly distinguished himself in so doing; for **Nicandra** not only commemorates a worthy name in old Greek plant lore, but the type that was to bear the name was with an exquisite sense of fitness chosen from out the family of the nightshades.

Marcus Porcius Cato (B. C. 235–150).—An illustrious Roman of vigorous mind and great originality, serving the public in the most exalted and responsible offices with great acceptance, affluent as to means, he lead a life of great simplicity, temperance, and frugality, delighting in nothing else so much as the training of his children in virtue, and cultivating flowers and fruits. Practically a philosopher indeed himself, Cato held in abhorrence the philosophies of the Greeks, was strongly averse to the introduction of Greek art and Greek customs into Rome, apprehending the destruction thereby of Roman valor and simplicity, and recalling his son from the study of Greek. Later in life Cato must have fallen captive to the charms of Greek erudition; for he himself mastered the language, and on a visit to Athens addressed a concourse of the people in their own tongue; and it is observed that his own writings have quotations from Greek authors.

The literary monument that immortalizes Cato the Censor is his *De Re Rustica*, a treatise on farming, gardening, fruit growing, etc. It is the oldest book of its kind in Latin literature, and therefore is of botanical interest. We learn from its pages that almost every method of propagating choice varieties in use with twentieth-century pomologists and vineyardists was practised by Cato long before the beginning of our era, even to the different modes of grafting; and there is no intimation that any of those methods were other than ancient at that time. The number of named varieties of things which they had and were careful to perpetuate is also sufficiently interesting to merit such exemplification as I here subjoin, culled from Cato's book:

Brassica crispa
Brassica erratica
Brassica lenis
Brassica lævis
Olea albiceris
Olea Colminiana
Olea conditiva
Olea Liciniana
Olea Salentina
Olea Sergiana
Myrtus alba.
Myrtus nigra
Myrtus conjugalis.
Ficus marisca.
Ficus Africana.
Ficus Herculana.
Ficus hiberna.
Ficus Saguntina.
Ficus Telana atra.
Vitis aminea majuscula.

Pirus Aniciana
Pirus cucurbitina
Pirus mustea
Pirus sementiva
Pirus Tarentina
Pirus volena

Vitis aminea minuscula.
Vitis Apiciana.
Vitis gemina.
Vitis helvola.
Vitis helvola minuscula.
Vitis Lucana.
Vitis Murgentina.

All this would pass readily for good twentieth-century botanical nomenclature; but these names are easily two-and-twenty centuries old.

No fewer than five genera dedicated to Cato have been proposed, by as many different botanical authors, each apparently unaware of the attempts of the others. The Catonia of Patrick Browne (1756) has priority.

Marcus Terentius Varro (B. C. 117–27).—In so far as the mastery of human learning gives distinction, Rome had in Varro the most distinguished personage of all whose names adorn the pages of her ancient history. One Symmachus, who lived four centuries later, and whose letters are extant, wrote to a friend: "You know the writings of Terentius, not the comedian, but the Reatine, the father of Roman learning."[1] This Terentius was sometimes called the Reatine in allusion to his birthplace, which was the small village of Reate—now Rieti—some ten miles north of Rome. The family was plebeian, but there had been gifted scions of it before this one, and there were others after him. A century before him there had been a consul Caius Terentius Varro, chosen by the tribunes of the people for the reason that he was of the common people.[2]

Concerning the childhood, youth, and even the early manhood of Varro, and under what conditions the passion for learning was developed, nothing seems to have been recorded; and we seem to obtain our first certain view of him as in the public service under Pompey in the war against Mithridates; but he is then fifty years of age. Also at seventy he is still a naval commander. Being a man of great wealth, owning extensive landed possessions in several provinces, and having acquired so costly a thing as a great library was at that time, the fact of his having devoted his energies to the military service of the Pompeys and Cæsars for so long a period has not seemed easy to account for. In these chances of war certain of his richest estates were confiscated, and his library

[1] Quoted by Meyer, *Geschichte der Botanik*, vol. i, p. 356.

[2] Livy, vol. xxii, chs. 34, 35.

was plundered, this event entailing the loss of not a few of his own writings. And all the while his own tastes and preferences were for the life of quiet study, with rural avocations for his pastime.

Varro was more than seventy years of age when Julius Cæsar, returning to Rome as the great victor, recognized him as the most learned man of his time, and charged him with the work of collecting and arranging a great library; a noble scheme which Cæsar's assassination a year or two later brought to naught. After that, under the triumvirate of Antony, Augustus, and Lepidus, Varro's name was placed on the list of the proscribed; but by help of his friend Calinus, who concealed him in his own villa, his life was saved until this storm was past. The remainder of his life was given undisturbedly to literary work. His industry as an author has made Varro a wonder to succeeding generations. He wrote long treatises on Antiquities, a History of Literature, another of Philosophy, another of primitive Rome, a History of Religion, a volume on Education, a Latin Grammar, a book on Navigation, and unnumbered other treatises, all, or nearly all, long since lost, though referred to by many contemporaries. His treatise on Agriculture, in three books, almost alone of all his writings, has survived. He tells us in the first chapter of the work that he begins the writing of it in his eightieth year. It is replete with learning of all kinds, and is still a practical treatise; yet also evincing the author's familiarity with those Greek authors who, like Aristotle and Theophrastus, wrote on the theories of plant life and form.

As compared with Cato, the list of Varro's cultivated plants is not as long, and he does not enumerate as many varieties of Brassica, Pirus, Myrtus, and other genera. The choice varieties of cultivated cherries, long known in Pontus, Varro adds to the list of Roman fruits.

He is first among Roman authors to take note of certain phenomena of plant life, such as the growth and development of leaves and flowers, and also certain movements. The leaves of the olive, white poplar, and willow, whitened underneath, are apt to become inverted so as to show the lower face, and this at about midsummer, which phenomenon they take for a sign of the arrival of the solstice. The flowers of heliotrope follow the course of the sun from morning until nightfall; and there are other kindred observations, with even a hint that there is a physiology of such things that it might be interesting to know something of. In a passage on cattle raising one of Varro's interlocutors is represented as saying: "The thing is so, but why it is so, that is your affair, you who read Aristotle."[1] By such tokens is Varro more a botanist than Cato. True, he has derived every one of these ideas from the Greeks, not one of the observations being original with himself; but it was something to have been first to call the attention of Romans to them.

Patrick Browne (1756) sought to establish a genus VARRONIA. It seems that the name is untenable; the same genus having been named *Cordia* more than a half century earlier.

PUBLIUS VIRGILIUS MARO (B. C. 71–19).—Virgil, who has often been designated the prince of Latin poets, was born at Andes, a small village near Mantua, some seventy years before the beginning of the Christian era. His early years were passed at Cremona, where his father had valuable landed possessions. These were among the lands which, after the battle of Philippi, Augustus Cæsar confiscated, distributing them to his veteran soldiery. On this occasion the future poet was near losing his life through attempting to dispute with the soldiers, the possession of his fields. He escaped by swimming across a river, and then Virgil with his father repaired to Rome. It was the beginning of his greatness. His presence, manners, and accomplishments recommended the young man to the great Mæcenas, the power behind the throne of Augustus, and the latter soon restored to Virgil his lands; and the emperor's reward for this kindness was the ten pastoral poems (*Bucolica*) composed in the course of the next three years, and dedicated to the imperial benefactor.

After these followed the *Georgica*, accounted the most perfect and finished of all Latin compositions.

The simple narrative of the poet's career at Rome, and elsewhere until his rather early death, is one of the most fascinating and beautiful chapters in all history, but for several reasons must not here be presented anew.

The *Georgics*, by which Virgil is even more favorably if less universally known than by his unfinished epic, the *Æneid*, treat of agriculture and gardening; but also again one must refrain from anything like a botanical analysis of the poems. It may suffice to indicate how prolific a field for botanical research the poems of Virgil long have been. The following list of works on the Virgilian botany is doubtless incomplete.

(1) *Virgilii Georgicorum Libri IV.* The Georgics of Virgil. With an English translation and notes by J. Martyn. London, 1741, 4to.

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[1] Varro, *De Re Rustica*, Book ii, ch. 5.

(2) *Virgilii Bucolicorum Eclogæ X.* The Bucolics of Virgil. With an English translation and notes, by J. Martyn. London, 1749, 4to.

John Martyn was a physician, and Professor of Botany at Cambridge, the friend of Sherard, Sloane, and Dillenius, and was the first to establish the fact of this poet's profound knowledge of plants. Both the works named were in so great demand among men of erudition as to have been several times reissued, and in octavo form. A German translation of Martyn's edition of the *Georgica* was published at Hamburg in 1759.[1]

(3) *Flora Virgiliana.* Eller forsök at utreta de wäxter som utföras i P. Virgilii Maronis Eclogæ, Georgica och Aeneides. Jämte Bihang om Romanes Matwäxter, by Anders Johan Retzius. Lund, 1809, 8vo.

(4) *Flore de Virgile.* Composú pour la collection des Classiques Latins, by A. L. Fée. 1822; also again in 1837.

(5) *Osservationi sulla Flora Virgiliana*, by M. Tenore. Napoli, 1826.

Although Virgil was by profession a man of letters and a poet, he nevertheless exceeds the other agricultural writers of Roman antiquity in the number of different plants which he knows, and of which he makes mention; for Cato (B. C. 235–149) knew 125 kinds, Varro (B.C. 117–27) mentions 107, Virgil (B.C. 70–19) 164. Yet the sum total of the plants of these Romans, 245, is only about half the number that had been known by Theophrastus some 300 years earlier.

The celebrated Lamarck (1793) dedicated to Virgil a new genus of African trees under the name VIRGILIA.

LUCIUS JUNIUS MODERATUS COLUMELLA.—This very celebrated Latin writer on agriculture and horticulture flourished in the next generation after Virgil, during the reign of Tiberius Cæsar, and may have been in the midst of his years at the opening of the Christian era. He was a native of Cadiz in Spain, and was educated by his father, whom he characterizes as having been a man of erudition and also an experienced practical farmer.[2] The son declares himself to have been what one would now call an omnivorous reader, and before settling in Rome had travelled somewhat widely in Greece and Syria.

Columella is the most voluminous of all the classic Roman authors on rural topics. There are thirteen books, and these

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[1] Haller, *Bibl. Bot.*, vol. i, p. 68.

[2] Columella, *De Re Rustica*, Book ii, ch. 16.

aggregate some two hundred and sixty chapters, mostly long ones, and if none of the chapters are very strictly botanical, the whole work is a treasury of information about ancient husbandry, and the treatment of cultivated plants and trees. From him we have the earliest account of the device called a hot-bed, heated from beneath by a mass of fermenting manure, and protected from the rigors of winter weather by panes of glass. He reports that by these means Tiberius Cæsar raised cucumbers all the year around.[1]

To Columella we are again indebted for a complete account of the methods of grafting as practised by the ancients.[2]

Sprengel has given a considerable list of plants and trees that are first brought to notice by this author.[3]

Near the end of the eighteenth century two distinguished botanists almost simultaneously bethought themselves of the propriety of consecrating a genus to Columella. The COLUMELLIA of Ruiz and Pavon (1794) appears to have the priority.

PEDANIOS DIOSCORIDES (about A. D. 64).—If to have written the most practically serviceable book of botany that the world of learning knew of during sixteen centuries were the best title to botanical greatness, to Dioscorides would readily be conceded the absolute supremacy over all other botanists, not only of antiquity but of all time. Concerning the duration and the absoluteness of his supremacy Sprengel has the following: "During more than sixteen centuries he was looked up to as the sole authority, so that everything botanical began with him. Every one who undertook the study of botany, or the identification of medicines swore by his words. Even as late as the beginning of the seventeenth century both the academic and the private study of botany may almost be said to have begun and ended with the text of Dioscorides."[4]

Almost volumes have been written in controversy as to the time when Dioscorides lived; though the extremes of opinion do not assign him an earlier date than B. C. 30, or a later than A. D. 98[5]; and the most probable seems to be that which locates him in about the middle of the first century of our era. That he lived in the time of Nero is inferred almost to a certainty from remarks of Tacitus and of Galen.[6]

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[1] *De Re Rustica*, Book xi, ch. 3.

[2] *Ibid.*, iii, ch. 11.

[3] *Hist. Rei Herb.*, i, 149–151.

[4] *Ibid.*, p. 151.

[5] Meyer, *Geschichte der Botanik*, ii, 96–100.

[6] Sprengel, *Hist. Rei Herb.*, i, 152.

Though in relation to this author biographic dates fail us, the land of his nativity does not. It is well established that he was a Cilician Greek, his native city being Anazarbos; for in order to distinguish between him and others named Dioscorides, eminent writers referred to him as Dioscorides Anazarbæus, the Anazarbean Dioscorides. It is clear also from his own writings that he was a learned physician and practised medicine; also that he had travelled widely to study plants, and obtain knowledge of other than vegetal remedial agents. In these travels he came to know many plants before unknown to Greek and Roman physicians, and was at the pains of describing many such; that is, of indicating not only their qualities and remedial effects, but also something of their aspects and morphology as living plants; describing their roots, stems, foliage, and even sometimes their flowers; and the number of plants and plant products of which he gives account is about 600. Such a list of merely medicinal and alimentary plants is by more than 100 greater than the sum of all plants known to Theophrastus three centuries before Dioscorides. And it was because he had described so many, and often so well, that in after ages he came to be regarded as the supreme botanist. The usefulness of his medical botany, from the phytographic point of view, was not only fully realized, but also enthusiastically somewhat overestimated. The scientific botanist among the Greeks was Theophrastus; and there is no comparison between him and Dioscorides, whose theme was medical botany; but, quite as usual, the man of "applied science" was the one to meet with general appreciation and approval.

So highly esteemed was Dioscorides during the middle ages, that early after the invention of printing, his work, though in Greek, obtained an editor and a publisher at Venice as early as the year 1499.[1] This edition must have obtained a ready sale, for in 1518 it was repeated. A third Greek edition appeared at Basle in 1529.[2]

Latin being the universal language of the schools, Latin versions of Dioscorides were in demand, and early became rather numerous. The very first of these, a book rare and obscure, purports to bear the date 1478,[3] thus antedating the first Greek prints. But from the year 1516, when the first excellent translation by Ruellius appeared, Latin versions became numerous; and for a whole century thereafter the most voluminous and most useful books of botany were in the

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[1] See Pritzel, *Thesaurus*, 2d ed., p. 84.

[2] *Ibid.*

[3] Said to bear the name of Petrus Paduanensis; see Pritzel, 2d ed., p. 85.

form of commentaries on Dioscorides. Such in large part are the works of Anguillara, Matthiolus, Maranta, Dodonæus, Cesalpinus, Fabius Columna, and the Bauhins. In several of these the annotations and comments quite exceed in bulk the Dioscoridean text, and are replete with new botany; that is, they contain the names and descriptions of many plants which the commentators are convinced Dioscorides did not know, and which they therefore judge to be new. One may fairly say that the greater part of all the new botanical matter published during the whole of the sixteenth century, and a part of the seventeenth, came out in the form of annotations upon the text of Dioscorides. Thus it appears that the Greek, who only meant to provide medical students with a full compend of remedies, and of the marks by which to know them, became incidentally the first master of phytography; the one every line of whose plant descriptions has been more attentively studied word by word, and that by a greater number of erudite men than any other book about plants that has yet been written; unless one should possibly be obliged to make an exception of Bauhin's Pinax. But even that is, first of all, a compend of Theophrastan and Dioscoridean phytography, together with such augmentations and improvements as in the year 1623 were found necessary.

Latin editions of Dioscorides are too numerous to be given a reckoning; and almost the same may be said as to early translations of him into modern tongues; for between the years 1555 and 1752 there were at least twelve Spanish editions, as great a number in Italian, and there were editions in French in 1553, 1559, and 1580. There was one translation into German as early as 1546, another in 1610, and this last appears to have been issued again in 1614.[1]

Little in the way of botanical taxonomy will be looked for in a work on pharmacy that is nearly nineteen centuries old. The most comprehensive of his groups are formed according to properties; thus his Book I is devoted to the consideration of plants that are of merely aromatic rather than medicinal qualities; growths that furnish oily, gummy, or resinous products, such as enter into the composition of salves and ointments; and after these follow the trees that yield fleshy fruits of grateful though not specifically aromatic flavors. Book II, beginning with animals, and animal products that are of dietetic and medicinal use, ends with the cereals, the leguminous, malvaceous, cruciferous, and other garden herbs. Then Books III and IV deal with a vast number of plants more distinctively medicinal.

[1] Pritzel, *Thesaurus*, 2d ed., pp. 86, 87.

It is to be observed that he does not treat first of herbs consecutively, then of shrubs, then of trees. That would have been the formal adopting of a piece of regular taxonomy even in his day, as we have seen, already long established. There are merely spicy things that are trees, others that are shrubs, and some that are herbaceous; and the same is true of things alimentary and of things medicinal. This qualitative classifying is better suited to his purpose. But that even the more strictly botanical taxonomy may now and then gain a point by ignoring herb, shrub, and tree as fundamental divisions, comes out interestingly in Dioscorides' chapter on *Sambucus.*[1] He has two species, one of which is only an herb, the other woody and almost a tree. We shall see later that, after the revival of botany, at a comparatively modern period, such a hold had been gained by the old distinction between the herbaceous and woody that in deference to it systematists almost with one accord divided Dioscorides' *Sambucus* into two genera and separated them widely, *Sambucus* being located among the trees, and *Ebulus* among the herbs; and that hardly after Bauhin as late as 1623[2] had followed Dioscorides in writing them as one genus was the botanical world of that time ready to accede to a proposition so subversive of what was deemed fundamental in taxonomy, *i.e.*, that a tree and an herb could not be congeneric.

Subordinately to the more general and qualitatively outlined divisions of the work, Dioscorides recognizes all the more familiar natural families of plants; that is to say, within each Book there is a line of labiate genera, another of the leguminous, another of the umbelliferous, and the succession of cognates is not often interrupted by the intrusion of a genus not of such natural alliance. Even among the composites *Anthemis*, *Parthenium*, and *Cotula* are in close conjunction on his pages, as are *Anchusa*, *Lycopsis*, and *Echium;* and the succession of the representatives of five or six genera of solanaceous plants is only broken by the intrusion of *Cardiospermum*. Examples of this need not be multiplied. It is propagating fable in place of history to affirm that natural families were first recognized and indicated by any Linnæus, or Adanson, or Jussieu of the eighteenth century.

The whole subject of Dioscorides as a taxonomist merits a fuller development. A thorough study of his text might show that classification had progressed somewhat during those three centuries that had then elapsed since Theophrastus.

[1] Diosc., Book iv, ch. 155.

[2] C. Bauhin, *Pinax*, pp. 455, 456.

The services of this next to the last of great Greek physicians to botany are well commemorated in that fine generic type DIOSCOREA dedicated by Plumier in the year 1703.

CAIUS PLINIUS SECUNDUS (A.D. 23–79).—Two venerable cities of Italy contend for the honor of having been his birthplace, namely Como and Verona. But wherever he was born, the elder Pliny lived in Rome and called himself a Roman. He is known as Pliny the elder, to distinguish him from a nephew of his who bore the same name, and is also an author of some note. But the elder Pliny is one of the remarkable men, and among the most voluminous authors, of ancient times. His character, his manner of life, and his rather early martyrdom to knowledge, were written of after his death, by his nephew and namesake in a letter to a friend:

"I rejoice exceedingly that thou readest so eagerly my uncle's books, that thou wishest to obtain a complete set of them, and makest inquiry concerning all of them. I will serve you in the capacity of an Index, and shall even indicate to you the order in which they were written; for to know that is a matter of interest to the learned. *Spearcasts of the Cavalry.* One Book. He wrote this while in command of a company of cavalry, where his leadership was marked equally by courage and prudence. *The Life of Pomponius.* Two Books; a tribute to the memory of his best friend. Twenty Books *Of the German Wars*, embracing a full account of all our wars with the Germans. He undertook this work while in military service in Germany, and it was suggested to him in a dream. Three Books of *The Student*, so comprehensive as to fill six volumes, wherein the aspirant to oratory is advised and directed from the cradle forward. Eight Books on *Hesitancies in Public Address.* These were written under Nero, when anything like bold or unrestrained writing was unsafe. Thirty-one Books supplementing the (historic) *Work of Aufidius Bassus.* Thirty-seven Books of *Natural History;* a work as comprehensive, learned, and many-sided as nature itself. Thou wilt wonder how a man of so many business affairs could bring to completion so many volumes, and on such difficult subjects. You will be still more surprised to learn that for a long time he was a busy lawyer, that he died in his fifty-sixth year, that he had been much occupied with the duties of the most responsible public offices, and that his time had also been heavily taxed by the exactions of the friendship of princes. The explanation is, that he was of the keenest intelligence, and his economy of time something almost beyond belief, even to the reducing of his hours of sleep to the shortest limit. In the winter, he was accustomed to work until between one or two o'clock in the morning, or at the shortest until midnight. He could fall asleep at once, no matter at what hour; sometimes even at work he would fall asleep for a few moments and awake again. Before the break of day he would go to the Emperor Vespasian —for he too used to work at night—to receive his orders or to fulfil some commission. Returning home he would study until breakfast time. After a light breakfast, if it was summer, and he had a little leisure, he would lie down in the sun and have a book read to him, taking notes and extracts; for he read nothing without making some excerpts, being accustomed to say that no book was so bad as not to contain something useful. After sunset he would usually take a bath, then recreate and sleep a little. After that, as if another day had dawned, he studied again until dinner time. Even at this principal meal a book was read, and comments written, and this without interrupting the reading. I remember that once upon a time one of his friends present checked the reader, who had given a wrong inflection, and had him read the line over again. 'But you understood the meaning at the first reading, did you not?' my uncle interposed; to which the other nodded assent. 'Why, then, did you call for the repetition? We have lost the time it would have taken to read ten lines, by this interruption.' So avaricious was he of time. He arose from the dinner table, whether while it was yet daylight in summer, or when in winter it was after dark, always with the same promptitude, as if compelled by law. This was his manner of life amid the business and turmoil of the city. In the country the only respite he allowed himself was that of the daily bath; and when I say that I mean the actual time of the bath; for while the drying and dressing was going on he was either listening or dictating. On his journeyings, as if putting out of mind all business cares, he did nothing else but that; keeping always close beside him a rapid penman, a book, and a writing tablet. . . . For the same purpose even in Rome he had himself carried from place to place in a sedan. I remember well how once in meeting me when I was walking he said: 'You ought not to lose these hours'; for he reckoned all time lost that was not given to study. It was by such exertions as these that he brought all those volumes to completion."[1]

We have from another letter by this same kinsman annalist—a letter to the historian Tacitus—an account of the circumstances of

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[1] Pliny the younger, Book iii, Epistle 5.

Pliny's untimely death.[1] The facts must here be presented in brief.

In the summer of A.D. 79 Pliny had command of the Roman fleet, then in the Tyrrhene Sea, at anchor behind what is now known as the Punta di Miseno. On the 22d of August of that year, at about noonday a terrific earthquake and rain of ashes accompanied that frightful eruption of Vesuvius which buried the cities of Herculaneum and Pompeii. A cloud in the shape of a huge Italian pine, with straight trunk and horizontal branches, seemed to arise from the crater of the volcano. Immediately, on a small and light sailing vessel Pliny set forth in the direction of the mountain to gain a nearer view of the phenomenon. Soon discovering that by the dread progress of the eruption the lives of the whole population along the mountain's base were imperilled, and that they had no way of escape but by sea, he ordered the larger ships to sail to the rescue, and went with them. At one time, when a shower of stones and ashes was falling on his ship, and the sailors were trembling in mortal terror, the commander was seen to be quietly dictating his own observations to the amanuensis. They made land at a point where some friend of Pliny had a country seat. The villagers were found already provided with the means of escape by sea, only a strong contrary wind prevented embarkation; meanwhile the dangers of remaining on land increased with every hour. Pliny, when nightfall came, as if to inspire courage in others, took his bath as usual, then dined, and composed himself as for a night's rest; but the inhabitants of the place remained awake. Towards morning the fall of stones and ashes so increased, and the earthquake shocks became so much more frequent, that the people awakened Pliny, fearing the closing up of the porch on which he had made his bed. The demolition of buildings appeared imminent, while out of doors the stones fell still more thickly. People bound cushions and pillows about their heads and rushed down to the beach; but winds and waves are still high and there is no boarding the ships. It is now daytime, but the blackness of night is still over land and sea, interrupted only by flashes of lightning or flames bursting forth now and then from the fissures of the earth. The people rush to and fro in the frenzy of despair. The fumes of sulphur threaten to suffocate them. Pliny, helped by two slaves, arises from his couch, and falls dead.

The *Historia Naturalis* is Pliny's monument; no other work of his having survived. The following outline of the contents of the work may be useful here. Book II, Cosmology and Meteorology;

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[1] Pliny the younger, Book vi, Epistle 16.

Books III to VI, Geography; Book VII, Anthropology; Books VIII to XI, Zoology; Books XII to XXVII, Botany, in the main agricultural, horticultural, and medical; XXVIII to XXXII, Zoology again, but in relation to medicine; as also in connection with aquatic animals some paragraphs on the remedial efficacy of certain waters; Books XXXIII to XXXVII, Mineralogy, more especially as applied to medicine, painting, plastic art. Book I of the work is prefatory and introductory to the work as a whole.

At first thought it may seem extraordinary that so vast a work, filling so many volumes, should have escaped the fate of many hundreds of less voluminous things, and should have reached modern times in its completeness. The probable explanation has been suggested by the historian Meyer, who thinks that its very voluminousness was its safeguard. The copying of the complete *Natural History* by hand was a large enterprise, and the manuscript when done was very costly. Men take care of that which is worth much money.[1] In the middle of the nineteenth century, an excellent authority reported the existence of more than sixty manuscripts of Pliny of greater or less antiquity.[2] Since the invention of printing more than eighty different editions have been printed of which number as many as eighteen appeared in the fifteenth century, and more than forty in the sixteenth.

The high prestige held by Pliny throughout mediæval times was due to the fact of his having written in Latin. All the other authors of greatest importance as to natural history had written in Greek; and Latin was the language of the middle ages. The work was also extensively, if not mainly, a compilation, and was made up in large part of translations into Latin from the greater Greeks, Aristotle, Theophrastus, Nicander, and Dioscorides, and so had much the character of a compend of all natural history.

As a Roman among Romans, Pliny was of the utilitarian bent of mind; even a subscriber to the dogma, then antiquated, that all things that are upon earth are here for the sake of man; not that he so holds it as piously to praise nature for universal benignity or generosity; for in one of his earlier volumes he says: "If nature appears to have produced everything for the sake of man, still man is often obliged to pay rather dearly for her gifts; so that it is not so easy to decide whether she has more the character of a benign mother than of an unkind stepmother."[3]

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[1] Meyer, *Geschichte der Botanik*, vol. ii, p. 125.

[2] Julius Sillig, quoted by Meyer, *Ibid.*

[3] Pliny, *Hist. Nat.*, Book vii, ch. 1.

Whatever nature may or may not be to man, it is impossible to establish any natural order and sequence of things on those utilitarian principles; and he who looks into Pliny's botanical volumes for anything like what we understand by taxonomy will be disappointed. Here is what a distinguished botanical systematist of the eighteenth century said of the Roman's plant classifying: "Pliny, the indefatigable compiler, published in fifteen books all that Theophrastus, Dioscorides, and their predecessors had said about plants. But he treats this matter in a manner so strictly historical, although in such flowery language, that one may well say of the whole that it is in beautiful disorder."[1] True it is that Pliny begins his history of plants with the discussion of trees. The philosophic Theophrastus had done so; but it was for the reason that trees seemed to him to claim the first place as being the most highly organized type of plants. Pliny begins with trees because he judges them to be, on the whole, more useful to man than herbaceous plants. Similarly everywhere in his writings the thread of the economic rather than the philosophic is that by which one is to trace whatever of system there is in his treatment of plants and plant lore.

It has been claimed by some, and disputed by others, that Pliny was more than a compiler, and that some of the facts which he records were from his own observation. A renewed and thorough study of all his botanical books, with such a question foremost, is still called for; but rather unusual accomplishments are also demanded on the part of the investigator; those of the ripe classical scholar and of the master botanist combined. Adanson, who if I mistake not had for his thesis inaugural a study of Pliny, accredits him as discoverer of the distinction between growth buds and fruit buds in trees, and says that he named the former kind germen, the latter gemma.[2] It was a botanical discovery of high rank, whoever first announced it; but I should suspect it of having been made anterior to Pliny.

As if to crown Caius Plinius Secundus with a wreath of myrtle, Plumier (1703) dedicated to him a genus PLINIA belonging to the family of the myrtles.

CLAUDIUS GALENUS (A.D. 130–201).—In respect to natural endowments, wealth of information acquired by study at home and travels abroad, and fertility of able and learned authorship, Galen was one of the great celebrities of antiquity; as a physician

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[1] Michel Adanson, *Familles des Plantes*, Preface, p. vii.

[1] Adanson, *Histoire de la Botanique*, p. 94.

ranking above all others figuring in the early history of medicine except Hippocrates.

Galen was a Greek, born at Pergamos in the year 131 of our era. Ancient annalists take note of the fact that Pergamos was the seat of the most celebrated of temples to Æsculapius; therefore of good augury as the birthplace of predestined physicians. Nico, the father of Galen, was skilled in philosophy, geometry, and astronomy, and was an architect; also evidently interested in plant life from the philosophic point of view, for the son reports it that Nico, as if doubting about the transmutability of one species into another, made some experiments with grain sowing, the result being the removal of all doubt concerning the changeability of certain grains into chess, or darnel.

The career of the son seems to indicate that the architect Nico was a man of wealth; for the child received most careful education under the best masters at Pergamos. At the age of seventeen years, having chosen the profession of medicine, he was sent upon his travels, and continued them until the age of twenty-eight. He spent several years in Ægypt, with headquarters at Alexandria, passing thence into Bithynia, Palestine, Thrace, Macedonia, Italy, and the islands of Crete, Cyprus, and Lemnos, these all noted in that early time for the wealth of drugs, and the good quality of them, that were imported from them into all the cities of the then known world. The high distinction to which he afterwards attained attests the improvement made of those rare opportunities that wealth had afforded him. One object of these prolonged journeyings had been that of profiting intellectually by converse with learned men, and the most noted physicians of every land. He was reputed also to have mastered all the dialects of Greek, as well as the Latin, Persian, and Ethiopian tongues. Also everywhere he sought the most perfect knowledge of every plant anywhere in use remedially. It was not enough that a given remedy might be purchased from any druggist. The physician ought to know all about the plant, even as living and in its native soil, and thus become qualified to distinguish pharmacologically between the false and the genuine, and to detect adulterants.

Galen's years of travel were concluded by a considerable sojourn in Ægypt, at Alexandria, then the world's greatest center of art and erudition; and thence he returned to his native Pergamos, where for some years the surgical and medical care of the gladiators was committed to him. In the year 164, when he was about thirty-three years of age, a revolutionary disturbance in his native city was the occasion of his be aking himself to Rome. Here he now practised medicine with distinguished success, and gave lectures on anatomy; all this, however, to the arousing of a storm of jealousy on the part of the native Roman physicians, to whose unrelenting sallies, or else to a malignant outbreak of the plague, or to both forces combined, he yielded at the end of three years and once again returned to Pergamos. The stay there was this time short, for the Roman Emperor Marcus Aurelius, with his imperial brother Lucius Verus, going to war with the Quadi, Parthians, and Marcomanni, desired his services as their physician on that expedition. The plague broke out in the country of the attempted conquest, the expedition became a failure, and the imperial majesties with their Greek physician began a retreat to Rome. Lucius Verus, stricken with apoplexy, died on the way, and the imperial philosopher and Galen reached Rome in safety. Here the latter now engaged actively and industriously in medical authorship during a number of years, and when in the year 172 Marcus Aurelius set forth on a second attempt to go and conquer the Marcomanni, desiring Galen to accompany him as his physician, the latter interposed, piously, that the god of his native city, the revered Æsculapius, to whom he was under solemn vows, had decreed otherwise. The excuse availed, and gave no offence to the pious Aurelius; so that Galen continued in Rome, as physician to Commodus, son of Marcus Aurelius and heir to the empire, then very young. Later, and at a date unknown, Galen returned to his native city, where he ended his long, laborious, and most distinguished career, at the age of 70 years, or, as some authorities say, at 90.

Some idea of Galen's industry as an author may be conveyed by a note or two on editions of his works issued since the invention of printing. One published at Basle in the year 1538, and containing the Greek text only, fills five folio volumes.[1] An edition given forth at Leipzig between 1821 and 1833, embracing both the Greek text and a Latin translation, is in twenty octavo volumes.[2] And such editions do not include certain of his works the Greek originals of which have been lost, and only the Latin versions of them have been handed down from early times; much less others which have been lost altogether. Even in Galen's lifetime certain books of his, kept in the Temple of Peace at Rome, were destroyed in a conflagration of that building and were never reproduced.

[1] Edited by Cossæus, Fuchsius, and Gemusæus.

[2] Claudii Galeni, *Opera Omnia.* Editionem curavit C. G. Kuhn, Lipsiæ, Tom. i–xx. 1821–1833.

A man of as profound erudition, and of such high attainments in botany as the celebrated Albert Haller, conceding the supremacy of Galen as a man of genius, and a great master of everything relating to the healing art, seems to deny that he was anything of a botanist, affirming that almost all his botany is borrowed from his predecessors, and chiefly from Dioscorides.[1] This criticism, in as far as it is adverse, touches the writings of the man as a possible contributor to botanical knowledge. It does not—or if it does it ought not to—stand as an impeachment of his knowledge of plants. It is incautious to pass judgment against any man's attainments in a subject until he has at least in some way expressed himself on that subject. To know many plants familiarly and well is to be something very like a botanist, whether one ever write a paragraph of botany or not. A familiar knowledge of many plants Galen not only urged upon the whole medical profession; he claimed that he himself possessed such knowledge. "In as far as possible the physician ought to know all plants, and if not all, the greater proportion, and those most useful. . . . He who knows the different kinds in all their states from young and small to fully grown, and can so distinguish between them, will in many places find certain useful plants, as I have done in various parts of Italy, where he who knows them only in the dead and dry, would never recognize them whether in the young state or the mature. There is no quacksalver who does not readily identify the herbs that are imported from Crete by their fruits; but that some of these selfsame things might be gathered on the outskirts of Rome they do not know, because the season of their herborizings does not correspond to that of the maturity of these plants. But that time is well known to me, and I go in quest of Chamæpitys, Chamædrys, Centaurium, Hypericum, Polium, and others of that kind, at just the right time, and gather them in their perfect maturity, neither waiting until they are past that, and are sunburnt, nor going too early, that is, before the fruit is well formed." It has well been observed by one of the historians of botany, that "The man who wrote thus must have been either a consummate charlatan, or else a man of deep and thorough knowledge, and a charlatan Galen was not."[2]

When one reads in Galen over and again such commendations of an intimate knowledge of many living plants, and when it is remembered that he made long journeys by sea and land in the

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[1] Haller, *Bibliotheca Botanica*, vol. i, p. 111.

[2] Meyer, *Geschichte der Botanik*, vol. ii, p. 191.

endeavor to familiarize himself with the medicinal plants of different climes, and when one has marked the keenness of his powers of observation everywhere, it is impossible to agree with the learned Haller that in botany Galen was inexpert; nor can it reasonably be questioned that had he betaken himself to phytography, he would have laid all botanical posterity under deep obligations to himself. Now, that he did not describe plants, but was accustomed to give their names only, or but little more, one might have been disposed to charge to the fact of his having flourished in the very next century after Dioscorides whose 600 species, embracing the whole vegetable materia medica, may have been for the most part well identified at Galen's period, so that the mention of a name only would sufficiently recall a species. But such apology for Galen would be superfluous. The truth seems to be that he had next to no faith in phytography at all. He takes openly the ground that "The identification of plants is better accomplished by the actual observation of them under the help and guidance of a teacher, than by that method which may be likened to the attempting to learn to navigate the seas by studying books on navigation."[1] This, then, is the main reason why Galen almost abjured plant description.

The passage is luminous with historic information about the study of botany in the Rome of eighteen centuries ago. We know already that at this period the occupation of a well trained physician is lucrative. There are many of them; therefore the candidates for the profession are not few. The remedies in use are almost all botanical, and they all study botany; quite otherwise, by the way, than botany is studied in twentieth-century schools of medicine, and less perfunctorily. Unless in their practice of medicine they are to be at the mercy of the unscrupulous among herb gatherers and drug vendors, they must know the marks of the genuine thing. Therefore important among their regular exercises is that of identifying plants, the book open before them, the specimen it may be a withered and shrunken root or rootstock, not improbably supplemented by a fresh one newly brought in from its native soil, or from some drug garden. The standard botanical work, descriptive and pharmaceutical, is Dioscorides—its author hardly a century dead—and there are others. The descriptions are mostly brief and often inadequate, so that mere guesses at the identity of things frequently pass instead of certainty, and about the identity of some that are of remedial importance the whole fraternity —Galen himself perhaps excepted—is wrong. At all events none

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[1] Galen, ed. Kühn, vol. xi, p. 96.

but he is so discouragingly aware of the multitude of errors that have originated through placing dependence on descriptive botany. He thought there was a better way; but this proposal of his seems to imply on his part an overweening confidence in the perpetuity of things. He did not foresee a time when the race of capable phytognosts would fail, and when in default of such teachers for the identifying of plants there would be no other dependence at all but the old and often imperfect descriptions.

Men follow great leaders when the leaders are in the wrong, about as faithfully as when in the right; and if, during several centuries after Galen, lesser lights continued to mention plants hardly more than by their names and remedial qualities, it was after the example of his authority as supreme. In such manner may the most expert man of science chance to antagonize the best interests of that science, and heavily impede its progress along one line while advancing it in a different direction.

His indifference to phytography notwithstanding, Galen has been credited with having made some few additions to the list of known plants by new name, and by some sort of description.

Michel Adanson attributed the discovery and the naming of two new genera to Galen. They are *Lycopersicon*[1] and *Arctostaphylos*.[2] Both names are now in use for genera, but it is impossible to identify either one with the type which Galen had in hand; but from such description as the Greek gave out, his *Arctostaphylos* would be *Vaccinium Arctostaphylos* rather than *Arctostaphylos uva ursi*.

In the year 1737 Linnæus dedicated a genus GALENIA to the memory of Galen.

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[1] Adanson, *Familles des Plantes*, vol. ii, p. 572.

[2] *Ibid.*, p. 165.

CHAPTER IV

INTRODUCTORY TO THE SIXTEENTH-CENTURY GERMAN FATHERS

THAT long course of ages intervening between the last decline of the Roman empire and the revival of learning in the fifteenth century is chiefly distinguished botanically by what we do not know about it. Even the historians of botany, with hardly more than a single exception,[1] instead of making intelligent and unimpassioned use of the scattered fragments of botanical record for the period, have done what they could to perpetuate their own hereditary prejudices against the whole period.[2]

However, he who is in quest of landmarks chiefly will be absolved from the task, interesting though that would be, of following the vicissitudes of botany through the middle ages. The period has not apparent landmarks of botanical history.

The tenor of the German writing of its history is, that the science of botany was born again, as it were, in the year 1530 and in Germany, by the publication of Otho Brunfelsius' folio entitled *Herbarum Vivæ Icones*—Living Pictures of Herbs. The Germans have always been and are the chief historians of botany. I pay full tribute of acknowledgment to their supremacy in this field of high endeavor when for the heading of this chapter I adopt what is become their own favorite caption. All of them use it: Kurt

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[1] Meyer alone (*Geschichte der Botanik*, vols. iii and iv) has treated the subject of botany in the middle ages with impartiality.

[2] Emphatic examples of this kind of writing in the name of history are in Sprengel's *Historia Rei Herbariæ*, vol. i; particularly his chapter on "Monastic Botany," pp. 222–228, and on the "Latinobarbarous Age," pp. 274–299; wherein even concerning the botanical volume of Albertus Magnus he says, "Let him read it who has time to throw away"; though Meyer, only a half-century after Sprengel, and as much an antimonachist as he, devotes seventy serious pages of his history to the merits of this same Albertus of the middle ages.

Sprengel,[1] Ernst Meyer,[2] Emil Winckler,[3] Julius von Sachs.[4] All of them name Brunfels, Fuchs, and Tragus (Bock) as the fathers of the new botany of modern times.

It has been indicated in a preceding chapter of these Landmarks that the real father of botany as a science was Theophrastus of Eresus. If he is the father of the science he is the father of even modern botany, though not of those developments of it that have been the peculiar achievement of modern botanists. Science is truth. The foundations of a science are its fundamental truths, and so the foundations of a science once laid are laid forever. These things are self evident.

We shall not be able to realize in how far the "German Fathers" contributed to the superstructure of modern botany until we have examined with great care and diligence their best works; and this is something which, I shall make bold to say, not even the German historians have been at the pains of doing; though Sprengel, first of their lineage, did much and well in this direction, while also leaving very much for others to accomplish. Julius von Sachs, the latest in the line, copied Sprengel's caption "The German Fathers," etc., but knew next to nothing of their works, even rating as unimportant Valerius Cordus,[5] who was immeasurably the greatest of them all.

The four now named represent two rather distinct kinds or grades of botanical work. Brunfels and Fuchs busied themselves almost wholly with medical botany. It is a rare thing with either of them to mention a plant of unknown or even uncertain medicinal or alimentary qualities; and their plant descriptions are almost as uniformly either compiled or literally copied from authors of centuries and even almost thousands of years before them. The books of Tragus and of Cordus abound in new and original descriptions. These demonstrate that these two men examined plants with their own eyes, and for the love of them as plants, and that they saw many things about the structure and the behavior of them to which the other two men, and even all botanists before them, had been blind.

There is another contrast. Brunfels and Fuchs, realizing the defects of many of the ancient descriptions, sought to render the

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[1] *Historia Rei Herbariæ*, 2 vols., 8vo, 1807–1808.

[2] *Geschichte der Botanik*, 4 vols., 8vo, 1854–1857.

[3] *Geschichte der Botanik*, 1 vol., 8vo, 1854.

[4] *Geschichte der Botanik vom 16 Jahrhundert bis* 1860, 1 vol., 8vo, 1875.

[5] *Geschichte der Botanik*, p. 31.

identification of remedies more easy and certain by supplying engravings of the plants. This idea was very far from being new; indeed, it was almost as old as botany. Pliny knew as many as three Greek authors who, before the Christian era, had illustrated their manuscripts by paintings of the plants.[1] The traditions of still others have been brought to light. In the middle ages early and late rare manuscripts of old botanical authors illustrated by drawings or paintings of plants were known and referred to. The most noted of such ancient manuscripts, now some thirteen centuries old, has been reproduced photographically, and in this way actually published since the beginning of the twentieth century.[2] Even forty or fifty years before these fathers of plant iconography, there were printed copies of the *Hortus Sanitatis*,[3] and of its German version, *Gart der Gesundheit*, illustrated by some five hundred wood engravings of plants. Doubtless the wretched character of those first printed plant pictures, along with the fact of the great popularity of the books containing them, were what moved Brunfels to undertake the production of the *Herbarum Vivæ Icones;* and the success of his enterprise stimulated Fuchs to inaugurate a larger one. These two might worthily have been styled Fathers of Plant Iconography, but to name them the German Fathers of Botany is superlative; for it will have to be admitted that the mere publishing of plates of plants, with names of said plants and their uses, is not in itself the setting forth of any scientific principles beyond the few taxonomic ideas which the mere grouping of the plates may chance to indicate. What are plant picture-books for? In the case of the authors of them, they may be the refuge of those who can not describe, or, with such as can describe, they are a condescension to such as can not read; also to others who are

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[1] Plin., *Hist. Nat.*, Book xxv, ch. 2; see also Meyer, *Geschichte*, vol. i, 250. The names of the ancient painters were Cratevas, Dionysius, and Metrodorus.

[2] A celebrated Greek manuscript of the *Materia Medica* of Dioscorides, known as the *Codex Aniciæ Julianæ*, in which each plant is represented by a painting of natural size. The manuscript dates from the sixth century and was done at Constantinople. It has long been in the Imperial Library at Vienna. The whole has lately been reproduced photographically. The title page of the published work has the following:

"Dioscurides. Codex Aniciæ Julianæ picturis illustratus, nunc Vindobonensis Med. Gr. I. photographice editus. Moderante J. Karabacek. Prefati sunt A. de Premerstein, C. Wessely, J. Mantuani. Lugduni Batavorum, A. W. Sijthoff, 1906."

[3] For some account of these earliest specimens of printed books of popular medicine chiefly botanical, the reader is referred to Pritzel's *Thesaurus*, 2d ed., pp. 364–368; also Meyer, *Geschichte der Botanik*, vol. iv, p. 189.

incapable of mentally imaging a thing from the verbal description of it. By the large picture-books of Brunfels and of Fuchs all sorts and conditions of men, lettered and illiterate, could identify some hundreds of useful plants; a thing which never had happened in the world before that day. For this they deserve only praise. Nevertheless, had no books of botany been issued in the sixteenth century essentially different from those of the two authors named, it is difficult to see how botany could have progressed a single stage within that century.

In the works of Tragus and of Valerius Cordus we have books in character essentially different from those of the two aforenamed. Both these were deeply interested in plants of all kinds; were given to examining their organs minutely and marking the behavior of certain growths at different stages, and all this before ever having thought of writing books thereon. Also when they betook themselves to writing it was without any purpose of calling upon artists to make pictures remedying the defects of their descriptions. They were under the inspiration of a new idea in botany, namely, that plants might be so described as to be identifiable by description. Galen's dream about a kind of apostolic succession of living teachers, one generation of whom should forever teach the next to know the medicinal plants by their right names[1]—all that had proven a very idle dream. Thirteen changeful, turbulent centuries had now passed since Galen. The succession had been obsolete a thousand years, and the world botanical was far at sea as to the true identity of many important plants. There must be descriptions; and they must be better than those handed down from ancient times. I should not venture to credit the erratic and garrulous Tragus with having known the history of botany so well, or having planned the opening of a new era in descriptive botany. We shall probably see, by the perusal of his book, that what he achieved here, and it was not a little, was but the spontaneous outcome of his admiring curiosity about plant structures. On Cordus' part, it is unmistakable, there is the deliberate plan of creating a new phytography. Therefore, and by a study of the men and their books, I think we shall perceive that in the Germany of the first half of the sixteenth century, there were two fathers of plant iconography and two fathers of descriptive botany.

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[1] See page 165 preceding.

CHAPTER V

OTHO BRUNFELSIUS, 1464–1534

First in point of time among the German botanical reformers of the sixteenth century, Brunfels is also easily first in rank respecting those educational and literary qualifications which go to the making of what one calls a scholarly book. In this particular his one botanical treatise, the *Herbarum Vivæ Icones*, is peerless among the several books of botany that appeared in middle Europe within the first half of the sixteenth century. Others produced more and better botany; but there are marks of a dignified and conservative erudition that are characteristically Brunfels' own.

Life. His career was a long one, at least for a consumptive,[1] and was singularly varied. One need not here analyze the motive of that countryman of Brunfels who pretended that the man's professional life might be summed up in one sentence like the following: "At first a schoolmaster at Strassburg, then a physician at Berne."[2] This would be good language in which to epitomize the professional life of one who had been at the early outset a schoolmaster, after that a university graduate in medicine, and then a practitioner. Such would be the natural interpretation of a sentence like that quoted; and the trouble with this pretended epitome is, that it leaves completely out of view Brunfels' occupations during the first fifty years of his life, revealing only the last twenty; for certain it seems to be, that when in default of other means of a livelihood he opened at Strassburg a school for boys, he was well past fifty years of age; also that when at the University of Basle he won the degree of Doctor of Medicine, he was sixty-five.

In the history of botany Brunfels will hold in the future, as he has done in the past, a somewhat distinguished place among the notabilities belonging to his century; and we must review, as well

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[1] Brunfels died of consumption at Berne, Switzerland, probably at the age of about seventy years.

[2] Sprengel, *Hist. Rei Herb.*, vol. i, p. 311.

as at the distance of four hundred years we may, the incidents that had to do with the moulding of the youth, and helped to establish the character of the man.

The birthplace of Otto Brunfels was Mayence, or Mainz. The family had taken its name from Castle Brunsfels[1] not far from Mainz where the earlier ancestry of the botanist had lived. At Mainz, John Brunsfels, the father, was in the business of a cooper; appears to have been in comfortable if not affluent circumstances; was well known and much respected as a man of character and high integrity; also, as we learn by his opposition to young Otto's plans, a man with a will and purposes of his own; qualities inherited by the son, as we shall see. Otto was the only son, and entertained the thought of devoting himself to the service of the Church. At that time Martin Luther was yet unborn and all Germany was Catholic.[2] A Catholic father of that period, if rich or well to do, would have been a marvel of pious unworldliness, if he had been willing that his only son should become a clergyman; for that would mean the immediate extinction of his own branch of an ancestral line. This father of young Otto Brunfels was resolute and persistent in his opposition to the son's wish; and naturally so; and this must have continued until the son was of legal age; for at last, hopeless of otherwise attaining to the priesthood, he left home and became a novice in the Carthusian monastery that was in his native town. This he would not have been permitted to do had he been a mere youth, unless the father had given consent.

Meyer's inference that Brunfels remained but three or four years an inmate of the monastery[3] proceeded from several misunderstandings, one of which was that the man had not been born until a little before the year 1500. But there is now good authority for our accepting 1464 as the year of the botanist's nativity; so that in 1500 he was already thirty-six years old. Then, since to assume a part in the new Lutheran movement was the object of his secret flight from the house of the Carthusians, and that movement was hardly well under way before 1517, it becomes highly probable that the man was fifty-three years of age when, renouncing monasticism and giving his learning and talents to the support of Luther's cause, he took up the sojourn at Strassburg. He betook himself

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[1] Brunsfels, rather than Brunfels, was the family name. In some of our author's earlier works he wrote it Brunsfelsius; but later he appears to have changed it to Brunfelsius.

[2] Otto Brunfels was born in 1464, Martin Luther in 1483.

[3] *Geschichte der Botanik*, vol. iv, p. 296.

to school teaching only after his voice had failed him, so that he could no longer preach. Here again the historian Meyer draws an inference. It is this, that his school must have been a financial success, because at the end of nine years at teaching he had saved money enough to pay the expenses of his degree at the University of Basle. Without doubting the financial success of Brunfels' school it is next to certain that he realized a much more considerable income from the sale of his rather voluminous Protestant theological writings; for these included, besides learned commentaries on certain books of Scripture, pamphlets for popular reading, and a catechism for children. There is a long list of them in Conrad Gesner's *Bibliotheca Universalis*. Altogether his two vocations of teacher and theological author must have yielded him a very fair income during these first nine years at Strassburg; for he was able to give employment to the best engraver of Strassburg, Hans Weydiz (Latinized Guiditius), who did the engraving of the *Icones*, and is a man of distinction in the history of wood engraving.

It must have been after having taken his degree in medicine, and within two or three years from the time of his death, that Brunfels made a journey from Strassburg to Hornbach for the purpose of personally urging Jerome Bock (Tragus) to write a book of botany for German readers. For the record of this visit history is indebted to Tragus himself. In the thirteenth chapter of his preface to the *Stirpium Historia* he says: "When information about the labors and the journeyings which I had undergone in behalf of plants had in some way been conveyed to the most learned Otto Brunfels of pious memory, he himself came journeying all the way from Strassburg to Hornbach, that he might see my gardens and collections. These things pleased him so much that from that day forward he ceased not to exhort, as did also others by letter, that I would reduce all this matter to order, and give it to the German public."

Not one of even the compatriot German historians of botany, in so far as I am aware, has set before us this evidence that it was to Brunfels' personal influence over Tragus that the writing and publishing of Tragus' work was due. How much botany owes to Tragus' unusual powers of observation and description we shall learn later; for the half of that story has never yet been told.

It is well worth noting that this visit to Tragus, with its fruitful consequences, was the last service which Brunfels rendered to botany. The visit must have been made as late as the year 1532; for not until that year was Tragus settled at Hornbach; and in the next year Brunfels, now newly-appointed physician to the city of Berne, removed thither; where also after only a year and a half of service he died in 1534.

After having ceased from theological authorship, and subsequently to his having taken a degree in medicine, Brunfels published several medical works; but both theology and medicine appear to have forgotten his name. In the history of botany only is he immortal; and this because he was intensely a lover of nature and of plants. His book gives proof of this, although the figures are the best part of it. It was because his love of plants could not tolerate the absurd pictures then common, that he resolved to produce something in that line true to nature, despite the cost; for the employing of the best artist of his time can not have been less than very expensive to him, and there may have been no clear prospect of any return, even of that which the plates cost him. Indeed no one can assert that there ever was any. But here was devotion to an ideal; a love of plants that was bent upon procuring faithful representations of them in books. And so a well marked epoch in the study of the plant world dates from Brunfels and the year 1530.

To the botanical memory of this ex-Carthusian, the Franciscan monk Charles Plumier dedicated the genus BRUNFELSIA in the year 1703.

Phytography. If by a man's phytography is meant his manner of describing plants, that is his word-picturing of them, it cannot be said of Brunfels that he has any; and Julius von Sachs was never farther from writing history than when he set this man forth as among those who "went straight to nature, and described the wild plants growing around them."[1] Brunfels publicly disclaims all purpose of writing verbal descriptions of any plants whatever, and in the following terms:

"In this whole work I have no other end in view than that of giving a prop to fallen botany; to bring back to life a science almost extinct. And because this has seemed to me to be in no other way possible than by thrusting aside all the old herbals, and publishing new and really life-like engravings, and along with them accurate descriptions extracted from ancient and trustworthy authors, I have attempted both; using the greatest care and pains that both should be faithfully done."[2]

His meaning as to phytography is plain. He will describe

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[1] Sachs, *Geschichte der Botanik*, p. 4.

[2] Epistle Dedicatory, to the Senate of Strassburg, second page.

nothing anew. He will ignore the contents of wretched mediæval herbals like the *Hortus Sanitatis*. He will reproduce the standard descriptions of classic Greek and Roman authors. For an example, take his presentation of the two water lilies, the white-flowered kind and the yellow. He figures them admirably, and, not having a word of his own to add to that knowledge of them which has been the common property of botanists for a thousand years and more, he supplements the two plates by three folio pages of quotations about them, taken from a list of eleven of the most approved botanical authors, ancient, mediæval, and contemporary. Here is the list:

Theophrastus
Dioscorides
Plinius
Apuleius
Georgius Valla
Avicenna
Serapion
Simon Januensis
Rases
Joannes Vigonius
Hieronymus Herbarius

And what is true as to his presentation of the water lilies holds good in the case of almost every other genus that he takes up. Rarely does he append to such a succession of quoted paragraphs a few remarks of his own; and these always indicated as his by the special caption, "Sententia nostra," or "Sententia Othonis;" nor are such original paragraphs really of the nature of descriptions. They usually express some opinion as to the identity of the plant in question; have reference to the correct application of a classic plant name. As to phytography, therefore, the Brunfelsian volumes are a treasury of select quotations from a long line of books many of which are now seldom seen. But there are no new descriptions in his volumes; and it may be doubted whether upon the whole he directly advanced the art of plant description by a syllable.

It is no impeachment of his erudition to question that he had the ability to describe plants well. There is evidence that he had not the faculty of mentally imaging an unknown plant from its description; and the ability to describe, and that of making effective use of a description are twin accomplishments, if indeed they be not almost one and the same, so that he who has the one has also the other. Certain it is that Brunfels read and studied here and there a classic plant description to little purpose. Bringing together in one chapter the classic descriptions of *Aristolochia*, the figures by which he illustrates the genus are *Corydalis bulbosa* and *C. Halleri*. So gross an error explains itself in this way. The aristolochias were of southern Europe and not found in Germany. Here, however, the roots of the fumariaceous perennials, not so unlike those of the principal aristolochia, had usurped in medicine both their place and their name. Brunfels, though professedly reforming German pharmacy by the correcting of just such blunders, did not detect this one Yet the very descriptions of aristolochia which he reprints from Dioscorides and Pliny must have shown, had he really read them, that these things could not be aristolochias. Other such errors also remained undiscovered by him, and as inexcusably; so that when his countryman and contemporary Fuchs remarked that in Brunfels the descriptions and the plates accompanying them are not in all cases at agreement,[1] he was passing but a gentle criticism on his neighbor's phytographical shortcomings.

Anthology. I have met with no evidence that during the fifteen centuries intervening between Dioscorides and Brunfels there had been any progress made in the knowledge and understanding of floral structures. There were several of Brunfels' younger contemporaries who, after the year 1530, added somewhat to anthology; but the time was yet more than two generations distant when the science of the flower was to become so far developed as to begin happily to revolutionize plant classification. There is no sign in Brunfels that such a day is near its dawning. In his attempts to range plants in groups he is no more influenced by considerations of floral structure than were the medical botanists of remote antiquity; even less so than Dioscorides, who, as we shall see, could not abide the placement of the bilabiate-flowered dead nettles in the same genus with real nettles, but segregated them, on account of their two-lipped corollas, and assigned them a new generic name of their own, and framed to express the peculiarity of their flowers. It is possible to rate the Brunfelsian anthology as more antiquated and imperfect than that of Dioscorides; for he of the sixteenth century less openly recognizes as generically distinct the Galeopsis and Lamium "nettles" and the proper *Urtica*[2]; and if he figures the thistles, the anthemideous composites, the principal borragineous plants, the bulk of the labiates, and some other such, each as a group by itself, it is done without any particular reference to floral structure, at least on Brunfels' part; for in all these instances he is but continuing groupings which the ancients themselves had indicated as being natural, and had well established.

Taxonomy. Brunfels adopts without hesitancy the ancient primary classification of growths as herbaceous and woody. When,

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[1] Epistle Dedicatory, in Fuchsius *Hist. Stirpium.*

[2] *Herbarum Vivæ Icones*, vol. i, pp. 151–154.

however, it comes to that apparently quite as ancient division of the vegetal kingdom into things cultivated and things wild, he deliberately ignores it. His first three plates represent three most common and homely wayside weeds, members of the genus *Plantago;* and thenceforward throughout his volumes he deals much more extensively with wild plants than with the domesticated.

Now this eliminating of the distinction referred to is not to be attributed to any following of the suggestions of Hippon, who some two thousand years before had declared plants wild and domesticated to be all of one lineage. There is no intimation that Brunfels had made tests, and proven out of the book of nature that this old-time grouping must be abandoned. The thought had come to him solely as a deduction from theological premises. The polytheistic ancients had held that the different alliances of cultivated plants and trees were each the creation of some beneficent particular divinity; and that the less useful or the altogether useless had hardly been created at all. The theology which Brunfels accepted, and, as a profession, taught, was monotheistic. One Divinity had made all the plants that are—the wayside weeds, the homely remedial herbs, as well as the beautiful things of the field, the garden, and the orchard. Such doctrine of the equality of all plants as to one divine origin finds expression in the last one of Brunfels' several prefaces, which contains a prayer, after which one reads his apology for giving to those common, lowly, and weedy things, the plantains, the foremost place in his system of botany. "They are the very commonest of plants," he says, "and are known to everybody; and being both lowly and also singularly useful, they are most apt to recall to mind the thought of God, whose way it is to work wonders through means that are usually accounted insignificant, passing by such as make more display, and which men therefore hold in more esteem."[1] This is even showing a preference for wild growths before those that have undergone domestication; a kind of preference that has been felt by the great majority of philosophic botanists from Brunfels' time to ours; and by virtue of his being the first propagandist of this new idea he sets up another landmark in the history of botany.

This idea of the equal genetic dignity of all plants seems to have come to Brunfels as a deduction from a theologic principle, rather than inductively from the study of nature; but whence he derived it signifies nothing to the disparagement of the idea itself; especially now, after all the world has come to concede its truthfulness. But

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[1] *Herbarum Vivæ Icones*, vol. i, p. 22.

it was not at once approved in Brunfels' time. There were learned men among his contemporaries who were at first startled by, and then made light of his having brought forward some of the most plebeian and beggarly roadside pests, and introduced them as upon an equality into the company of the nobler growths of the fields and meadows, and of the vegetable and drug gardens. Among the more serious faults that his contemporary Fuchsius found with Brunfels' work, one was "That he sometimes takes for subjects the most common weeds."[1]

By at least one other item of his method, over and above this of ignoring the old distinction between things as domesticated and wild, does Brunfels commend himself as a believer in some kind of a natural classification. He declines to adopt anything like an alphabetic sequence of genera; a kind of arrangement which was adhered to by several of his noted botanical contemporaries, as we shall see. He prefers freedom to express, if but tacitly, some ideas of a more rational grouping, such as the alphabetic succession of names almost wholly precludes; and, with the medical botanist, that arrangement may be most convenient, if not even in a sense natural, in which plants, whether alike or unlike as to morphology, are held in juxtaposition by agreement as to what are taken to be their medicinal virtues.

For an example of this kind of classifying carried to an extreme, take his two genera of liverworts, *Hepatica* and *Jecoraria*. The former is that anemoneous herb that has retained in later times the name *Hepatica;* the other is the common *Marchantia polymorpha*, a cryptogam. The two are figured and described on opposite pages, and their medicinal uses are said to be the same.[2] It may be noted that each bears alike, even in our time, the common name of liverwort. Before Brunfels *Hepatica* usually meant the plant *Marchantia*, which was also called *Jecoraria*, and the restriction of the name *Hepatica* to the genus of anemone allies, and of *Jecoraria* to the lichenoid hepatic, seems to date from Brunfels, and was a distinctly taxonomic movement on his part; as if his judgment had been that types so very unlike morphologically ought not to be treated of under one and the same generic name.

Because of their having been employed interchangeably in medicine, under the common designation of *Verbena*, our medical botanist figures and discusses, one next after the other, *Verbena officinalis* and *Senecio vulgaris*.[3] The botanist of a later time will

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[1] Fuchsius, *Hist. Stirp.* in Epistola Nuncupatoria.

[2] *Herb. Viv. Icon.*, vol. i, pp. 190, 191.

[3] *Ibid.*, pp. 119–123.

see no likeness or other sign of true affinity between these; and it is very probable that Brunfels himself realized how very distinct they are when considered from the morphological rather than the remedial point of view; for when he formally designated one of them *Verbena mas* and the other *Verbena fœmina* it is beyond question that he was purposely indicating the morphological distinctions between them; quite as he had done in assigning to those two morphologically dissimilar liverworts each a generic name of its own. And according to the usage of Brunfels' time, as well as for two centuries later, binary generic names like *Verbena mascula* and *Verbena fœmina* were thought as suitable, and were as freely made and admitted, as those of one word only.

Of such attempted improvements in classification by appeal to considerations of morphology, one may come to a fuller appreciation by looking into Brunfels' way of presenting those many herbs which, in his time, had long been reputed to be good vulneraries, and had therefore passed under the medico-generic name of *Consolida*, with which *Symphytum*, *Sanicula*, *Vulneraria*, and *Solidago* were synonymous, each such name indicative of the property which these plants all had, or were believed to have, of promoting the closing-up and healing of cuts and wounds. Here is a partial list of these plants under their mediæval names, with their equivalents in modern nomenclature:

Mediæval	Modern
Consolida major	Symphytum officinale
Consolida media	Ajuga reptans
Consolida minor	Sanicula Europæa
Consolida petræa	Coris Monspeliensis
Consolida regalis	Delphinium consolida
Consolida rubea	Tormentilla erecta

One thus gains an idea of how great a diversity of plants passed with mediæval pharmacists and physicians under the generic name *Consolida*. And the list must now be given again, that the Brunfelsian taxonomic betterment of it may as readily be seen:

Mediæval	Brunfelsian
Consolida major	Consolida major
Consolida media	Consolida media
Consolida minor	Diapensia
Consolida petræa	Symphyton petræum
Consolida regalis	Consolida regalis
Consolida rubea	Tormentilla

One observes that out of the six Consolida names, three have been eliminated, and others brought forward to take their places. I say brought forward; for neither *Diapensia*, nor *Symphyton petræum* nor *Tormentilla* is coined and proposed as new by Brunfels. He picked them up every one out of the ancient and mediæval synonymy of the vulnerary herbs; from which also it will appear that other men who lived and wrote botany in times long forgotten—but the history of which times must none the less some day be written—thought as Brunfels did, that plants totally unlike in appearance, *i.e.*, morphologically very different, ought to be invested with names more than partially different, even when as to qualities and uses they were very similar. And these group names established upon the merely remedial virtues of things visibly most dissimilar must have been misleading and confusing in the extreme. It seems as if Brunfels realized this, and intended to suggest improvement when he set aside three out of the six Consolida genus names and wrote others in place of them. It is as if he had thought it out, that since the different kinds of plants can only be well distinguished and scientifically grouped through attending to their morphology, it is not well that they should bear names that point to their qualities rather than to their forms. Therefore, in the interests of a more sure identification of important plants, as well as at the same time encouraging the appeal to morphologic marks in classifying, it would be a good thing to at least place a check upon this multitudinous repetition of pharmaco-generic names, the first half of which is the same for a half-dozen very dissimilar genera.

If it be asked why he did not, while he was about it, proceed to the suppression of as many as five out of the six Consolida genus names—leaving perhaps one of the genera to bear the simple name *Consolida*—the right answer will seem to be that Brunfels was not of the temperament of the taxonomic revolutionist but only a reformer, and disposed to be somewhat conservative even as a reformer; between which character and that of the bold iconoclastic revolutionist there are differences.

Entirely consistent with his aversion for genera made up of plants qualitatively alike but morphologically unlike, is Brunfels' approval and adoption of some in which the species are qualitatively unlike, and at agreement morphologically. Such a genus as this is that which he fully illustrates under the classic name of *Urtica*,[1] which in the botany of to-day comprises only the true

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[1] *Herb. Viv. Icon.*, vol. i, pp. 151–157.

nettles. The specific constituents of his *Urtica* seem to be

Urtica dioica,
Lamium maculatum,[1]
Galeopsis Tetrahit,
Urtica urens,

and this, too, is the order in which they succeed one another in the book; first a real nettle, then two so-called dead nettles, the line closing with a second true nettle; a genus composed of two nettles, and two or three members of the very different family of the Labiatæ. And this, as intimated above, is a genus not qualitatively but morphologically constituted; a fact easily demonstrable when it is remembered that Brunfels had no anthology; that the flowers of plants not only were not at all understood by him, but were the least and the last parts of them to receive any consideration. So long as two or three herbs were alike as to roots, stems, and leaves, they might easily be designated by the same common, *i.e.*, generic, name. Forgetting, then, all anthologic differences between nettle and dead nettle, note how remarkably they are at agreement. The roots in all are small, fibrous, and not deep-seated. The stems of all are upright, almost or quite without a branch, conspicuously quadrangular, and the leaves they bear are opposite. The leaves in all are short-stalked, their blades of the same ovate or oval outline, serrate as to their margins, and are of much the same texture as well as form. The seeds in all—for though anciently flowers were neglected, seeds never were—the seeds were black, and were always clustered together in the axils of the leaves all up and down the stem. All these quite marked characteristics of all their vegetative organs *Urtica* and *Lamium* and *Galeopsis* have in common.

Since the thought is one far from being familiar to the botanical mind of the present, it must here again be insisted on, that the grouping together of several plants upon vegetative characters only, but under a generic name, is as exactly of the nature of a generic concept as that group which is rested on characters of flower and fruit only. By either method a genus may be circumscribed which shall be unnatural; and the idea is equally the idea of a genus in either case.

I do not see what chapters of any history of botanical science should be more profoundly significant, or of a more general interest, than those touching upon the development of men's ideas of a plant genus; for the idea of the genus seems to be first and last the type-

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[1] But this one not figured, though by implication included, as quoted from Hermolaus Barbarus by Brunfels, on page 154.

idea, if one may intelligibly so speak, of taxonomy. It was because this seemed to be true, that in our Introductory on the Philosophy of Botanical History, the earliest available expressions of such idea by even the primitive and the unlettered were dwelt upon somewhat at length; and for the same reason, one desires to examine with the utmost care leading expressions of the idea of a generic group as they occur in this almost the earliest propagandist of what has slowly developed into the thing known as modern botany. Brunfels was of thoroughly well educated mind, even a profound scholar, also naturally endowed with a keen insight into the beauties and the harmonies of plant life and form. On all these accounts it would be exceedingly interesting, if it were possible, to know just what his own opinion really was as to the philosophic tenability of such a genus as this which we have been inspecting; a genus *Urtica*, by name, but made up of species some of them urtical, but as many others labiate. If he has any taxonomic opinion different from that which, in as far as we have proceeded, he seems to have expressed, we shall be likely to find the evidence of it, if there be any, by reading as it were between the lines; for even a botanical genius, if writing as Brunfels professes to write, in the interests of medical botany only, inditing a work the readers and students of which are to be the physicians and the pharmacists, must not yield to every impulse he may feel to improve taxonomy; for such improvement commonly involves changes in nomenclature, and there is nothing of which the druggist, or other plant industrialist, is more intolerant than changes in names of his commodities.

The opinion, if Brunfels held it, that nettles proper and labiate-flowered nettles are generically distinct, was not original with him. We observe that Dioscorides as long ago as the first century of our era segregated the dead nettle as a genus, and under a name which pointed to the character of its flower, the name *Galeopsis;* and this proposition had evidently been acceded to by some of the mediæval Latin botanists, who, instead of the Greek γαλίοψις, had employed such Latin equivalents as *Urtica mortua*, *Urtica iners* and *Urtica labeo*, the last a most significant appellation, "nettle with a lip," evidently taking cognizance of the floral character, while the other two refer merely to the lack of stinging hairs. Now this mediæval synonymy of the plants is perfectly familiar to Brunfels. He formally quotes every item of it; and his approval of *Galeopsis*[1] as a proper genus comes out plainly enough,

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[1] In modern botany the genus is written *Galeopsis*. Dioscorides (Book x, ch. 80) wrote it *Galiopsis*, as did also Brunfels.

at least to the careful reader, and on this wise. In reproducing what Dioscorides had said about the labiate nettles he does it under the following caption: "De GALIOPSI simili Urticis herba, DIOSCORIDES."[1] Of such a caption the English version is, "Concerning GALIOPSIS of Dioscorides, an herb resembling the nettles."

Of course, that which resembles another thing *is not* that other thing which it resembles; and nothing that was ever printed in a book is plainer than that this author did not regard *Galiopsis* as congeneric with *Urtica*. When in his *Icones* he sandwiches the dead nettle in between two real nettles, and when as a heading to his Chapter XXIII., in which both kinds are discussed, he places that simple "DE URTICIS," he is purposely adapting himself to the understanding of the half-taught root and herb dealers, and the untaught old women, who call them all nettles indiscriminately. In a word, Brunfels is a man of some learning and insight in matters botanical, and also a man of discreet conservatism; holding it unwise to lay too openly before the general public every advanced taxonomic view that is his own.

In his indubitable though dissembled accepting of *Galiopsis* as distinct from the nettles he cannot but have been impressed by the fact that Dioscorides in making the segregation had done so in deference to its flowers, which he described as being "slender and purple"; and it may or may not have been in deference to similarity in floral structure that closely appended to the Urtica-Galiopsis series comes an unbroken line of three other galeate-flowered labiate types. If, however, this be an example of guidance by anthology to the recognition of affinity, still it is a guide which Brunfels is as far as possible from following steadily. The flowers of orchidaceons plants have as much agreement in character as have those of labiates; but when he comes to the grouping of what are known to us as the orchid genera we find that all those which have two or three large tuberiform roots are gathered into one place by themselves,[2] while their merely fibrous-rooted kindred form a group quite apart from these,[3] various wholly unrelated types intervening between the two orchid groups. This is all quite after the method of antiquity; the method of those who, heedless of flowers, to the knowledge of which they had not advanced, concluded things to be allied because they were alike as to roots, and, it may be, as to stem and foliage also.

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[1] *Herb. Viv. Icon.*, vol. i, p. 155.

[2] *Ibid*, pp. 103–110.

[3] *Ibid.*, pp. 181, 182.

Let us give a moment's attention here to another set of plants which he places in juxtaposition for the reason that they all exhibit a kind of tuberiform organs as developed among their roots. The group is

Brunfels	Modern
Scrophularia major	Scrophularia nodosa
Scrophularia media	Sedum Telephium
Ficaria	Ficaria ranunculoides

The point that is of special interest here is, that for the third member of the group Brunfels rejects that which was one of its common mediæval names, that is, *Scrophularia minor;* though more anciently, even with Dioscorides, it was called *Chelidonium minus.* What he did with this third plant of the list seems to attest that there was in him, botanically, as there was ecclesiastically, something of the spirit of the revolutionist, or reformer. If there had not been, he would have been almost sure to have called this ranunculeous herb by one or other of its ancient and mediæval names rather than startle the herbalists and pharmacists of his time by that new name, *Ficaria,* for a type so long known under very different appellations. We shall also, I think, miss a part of what was in his mind, if we do not read here the expression of an objection on his part against the old way of naming and grouping of plants conformably to their medical qualities rather than according to their morphology. All three of the plants had been called kinds of Scrophularia, because they were believed to be efficacious against scrofula; and there is with me no doubt that Brunfels in displacing one of the old Scrophularia names by the new generic name Ficaria is to be understood as mildly protesting against qualitative criteria of plant affinities, and affirming the need of appealing to the morphologic.

We were observing above how Brunfels might be said to have limited his group of the orchids to such genera of them as have a certain kind of underground organ; that he excluded from the group such as have only fibrous roots, himself all the while oblivious —as all the world before him always had been—of the flowers by the structure of which all stand at agreement. Let us now observe him locating as far away from each other two groups of genera known to us as borragineous plants. In this instance he does not separate on ground of differences as to roots, or form of leaves, but of pubescence only, that is, over and above certain qualities common to all. Upon such principles are *Echium, Cynoglossum,* and *Borrago* made to form a group of genera.[1] We of to-day, after four centuries of taxonomic progress, concede that Brunfels was correct in apprehending a very intimate consanguinity between the three. But we hold them in juxtaposition on quite other grounds than those which had weight in the early sixteenth century. We judge them near allies because the plan of their flowers, and the common characteristics of their fruits are the same. With Brunfels the flower was so almost wholly unknown that no such thing as the plan of a flower had been thought of. And, viewed superficially—the only view that had yet been taken of flowers at all,—they were very notably dissimilar. The corollas of the genera are of remarkably distinct types, that of *Echium* being narrowly tubiform below, with an irregular almost bilabiate limb; that of *Cynoglossum* is short-salverform, perfectly regular; that of *Borrago* broadly and flatly star-shaped. Few families of plants present three genera so unlike as to the cut of their respective corollas as these three. We therefore seem to infer to a certainty that in collocating these three generic types, he had had the utmost regard to their likeness as to roots, stems, foliage, and especially to that armature of harsh somewhat stinging bristles wherewith all three alike defend themselves; and that in the process of his reasoning the corolla, *i.e.*, the "flower," was not at all considered. And, as if to place this beyond dispute, two other borragineous types are relegated to another part of the book. One is a *Cynoglossum*, the other a *Myosotis*.[2] Both differ from the other group in that they show no trace of the stinging-bristly or any other rough indument. They are almost silkily soft-hairy. Had he not held such differences to be most significant, taxonomically, it is impossible to see why he separated so widely these two groups of what we of to-day understand to be near allies.

If one is to follow the progress of plant taxonomy from the year 1530 forward, it will be needful to bear in mind such things as Brunfels' failure to apprehend the consanguinity of all the borragineous genera that he knew; as well as to note, if perchance one may discover the reason, why he failed. Then afterwards it must be observed how those who came after him, one after another and little by little, brought the other genera of such a family into a continuous sequence; also all the while attending to—even carefully noting—the development of new principles, whatever they may have been, in accordance with which the better taxonomy of more

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[1] *Herb. Viv. Icon.*, vol. i, pp. 111-113.

[2] *Ibid.*, 175–177.

recent centuries has been attained to. Thus may we learn, and thus only, the lesson of the modern development of the very old idea of plant families.

Nomenclature. All plant names are names of groups; and to group things together under a common name is to classify. Nomenclature and classification are therefore so intimately connected that neither topic can be fully discussed apart from some consideration of the other. They can not be completely divorced; and so it was inevitable that something in relation to Brunfels as nomenclator should be brought out under the heading of taxonomy. It will be useful, however, to epitomize his work as nomenclator, and particularly since he now and then evinces a disposition to amend and improve upon ancient and mediæval names and name-making methods; or, it might perhaps better be said, a disposition to return from mediæval to ancient methods; for what I have in mind is something like a distinction which, in a general way, holds between what may be termed the ancient and the mediæval plant naming. It is, however, not much more than a difference between the genius of the Greek language and that of the Latin as to manner of framing distinctive names for things.

In Greek the noun and adjective readily combine to form a single word, such word beginning with the adjective part and ending with the noun; whereas in Latin noun and adjective are kept as distinct words, even with the noun rather than the adjective standing first. To make this as plain as possible let us use a few examples:

Greek	Latin
Leucoion	Viola alba
Melanion	Viola nigra
Chrysion	Viola aurea
Herpetion	Viola repens
Chelidonion	Viola hirundinaria

By many scores of such one-worded Greek plant names which by translation into Latin become binaries, there is revealed one of the misfortunes under which mediæval and early renaissance botany labored everywhere—for mediæval botany was Latin botany —that of having in its employment hundreds of binary names, some of which were of specific import, while as many more were but the names of monotypic genera.

The continual perplexities involved in this phase of nomenclature seem to have exercised the mind of Brunfels to a degree, so that he made bold to displace here and there some binary generic name, substituting one of a single word. A few examples of such action on his part were brought forward under the heading of his taxonomy. A more considerable exemplification of this practice is given below, in a selection made from the first volume of the *Vivæ Icones:*

Early binary generic names		Brunfelsian substitutes
Fumus terræ Fumaria herba	=	Capnos
Ferraria major Consolida minor	=	Sanicula
Testiculus canis Testiculus vulpis	=	Satirion
Lingua bubula	=	Borrago
Sacra herba Verbenaca supina Cincinnalis herba Lacryma Junonis Herba sanguinalis Sanguis Mercurii Mustelæ sanguis Crista gallinacea	=	Verbena
Trixago minor Quercula minor	=	Chamædrys
Scrophularia minor Chelidonium minus	=	Ficaria
Herba Apollinaris Faba suilla	=	Hyoscyamus

The credit of having reformed the nomenclature of genera by the exclusion of names made up of two distinct words has been given to Linnæus, who, in the year 1751, is thought first to have laid down such a principle.[1] But the actual reform had been quietly inaugurated by Brunfels two hundred and twenty years before Linnæus came forward with his *Philosophia Botanica.*

Sprengel, the one nineteenth century author of a *Genera Plantarum* who has observed the law of priority in the crediting of generic names, ascribes to Brunfels the authorship of the following:

Ammi
Calendula
Fragaria
Linaria
Pyrola
Sanicula

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[1] Nomina generica ex duobus vocabulis integris, ac distinctis facta, e Republica Botanica releganda sunt." Linn., *Philosophia Botanica*, Art. 242.

Carthamus
Castanea
Euphrasia
Melissa
Parietaria
Potentilla
Spinacia
Scrophularia
Valeriana

To this list of fifteen, credited to this author by Sprengel, I find two more to be added, namely *Hepatica* and *Ficaria*. Sprengel's reason for not taking them into the reckoning was simply this, that he did not admit the types as worthy of generic rank, but held with Linnæus that the former was but an *Anemone* and the latter a *Ranunculus*.

Now when Sprengel and other advocates of priority credit such genera to Brunfels, it is not to be understood as their meaning that in his book these types are for the first time named and defined. The truth is, that all of them had been known before Brunfels, and some of them had been much written about, under different names. For a heading to each chapter in which a genus is discussed, Brunfels selects, out of the several names current for that genus, the one that pleases him best; and, by virtue of the great prestige which his book obtained, the plant names in it were continued in use by other authors. Therefore they who credit *Sanicula*, *Potentilla*, *Fragaria* or *Hepatica* to Brunfels affirm no more than this, that each such name, as the fixed appellation of a certain generic type, is traceable back to Brunfels.

In his researches upon native German plants he came to know here and there a type which, after the most diligent comparison with all the classical plant descriptions, he felt certain had not been known to the ancients, neither been described by any one. They were new generic types; and to such he never assigns any name at all, other than that by which it is known to German peasants. There is beautifully figured in one place a flowering plant of *Cardamine pratensis*.[1] Above the figure the German name Gauchbluem is inscribed; beneath it the statement in Latin that the plant was unknown to the ancients, though common enough in Germany, and native. One page is occupied by a most accurate and life-like representation of *Anemone nemorosa*, with the legend: "A wildwood herb, the name of which is unknown."[2] Nor is there any other mention of the plant; not so much as a record of its being known by a vernacular name. This is doubtless the earliest publication of the Wood Anemone.

Out of such namelessly figured types there might here be gathered surprising items of plant history. For one instance: any one

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[1] *Herb. Viv. Icon.*, vol. i, p. 218.

[2] *Ibid*, vol. ii, p. 80.

informed as to how familiarly hundreds of useful plants were known two thousand years before Brunfels would expect to find so common and important a plant as *Trifolium repens* among that number. Nevertheless, it is one of the things which Brunfels presents as new to botanists.[1] He says it is well dispersed throughout Germany, chiefly in meadows, and is known to the common people by the name of Weiss Fleischbluem; also that his engraver brought him the plant under that name. That Old German name of the plant, and Brunfels' brief remark upon it, both printed on the plate page, seem to constitute the earliest publication of *Trifolium repens*.[2] Both the botanist and the artist seem to have agreed in the opinion—a purely philosophic one—that no little weed was beneath botanical notice, and between them they have given us the beginning of the history of *Draba verna*. The plant is elegantly figured under the vernacular designation of Gensbluem; but not another word is said about it.[3] It is, however, the first record, and a perfectly definite record, of an interesting though diminutive type; one that within the last century has been much discussed by very able botanists who have investigated it morphologically, taxonomically, and even as to its rightful name; and that Old German popular name Gensbluem—in later German Gansblum—has proven a somewhat fateful appellation. More than two centuries after Brunfels had printed it, Michel Adanson proposed its adoption as being by right of priority the lawful generic name.[4] For two reasons, not calling for mention here, Adanson's movement failed of any public approval. Yet once again, in the end of the nineteenth century, Otto Kuntze renewed he Adansonian proposition;[5] but the attempt to reinstate Gansblum was again fruitless, at least as to gaining public approval. It was not a Latin-made name. Probably it did not occur to Brunfels' mind that a little weed, of no use in medicine or any art, needed to be dignified by any other name at all than that by which the country people of Germany knew it.

To the nomenclature of species it is evident Brunfels gave no thought; nor was there any reason why he should have given it any attention. Most of the genera, with him as with the botanists of antiquity, were monotypic, and the generic name was all that was

[1] *Herb. Viv. Icon.*, vol. ii, p. 55.

[2] The botanists of remote antiquity knew but one plant which they called *Trifolium*. It figures in modern nomenclature as *Psoralea bituminosa*.

[3] *Herb. Viv. Icon.*, vol. ii, p. 34.

[4] Adanson, *Familles des Plantes*, vol. ii, p. 420 (1763).

[5] Kuntze, *Revisio Generum*, vol. i, p. 29 (1891).

needed. There was not the shadow of a reason for appending a second name; and he, no more than hundreds of botanical writers before his day, ever thought of such a thing. Sometimes when there are one or more notable modifications of a type—varieties or species of it—the original goes by the generic name only, while the others have each its own cognomen. Of this sort is his nomenclature of three buttercups which he figures and gives account of.[1] In the nomenclature of to-day they are (1) *Ranunculus acris*, (2) its double-flowered garden variety, and (3) *Ranunculus bulbosus*. The generic name which Brunfels adopts is *Pes corvinus*, *i.e.*, Crowfoot, turned into Latin. With him the first species is simply Crowfoot, its variety of the gardens is Full-flowered Crowfoot, the third plant is Lesser Crowfoot.

This early practice of leaving the one original representative of a genus without any cognomen, even after said genus has ceased to be monotypic, is a practice doubly suggestive in relation to the philosophy of nomenclature; for, in the first place it plainly reveals the antiquity of the idea of generic types, and emphasizes it. In the second place, the failing to assign a cognomen to the type species entails a difficulty; becomes a possible source of ambiguity and perplexity; for, *Pes corvinus* being mentioned, the question may chance to be asked: Which one of the three? That question is virtually a demand, and a most reasonable one, that the type species have also its particular cognomen. That botanists of fifteen centuries anterior to Brunfels had seen this to be desirable, one may infer from the nomenclature of *Plantago*. Two species of this genus were known to Pliny; and he had a specific cognomen for the type species as well as for the other. They were *Plantago major* and *Plantago minor;* and Brunfels follows Pliny in this. His type species is not simply *Plantago;* it is *P. major*, which name, as well as *P. minor*, the German father duly credits to the Latin author of the olden time. He uses, then, a specific name for the original representative of a genus when there is classic authority for so doing; but I have not observed him taking the initiative in this course by actually himself assigning to the type species of any genus a cognomen.

These paragraphs on Brunfels as nomenclator ought not to be concluded without our having taken a briefly comprehensive survey of his principles. These principles, such as he was more or less ruled by, will be all the more suggestive to us from the very fact that he did not professedly have any; for doubtless he had never

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[1] *Herb. Viv. Icon.*, vol. i, pp. 143–150.

so much as heard, or even thought the phrase "botanical nomenclature." The laws governing the naming of plants were not different from those observed in the naming of other things. All that we may gather by observing his procedure along these lines may be the course which a cultivated and philosophic mind, unhampered by prejudices, will naturally take. But such a study will be well worth while; because one does not often meet with an author who so nearly antedates all our stereotyped conventionalities, and takes his own course so little influenced by traditions and prejudices. Without having enunciated one of them, he seems to have been more or less under the guidance of principles like the following:

1. That for the science of botany there is an initial book; that is the *Historia Plantarum* of Theophrastus of Eresus. He quotes that work constantly, but never, I think, any earlier book or author. Others of Brunfels' time and a little later we shall find citing Moses, Solomon, and other Hebrew writers as if these had been botanists; but not so Brunfels, who, notwithstanding his training in theology, and the distinction he had won as a Biblical scholar and commentator, does not intimate that he has found botany in Holy Scripture, and never cites an author who antedates Theophrastus. It will not, however, follow that he must adopt Thephrastan generic names in such wise as to make that author's monumental work the point of departure for nomenclature. The existence of an historically first book of botanical science is one thing. The having a starting point for an universal nomenclature of botany is quite another; and the two are both logically separable and historically separate. Brunfels was well informed about the historic beginnings of botany; but the idea of an universal system of nomenclature for groups of plants had not in his day been conceived.

2. Brunfels writes in Latin. The text of his book is for those who know Latin, and, knowing it, know things by their Latin names. The writer is under the necessity of using the Latin names of plants rather than those by which the same plants are known in Greek or Hebrew, Arabic or Persian. If a man pretending to write in Latin about animals should write *hippos* instead of equus, or *alopex* in place of vulpes, he would stultify himself; would be writing unintelligibly, absurdly, and ridiculously. It is not imaginable that Brunfels, in a Latin book of botany, should have done so insanely as to write *drys* instead of quercus, or *kittos* in place of hedera, *ion* rather than viola, or *arnoglossa* rather than plantago.

Nevertheless he might have done so and most reasonably; indeed he must have done so, had the ideas of universality and priority in nomenclature been conceived and approved by him; because botany is of Greek rather than Latin origin, and so the Greek names of plants happen to be older than the Latin names. It was needful here to take a survey of the whole situation; for from Brunfels forward we must be looking for adumbrations of any of those principles which in our time have come to rule—or misrule—biologic nomenclature.

3. Even as a Latin writer, and using none but Latin generic names as headings for his chapters, Brunfels does not pay respect to priority. He readily adopts, out of several Latin names, ancient and mediæval, not the oldest, but the one that best suits his own purpose or fancy. From before the Christian era until six or seven centuries after it the water lilies had been known as the genus *Nymphæa*. Then from the eighth century forward to the thirteenth and later the Arabic name *Nenuphar* had usurped its place in Latin botany generally. Brunfels adopts *Nenuphar* and writes *Nymphæa* down among the synonyms; this manifestly for the reason that most of the botanists and druggists of his own time knew the plants as *Nenuphar* and would be disturbed if he should restore the classic name. Here, then, we have

4. The principle that the name by which a genus is known to most of one's contemporaries is the one to be taken up, there being no other objection against the name.

5. That a later name consisting of one word only is commonly permitted by Brunfels to supplant a very ancient one made up of two words has been already quite clearly demonstrated.

6. A species name, or cognomen, is not assigned the type which alone represents its genus.

7. While plainly favoring the selection of the best of several names as the one to be perpetuated, Brunfels, as if realizing the inconvenience of having many synonyms, is moved to use the greatest care and caution against creating them; that is, against creating Latin synonyms. This is well shown by his great aversion to assigning Latin names to types which to him appear undescribed. He publishes freely the engravings of such, but is careful to label them with no other than the German vernacular names. I have not found him once deviating from this very conservative practice. And, under his beautiful plate of *Pulsatilla*, in a long paragraph he explains why he holds to such a course. In none of the authors whom he has been able carefully to study has he found any descrip-

tion of this sort of plant. It may have been named and described somewhere nevertheless. He is resolved to print the figure, and leave it to others who have more leisure than he, to study it in the light of all descriptions to them accessible.[1] Meanwhile the thing may be known by one or other of those German names by which the common people know it.

8. The student of botanical nomenclature should here note well the distinction which Brunfels tacitly makes between the Latin names used by Latin botanical writers, and those invented in their mother tongue by the common people. It is plain that with him they have not the same status. The vernacular name cannot figure among the Latin synonyms. It is upon no equality with them. His action and his words together bring it out clearly that, in his mind, there is a botanical nomenclature, and synchronously with it a kind of plant naming that is not valid scientifically. The botanists of antiquity had not, and hardly could have had this thought. Is the expression of it new with Brunfels? He who is to answer this question must first learn pre-Brunfelsian and mediæval botany. The prevalence of that opinion is long since become universal, despite its having been ably disputed two centuries after Brunfels. It will be important to the history of nomenclature that one trace its progress from Brunfels forward.

9. In respect to the nomenclature of species it should be observed that what is often spoken of now as the phrase name, or more unadvisedly the "polynomial," and commonly attributed to all botanists preceding Linnæus, is a thing unknown to Brunfels. In genera of several species I have not found him using in a single instance any name that is more than binary. Where there are three words to a name the first two are the generic name.

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[1] *Herb. Viv. Icon.*, vol. i, p. 217.

CHAPTER VI

LEONHARDUS FUCHSIUS, 1501–1566

An early and a clear vindication to Brunfelsius of the honor of having made an epoch in the history of botanical iconography is the fact that his *Herbarum Vivæ Icones* inspired a younger countryman of his to embark at once in a still larger enterprise of the same kind; this with the manifest purpose of outdoing the originator of the movement. Brunfelsius and Fuchsius were alike in that they were college bred Germans and university graduates; their early and also their later academic and professional training having been acquired in Catholic schools, and mostly while they themselves were yet Catholics; and they were witnesses of the beginnings of the Lutheran movement, and both became partisans of the Augustinian; Brunfels with voice and pen actively and zealously furthering the movement, and Fuchs so expressly in sympathy with it as to have forfeited thereby the professorial chair with which his alma mater had early honored him. Both were regularly graduated medical practitioners, and both eminently successful, even famous, as physicians; though this good fortune came to the elder of the two only very late in life, and after he had abandoned theology and polemics.

Life. Fuchsius was a native of Memmingen, Bavaria. The father died when the child was in his fifth or sixth year. The small boy must have been precociously intelligent. The care and cost which the widowed mother bestowed on his education, and the academic honors conferred on him in boyhood, youth, and very early manhood attest this. At the age of ten he was sent away to a noted school at Heilbronn, at eleven to Erfurth where, after a year and a half of very special and zealous devotion to what were already his favorite studies, the ancient languages, the university conferred on him the degree of Bachelor of Philosophy; this before he had completed the thirteenth year of his age. During a year and a half, and that while he was somewhere between the ages of fourteen and eighteen years, he was master of a private school in his native town; to the success of which undertaking on the part of one who was a mere boy in years, a precociously large stature, quiet seriousness of mind, and a manly dignity of bearing are said to have contributed.[1] At the age of eighteen he again left home, and this time to enter the university at Ingolstadt, where at first he applied himself to advanced classical studies, and two years later obtained the degree of Master of Philosophy. Entering at once upon the study of medicine at the same institution he won the doctorate in the year 1524, at the age of twenty-three. He undertook to establish himself in the practice of medicine at Munich; but after a residence there of some two years, within which time he had married, he was called to the Professorship of Medicine at Ingolstadt. Here another and more honorable place was soon tendered him, and he became physician to the Margrave George of Brandenburg. During an outbreak, at Anspach, the residence of this prince, of that very fatal epidemic which one reads of as the plague, Fuchsius acquired reputation by the success that attended his treatment of the disease. He remained physician to George of Brandenburg some five years, and it was during this period that his career as an author began. He published a Compend of Medicine, then a translation from the Greek of one of the books of Hippocrates. He was now called a second time to the Chair of Medicine at Ingolstadt. The call was accepted; but again the stay was short. This university still remained one of the strongholds of the old faith. Doctor Fuchsius let fall expressions of sympathy with Luther's movement. Within less than a year he withdrew, returned to Anspach, where the Margrave George gave him welcome, and reappointed him body physician. The next year witnessed another outbreak of the plague, and this time Doctor Fuchsius with his wife and children fled the place.

In the year 1535 he received a call to the Chair of Medicine in the then newly established Protestant university of Tübingen. Here he remained to the end of his life, that is, for thirty-one years; and they appear to have been years of the most arduous and unremitting activity. His lectures on medicine were extraordinarily popular, and the intervals between lectures were occupied by the duties of the practitioner. He declined one offer of a professorship in the celebrated University of Pisa, and another to the office of Physician to the King of Denmark.[2]

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[1] Melchior Adam, *Vitæ Germanorum Medicorum.*

[2] Meyer, *Geschichte der Botanik*, vol. iv, p. 311.

As to when, and amid what surroundings Fuchsius became interested in botany, I have met with no record. It may, however, safely be assumed that no passion for nature study, or for plants in particular, was congenital with him. There is internal evidence in his book that as a botanist he was not born but made. The curricula of the schools of medicine at that period offered the possibilities, at least, of the making of botanists. The medicines in use were still chiefly plant products, either native or imported from Asia. The names of them were plant names. Each was the subject of a chapter in the standard books of the materia medica. Those books were all ancient, and had been written by the Greeks, Hippocrates, Theophrastus, Dioscorides, Galenos. Their chapters were the texts on which university professors of medicine lectured to their students; and the identification of the plants more or less succinctly described in those classic chapters was a part of the regular work. It was the examination of plants in the light of what purported to be the original and authentic descriptions of them. Critical work of this kind may be done by a student as a piece of drudgery, or it may become an inspiration. To those not too sluggish it must have been stimulating to be able to demonstrate by Greek texts ten or fifteen centuries old that the vendors of drugs were selling important medicines under wrong names; that what they sold under the Greek name *aristolochia*, for example, was not in the least like what the great Greek physicians had used under that name either morphologically or qualitatively. And if such questions took them to the drug gardens, or led them afield into wild places in search of medicinal plants in their fresh and growing condition, all this would tend to the fuller development and the deepening of a sincere interest in botany.

There is every reason for believing that Fuchsius' interest in botany was thus awakened. His earliest botanical publication fully substantiates this view. It was issued by Brunfels as an appendix to the second volume of his *Icones* in 1531, that is, ten or eleven years earlier than the appearing of his principal botanical work. Its title translated is *Leonard Fuchs' Notes on certain Herbs and Simples not yet rightly understood by the Physicians.*[1] It consists of thirty-four long chapters upon more than as many plants and plant products then in use; dealing mainly with the right application of ancient names; often quoting the language of authors whom he

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[1] "Leonardi Fuchsii Annotationes aliquot Herbarum et Simplicium, a Medicis hactenus non recte intellectorum." In Brunf. *Icon.*, vol. ii, app., pp. 129–155.

charges with having brought in this confusion under which the pharmacy of his time is laboring, and denouncing the errors of such authors with scathing sarcasms. The aim of the essay is the elimination of gross errors from the pharmacopeia, and all the subjects are plants that have been in use for ages. Nothing new is added; neither is there any trace of the philosophic investigation of the plant world as such; or the revelation of any interest in plant life and form in themselves considered. But this is Fuchsius' juvenile botanical production. Will there be awakened within him later an interest in plants as plants rather than as drugs?

There is no evidence that such an awakening ever came; or that any considerable part of his work with plants had other than utilitarian ends primarily in view. In the last chapter of this earliest piece of his botanical writing he expresses the design of going through the whole of Greek and Latin botany, correcting errors and giving the right identification of everything, after the method exemplified in the treatise he is now concluding; even adding that he has been urged to do this by those fully convinced of the great need of such a work. But this promise remained unfulfilled. The twofold duties incident to a professor's chair and an extensive medical practice claimed his energies, and the twofold emoluments enabled him to undertake a line of botanical work—botanical recreation, rather—which it is improbable he ever would have thought of but for the great success which had promptly attended the publication of Brunfels' *Icones*. He employed two painters, and also the best engraver in Strassburg,[1] and set them to work figuring plants. Thus within seven or eight years after the appearing of Brunfels' work Fuchsius had ready for the press his great volume of the *Historia Stirpium;* though it was not issued from the press until some four years later, that is, in 1542. Its success seems to have been speedily assured, and was really wonderful. To a generation that had been accustomed to such books as the *Hortus Sanitatis*, filled with the most wretched caricatures of plants in place of true representations of them, this great book by Fuchsius must have appeared as nothing less than luxurious; and the epoch which, ten years earlier, Brunfels had introduced by his 135 good illustrations of as many plants, was strongly accentuated by the appearing of this new volume with upwards of 500 large plates more than equaling, on the average, those of Brunfels.

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[1] The portraits of these artists, with their names, Heinrich Füllmaurer, Albert Meyer, and Veit Rudolf Speckle, are appended to the first edition of the *Historia Stirpium*.

Within a year, or a little more, there was issued an edition in German, this augmented by six more plates. Then in 1545 there came out, as if in condescension to the class of the unlettered, an inexpensive edition of the plates only, and this was so successful that a second issue was made in that same year; both these were in octavo. After that there were not a few small-sized and cheap editions brought out, with Latin text, but with figures so greatly reduced in size as to be of little use. With these, however, Fuchsius had nothing to do.

During the long tenure of the professorship at Tübingen, which covered nearly half his lifetime, there was no return to that critical work upon the history of medicinal plants with which he had inaugurated his career as botanical author; nor were there any more than casual questionings of nature even as to the affinities of plants. But the botanical artists were kept at work. To have more plants figured, and to formulate a page of text to accompany each plate, gave him pleasant respite from professional work, and promised greater fame and fortune. Before his death he had ready for the press the plates and descriptive texts of fifteen hundred plants; a work which, if it had been printed, would have made three folio volumes as large as the *Historia Stirpium*. But when all was done, no publisher could be found who would undertake the issuing of so vast a work without the advance of a considerable sum of money. This the author would not—perhaps could not—accede to, and the manuscript remained unpublished.[1]

In the original Latin edition of 1542, the Introductory Epistle, addressed to the Margrave of Brandenburg, is a document deserving fuller notice than can here be given it. It is a rather lengthy discourse, but withal instructive as to the condition of botany at the time, and exceedingly well written; amounting to something like an abstract of the history of medical botany from the earliest times down to his own date. As a piece of writing it reveals in its author general abilities altogether superior to what I can not but consider the mediocrity of his gifts as a botanist. It is in these introductory pages that he earns for himself the praise of being a fair and equitable judge and critic of the work of others, of whatever race, religion, or nationality.[2] At a time when it was usual in Germany to depreciate, if not to denounce, all French and Italian efforts to restore botany, Fuchsius proclaims it that they are all inexpressibly indebted to such great scholars as Hermolaus

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[1] Meyer, *Geschichte der Botanik*, vol. iv, p. 313.

[2] "Æquissimus majorum suorum judex." Sprengel, *Hist.*, vol. i, p. 324.

Barbarus, Marcellus Virgilius, Ruellius, and others who, through corrected texts and Latin versions of the Greek fathers, first placed them at the service of the botanists of every country. He takes pains to defend them one and all against aspersions that have been cast upon their works by men incompetent to criticise them, and recommends the study of them to all.

When, in their turn, his own countrymen and contemporaries, Brunfelsius, Cordus, Tragus, come up for mention, it is always most respectful and even honorable mention; and this despite the fact that among them there is a rival or two whom he fears, and has good reason to fear, still is he solicitous to be just to each, and to speak out the favorable things concerning their work which may be said.

A representation which he makes in this preface, of the low estate into which the pharmacy of his time had fallen, I must in substance reproduce. "The times once were when not only great philosophers and poets but kings and princes both investigated plants, and favored others so occupied. But in our day even the physicians are so much averse to that kind of study that you will hardly findone among a hundred of them who has correct knowledge of even a very few kinds. They appear to think that this kind of information does not belong to their profession, and to judge that it would be condescending from their proper dignity to entertain doubts about the accuracy and trustworthiness of those who buy and sell such things. And so it comes to pass that the druggists —God knows that they themselves are for the most part an illiterate set—leave all this to the foolish and superstitious old women who gather herbs and roots. Error is therefore heaped on error, and will be so long as the identification of vegetable medicines is left to rustic and vulgar ignorance."

The superb South American genus FUCHSIA was dedicated to this, the second father of plant iconography, by Plumier in the year 1703.

Vegetative Organography. Fuchsius has a very instructive and useful introductory chapter which he styles "An Explanation of Difficult Terms." From the historian's point of view this will be regarded as most valuable. It is the earliest vocabulary of botanic terms that I have met with thus far; and no historian that I know of has made mention of it. One gathers from this vocabulary good information of progress gained—and also of retrogressions made—in descriptive and organologic botany in the seventeen centuries and more between Theophrastus and Fuchsius. True to his title, our author omits all easy and familiar terms; does not define anew the root, the stem, the leaf, the flower. By this we know that he has nothing to add to the ancient and classic diagnoses of these organs. He does, however, define a bulb. "Bulbs are roots that are round and tunicated; such are those of the hyacinth, asphodel and colchicum." There is here a retrogression from Theophrastus, who doubted that the tunicated mass ought to be considered a root, and who also mentioned that the tuft of fibres descending into the ground from underneath are undoubted roots. Neither of these considerations affects the mind of stolid Fuchsius. Bulbs are roots that are rounded and tunicated. I do not recall having met with an earlier use of the word tunicated as describing certain bulbs. It is very apt, and has now long been everywhere in use as definitive of one kind of bulbs. With him, however, the employment of it is unfortunate; for it makes the tunicated structure to be characteristic of all bulbs, which is a bad mistake, as excluding the scaly kind, like that of lilies, from the category of bulbs; for not the crudest morphologist could call a scale a tunic. And Fuchsius proves his definition fallacious by stating, when he comes to the figuring and describing of the true lilies, that they have bulbs.[1] His referring to the asphodel as an example of a bulbous plant will be misunderstood. He has not at all in mind that plant which in later times has been identified as the famed asphodel of antiquity, the underground parts of which have nothing that is in the nature of a bulb of any kind. That which Fuchsius believes to be the asphodel, and figures for it, is a lily, and its scaly bulb is well shown.[2] If his third familiar example, colchicum, illustrates to us what we distinguish now as a corm, it is at least fibrous-coated on the outside, and would therefore answer at least to the letter of his diagnosis of a bulb.

There is one term in use in the sixteenth century in connection with certain bulbous plants which has not survived; that is, the neck (cervix). Fuchsius defines the *cervix* as "an elongated and cylindric body intervening between the summit of a bulb and the tuft of leaves, and has the appearance of a neck." From its position, and its external appearance as cylindric and supporting leaves in onions, leeks, daffodils, and their kindred, one might have expected to find it designated as a stem. That it was not, is a circumstance that must convince us of two things: first, that Theophrastus' immortal definition of a stem as made up of bark, wood, and pith, was a part of the very alphabet of botany in Fuch-

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[1] *Hist. Stirp.*, p. 366.

[2] *Ibid.*, p. 115.

sius' time as it is in ours; and second, that the stem-like cylinder surmounting onion, leek, and daffodil bulbs must have been examined in cross section and found completely destitute of every characteristic of stems in general; discovered to be made up of nothing else but the compacted bases of the leaves themselves. It looked much like a stem. Investigation proved it wanting every claim to that title; and they named it cervix, the "neck" of a growing bulb. The word was not destined to a permanent place in the vocabulary of the science. When at length it came to be seen that the bulb itself, as well as the cervix, was also but a mass of leaf-bases, and therefore no root at all, the term lost its particular significancy and disappeared. And this very fact of the invention of a new word that seemed to be called for, and its later passing into desuetude, is an interesting kind of episode in the history of morphology, and well merits notice in a place like this. We shall meet with other instances.

Respecting that more marked phase of underground stem, the rhizome, or rootstock, Fuchsius appears quite securely to rest in a position which Theophrastus had held with wavering. Fuchsius denominates them all *radices geniculatæ*. The Greek had realized that they have rather too much in common with stems.

In his treatment of stems in general, one observes in Fuchsius some divergencies from, even here and there some little advance ment beyond, the status of these things in the minds of the ancient authors. The word *culmus*, modified from the Greek calamos, is his term for the stems of grass-like plants. The first and largest divisions of tree trunks are denominated *brachia*, arms, though not unless such diverge from one another rather strongly, suggesting, as he says, arms of the human body when extended. Others had always noted what they called the knots, or nodes, of stems. Fuchsius uses, and even defines, the good term *internodium*, internode; though I much doubt his having invented it. He also observes in trees and shrubs the occasional development of long and vigorous shoots from trunks and main stems; points at which branching is unusual if not abnormal. He names such shoots *adnates*. Botany still recognizes this class, and knows them as adventitious shoots, from adventitiously formed buds

One reads in this author and in others of his time of such things as the *alæ* of stems. The usual meaning of alæ is wings, as of a bird; but in ancient Latin the term also meant the armpits; and quite like this is the sixteenth-century use of it in botany. Fuchsius defines the alæ of stems as being a kind of sinuses from which new growth proceeds; they are the axils of modern terminology. It is also here that I meet with *scape* as a botanic term; and the application is just that now in use, designating an elongated peduncle arising from under the ground; though neither Fuchsius nor his contemporaries so understood it. They regarded it as a true stem without nodes. But Fuchsius' "scapus" was not at once adopted; until long after his time it was usually denominated a *stylus*.

Two of the several modes of leaf arrangement are named and defined in this vocabulary, the decussate and the verticillate; but there is yet no one word in use by which to distinguish leaves as opposite. A phrase is required to express that. Any leaf margin that is evenly indented is described as crenate, or as serrate, quite indiscriminately, the terms being treated as synonymous; but if serratures be quite deep and close, as in the nettles, the leaf is fimbriate. Pediculus and petiolus, *i.e.*, peduncle and petiole, are employed as indiscriminately, either one applying to leaf-stalk or to flower-stalk. The word *stipula* also makes its appearance in Fuchsius' vocabulary, but with nothing like its meaning in more recent botany. His definition proves it to have been in his mind merely a special name for the peculiar leaf of grass-like plants, not a part of such leaf, but the whole of it.[1] It is a definite proposal that, since the stems of grains and grasses have the special name of culm, the leaves of the same class of plants ought not to be called leaves, but should have their own special designation—should be called stipules; and this is perfectly logical and consistent; for the leaves that grow on culms are quite as unlike all other leaves as culms are unlike other sorts of stems. It will be recalled that Theophrastus had named this entire group the *Calamophylli* in allusion to the remarkable characteristics of the foliage. But Fuchsius does not seem to have met with success in this endeavor to have grain leaves and grass leaves become known by the name of stipules; and, more than two centuries later, Linnæus picked up the old term *stipula* and applied it anew, and with perfect success.

Fuchsius tried also to invest the compound leaf with a name of its own, as a thing too different from the simple leaf. The distinction itself, as we know, was perfectly and for all time made by Theophrastus, who discovered things and left them nameless. The German father would have the compound leaf called a *frons*, *i.e.*, frond, thus restricting the other Latin word *folium* to the simple leaf and the individual leaflet of the compound. But this also fell short

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[1] "Stipulæ sunt folia culmum ambientia." Fuchs.

of general adoption; and long after Fuchsius, frond came into use as designating the peculiar foliage of ferns. It must be said of Fuchsius' application of frond that it was the more correct; for frons with the old Latins meant the leaves of trees, or even leafy twigs of trees, such as, anterior to Theophrastus, and by thousands in later times, the compound leaf was believed to be.

Inflorescence. The term inflorescence of course did not appear in botany until long after Fuchsius; but the thing had been of necessity both observed and discussed. The using of words definitive of the various clusterings of fruits and flowers must be older than history. Perhaps few if any of those defined by the German father were newly coined, or even otherwise applied than they had been in far earlier times. But here in the *Historia Stirpium* we have a goodly number of them brought together, along with not indefinite statements of what that author understood to be their meanings. And what must vouch for the importance of this paragraph of history is the fact that not one of Fuchsius' terms relating to inflorescence bears with him the meaning which the same term has in the botany of our own time.

Take the word *thyrsus*, which at its first origin in Greek and Latin was but a synonym of *caulis*, any stalk or stem; though later, and still in ancient times, it acquired a special significance; while with botanists of our time it means a particular kind of inflorescence. There is with Fuchsius no kind of a flower clustering that is called a thyrse; yet he essays to define the term as if in the botanical terminology of his time it had gained some new shade of meaning. From his definition itself nothing of the kind is apparent; but at definition Fuchsius is no adept; and when he says a thyrsus is a straight wand-like or arrowy stalk he has hardly departed from the earliest of ancient definitions. But when we make search for his practical use of the term we find that it has with him a meaning which he had not indicated or intimated in his definition. In the description of the hyssop he uses the expression· "Flowers purplish-blue, investing the thyrses like a spike."[1] Here it is plain that the thyrsus is the axis of a spicate inflorescence; that which in much later botany is become the rachis. But it is only now and then that he notes the arrangement of flowers; though the clustering of fruits is much more frequently taken into account.

The term *racemus* occurs not infrequently; but I think only as specifying the arrangement of some berry-like kinds of fruits. The type of the raceme is the grape-cluster; but in his definition

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[1] *Hist. Stirp.*, p. 840.

of *racemus* he says it is the fruit cluster not of the grape only, but also of the ivy and of any kind of herbs or shrubs that have bunches of berries. In practice we shall find him writing of the compactly spicate berries of the arum as forming a *racemus*.

As with the ancients, so with Fuchsius the spike is rather more a taxonomic than morphologic term. His definition of it is the most concise of all. "A spike is that which a culm bears at its summit. . . . It consists of three parts, grain, glume, and beard. A muticous spike is one that is beardless." Plainly, then, the spike with him is the peculiar inflorescence of grains and their natural allies. The typical form of a spike, that in which as in wheat and barley there is a simple rachis—thyrsus, he would have called it—up and down which the several parts are sessile, is not alone a spike. The one prerequisite of a spike is, that it shall crown the summit of, or at least be connected with, a culm. And so we find him naming as spikes the inflorescences of broom corn,[1] and also of maize.[2] In modern botany they are panicles. Only thus far, however, does he abide by his own diagnosis of the spike as the fruiting cluster of grass allies; beyond this point, and here and there, we find him overstepping the bounds which he himself has set to the application of that term. *Lavandula stoechas*, an aromatic shrub common in European gardens of that time, is of the family of the labiates, the flowers of which are congested within a somewhat elongated and cylindric involucre of chaffy and overlapping scales. This involucre vividly recalls the head or ear of some short-spiked kind of wheat; and Fuchsius transgresses his definition boldly enough by calling the involucre of this inflorescence a spike.[3] In justice to the botanist it must, however, be admitted that he was following popular precedent. The inflorescences of not only this but a number of other labiates had long been called spikes. There is then traceable in Fuchsius a tendency toward a point that was not actually gained until more than two centuries later, of defining terms by morphologic rather than any other characteristics; of naming a raceme from its structure rather than from the fact of its bearing berries and not capsules; and a spike not as the fructiferous terminal of a culm, but as an axis, bearing up and down its length sessile flowers or fruits, this irrespective of the family of plants in which it may occur.

The *panicula* is almost unique among Fuchsian inflorescences in

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[1] *Hist. Stirp.*, p. 772

[2] *Ibid.*, p. 824.

[3] *Ibid.*, p. 777.

that it is not made the mark of any particular taxonomic group, and is defined in quite strictly morphologic terms. As far as possible from the panicle of modern vocabularies, it consists of almost any very compact cluster that is somewhat elongated and, at the ends, well rounded. The first example given is that of the cones of spruces, which are not conical, and therefore would not have been called cones by botanists of antiquity, nor by their disciples of the sixteenth century. But it is said by our author that the Latins applied panicle mostly to such as, being of the requisite form and density, were also appendaged by some sort of coma; and so, among the Fuchsian panicles, one finds the bristly hairy spikes of millet, and the elongated furry heads of the mouse-ear clover, otherwise called lagopus, *i.e.*, *Trifolium arvense*. Also individual spikes of scirpus, eleocharis, and others of their tribe—the *individual spikes*, I say, and not the whole inflorescences—are panicles.

The umbel, though defined morphologically as a flower and fruit-cluster constructed upon the plan of an umbrella, would never be applied to an umbel of berries. The umbels with Fuchsius are the inflorescences of the family of umbellifers, or at least of dry-fruited plants, exclusively. The flat-topped clusters of certain anthemideous composites like millefolium he speaks of as umbels; though they are not really umbels, but corymbs; a distinction that had not then been made.

Anthology. In the vocabulary of Fuchsius there is vouchsafed a perfectly intelligible definition of what he calls the calyx. It is a kind of "bag within which at first the flower, and after that the seeds are enclosed." Note first of all, that such a calyx as this can be no part of a flower. It can not be determined to be a calyx until it has shown itself permanent; until the seeds have ripened. A deciduous calyx would be a contradiction in terms. A circle of green sepals behind a flower, even though at first enclosing the "flower" does not constitute a calyx; at least if it fail to persist and to enfold the seeds after the other parts have fallen. Under this definition all mintworts and sages, the borrageworts and other synsepalous things have a calyx, while the poppies and the buttercups and their allies have none. The pomegranate and all pomaceous fruits are furnished with that organ; the olive and all drupe bearing trees are destitute of it. This appears to be the earlier idea of a calyx; the first movement toward the bringing in of that green-leafy circle close behind what was called the "flower," to where it should be recognized as a part of the flower. But his having technically defined the calyx does not preclude his occasional employment of the term in an untechnical sense, that is, as designating some other organ that happens to be shaped like a cup; thus we find him calling the lily flower a calyx,[1] *i.e.*, a chalice-shaped flower.

His doctrine of the flower in general is that of Theophrastus hardly improved upon. There are two kinds, the leafy and the capillary; but both are united in, for example, the rose. The term petal is still wanting in botany. Its introduction into the vocabulary will not be proposed until two generations after Fuchsius. The foliar parts of a flower are still leaves only. Yet what is curiously interesting is, that already as the green leaf is seen to have usually that which they have called its petiolus, or pediculus, so the flower leaf is credited with having its unguis, or claw; the more or less narrowed basal part by which it is attached to its receptacle. Fuchsius defines well enough this unguis, even remarking that in the flower of a red rose this claw is white. And so the distinguishing of the two parts, blade and claw, in this organ historically antedates the naming of the whole organ as petal; and Fuchsius, so far from affirming this to be a new distinction of his own making freely attributes it to "the ancients."

In this Fuchsian vocabulary occurs what is perhaps the earliest botanical definition of stamens. There is so much of history in it that one must reproduce it as literally as may be. "Stamens are those apexes that come forth from the middle of a flower-cup; and are so called because they rise up like filaments out of the inmost bosom of the flower."[2] As a definition this is illogical and bungling; for both the anthers and the filaments are separately called the stamens; not by any means the two parts that go to the making up of stamens; either one alone is stamen according to his absurd statement. In the first clause the anthers, *apices*, are the stamens; in the second, the filaments are the stamens. That what he denominates apices are the anthers is clear as day from his description of the flower of the common white lily, where he remarks that in this the apices are yellow; for no other parts of the lily blossom are yellow but the anthers.[3] The term *filamenta* by its very meaning applies to no other organs but the filaments and styles. Let us note here that there is yet no description of *a stamen*. The author neither thinks nor speaks of the thing but in the plural. What he

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[1] *Hist. Stirp.*, p. 363.

[2] "Stamina sunt qui in medio calycis erumpunt apices; sic dicta, quod veluti filamenta ab intimo floris sinu prosiliant." Fuchs, in the vocabulary.

[3] *Hist. Stirp.*, p. 363.

has seen, and what all the botanical authors before him had seen, is a tuft of delicate things standing up from the midst of the circle of colored flower leaves. The ancients had written of them as *flocci*, or as *capillamenta*, still only in the plural. No one had ever looked into the individuality of one of those flocci or capillamenta. To have done that would have been to lead the way to the discovery that the members of the filamentose tuft are not all alike; that at least the one central member—if not the whole central membership of the tuft—is different from those that stand between them and the circumference of the flower. There would soon have been two kinds of stamens to describe if one stamen had been defined, because of those—the styles and their stigmas—which would not have answered to the definition.

Now Fuchsius in practice writes of these tufted things in the middle of a flower as the apices. This is a distinct departure from the terminology of antiquity, and is withal a departure in the right direction; for the ancients had seen and written of flocci, capillamenta, and the German had seen—perhaps by some unknown mediæval botanist had been taught to see—the little knots that surmount the outer set of the flocci, and from these little apical knots the whole stamen-tuft had been named anew, "apices." This term, whensoever it made its appearance, came in like a kind of prophecy that the terminal knots were one day to be received as the only essential parts of the tuft. One would willingly concede to Fuchsius the invention of this term which shows that anthers are being noticed; but he was in no sense a botanical discoverer, and he availed himself of many an old book and manuscript of which we have no knowledge.

For the staminate tassels of hazel, walnut, and oak trees he has also now a name; whereas the ancients seem to have had none. But he has no more idea what these tassels are for than the ancients had; though he ventures the guess that they are instead of flowers; thereby proving that he had never seen the pistillate or real flowers of any of them. He calls the pendents *nucamenta*, nut-tassels, and describes them in language borrowed from Theophrastus who, as we have been learning, had a much better knowledge of nut-tree flowers then Fuchsius ever attained to.

Fruit and Seed. The Greek philosopher's comprehensive and classic definition of a fruit is either unknown to Fuchsius, or else he purposely condescends to the popular notion; for he says that a fruit is something made up of flesh and seeds. This is quite in keeping with his definition of a raceme as being a cluster of fruits.

i.e., fleshy fruits, rather than a particular mode of clustering in both flowers and fruits. Seed coverings other than fleshy are disposed of by him under two or three distinctive terms. There is the *silique*, the *vasculum* and the *caput* as well as its diminutive, the *capitulum*. The legumes he says are siliques, and yet many other herbaceous plants and even shrubs bear siliques; and we infer his silique to be any kind of an elongated and two-valved pod. He ventures upon no definition of what he calls vascula, beyond this, that they are the coverings of seeds. His account of head and capitulum is, that they apply to any well rounded and solidified part of a plant, whether basal or terminal. An onion-bulb, resting wholly above ground and therefore not a root, is a head, a *caput;* and so is the head of a cabbage, and also the round indurated capsule of a poppy. If he knew anything of Theophrastus' technical definition of a fruit as composed of pericarp and seed, he does not appear to me to have made use of it, or of the term pericarp.

The seed is not defined in our author's vocabulary, or even mentioned there otherwise than incidentally.

Phytography. There has already been cited this opinion of phytography from Brunfelsius,[1] that the best way to reform and improve upon mediæval plant description would be to restore word for word the descriptions of the ancients. At a later time it was thought—and the thought was carried into action—that the only possibility of improvement in phytography lay in wholly disregarding the classic texts, and writing all plant diagnoses anew. Such a proposal as this last would have filled either Brunfelsius or Fuchsius with amazement; and also not unreasonably, for neither of them had in view the reformation of botany in general. Both were aiming at the correction and improvement of that which we of today would speak of as the botanical part of the pharmacopeia. They were interested in phytography, because it is one of the necessities of medical botany. The most trying part of their work was that of the identification of ancient remedies; and their only clues to the identity of any of them were in the ancient descriptions. We have followed Brunfelsius in his giving, for some plant newly figured, page after page of different descriptions literally quoted from those whom he regards as standard authors, not willing to divert the attention of the student by a single line of his own; willing that they who study his book shall judge for themselves whether what he has figured under a given Theophrastan, or Dioscoridean, or Plinian name has been rightly identified.

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[1] See page 172 preceding.

The ideal of Brunfelsius is high. His book is for scholars; but Fuchsius plans to be more popular. He will publish twice as many plates as the former had done, and for the sake of economy must reduce the number of pages of printed matter. The average is not much more than a half-page to a plate; yet the descriptive part of Fuchsius' volume is not in all respects insignificant when compared with that of Brunfelsius. There is a falling off in the bulk of interesting and useful matter; but some good phytographic distinctions are made which do not appear in Brunfelsius.

Every chapter of Fuchsius is divided into separate and separately named paragraphs, of which always the first, headed *Nomina*, is devoted to the name and synonymy of the genus, and contains nothing else. In the case of every monotypic genus the second paragraph is headed *Forma*. In this we read always the morphologic marks of this type; at least such of them as Fuchsius can copy out of standard authors. When, however, a generic type is known to be made up of two or more species or varieties, then this morphologic paragraph is not second in order but third; the second being now given to the naming and defining of the species or varieties; and this occasional second paragraph is always under the caption *Genera*. In an earlier chapter an explanation has been given of the primitive use of the term genus, and its plural genera, as meaning nothing more nor less than the kind or kinds of a thing.[1] It is plain that Fuchsius' "genera" are the species and varieties, while under *Forma* he gives the morphology of the genus as a whole. The placing of the descriptions of species and varieties first, and that of the genus next below is illogical in the extreme; but there are still other intimations that a logical mind was not among the learned Fuchsius' natural endowments. But this segregating of the morphology, the ecology, and the properties of a plant, and the relegating of each to its own paragraph is definitely an improvement in phytography, and is perhaps an invention of his own.

Taxonomy. We are learning that there was in Fuchsius nothing of the plant anatomist or physiologist, something of the organologist, but that he was in no wise given to philosophizings about plant life and form in general; that he was a medical botanist, dealing with plants from the utilitarian point of view. He would not have appeared as a taxonomist had not taxonomy been inevitable wherever more than one individual plant is treated of.

Concerning the larger and more comprehensive groups, the

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[1] See pages 115, 116 preceding.

genera majora of the botanists of a somewhat later period, the orders or families of to-day, there is little to be read in Fuchsius; for his sequence of genera is alphabetic, a kind of arrangement which precludes the grouping of genera into families. It does not, however, in any way influence the definiteness of the genera themselves, or of the species composing them. The work is divided into 343 chapters, and each chapter is devoted to one genus and no more; so that the number of genera treated of is the same as the number of the chapters. Many of the genera are represented by only a single species, many by two or three, several by four or five, and one has seven species, all described and figured. But one must not pass to the study of his genera without noting certain lapses from the alphabetic arrangement which are made in deference to what are held to be the affinities of a genus. These are not numerous, but they are enough to show Fuchsius as susceptible of being influenced by the idea, even in his time an old one, that some genera are interrelated. Here it must also be observed that the alphabetic order he follows is that of the Greek rather than Latin names of genera. For medical botany, all through the ages and down to Fuchsius' time, Greek rather than Latin was the preferred language of nomenclature, at least with those best educated; because all three of the most venerated authorities upon that part of botany, Hippocrates, Dioscorides, and Galenos had been Greeks and had written their treatises in Greek. Now while in his Latin text Fuchsius uses the Latin names of things, quite as he ought, the generic names heading the chapters are the Greek generic names; and the sequence is that of Greek letters of Greek names; so that the chapter on the genus of the elder trees or bushes is headed not *Sambucus* but *Acte*, and that of the plantains not *Plantago* but *Arnoglosson*.

Now for a sequence of genera in a book of sixteenth-century botany, the choice of the Greek alphabetic order left its author certain liberties. All Latin names are naturally exempt from such a rule; and there were now in Fuchsius' time not a few plants holding places in the pharmacopeia which had not been known to the Greeks of old, and which therefore had only their Latin and their vernacular appellations. Since the Latin names of these may not consistently head chapters where the headings are professedly Greek, Fuchsius is apt to use these as occasions for giving expressions of opinion about natural affinity by placing them in the line of what he conceives to be their real kindred. For one instance, take his placing of *Datura Metel*, a plant then newly introduced into Europe, where it was known only by the Italian name of Stramonia. Since it has no Greek name he locates it where he will; and we who have been taught that there was no natural classifying of plants until well toward the end of the eighteenth century may well be surprised that Fuchsius places this at the end of a line which begins with *Solanum nigrum*, such perfectly solanaceous types *Solanum melongena*, *Physalis somnifera*, and *Atropa Belladonna* intervening.

That very early in the sixteenth century there was already in exercise the taxonomic skill to put together as under one comprehensive natural genus such diverse-looking consanguinities as the five plants here named is something that merits more than passing notice. It is another of those forceful intimations that much of the botanical history we once learned must be unlearned. Let us remember that we are now at a point more than one hundred and fifty years anterior to those great lights, Morison, Ray, and Tournefort, and some two hundred anterior to Haller, Jussieu, and Adanson, to which latter trio is usually accorded all the glory of having first outlined such natural groups as this. But Fuchsius himself intimates that from very ancient times there had been a somewhat familiar knowledge of four out of these five plants, and says that both Dioscorides and Galenos had held them to be kinds of *στρύχνον*, *i.e.*, *Solanum*.[1] But the referring of so marked a new type of plant as *Datura* to the anciently recognized group of the *Solanum* allies by men of the sixteenth century will hardly fail to suggest to some that there must have been, after all, some appeal to anthology. Certainly the conventional nineteenth-century botanist finds no stronger links uniting these two plants to one family than the symmetrical pentamery of the flower, coinciding with a plicate præfloration and superior ovary. It is none the less well beyond question that not a single point anthological had influence in determining the affinities of *Datura*. The corolla has not yet, at this period obtained its name. It is still the "flower" simply. Neither stamen nor style had been taken note of as an individual organ; much less had the two been distinguished, or the members of either set been counted. Floral symmetry was yet unnoticed; and, marked as is the æstivation, or præfloration of solanum and all its allies, the very topic of præfloration was not yet heard of in Fuchsius' time, nor did it begin to figure in taxonomy until some two centuries after his demise. According to Fuchsius,

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[1] *Hist. Stirp.*, p. 691.

his best reason for locating this type in the line of the nightshades is the fact that its herbage exhales the narcotic odor characteristic of all other nightshade allies. Plant classification, then, in Fuchsius' time has not yet emerged from the period of antiquity; and botanists are still considering agreement among plants as to their vegetative and qualitative characteristics, giving little heed to those of flowers or fruit. And a fair measure of success not rarely attended taxonomic effort guided by these criteria. But the success, however marked in the case now in mind, was not quite complete. Capsicum is undoubtedly a solanaceous type, but this fact Fuchsius failed to apprehend; yet the failure is not unaccountable, notwithstanding that, viewed morphologically, capsicum is much more plainly akin to nightshade than is *Datura*. The plant was a new one in German gardens of the sixteenth century as was also *Datura*. It had been as unknown to the ancient Greeks, and had no Greek name; therefore Fuchsius could have placed it next *Solanum*. The fact is, no one had yet seen its relationship. The plant lacks the narcotic odor. Every part from leaf to seed is of a peppery odor; and this quality, amounting to a burning pungency of taste, joined to the peculiar medicinal qualities, blinded every one to that affinity for *Solanum* which every one now sees in *Capsicum*.

It must not, however, be inferred that this principle of qualitative agreement is held an inviolable rule in this early taxonomy. With botanists of that time, quite as with those of every later generation, the endeavors to form groups are dominated sometimes by one principle, sometimes by another. It is easy to bring forth out of Fuchsius instances of putting things together as of the same genus regardless of odors and flavors and of almost all other marks save those of roots and leaves and general mode of growth. Under the genus *Verbena* he figures two species. There is the most deceptive likeness between the two as to roots, stems, leaves, and a slenderly spicate inflorescence of small flowers; but one of these verbenas is *Verbena supina*, the other is *Sisymbrium Lœselii;* a true verbena and a crucifer made congeneric; in the Fuchsian binary nomenclature *Verbena recta* and *Verbena supina*.[1] It will not be easy for the systematist of to-day to imagine so natural an alliance as that of the crucifers remaining unrecognized by those who had already recognized the solanaceæ; yet as to roots, foliage, and habital characteristics the crucifers differ among themselves a hundred-fold more widely than do the solanaceæ.

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[1] *Hist. Stirp.*, pp. 591–593.

Despite what we of later times perceive to be the naturalness of the cruciferous group, its recognition had to await the development of anthology; and I have met with no evidence that up to the time when the career of Fuchsius was ended there had yet lived a botanist who had known the floral structure of a crucifer. Even the Theophrastan elements of anthology seem to have been sunken in oblivion ages before the birth of Sprengel's and Meyer's and Sachs' "German Fathers of Botany," one or two of whom were to renew the investigation of floral structures; though Fuchsius was hardly one of these.

The contemplation of these quaint herbalistic genera based on vegetative characters and ignoring flowers and fruits is both entertaining and instructive. It is as if one had discovered in these antiquated tomes a fossilized and now extinct system of plant classifying; and the reader will not fail to be interested in glancing at the outlines of other such genera. Fuchsius has one little genus for which he brings forward into print, from out an unpublished manuscript, the name *Pilosella*. Here is the composition of the genus:[1]

Fuchsian	Recent
Pilosella major	Hieracium Pilosella.
Pilosella minor	Antennaria dioica.

He has tried to make one of the plants answer to the *Myosotis* of Dioscorides. It does not well agree, and he is confident that neither of the two was known to the ancients. Both are well known in Germany, and of repute as vulneraries, on which account he must not omit them. The German herbalists of his time know one of them by the name Pilosella. That will suffice for a formally generic name, and with it he has already headed his Chap. ccxxx. "Two kinds of it are found, differing in nothing but the flowers. One of them has leaves that are larger, and do not lie flat upon the ground. Its flowers are yellow, and it is named Pilosella major. The other has smaller leaves that lie flat upon the ground, and purple flowers which disappear with a pappus. The Germans have for this the names Little Mousear and Rabbitsfoot. In a manuscript herbal I find these plants disposed of as Pilosella major and minor." I thus present a literal version of what our author had to relate respecting the components of this his genus *Pilosella*, even to the interesting admission that the whole chapter had been borrowed

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[1] *Hist. Stirp.*, pp. 604–607.

from an old herbal in manuscript—illustrated by drawings, no doubt—which had been available to him. Even the specific names *P. major* and *P. minor* were from said manuscript. Contemplating these two plates, whether the proposed genus consisting of a hieracium and an antennaria seem rational or absurd will depend entirely upon whether one view the types with the eye of the modern botanist, or with that of him of Fuchsius' time. The modern, taught by the traditions of not more than five or six generations of his more recent botanical ancestry, looks at the flowers only, and in consequence realizes only that the two must be regarded as of widely separate genera; for one has the flower head of a hawkweed, the other a congested bunch of cudweed heads; and as to the structure of their individual flowers they represent something like two extremes. But it will be both illogical and unfair to test the consistency of the earlier classification by presuming to hold it amenable to modern taxonomic principles; though this seems to be about what the readers of old botany, and even the historians, have always hitherto been doing. The consistency of the Fuchsian *Pilosella* is readily seen if, blinding ourselves as he and his forbears were blind to small floral structures, we look at and compare those parts of the two plants which they looked upon as bearing marks of consanguinity. Both the antennaria and hieracium are small perennial herbs of one and the same mode of growth, and that mode rather exceptional. Each has its leaves mostly in a basal tuft, and bears its flowers at the summit of scapiform stems. A number of depressed stolons leafy with a smaller foliage radiate from the base of the stem of each, so that both in the same fashion propagate vegetatively and form colonies. Add to these and other points of morphologic agreement the consideration that both were received as possessing the same remedial virtues, and we have a rational genus according to all the leading principles of sixteenth-century taxonomy.

Thus comprehending the situation—realizing that these groups that look so strange and motley have not been formed at random, but rather under guidance of definite principles—every such group acquires a new and even a lively interest. Let us open the book at its initial chapter. The name of the first genus is *Absinthium*. Three species are described, two of them figured well. They are:

Fuchsian	Recent
Absinthium vulgare	Artemisia Absinthium.
Absinthium Seriphium	Sisymbrium Lœselii.

Thus a cruciferous plant, common in German vineyards and hedgerows, is made congeneric with wormwood because it has similar foliage, and also is thought to answer to *Seriphium* in several other particulars. It is a good example of futile effort to identify with the *Seriphium* of Dioscorides a very different plant of central Europe which Dioscorides never saw, and which, by the way, if he had seen it, he would never have thought of as a kind of wormwood; for, as we have seen, the Greek did not quite disregard floral structures, but could distinguish genera by anthologic marks.[1]

But Fuchsius' disregard of flowers in these generic groupings is manifest again close by *Absinthium*, in his *Anthemis*. The name of the genus and those of all three species are taken up from Dioscorides, as he says, and the following is his identification of them, the names at the right being those now in use.

Fuchsian	Recent
Anthemis leucanthemon	Matricaria chamomilla.
Anthemis chrysanthemon	Anthemis tinctoria.
Anthemis eranthemon	Delphinium Consolida.

Judged by the modern and improved standards, the locating of a larkspur as congeneric with chamomile is the worst of conceivable taxonomy; and it is impossible that in this Fuchsius interpreted Dioscorides otherwise than erroneously and even almost stupidly. The Greek had habitually looked at the flowers of things, and had shown clearly a tendency to regard the forms of flowers as taxonomically significant. Concerning *Anthemis* in particular he declares that the flowers in the three species differ only as to the color of the little leaves that encircle the centers of the flowers, which central part he says is yellow in them all.[2] Now concerning the identity of the white-rayed and the yellow-rayed Anthemis species of Dioscorides there never had been any doubt; therefore as to these two Fuchsius was but reiterating the expression of a common opinion. The third member, however, that is, the purple-rayed anthemis, was problematic; for there is not known in those regions where the ancients botanized a purple-rayed composite having the foliage of an anthemis. But there need not be; for any ranunculus-flowered or anemone-flowered branching herb, if it had the foliage of anthemis might have been relegated to that genus. It is only to the trained eye of the modern botanist that the rayed head of a

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[1] See page 180 preceding.

[2] *Dioscorides*, Book iii, ch. 130.

composite and a ten-petalled flower of an anemone or a buttercup seem very unlike. By the ancients, and by everybody down to Fuchsius' time and much later they were not regarded as being different. The superficial likeness between the two is great. In either type there is a yellow center made up of little things compacted together, and this is encircled by rather many narrow leaves apt to be different in coloring from those of central tuft, or mat, or cone. The different constituency of those yellow centers in anthemis and anemone no one had yet perceived, even in Fuchsius' time. With him, as with all botanical antiquity, the yellow middle part was made up of the "stamina," the "capillamenta," the "flocci," in anthemis as in anemone: and that the circle of colored leaves is a circle of ray-flowers in anthemis, and of petals or sepals in anemone and buttercup—that is a refinement anthological of which neither Dioscorides in his day, nor Fuchsius fifteen centuries later, had ever dreamed. But in the grain fields of Greece and Italy there grew in abundance one anemoneous herb with perfectly anthemideous habit and foliage, and flower leaves dark-red—easily within the wide range of the purples of the ancients. Must not this have been the *Anthemis eranthemon* of Dioscorides? Its modern name is *Adonis æstivalis*.[1] Certainly this, rather than *Delphinium Consolida*, is the third anthemis of the Greek physician. And the fault of Fuchsius is his utter disregard of those floral marks in respect to which the Greek had said that all three species of anthemis were at agreement; though as to mere foliage, and the annual root as well as more of growth, the larkspur-anthemis of Fuchsius answers well enough to the other kinds; and this would have been the apology for Fuchsius' erroneous determination of the plant, had he not virtually disclaimed it for himself by attributing it to some unknown earlier botanist whose anonymous manuscript has been at his disposal. In this manuscript he says, "there is an exquisite drawing of this plant, which is commonly called consolida regalis," and then he proceeds to quote, from this unknown author, the following: "Some call the herb Monachella or else Capuciaria, doubtless in allusion to the hood of the monks, which the flower recalls. Dioscorides calls it Eranthemon, and it is one of the kinds of Anthemis, having the foliage of the chamomile, though of a darker green; but the flower is rather like that of a violet." [2]

Evidently the author of that unpublished commentary had been

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[1] See Matthiolus, *Comm.* (ed. of 1565), pp. 904–906, with fine plate of *Adonis*, in between two plates of anthemids.

[2] *Hist. Stirp.*, p. 28.

too superficial in his knowledge of the wild plants of Mediterranean fields to well interpret Dioscorides. But his identification of the third anthemis appears to have suited the fancy of Fuchsius and he approved it. His former rival, Brunfelsius, a dozen years earlier had maintained that the *Consolida regalis* was *sui generis*, and did not think it had been known to any of the botanists of antiquity.[1]

There seems to be evidence within the book of Fuchsius itself that the work was long in preparation, and the middle and later chapters printed so much later than the earlier as to bear intimations of his mind's having changed somewhat in the direction of an appreciation of floral structure as having some value in the determining of plant relationships. While his contemporaries Brunfelsius and Tragus hesitated to distinguish generically between the real nettles and the labiate-flowered dead nettles, Fuchsius separates them widely, and under the names of *Urtica* and *Lamium*[2]; this, however, not as an original proposition, but as adopted from Dioscorides and Pliny.

In his description of the genus *Pisum*, the garden pea, he says that its flower is shaped like a butterfly; but I do not find him using the expression in describing other plants of that family; and while this is the earliest mention of the papilionaceous corolla form that I have met with, I still think it improbable that it was original with Fuchsius.

Among several new genera proposed by this author there is one, namely *Digitalis*, which he establishes almost upon the form of the corolla alone.[3] This genus of two species, which he names respectively *D. purpurea* and *D. lutea*, practically concludes this volume of more than 900 pages; and so a course that began in almost total disregard of anthological considerations, ends in the admission that floral structure may upon occasion be of such high taxonomic import as to furnish the most essential character of a genus. Contrasted with the beginnings of the volume, this conclusion of it is taxonomically very significant; even prophetic. It forecasts the time a hundred and fifty years later when Tournefort, running to another extreme, would essay the systematization of all petaliferous plants, almost by the corolla alone.

Nomenclature. All the unconventionality, simplicity, and brevity of a primitive and even a classic nomenclature marks the

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[1] Brunfelsius, vol. i, p. 84.

[2] *Urtica*, *Hist. Stirp.* pp. 105–109, three species; *Lamium*, *Ibid.*, pp. 468, 469, also three species.

[3] *Hist. Stirp.*, pp. 892–894

plant-naming of Fuchsius. As already indicated he heads each chapter with the Greek name of the genus, unless perchance the plant was unknown to the Greeks and has no Greek name; then the Latin name is used. For every one the Latin and the German synonyms are given, and that in the first paragraph of the chapter, under the caption "NOMINA." Still more carefully considering the convenience of pharmacists and the untutored collectors of simples, he causes to be printed on each plate the Latin name of the species at the lower left-hand corner, and at the right-hand corner the German. Quite as we expect to find it, we read on plates representing monotypic genera no name at all but the generic. Commonly such names consist of but one word. Such are *Adiantum*, *Althæa*, *Anethum*, *Asarum*, *Asparagus*, and scores of others as familiar and as ancient. But there is also no dearth of purely generic names for monotypic genera that are of two terms. There are *Acorus officinarum*, *Tagetes Indica*, *Plantago aquatica*, *Aster Atticus*, *Barba Capri*, *Vitis vinifera*, *Vitis alba*, *Vitis nigra*, *Viola purpurea* (=*Viola*), *Viola alba* (=*Matthiola*), and very many more like these. There is no ground for questioning that every one of these names is purely generic. There is no warrant for denominating so much as one of them "a pre-Linnæan binomial," that is, a name of which the first word is, in the accepted sense, generic, the second specific. Names binary, and of just this last named quality do also abound in Fuchsius; but these now cited, and also a host of others like them, have nothing in them of the generico-specific meaning which all binaries in use to-day convey, and are understood to convey. Now that Fuchsius does not mean by *Plantago aquatica* any species of the genus *Plantago* is put beyond all doubt or cavil by the facts, first, that he takes it as the topic of a chapter apart from that in which *Plantago* proper is discussed[1]; and second, that the officinal, or more properly scientific (Greek) names heading the chapters are totally distinct, the one being *Arnoglosson*, the other *Alisma*. Concerning *Vitis vinifera*, now long in the status of a generico-specific binary, it is as readily demonstrable that with Fuchsius it is purely generic. Assuming that in primitive times the one word *Vitis*—in Greek ἄμπελος—was, as it now again is become, the generic name for grape-bearing shrubs, there came in later such two-worded generic names as *Vitis alba*, *Vitis nigra*, *Vitis Idæa*, etc., each standing for a completely and widely different genus. It is easy to see that such multiplication of generic names beginning

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[1] *Plantago* (of two species) is the topic of ch. xi, pp. 40, 41; *Plantago aquatica* (monotypic) is of ch. xii, pp. 42, 43.

with *Vitis* rendered it needful that the original Vitis should receive also a second and modifying term in order to avoid confusion; and thus there came into existence, and of necessity, the generic name *Vitis vinifera* instead of the simple and primitive *Vitis*. Let us see what Fuchsius' genera of which this term is a part of the name, really are:

Fuchsian	Recent
Vitis vinifera	Vitis
Vitis alba	Bryonia
Vitis nigra	Clematis

Specific names, with this German father, quite as with all his forbears that I know aught about from Theophrastus forward, are strictly binary. One simple word, almost always adjective, constitutes the specific term of such binary, and is the cognomen, as some called it. It happens, indeed, with many a such binary name that it is composed of three separate words; but that is always for the reason that the generic name is of two words. Not much less than a hundred of Linnæus' trivial names of plants are of three distinct words; but this is because he makes the specific part of such names to consist of two words, and never the generic part. This is precisely the difference between the Fuchsian and the Linnæan binary nomenclature; and there exists no other difference. We amend Linnæus by connecting by a hyphen his two-worded specific names. This is done in order to preclude if possible any questioning of the fact that the two words which we have hyphenated are to be thought of as one. It were equally in place to hyphenate the terms of Fuchsius' two-worded generic names. This done, he who ran might read the truth that all Fuchsian plant names not those of monotypic genera are as strictly binary as those of Linnæus; with even this one difference in Fuchsius' favor, that he has no two-worded specific names.

As to general principles of botanical nomenclature, those of Fuchsius seem few, and easily ascertained. Those principles appear to be convenience, etymological suitability, brevity.

If all his names are binary, and, as being the mere names, hold places entirely apart and distinct from the descriptive paragraphs, as they always do, then there is not even the suggestion in Fuchsius of those "phrase names," so called, which became a burden upon the phytography of two centuries later; and it may be said of his work that it is quite a model of brevity in nomenclature.

Inasmuch as the Greek names of genera are older than the Latin, and he gives to them a kind of precedence, making them the headings of his chapters, there might arise a question whether the Greek names were preferred on account of their right of priority. But there is other evidence that the principle of priority in nomenclature obtained no recognition with him; was perhaps never once thought of. The standard works on the materia medica in use everywhere in Fuchsius' day had been written in Greek; which fact alone would give the highest prestige to Greek plant names. Their Latin names, needful to be used for the convenience of the many who knew Latin but not Greek, were in every way as valid as the Greek names. Both were used, chiefly as a matter of the greater convenience to readers and students as a body. That he never thought about rights of priority as worth contending for comes out as clearly as possible in his presenting a new genus with the new generic name *Digitalis*. "We make use of this name, until we ourselves or some one else shall have invented a better one."[1]

The above remark attests its author's opinion that there could be appropriate names and inappropriate, and that names either bad, or even not very good, might well be suppressed in favor of new ones more suitable. Even the principle of convenience, which always favors the retention of an established name whether bad or good, may be overruled for the rejection of a name that is ill constructed, and the substitution of a new and better one. There is one generic name that had held good for some fifteen centuries, a Greek name too, which he declines to adopt as the heading of its chapter, evidently because etymologically distasteful to him. The name in Greek is *Ocimoides*, formed by the addition of *oides* to the generic name *Ocimum*. Instead of the ancient and established *Ocimoides* he writes for a heading to the chapter the new name *Ocimastrum*[2]; an initiative in the reform of generic nomenclature which Linnæus two centuries later was to carry forward with universal approval.

He who thinks that nomenclature, like the science itself, should be subject to advancement and improvement, must be believed to have his reasons; though Fuchsius does not appear to have declared his. One thing, however, we observe, and that is that all the names he uses have their meanings. A genus is named in allusion to some morphologic or qualitative characteristic; or else in honor of some personage who had to do with botany; or rarely,

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[1] *Hist. Stirp.*, p. 892.

[2] *Ibid.*, p. 895.

from some foreign country whence the plant or tree had been introduced into Europe. But there is nothing of the meaningless and cabalistic in any of the 343 generic names that head as many Fuchsian chapters. The meaning is not in every instance plainly to be read in the name itself. That which it once had when newly coined, has now and then become obscure, if not quite lost, through the lapse of ages. But there are not yet in botany anagrammatic names, nor any that had been framed by the putting together of two or three meaningless though perhaps euphonious syllables. The beginnings of such an epoch in nomenclature we shall not look for until after the time of Linnæus.

In specific nomenclature, however, we note in Fuchsius a free use of cabalistic names. In genera of two species one is very apt to be named *mas*, the other *fœmina*, and since at that time nothing was known about sexuality in plants, such names had little meaning. But quite as frequently the first species is called *prima*, the other *altera;* and in the case of his fine plates of six species of *Geranium* they are named *G. primum*, *G. alterum*, *G. tertium*, *G. quartum*, *G. quintum* and *G. sextum.*[1] Though without a trace of diagnostic significancy, and purely cabalistic, we shall find that this kind of specific adjective came into use very extensively in the works of botanists of a generation later than Fuchsius.

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[1] *Hist. Stirp*, pp. 204–210.

CHAPTER VII

HIERONYMUS TRAGUS 1498–1554

An original and even eccentric character, singularly gifted as a botanist, was Hieronymus Bock, whose names in literature are several; for his earliest publications were made in Latin, under the name Hieronymus Herbarius. Brunfels in his first volume publishes a number of paragraphs the manuscript of which had been furnished by his friend Bock, and these are all credited to him as Hieronymus Herbarius—Jerome Botanist, or Jerome Herbalist, as you like. So again, in the Appendix to Brunfels' second volume, there is a document of some length, entitled *Apodixis Germanica.*[1] This is in German, and is by Tragus, but still under the name Hieronymus Herbarius. In the numerous German issues of his principal work he figures as an author whose name is Hieronymus Bock. When the great botanical merit of this work had been intimated abroad, and a Latin edition of it was thereby called for, in this he appeared under the Græco-Latin name Hieronymus Tragus. By this name therefore is he known in the botanical world in general. The genus that was dedicated to him by Father Plumier therefore necessarily took the form of Tragia.

Life. No adequate biography of this interesting character seems to be known. We trust that we have to a certainty the year of his birth; we have the date of his marriage, the maiden name of the bride, the names of her parents, and even the number of guests that were in attendance; and all this out of Tragus' own diary.[2] Neither is there any disputing the date of his death, the place of his burial, or the name of the preacher who delivered the funeral panegyric. But all these are matters which, in the biography of a reputed scholar, a practicing physician, and the beneficiary of a lucrative parochial endowment, are of subordinate interest. What one most wishes to know are the names of the colleges and universities at which the man studied, the schools whence he had his

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[1] Brunfels, *Viv. Icon.* vol. ii, pp. 183–199 (in my copy).

[2] Melchior Adam, *Vitæ Germanorum Medicorum*, p. 68.

degrees in medicine and in theology. In other cases there is ordinarily no doubt of the date of the conferring of a degree, or of the admission to sacred orders. Upon these several points, each of prime importance to even the briefest sketch of the career of a noted scholar, physician and clergyman, Tragus' biographers from Melchior Adam away back in the year 1620, down to Ernst Meyer in 1857, are silent. Not one of them names a school of any grade, or of any profession, which the man was known to have attended; and if any should suspect Tragus of having been a medical practitioner without a diploma, and of having enjoyed as a Protestant layman the income of a large Catholic parochial endowment, there is not a line in the most authentic biographies, as I have read them, by which to allay either suspicion.

Tragus was born at Heidesbach, not very far from Heidelberg, in the year 1498. Concerning the estate of his parents as poor, or as in easy circumstances, nothing is known. As a youth he was found uncommonly well educated; whence Meyer inferred that the parents were well to do. That inference may as easily be wrong as right.[1] At the monasteries of the period there was free education for boys of intelligence and promise, if their parents were poor. Meyer, writing in the middle of the nineteenth century, should have bethought himself of this, that in the Germany of Jerome Bock's boyhood Luther and the reformation had not yet appeared, and monasteries were everywhere. The biographers all affirm the disappointment of the parents at the young man's final and very fixed determination not to become a monk; and the disappointment seems to imply that he had been placed under monastic influence, and on their part hopefully.

The considerable views which we obtain of Tragus' life here and there do not show him always happily circumstanced, but rather more commonly as acquainted with adversity. Physically he was a consumptive. Even out of the ten children born to him eight died young; and he survived also the mother of them. It is in the year 1523, when he is twenty-five years old, that we find him settled in Zweibrücken, under the appointment of the Palgrave Ludewig, as a school teacher, and also as in charge of gardens of this prince, which he is said to have enriched with many plants.[2] This favorable incumbency Tragus seems to have held for some nine years, and came to an end in 1532 by the death of the Palgrave Ludewig. Quite before this date we should have found Martin Luther a central

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[1] *Geschichte der Botanik*, vol. iv, p. 303.

[2] Meyer, *Ibid.*

figure before the German public, the ecclesiastical revolution in progress, and Jerome Bock a zealous and outspoken Protestant. Prince Ludewig's successor, Friedrich II., was either still firmly a Catholic, or else unwilling to declare himself a partisan of Luther; and Jerome Bock, botanist, lost his position and became penniless with a young family on his hands.

A plan inaugurated by some sympathizing friend for his relief is one that ought to have been regarded as somewhat hazardous, and indeed may have seemed so to them; but it was attempted. A beautiful country place called Hornbach was the site of a Catholic church dedicated to Saint Fabian and, long before Tragus' time, richly endowed by private munificence. The parish was vacant. Luther's influence had been felt there and the membership was divided between allegiance to Rome and sympathy with Luther. It was hoped, so says Melchior Adam—himself strongly a Lutheran partizan—that the "old superstition" had been banished to such a degree that the new incumbent would be able to complete the "reformation" of St. Fabian's. The event proved otherwise, and the incumbent was obliged to retire; not, however, very promptly, nor until after he had done much botanizing in the wild rich regions roundabout; such journeyings being made, as he informs us, disguised as a peasant.

On his enforced retirement from this anomalous and only quasi-official incumbency he was again in the straits of poverty extreme; until relief came in the form of a bidding to make his home in the castle of Count Philip of Nassau, who remembered our botanist as having once brought him safely through a dangerous illness. His sojourn with this friend seems to have continued through several years, though for precisely how long can not be ascertained. During this interval, however, the affairs of St. Fabian's Church at Hornbach had undergone a change, probably that of the elimination of the Catholic element; for a way was now open for Tragus' return to the enjoyment of that benefice. He is said to have received a hearty welcome back by those remaining, and that he ended his days there not very long after his return is certain. He died of consumption early in the year 1554, at the age of about 56 years. A half-century or so later when Melchior Adam was engaged upon his biographies of German worthies, a search appears to have been made for Tragus' tomb at Hornbach. On the site of St. Fabian's Church nothing remained but a mass of ruins; but forth from under the fallen rocks they were able to bring a memorial tablet bearing this inscription: [1]

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[1] Melchior Adam, *Vitæ Germanorum Medicorum*, p. 70.

"Anno Dom. 1554, 21 Febr. Hieronymus Tragos, animæ corporisque medicus, et canonicus huius ædis, in Domino Jesu obdormivit; cuius anima in consortio beatorum quiescit. Amen."

Phytography. The third in order of time among the more noted German botanists of his century, Tragus is the first in their line to actually describe plants. Brunfels had caused pictures to be made, and then by way of comment on each picture had gathered together all that creditable authors in times past had written about the plant, repeating their language to the letter, and citing religiously the volumes and the pages. Fuchs had also pictured every plant, and then to lessen the cost of printing, had presented short descriptions, compiled from other authors, and for the most part given forth as his own.

Two circumstances, both beneficent, united their influences to make Tragus' work peculiarly and distinctively a book of plant description. First of all, the man was by nature an ardent lover of plants. He began to pursue botany for the mere love of it, without thought of thereby acquiring either fame or profit. He who loves things will see much in them to which the indifferent calm and cool observer is blind. If the born botanist—not the machine made one—write of plants he will find language wherewith to enable his readers to see what he has seen in a plant; and this is phytography. The second favorable circumstance was that of poverty. Tragus at first had no money with which to employ draftsmen and engravers. When he yielded to the entreaties of friends of botany that he would prepare a book, he wrote it in the German language, and with the intent that unfamiliar plants at least should be recognizable with German readers by his verbal descriptions of them alone. In passing at once from the descriptions in Fuchsius to those of the Latin edition of Tragus, one is impressed by the originality and the vastly superior excellency of the latter, it should be recalled that the high quality of them is largely due to their having been first written and published in German,[1] and without thought of their ever being accompanied by pictorial aids to the identification of the plants; and that it was their great success as descriptions that called for the republication of the book in a Latin version, so that the whole might be available to scholars everywhere outside of Germany.

While easily to be ranked among the fathers of phytography,

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[1] Both the *New Kreutterbuch* (1539) and the *Kreutterbuch* (1546) are in German; the former without illustrations.

and even as the first of them in point of time after Theophrastus, Tragus does not systematically describe all the plants which he finds occasion to present for discussion. The most familiar things of field and garden, such as every one has known from time immemorial he neglects, as to their organography, assuming, as did the ancient authors, that the names will recall the images of the plants to the mind of almost any reader. It is the wild things, the native growths of Germany, together with the more recent introductions into German gardens, which engage his powers of verbal delineation.

To the vocabulary of descriptive terminology Tragus does not make any considerable additions. He very commonly employs the old established system of comparisons; conveying an impression of the root, the stem and mode of growth, the leaf-outline, the inflorescence of a less known plant, by comparing them with the like organs in very familiar types. Sometimes he does, what every one besides had always done, that is, he repeats old comparisons that had been in steady use unaltered for a thousand years and more, and occasionally he makes bold to suggest a new one which he thinks better. Dioscorides had described in the following terms the foliage of a certain plant:[1] "Cyclamen has the leaves of the mature ivy bush, but colored purple, and variegated with whitish markings." Of course Tragus had pondered well this classic line, but thought it might be improved. Here is his own description of the leaves of what he takes to be the same plant. "The leaves of cyclamen are spread over the ground in a circle, are very similar to those of the ivy, or, as I think, of the asarum rather, of a dark green, underneath somewhat reddened, above more brownish, and marked with whitish spots."[2] This is liable to be promptly adjudged a better description than the other, at least for cyclamen leaves as most people are used to seeing them; and the cyclamen which Tragus knew in Germany has foliage assuredly more like that of asarum than like that of old ivy. But what renders the attempted amendment of the Greek author's diagnosis infelicitous is this, that Dioscorides never saw the cyclamen species that Tragus knew, and that at least some of the several Mediterranean cyclamens have leaves of more nearly ovate outline, and therefore better likened to bush-ivy leaves than to those of asarum. The audaciousness of Tragus is not diminished by the consideration that Dioscorides had not only also known asarum, but in describing its

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[1] *Diosc.* Book ii, ch. 158.

[2] *Stirp. Comm.*, p. 905.

foliage had definitely indicated wherein it differs from that of ivy[1]; so that if the cyclamens of Greece and Italy as he had known them had shown asarum leaf outlines he would have been likely to have said so. Tragus was deceived by his own gratuitous supposition that the cyclamen of Germany was the one only cyclamen existing.

The common mullein had been used in medicine from the time of Hippocrates; but in the books not much more than one line of description, and that of the leaves only, had been accorded it. Nothing even remotely approaching the following by Tragus had been written about it:

"A very notable thing in this plant is the long straight thick root, of a woody hardness. Its leaves, especially the earlier, lie close to the ground, are rather broad and long, of a whitish aspect and woolly, more so than those of helenium (that is, elecampane, *Inula Helenium*). Not until the second year does it send up its stem, full of a white pith within, like the elder, and sometimes attaining a man's height, clothed with leaves which gradually become smaller and narrower as they approach the summit. The flowers, yellow, woolly, and most sweet smelling are of five distinct leaves, and completely cover the stem from where they begin up to the very apex of it; which falling away are succeeded each by a woolly globe crowded full of seeds not unlike those of a poppy. When the plant is in flower it well resembles a beautiful torch, whence the name King's Torch has been given it."[2]

It is the earliest botanical account given of the mullein. The writer of it is manifestly a botanist; for he has busied himself with the investigation of this plant as a plant, not as a thing either useful or useless. The subject of this piece of research is but a weed, but he has followed it through its life history, examining its root, dissecting its stem, noting the norm of its foliage, and also the deviations from it, has counted the segments of the corolla, discovered that from within there is exhaled a pleasant odor, has inspected with care the seed vessel and its contents, likening the seeds within it to other seeds that every one is familiar with. There is even added an item of the folklore of the plant. The like of this comes very near to being something new in the history of botany; and the book abounds in plant descriptions of this new and original type.

For an example of his diagnosis of a plant never before described by any one, take that of the Lily of the Valley:

[1] *Diosc.* Book i, ch. 9.

[2] *Stirp. Comm.*, pp. 216, 217.

"From a fibre bearing an extensively creeping root somewhat like that of lolium there arise in the month of April green sprouts not so unlike those of asparagus, which turn out to be nothing else than a pair of green leaves closely wrapped together, and supported on a common footstalk. Now when these two begin to separate from each other they assume the appearance of a pair of leaves of the white lily; at the same time there is seen to arise as from between them a triangular peduncle ornamented by five or six little globes placed one after another, of about the size of a chick pea, which in the latter part of April expand to little cymbal-shaped flowers of a snowy whiteness, round, hollow, serrated around the lower part (*i.e.*, that part looking toward the ground), in the middle within marked with a purple spot. the whole flower most fragrant, but of a bitter taste. After the falling away of these they are succeeded, at the end of June, by a coral-red fruit not unlike that of the asparagus."[1]

The production of this fine word picture had been prompted by something more than mere admiration for a beautiful plant which, although unknown to descriptive botany, has risen to high repute in medicine, at least in Germany. A spirit of friendly rivalry seems to have added zest to those morphologic investigations from which the description followed. Brunfels, who had been first to figure the plant, had found no description of it anywhere, and left it without any. Concerning the medical authorities of the time, whose chapters he had ransacked in search of some account of it, he says he has found them "as silent as fishes."[2] We learn from Tragus that Brunfels afterwards changed his mind in so far as to doubt whether this might not be the *Hemerocallis* of Dioscorides[3]; and Tragus the more carefully studies lily of the valley from earliest spring to the end of the season, and describes it every part from root to fruit, that he may successfully controvert the view of that "man of pious memory, Otto Brunfels." The argument, given at full length, occupies a separate paragraph appended to the description

On his frequent excursions to the woods and other wild lands, Tragus had discovered many growths hitherto unknown; and while never until late in life having thought of availing himself of the engraver's art, he published a long list of such by verbal delineation only; and so well that plates, when at last in a new edition he

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[1] *Stirp. Comm.*, p. 572.

[2] Brunfels, *Herb. Viv. Icon.*, vol. i, p. 212.

[3] *Stirp. Comm.*, p. 573.

made use of them, were wellnigh superfluous, at least to all who were competent to read the descriptions. No greater praise can be given any man of that epoch, than will be rightfully accorded to Tragus in adjudging him to be, for the whole era of modern botany, the first father of phytography after Theophrastus.

Anthology. It was now quite time that some one should resume the investigation of floral structures; a part of botany in respect to which no advancement had been made for fifteen centuries. Neither Brunfels nor Fuchs had even so much knowledge of them as had been attained to by Theophrastus eighteen centuries before. The Greek had said concerning nut trees and oak trees that, over and above those vermiform tassels whose use and nature he could not explain, they have real flowers, things out of the very heart of which nut and acorn were developed. Fuchsius, the famous professor of medicine, physician to the titled and the affluent, and moneyed employer of draftsmen and engravers to figure plants, had boldly ventured the unwarranted opinion—contradictory to that of the great Theophrastus—that the tassels of oak and hazel take the place of flowers, and that such trees have no other kind at all.[1]

Tragus, the poor schoolmaster, the apparently unlicensed country doctor, the unordained preacher of the new evangel, welcomes this kind of an opportunity to assail the errors of the dogmatists who sit in exalted station; for, whatever else Tragus may or may not be, he is a botanist well worthy of the name. The combined botanical knowledge of all the Brunfelses and Fuchses of Germany is but a small fraction of what he has seen and taken note of in the book of nature. He has studied the hazel bushes both wild and cultivated in their several species, from the time of the lengthening of their aments in February, all through the spring, and the summer, and the autumn. Theophrastus had averred that, whatever the tassels might not be, the bush has flowers, demonstrably such by the fact that the fruits develop from them. Of the form and coloring of such flowers, and the time of their appearing, he had given no hint; and since at the period with which we are dealing, quite as during two thousand years before, the first idea of a flower was exactly our idea of a corolla, and none had ever seen such things on hazel bushes at any time of year, it was not so wholly inexcusable to deny that there were hazel flowers. And yet, the most cursory reader of Theophrastus' chapters on flowers must have seen that he recognized flowers as either petaliferous or apetalous,

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[1] Fuchs, *Hist. Stirp.*, p. 397.

even though he did not use those terms; and thus, if corylus failed ever to deck itself with corollas, the next thing in order would be to scrutinize the shrubs in quest of the Theophrastan "capillary" flowers. In such a quest as this Tragus was successful; and since he is the first to actually describe the fertile flowers of *Corylus Avellana*, his words have such historic value that we must quote them: "All kinds of corylus have diminutive red flowers, resembling the very short stamens of a crocus, which they display just before the unfolding of thei leaves. It is in the month of February that those aments which some erroneously suppose to be the flowers, acquire their yellow coloring. Theophrastus in the sixth chapter of his third book of the *History of Plants* makes mention of the proper flowers of corylus; nevertheless Leonard Fuchs and John Ruelle besides some others, persistently deny that corylus has ever any flowers; a thing which beyond doubt they would not have done, had they ever once looked into the book of nature on the subject."[1]

After so signal a contribution to the anthology of the hazel, and especially after such public castigation of Fuchsius and Ruellius, it was fateful that Tragus should err, and that ridiculously, in his philosophy of alder blossoms. Here, having denied that hazel aments are flowers, he is confident as can be that alder aments are true flowers; and he proves it in this wise: "The alder, at least when full grown, adorns itself in spring with brownish flowers, almost like those of the birch and the beech; but they do not fall off, as we have observed that they do in the beech, but are permanent, and come to be drawn together so that they at length acquire something like the configuration of an olive. And this is the fruit of the alder, which matures at the end of summer, and then falling sows itself, so that by this means new alder trees are produced."[2] Afterwards in giving account of the flowering and fruiting of birches he betrays the same nnocence of the real origin and history of the small seed-bearing cones[3]; inferring that they had been produced by a final contracting and thickening of the pendulous staminate tassels. A less erratic and more logical mind would not have been content with affirming that the loose pendulous aments of birch and alder are real flowers, and at the same time denying that the very similar ones of hazel have anything at all of the nature of flowers. Yet when it comes to the oaks, Tragus must again be given a credit mark as having detected, next after Theophrastus, their very

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[1] Tragus, *Stirp. Comm.*, p. 1095.

[2] *Ibid.*, p. 1085.

[3] *Ibid.*, p. 1114.

obscure fertile flowers. "The leaves of all kinds of oaks when young are very small and delicate, and there appear with them at this stage long yellow aments. . . . After the aments there come forth very small red flowers which subsequently transform themselves into acorns."[1]

Sexuality in plants never yet having been apprehended, Tragus had no conception of the fact of diœcism. He studied the catkins of willows, but without discovering that those of some individuals are promptly deciduous, and that only those of certain other individuals remain longer. He therefore wrongly attributes to willow catkins indiscriminately the quality of remaining on the tree until they have developed a kind of wool which sails away on the passing breeze. In just this connection, however, he makes a remark which reveals him for once in the character of a truly inductive philosopher, unwilling to venture a broad general conclusion from an isolated fact. "Whether this wool of willows be their seed or not I do not know, except as regards the fourth species, in the case of which it at least takes the place of seed; for in this I have caught the floating wool, have sown it, and have seen willows of this same species spring up from the sowing." [2]

As to certain particulars in the structure of petaliferous flowers the chapters of Tragus seem still more clearly to herald the coming of a new era in anthology. Not that he has any new doctrine of the flower. So far from it, he follows the universal and time-honored practice of calling, in the case of petaliferous flowers, that and that alone the flower, which only long after his day came to be known as the corolla. But he observes and takes note of things outside of this "flower," and of other things inside it, the tendency of which notes and observations is to raise a question as to whether there are not other things which, taken together with the circle of colored leaves, should all collectively be thought to constitute the flower. He does not formally propound any such question. Even that suggestion of it which his language carries, it is more than likely he himself did not perceive. Let us attend to a few instances of anthological comment that seem to be new and original with Tragus.

As subjects of brief description and of various comment the poppies are very ancient, and were so in Tragus' time; and he is the first man in all history to describe the plants as with the pen of a botanist; the pen of a man who had looked at them with his own

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[1] *Stirp. Comm.*, p. 1101.

[2] *Ibid.*, p. 1073.

eyes, and had seen points of morphology never mentioned before him. One of these points relates to the "flower" before its unfolding; or as we should now say, to the flower buds. No such expression as flower bud, however, was yet in use; and Tragus names the thing a *capitulum*, a little head. Poppy plants have what have been called from time immemorial their heads, that is to say their capsules, or seed vessels; and Tragus now attributes to them a second kind of head, one never before mentioned; and this is his language: "The head from which the flower is to break forth is covered with a pair of skins (cuticuli), green as to color, and also hairy. When the flower itself is ready to make its exit the two integuments separate and promptly fall away."[1]

He has several genera, and in all some eight species and varieties of poppy allies, and to the group as a whole he attributes this pair of caducous integuments. It is a significant item in the history of anthology. He has recorded the discovery of an organ, and has given at least the hint of its possible availability in taxonomy; but, as is usual with discoverers, he is in advance of the time in which his work will be appreciated. It will be yet two hundred and forty years before this pair of integuments to the poppy bud will acquire their name as sepals, and about as long before their caducous nature will be recognized as a good character for the family of the *Papaveraceæ*.

In respect to the forms of flowers, *i.e.*, corollas, there are evident traces of attempts—perhaps half-unconscious efforts—to generalize. As if the wild rose might have been looked upon as the most perfect flower-form, or at least as a most representative type, he is given to speaking of other broad petalled and subrotate flowers as being rose-like, or even as being roses. In describing *Pæonia*, which he figures in a single-flowered state, he twice refers to its "roses," hardly using the word flower at all. "In all our Germany you will hardly find a more elegant rose than that of pæonia"; and again he says that "toward the end of April a round head at the summit of each stem all at once breaks into a broad red rose."[2] In like manner he speaks of the flowers of hollyhocks only as roses, and is wont to denominate any large flower of five petals widely spreading as rose, or at least as rose-like. Smaller ones, with petals equally spreading, and especially if they be acutish petals, are his "stellate" flowers. But if the five spreading petals be unequal in some degree, and especially if one of them be at all prolonged at

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[1] *Stirp. Comm.*, p. 119.

[2] *Ibid.*, p. 582.

base into a hollow protuberance or spur, such a corolla is not with him a flower but a "violet." The proper violets, that is the acaulescent kinds, and the pansies or tricolor kinds, are regarded by Tragus as by all others before him, as of two genera with different generic names; yet flowers of both are spoken of not as flowers but as "violets"; and the same term is applied to the flowers of larkspurs.[1]

But there is a second and very different corolla-type which is also called by Tragus a violet-flower, namely, the cruciform. This came about by reason of the familiarly known fact that in the nomenclature of that period there was a genus *Viola purpurea*—sometimes called *Viola Martia* (=*Viola*), and also a genus *Viola alba* (=*Matthiola*), besides even a *Viola lutea* (=*Cheiranthus*). It is with this cruciferous viola type in mind that he describes the flower corymbs of *Viburnum Opulus* as adorned with "an outer circle of large white violets consisting of four leaves."[2] But among the crucifers particularly, the flowers of all that have rather large and showy petals he habitually speaks of as violets; for example those of the mustard,[3] the cabbage,[4] and the turnip,[5] with also that of celandine.[6] That it was not the merely cruciform arrangement of spreading petals that caused a flower to be called a violet of this type becomes apparent when one has observed that Tragus never applies it to the blossom of any small-flowered crucifer. In the case of the lepidiums, and *Bursa Pastoris*, and all others having diminutive petals, it is his custom to say only that the flowers are small and of such or such a color; never speaking of them as even small violets. For the crucifer-violet to be called a violet there must be some approximation to the size and showiness of the wall-flower and gilliflower.

But now, the learned philologist may interpose that, after all, both rose and violet, in the speech of primitive peoples are terms indicative of not particular kinds of flowers; that each, in its origin, is but the synonym of that other and later word flower. Nor may this be successfully controverted. The very nomenclature of scores of familiar flowers to-day attests the truth of it. There is Christmas Rose (=*Helleborus*), China Rose (=*Hibiscus Rosa Sinensis*),

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[1] *Stirp. Comm.*, p. 568, 903.

[2] *Ibid.*, p. 1002, under the name *Sambucus aquatica.*

[3] *Ibid.*, p. 100.

[4] *Ibid.*, p. 718.

[5] *Ibid.*, p. 728.

[6] *Ibid.*, p. 106.

Guelder Rose (=*Viburnum Opulus*), Rock Rose (=*Cistus*) Rose of Sharon (=*Hibiscus Trionum*); also there is Dame's Violet (=*Hesperis matronalis*) Dogtooth Violet (=*Erythronium*), and a great number of other such, for plants that have no affinity to either the rose genus or violet genus. Doubtless with some of the peasantry of several countries to-day the showy flowers they know may be found classified in their languages more or less definitely as roses, lilies, and violets. And something like this was most certainly true four hundred years ago in rural Germany where Tragus was born, and where he did all his botanizing. And although I find him twice using the term violet for small flowers that do not readily fall into either of his two definable categories of violets, I do not think that either one instances a lapse into that primitive usage under which the term is synonymous with smallish petaliferous flowers in general. If he calls the flower of the catsfoot, or ground ivy[1] a violet, the color of the flower, and the irregularity of its corolla-limb may have suggested it; and the corolla of a bryony[2] is not unlike that of the sterile outer row of those in viburnum[3] corymbs. That he in truth goes far on the way toward a convenient morphological classification of corollas is evinced by his giving diagnostic names to still other forms. Among the polypetalous he even distinguishes the rotate from the rosaceous. The flower of nigella he says is "round like a wheel,"[4] that is, the petals spread away in a flatly horizontal direction from their axis, a thing which can not be said of either rose or pæony petals. It is an excellent distinction, not noted even by Tournefort the great corollistic systematist of a hundred and fifty years later; for he describes the nigella flower as rosaceous.[5] The campanulate, or campaniform, was also named by Tragus. In his description of the plant hyoscyamus he says that it has little bells for flowers.[6] The corollas of campanula,[7] of digitalis,[8] and of vaccinium[9] are said to be campaniform. The funnelform is also alluded to, though under the term cymbaliform. He attributes that configuration to lily and morning-

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[1] *Stirp. Comm.*, p. 798.
[2] *Ibid.*, p. 819.
[3] *Ibid.*, p. 1002.
[4] *Ibid.*, p. 117.
[5] Tourn., *Elemens.*, vol. i, 225.
[6] *Stirp. Comm.*, p. 132.
[7] *Ibid.*, pp. 724, 926.
[8] *Ibid.*, p. 888.
[9] *Ibid.*, p. 974.

glory flowers. In a word, he is so definitely the first forerunner of Tournefort in the matter of observing and carefully noting a number of marked corolla forms, that it seems due him that a list of them should here be given synoptically.

1. Rotate, typified in Nigella.
2. Roseform, typified in Rosa, Pæonia.
3. Violetform (proper), typified in Viola, Delphinium.
4. Violetform (cruciferous), typified in Cheiranthus, Matthiola.
5. Stellate, typified in Sedum, Solanum.
6. Campaniform, typified in Campanula, Digitalis.
7. Cymbaliform, typified in Convolvulus, Oxalis.

Gifted with keen perceptibilities in the matter of floral structures, it is rather remarkable that Tragus did not assign names to such very strong corolla-types as the bilabiate and the papilionaceous; for I find in him no trace of any term by which he would designate either one. Even the later Latins seem to have denominated certain very nettled-leaved labiates as "nettles with a lip"; and this most lynx-eyed and original inspector of flowers seldom names anything more than the mere color of labiate flowers, once only—the case of glechoma above referred to—giving perhaps a hint of the form. In respect to the flower of leguminous plants he does not so much as second the suggestion of Gesner that the pea blossom has the form of a butterfly, but attributes, for example, to the pea vine the flower of genista, and to genista in its turn the flower of the pea, and so on to the end of the series. There was nevertheless in Tragus' time a German school boy who had already coined the term papilionaceous for these flowers, though only in manuscripts that were not published until after Tragus' demise.

Among those scattered anthological notes which, to his contemporaries and to himself, may have seemed of least moment were his various observations upon stamens. Their function had not yet been guessed at, or even by Tragus himself so much as thought about. The organ had not been even morphologically contemplated, in its individuality, up to the time when Tragus began to examine it in different flowers comparatively. From time almost immemorial they had been mentioned only collectively, in Latin writing, as the capillamenta, the flocci, the stamina, the apices; mostly a tuft of thready things, with or without knotted tips. It is altogether a new thing in botany for a man to write as follows concerning the white-lily flower: "From out the bottom of it there stand forth six apices, and these yellow; then prominently from the midst of them a kind of thickish stamen that is green and has a triangular head, the whole being shaped something like a walking stick."[1]

In a flower of the size of a lily such diagnosis of less obvious parts is easy, insomuch that one need not have been surprised if many a writer before Tragus had indicated them as well; but for similar inspection and diagnosis a gooseberry blossom was small and difficult for a botanist whose researches antedated the invention of hand lenses. The following is our botanist's account of it; and it is the oldest one extant:[2] "In March the bush puts forth its flowers, small, concave, purple, which if you examine them one by one you find to contain within five diminutive apices each supported on a hair, the whole resembling a little bell."[3] Another flower which he is first to describe is that of the hawthorn. This is not much larger than the gooseberry blossom, and of less simplicity in its structure; but he brings out its characteristics quite as clearly. "The individual flower is made up of five small white leaves, from out the middle of which there stand many white things like hairs supporting rose-colored apices, such as one observes in all pomaceous flowers."[4] Among his "pomes" Tragus includes also the drupe-bearing trees, and his account of their floral structure is given once for all under the caption of the Wild Plum. "The individual flowers of the wild plum consist of five bright-white leaves; and in the midst are seen about eighteen as clear white capillamenta or stamina each supporting its small yellow apex. Just about this is the structure of the flowers of nearly all the pomes, cherries, plums, pears, and apples, except that some of them exhibit a greater number of capillamenta and apices than do those of this wild plum."[5] From this point forward throughout the line of the pomaceous and drupaceous trees, he has little to say of the flowers beyond this, that "they are those of all the pomes," sometimes remarking that the "apices" are red, or that they are yellow.

In thus taking a census of individual stamens, recording the number of them as being constant in each different kind of flower, even distinguishing by name each of their two parts, it is evident

[1] *Stirp. Comm.*, p. 794.

[2] Fuchsius had said of gooseberry flowers only that they "are of a purplish green."

[3] *Stirp. Comm.*, p. 978.

[4] *Ibid.*, p. 934.

[5] *Ibid.*, p. 1016.

that Tragus on his own part was but entertaining himself and some of his readers with mere curiosities; that he had no notion whatever of the real importance, to the future of botany, of that which he was doing. Let us, then, take a still more careful survey of the things which he accomplished in this particular direction which were for the advancement of anthology, albeit he himself was all unconscious of their import. To begin enumerating the stamens of particular flowers, and to record in each instance the count, was to make a decided innovation upon the immemorially established anthology of Tragus' time, which had always treated of them in the aggregate only, as tufts of hairs or threads. If he announces that in every gooseberry blossom there are just five such hairs or knotted threads, that is announcing that he has made two different comparisons of them: first, a comparison of the five one with another, and then of these with two other hairs quite different from them which occupy the very center within their circle. He does not mention these, the styles, or style-branches, yet can not have failed to perceive them. But he now proceeds virtually to define a stamen. It is the little apex, and the hair on whose summit the apex is sustained. Elsewhere he repeats the definition more explicitly, and, avoiding the use of any new terms, he calls that the "capillamentum" which we now know as the filament, and "apex" is his application of an old term to what has become the anther of modern anthology.

Tragus is, then, the discoverer of the stamen as a definable organ made up of two separate and different parts, each part with its own name. The same is very near being true of him as regards also the pistil. We have just seen how accurately he could describe the style and stigma of a lily as things quite apart from the stamens; and as to the flowers ofcommon fruit trees,we must eel assured that he saw the one style of each plum and cherry blossom that he inspected, and the five of them that are conspicuous in the flowers of pear and apple trees. He did not mention them; but then, they are as plainly visible there as the stamens themselves, and both his enumeration and his definition of these prove that he did not confuse the styles with them. Curiously enough, after having located and well outlined the large lily style and its stigma, the very next account he gives of such central thing is in connection with a flower even smaller than that of a gooseberry, namely, of the little ericaceous undershrub then called Myrtillus.[1] Here the stamens seem to have escaped his notice, as they easily

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[1] The *Vaccinium* of recent botany.

may have done, being both minute and hidden away within the partly closed hollow of the corolla at its base, falling away with it; but the style is very obvious, and in describing this hollow bell of a corolla he says: "It has in the middle a red-brown something like the clapper of a bell, which, after the falling of the flower grows into a round reddish berry."[1] The Latin term used for this thing in the middle of the flower bell is *pistillum.* It is the first instance of the employment of that term to designate what has long since become universally known by that name.

On the whole, then, as by suggesting the classifying of corollas this author is the first herald of Tournefort; much more conspicuously as the first investigator, even fairly the discoverer, of stamen and pistil, is he the first forerunner of Linnæus. It was anthology which created the new botany of the eighteenth century and the nineteenth; and the beginnings of the modern anthology are with Tragus.

Fruit and Seed. Well in advance of classic antiquity in the knowledge of floral structure, Tragus not only added nothing to carpology, but had never learned either from Theophrastus or by research of his own anything like all that Theophrastus had attained to along this line. One may doubt if any other book was ever printed, or even written in any age, in which there find expression so many whimsical and superstitious fancies about seeds. In a general way they seem to fall short of having that value in the economy of plant life and plant distribution, in Tragus' opinion, which even a remote antiquity accorded to them. The transmutation of the seeds of cereals into germs of chess and darnel he accepts without the expression of a doubt. Abiogenesis, the doctrine of the origin of living things from lifeless matter, he accepts unwaveringly, and with a plenitude of faith probably surpassing that of the Greeks who aforetime invented the theory. He even defends it by theologic arguments[2]; and by it he explains the coming into existence not only of low and simple flowerless things, but also of some of the highest types, even of trees. The time seems to have been when he had thought that all willows had at first come forth spontaneously from the mud of river banks; for he knew them to be propagated by cuttings always, and no one had ever seen or heard of a willow seed. Even when with such truly scientific inquisitiveness he had planted some of the "white down" that he had gathered as it was about to float off from a willow, and had afterwards the satisfac-

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[1] *Stirp. Comm.*, p. 974.

[2] *Ibid.*, pp. 1125, 1126.

tion of seeing willows spring up as from that down, this neither convinced him that all the other kinds of willow might be raised in that way, nor even that any willow had ever borne a real seed. He says that one out of the four species of willows which he describes can be propagated in this way; but he vouches for none of the others; does not infer that the other three may be found producible from "willow down." Nor did he have any idea that he had planted willow seeds. "The down takes the place of seed."[1] There are certain kinds of poplar, and even of maple, which he thinks never bear seed at all, and which he therefore thinks came into existence abiogenetically. These were diœcious trees, none of them indigenous to Germany, and perhaps then existing there in only the male sex. He has seen the tassels of *Populus alba*, and knows them to be always deciduous; also no one knows of any seed as consequent to the flowering of *Acer pseudo platanus*. They are seedless, and therefore, to the faith of Tragus, abiogenetic in their origin. It even seems to be his opinion that certain plants plentifully seed-bearing may upon occasion spring up and mature in places where no seed of them had fallen. It is upon just this theory that he accounts for the occurrence of individual plants of many kinds on high walls of solid masonry and upon the roofs of buildings. No orchids, in his understanding of them, produce seeds at all. He is familiar with the fact that in autumn, as the plants are withering away, a very fine dust falls from where the flowers were, but he affirms that this perishes together with the season's growth of stem and leaves.[2] He presents to his readers a strange fancy about the primal origin and the perpetuation of this class of plants. He is the first author to mention, and may or may not have been first to observe in the flowers of orchids, resemblances to birds and other flying things; but he writes much as if he had been the inventor of the theory that this kind of seedless plants originates from certain excretions of birds. It is in the chapter that is devoted to the birds' nest orchis that he explains this belief. Such plants, he remarks, abound chiefly in and about thickets where small birds mate and nest.

The belief in all kinds of spontaneous generation, of even seed plants as well as the seedless, did not preclude all research on Tragus' part along these lines. There is a picturesque account of nightly vigils in search of the problematic seeds of *Osmunda regalis*.[3]

[1] *Stirp. Comm.*, p. 1073.

[2] *Ibid.*, p. 784.

[3] *Ibid.*, p. 544.

While it was generally held that all fern-like plants are seedless, this one seems to have been popularly credited with shedding seeds regularly on one particular night of the year. Thus runs Tragus' own story:

"Inasmuch as all writers about herbs have said that ferns produce neither flowers nor seeds, I have thought it worth while to place on record, for the information of botanists, an account of my own experiences, which prove the contrary. For four years in succession I kept vigil all the night preceding the Feast of St. John Baptist.[1] and always found in the very early morning, before the break of day, very minute black seeds, not so very unlike poppy seeds lying on the pieces of cloth, and the mullein leaves which, in order that I might not miss the seeds, I had placed under the plants beforehand. Some of the ferns had shed no seeds at all; others had deposited them by the hundred. Moreover, in these experiments, I employed no cabalistics, no conjurings, no incantations, no superstitious observances of any kind, nor did any one of the three companions of my vigils; but having made a fire we watched and waited, sometimes finding none, at other times a few here, and many there. Why there should be such a diversity in the yield of seed, and what the purpose of nature may be in all this matter, I do not understand."[2]

This account incidentally reveals it that in middle Europe in the sixteenth century there still flourished the ancient profession of the root and herb gatherer, in the practice of its old time superstitions; that men believed that under the sacred spell of the summer-solstitial midnight such flowerless and seedless herbs as ferns, by help of solemn incantation could be made to scatter seeds; these presumed to be efficacious in medicine or magic. Tragus, the inquirer and reformer, half believing and half disbelieving, investigated the matter, proving to his own satisfaction that ferns bear seeds; that they produce them naturally, without the promptings of conjuration; yet it seems not to have occurred even to our reformer botanist to look for fern seeds in the day time, or at night except on that immediately preceding St. John's Day!

The distinction, indicated so long ago by Theophrastus, between plants with one seed leaf and those with two, though never again brought forward prominently until long after Tragus, had not been ignored by him. It had been the cereals and their kindred to which the Greek had ascribed the one seed leaf as a universal

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[1] The 24th of June; otherwise called, at least in Old England, Midsummer Day.

[2] *Ibid.*, p. 544.

characteristic. There was now common in Germany one breadstuff plant which the ancients had not known, namely, buckwheat. Tragus describes and figures it among the proper cereals, but with the remark that it differs from all the rest of them in that it comes up from the seed with a pair of leaves instead of with one alone, in this respect more like a turnip or cabbage.[1] And although he was a student of wild plants rather than of cultivated, there are many instances of his concluding a description with the statement that the seedlings of such a plant come up with two leaves. Now and then he mentions the outline of such seed leaves in some particular plant as contrasted with those of some other; showing that he not only observed but compared them in different plants. But this does not seem to have had any purpose beyond that of gratifying his own curiosity and stimulating the like in his readers. There is no indication of his having apprehended the taxonomic significance of these distinctions between monocotyledonous and dicotyledonous seeds. The time for the birth of this great thought lay distant from Tragus a hundred years and more.

Taxonomy. In his Preface Tragus abjures the alphabetic arrangement of genera as unscientific, bringing in confusion where natural order ought to be. He is clear in expressing the determination to adopt a natural sequence. "In describing things, I come as nearly as I can to keeping by themselves such plants as nature seems to have linked together by similarity of form."[2] This was no new proposition. Ever since plants had first been observed philosophically, and written about, various groups, varying severally as to their extent and inclusiveness, had gained recognition as natural groups through resemblances in morphology. Tragus knew this well, and was only indicating his choice of natural method, in preference to the purely artificial alphabetic arrangement of genera, such as Fuchs and Gesner saved themselves labor by adhering to. Neither does he contemplate considerable innovations upon the long established method of grouping and arranging things. By his own frequent peerings into the curiosities of floral structures and recording what he saw, he has vividly suggested a new anthology. He has even begun it; and in the course of its future development it is going to revolutionize taxonomy completely and that twice over; yet nothing of this is even dreamed of by Tragus; and his superior knowledge of floral morphology has little real and almost no appreciable effect upon his own classifyings.

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[1] *Stirp. Comm.*, p. 648.

[2] *Ibid.*, Præfatio, ch. xiv.

Tragus' taxonomy, like that of all his forebears, is established upon characters of the vegetative organs, with no strong appeal to flower or fruit; and the primary grouping of things as trees, shrubs, and herbs is in his estimation natural and valid. Nevertheless a few exceptions are taken by him against this rule. An underlying principle of such a rule, or at least a logical deduction from it, is that no one genus embraces both trees and herbaceous plants. And really there was but a solitary genus known in early botany that was troublesome to those who regarded the distinction between woody plants and the herbaceous as being taxonomically fundamental. There was the tree *Sambucus* and the herb *Ebulus*. These are their classic Latin names, and the import of this nomenclature is, that they are of two genera. *Ebulus* is not even in the least degree woody or shrubby in any part. In texture and duration it is as perfectly a perennial herb as the common asparagus or rhubarb. Dioscorides, the great Greek physician and medical botanist, having regard to pharmaceutical principles as well as taxonomic, received *Ebulus* as a kind of *Sambucus*. They are alike, he says, in foliage, flower, fruit, and medical properties.[1] Tragus, consistently adhering to the fundamentals of classification there accepted, describes and figures *Ebulus* among the genera of herbs; for even his readers, every one, would look for it among the herbs and not among the trees; and *Sambucus* is treated of far away, in the third book, under the general topic of trees.[2] In both places, however, he ventures the opinion that the two are naturally of one genus. Under *Ebulus* he says, "If you consider its foliage, flower, and the heavy somewhat sickening odor of the herbage, you must regard it as nothing else but a smaller and herbaceous kind of *Sambucus*." Under the latter he says again, "As to its leaf, flower, fruit, and odor this is so exactly like *Ebulus* that the ancients were wont to receive them as of one and the same genus."

From a period too remote for precise limitation, only *Sambucus* and *Ebulus* seem to have militated against the taxonomic validity of that old distinction between herb and tree; and we shall be interested in following the subsequent history of this taxonomic puzzle to its final solution.

That old philosophy of the three grand divisions of Tree, Shrub, and Herb unquestionably carried with it the opinion, certainly not altogether unreasonable, that trees are of highest rank, and herbaceous plants of the lowest. Under this system it will be seen

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[1] *Diosc.*, Book iv, ch. 168.

[2] *Stirp. Comm.*, pp. 796 and 996.

that some authors began with the highest and proceeded to the lowest, and that other authors, beginning with the herbaceous genera, ended with the ligneous, the largest and most enduring trees coming last of all. This last is the order followed by Tragus; not, however, as one philosophically viewing the plant world from lowest type to highest as a genetically connected whole. It is, on the contrary, quite certain that that Aristotelian idea, only now very lately reinstated, never entered Tragus' thought at all. If he takes up herbaceous plants first in order, it may well be because they are both the most numerous as to the genera and species, and of the highest importance to man. Nevertheless we shall find him very much given to running like things together and thus forming groups within groups, lesser ones within the more comprehensive, whether he be dealing with herbs or with shrubs or with trees. This is taxonomic work; and this is the way in which he fulfils the promise made in his Preface about natural arrangement. We must follow him now for some distance, and very carefully, if we are to arrive at an understanding as to what botanical system really was, in Germany, in this first half of the sixteenth century.

In no author as early as Tragus is there given any introductory synopsis or tabulation of the system. Such convenient and helpful skeletonization is a later invention; and here one gathers information about the system, even to the principles that underlie it, only through following the author chapter by chapter from the beginning of the volume to its end.

For a work like this, of 1200 pages, the selection of 100 for such analytic study must suffice. They might be taken at random from any one of the three divisions of the treatise; but we shall select the first 100. Within these there are embraced figures and descriptions of some 74 species, distributed to about 31 genera. Now this proportion of something like two and a half species to a genus was something new in botany and is therefore one of the very significant features of the book; so much so that we must give it a moment's consideration before passing to a study of the sequence of the genera. In order to realize the meaning of my statement that, for the time at which he wrote, the ratio of two and a half species is so great as to amount to an innovation in taxonomy, a brief comparison must be instituted. The 500 or 600 plants that the ancients had dealt with represented, in the great majority of cases, what we of to-day are accustomed to speak of as monotypic genera. Not any very considerable number of their genera are defined as consisting of two or more species; so that they had but one and a small fraction species to each genus. Fortunately for our comparisons the volume of Tragus deals with not very far from the same number of species as that of Dioscorides, and seems to have rather more than twice as many species to each genus; or, to express it differently an equal number of species is distributed to only half as many genera by Tragus as by Dioscorides; and this must be regarded as having been somewhat revolutionary, especially in view of the fact that Brunfels and Fuchs had much less perceptibly, if at all, departed from time-honored usage in such matters.

There may be reason to doubt that such a movement originated in the mind of Tragus himself. There are intimations elsewhere that as a new departure it may have been suggested if not advised by a mind more philosophic than his own. But this is a matter the investigation of which may be deferred. What should here be remarked is, that almost every generation of active and leading systematists during now nearly four hundred years has been divided upon the question of whether plants are more philosophically disposed in few genera of many species, or in many genera of correspondingly few species; and in Tragus' book we are at a kind of starting point in this perhaps endless controversy about the delimitation of genera. As perceptibly inclining to reduce the number of them he is again the forerunner of Linnæus.

Surveying now somewhat closely 62 consecutive pages of the first 100, we enumerate 45 species all in one line, and with a solitary exception, all at agreement as to certain very obvious characteristics. All of them exhibit fibrous roots, quadrangular stems, and opposite leaves. In 41 out of the 44 the leaves are simple and in no wise divided or even cleft. Here is proof that things had been selected and brought together under the guidance of definite morphologic principle. Also much time and toil must have been bestowed upon the getting together of so considerable a number of square-stemmed opposite-leaved herbs all at agreement in a general characteristic of leaf-outline. Some of them are much branched herbs in which no trace of quadrangularity is seen on the maturer branches, but only on the growing twigs; and it was the judgment of something like a botanical expert that brought such into line among the square-stemmed. It is also worth noticing that 41 of the 45 are plants more or less aromatic.

The number of the genera among which the 45 plants are distributed is twenty. First in order stands *Urtica* with three species; then seventeen genera of the family of the *Labiatæ;* then the genus *Valeriana* with three species. As to the nettles proper and the labiate dead nettles, Tragus has order where in Brunfelsius there was confusion[1]; for the figures of the two nettles have *Urtica* printed over them, while the two dead nettles are respectively indicated as *Galeopsis* and *Lamium*. To these succeeds *Marrubium* with an array of four species. Only one of them is of that genus; two belong to a genus that had not at the time been proposed, that is, *Stachys;* the fourth is a plant that was destined to stand as prototype of the genus *Lycopus*. The figure well represents *Lycopus Europæus;* and this appears to be the first publication of the species, though he writes as if it were already familiarly known in the pharmacy of the time under the name *Marrubium palustre.*[2] It is in the course of his definition of this too amplified *Marrubium* that he describes the fruiting calyx with its four naked nutlets; one of the most important characteristics of the whole family of the labiates; though as we have said before he is far enough from realizing the taxonomic value of what he has thus been the first to discover and describe.[3] The presence of this pouch, as he calls it, with its four naked seeds occurring as it does in some herbs with dissected foliage, does not induce him to place any such in the same line with the nettle-leaved labiates. His mind upon these matters is the mind of all antiquity, and of his contemporaries, dominated by the idea that likeness as to foliage and stem and root, together with agreement in sensible qualities, more surely indicate consanguinity than do similarity in respect to seed vessels and seeds. We shall meet with plenty of proofs of this.

Next after *Marrubium* the first considerable genus is *Mentha*. Tragus is aware that it is difficult, and says that quite a number of plants which some have regarded as mints others have referred to other genera. He seems to have a new view of his own, namely, that no plant is properly a *Mentha* that has not an upright mode of growth, with flowers separate from the leafy part, and borne in naked pedunculate spikes at summit of the stem. It is a group of mints that is sufficiently natural, and has been recognized as such by all special students of the genus of later periods. Tragus, as his four plates show, limits the genus to this group, disposing somewhat variously of the equally numerous species that have all their flowers in the axils of the leaves. As good a species of *Mentha* as that called *Pulegium* is excluded, and held as a monotypic genus, doubtless partly on account of its peculiar odor and its

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[1] Page 179 preceding.

[2] *Stirp. Comm.*, p. 10.

[3] *Ibid.*, p. 8.

efficacy as an insectifuge, and partly because its stems are almost or quite prostrate; for in this antique classification by vegetative characters the very posture of stems was regarded as a weighty consideration, as we shall see later.

In the treatment of these plants now long known as the family of the *Labiatæ*, Tragus, quite as if he had recognized the family and wished to keep the members of it in as close juxtaposition as possible, again does violence to one of the very fundamentals of the old system to which he has professed fealty. Rosemary and lavender are genera of labiates, square-stemmed, opposite-leaved, aromatic, and have the flowers and fruits of labiates, but they are shrubs; at least rosemary is, and the lavenders are strongly suffrutescent; therefore the proper place for them is away in Tragus' Third Book, among the woody growths, where also we find plenty of growths that are both smaller and less woody than either of rosemary or lavender; but he has both these here in the First Book, at the end of the line of the labiates, all the rest of which are herbs.

With this ending of the series of the labiates we are brought to about the sixtieth of our one hundred sample pages. The number of genera embraced within the fifty-nine pages is eighteen. All are genera of *Labiatæ* except the first, and that is *Urtica*. We have already seen how nettles and dead nettles were primitively regarded as of one and the same genus. The conceding that the two were generically distinct did not necessitate any wide sundering of them. The close resemblance between them as to vegetative organs, and the clustering of the flowers in the leaf-axils, betokened still a close consanguinity. To Tragus and his contemporaries the transition from *Urtica* to *Lamium*, so far from seeming to be abrupt, was a perfectly easy and natural one

That close against the aromatic labiates of herb and drug gardens *Valeriana* should be located is not so difficult to explain, now that we have Tragus' point of view; for the valerians, at least as to their basal and underground parts, are notably odoriferous; they are not indistinctly square-stemmed, their leaves are opposite, their inflorescence is of the verticillastrate type, and their flowers are bilabiate.

At this juncture the series of the square-stemmed and opposite-leaved is briefly interrupted. The intercalated genera are *Asarum*, *Geum*, and *Ruta*. But in the primitive classifyings aromatic properties were much deferred to, and inasmuch as this whole series, all the way from *Lamium* to *Ruta*, is a line of aromatic plants, the three above named do not interrupt it save only as to stem and leaf morphology, which, at least for the moment, is held subordinate to the qualitative.

After *Ruta* there is at once a return to the line of the quadrangular stem and opposite leaf. The genus is now *Hypericum*, and five species of it are described and figured. These harmonize with the main series as to an undivided foliage set oppositely and at regular intervals up and down square stems; and also as being notably odoriferous even if not distinctly aromatic like labiates. At the same time they have manifest points of contact with rue, that genus which they immediately follow; for their leaves are dotted, and small flatly opening yellow flowers crown the stems and branches. Next after hypericum stands the undershrub, *Santolina*, an anthemideous composite; and, viewed in the light of twentieth-century taxonomy certainly rue and hypericum and santolina placed in line together make a motley order; but, what we are here in quest of is, the set of principles on which sixteenth-century taxonomy was grounded. We are certain that Tragus had his taxonomic reasons for locating santolina where he did, for he states them. Every one who knows the plant is aware of its being notably odoriferous; and he gives as one reason why it may well stand next hypericum the fact that its aroma is that of hypericum intensified. Now on the other side we shall find santolina flanked by two labiate plants; and in respect to its mode of growth, and its aspect as clothed thickly with small grayish foliage, Tragus says that in these things it well resembles lavender and hyssop and thyme. So then, judged by the criteria employed at that period, this was not a motley arranging of things.

From santolina there is a return to labiates. Two species are figured and three described. We wish to know why he thus separated them from the rest of their line. It would be interesting if we could learn his reasons for intruding almost into the midst of the line of the mintworts *Asarum*, *Ruta*, the whole series of the *Hypericum* species and *Santolina*. There is one thing which gives to these last members of the line of labiates an aspect very unlike that of the others; for their leaves are much dissected, while in the line of more than thirty that precede rue and hypericum there is not one that displays any other than simple leaves. This short concluding series consists of compound-leaved species of the genus *Teucrium*. Did Tragus, blinded by foliage so exceedingly different, fail to see that these are true allies of that simple-leaved series that has been interrupted by rue and hypericum? We have the most positive proof that he did perceive the relation; for he says that the stems are quadrangular, that the flowers are verticillastrate around the stems, and that each comes forth from what he calls the seed-pouch, in which particular he likens them to those of hyssop and satureja.[1] But in other instances besides this he is seen to pay such deference to the distinction of compound and simple foliage as to make use of it taxonomically. Of even this genus *Teucrium* one simple-leaved species is not only held generically distinct from those with dissected foliage, but is located 124 pages away from them, where, by the way, on account of its veronica-like habit and foliage it is associated with several veronicas to constitute a genus *Chamædrys*. In his Third Book, in taking up the natural series of the pomaceous and drupaceous trees, the compound-leaved genus *Sorbus* in three species heads the series, quite as if by virtue of its compound foliage it had been regarded as the highest or most advanced type of its alliance.[2] Again, the bulk of the umbellifers, all having pinnated or more dissected foliage, Tragus adopts as a natural alliance, following of course the botanists of remote antiquity; but *Bupleurum*, vested as it seemed to him in a perfectly simple and even entire foliage, he on that account excludes from the family. The genus *Achillea*, of the anthemideous composites, quite imitative of the umbellifers as to foliage and inflorescence, intervenes between *Bupleurum* and its compound-leaved affinities.[3] Other proofs need not be adduced; for we must return to that group of dissected-leaved labiates that close the line of their cognates. They have brought us to number eighty of our one hundred pages.

With page eighty-one, and thenceforward, one notes an abrupt change, at least respecting the morphology of things; for within the next one hundred it will be rare to meet with a plant square-stemmed and opposite-leaved. The stems are now terete, and the leaves alternate; and in place of aromatic odors there is now everywhere a peculiar pungency of flavor to the herbage. The genera and species are, for a time, those of the family of the crucifers. There occurs at first an unbroken line of seven of these. The student to whom the book is not available will be helped by a list of the names of the species:

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[1] *Stirp. Comm.*, p. 79.

[2] *Ibid.*, pp. 1008–1011.

[3] *Ibid.*, pp. 474–483.

	Tragus	Modern
1.	Nasturtium hortense	Lepidium sativum
2.	Nasturtium aquaticum	Nasturtium officinale
3.	Nasturtium pratense	Cardamine pratensis
4.	Thlaspidium	Lepidium ruderale
5.	Alliaria	Alliaria officinalis
6.	Thlaspi	Thlaspi arvense
7.	Lepidium	Lepidium latifolium

It will be seen at a glance that no taxonomic account is made of the pods in drawing up the line of those crucifers. Siliculose and siliquose genera are completely intermixed. It is just what Sachs, from his mistaken point of view, calls a "motley order." Yet there is a system in the arrangement of these seven crucifers, and it is plainly apparent. Draw a line between the numbers 4 and 5 and all above the line are compound-leaved, all below it simple-leaved. It is a natural arrangement, according to Tragus' notions—certainly very crude, sometimes—about the tests of affinity. From this point, however, the succession of members of this family is broken by the intrusion of eight species belonging to three other natural groups. The intruders are, two persicarias, five buttercups, and a rather peculiar plantain. Only at the end of such a line as this does he again take up the crucifers.

If in this procedure the German father seem chargeable with having abandoned the principle laid down in his Preface, I see no clear defense for him. If he had been intent upon collocating things by the criterion of "form," as he calls it, he must needs have kept the line of the crucifers uninterrupted by plants polygonaceous and ranunculaceous. He was botanist enough easily to have seen the marked contrasts of form between these plants and those crucifers that flank them in considerable file on either hand. Being such, these intrusions were not made by chance or whim. Apparently it is a case of the subordinating of taxonomic principle to the convenience of a multitude of those to whom morphologic marks would go for naught, and with whom a grouping according to useful qualities would be welcome. Let us understand how, with such a purpose before him, Tragus would not unnaturally break the succession of crucifers and interpose a line of buttercups.

Where the succession of square-stemmed opposite-leaved aromatic herbs ends in a fragrant teucrium, and the terete-stemmed alternate-leaved series begins with the garden peppergrass, I have characterized the transition as abrupt; but this was done with a mental reservation. Morphologically it is abrupt, extremely so. Qualitatively considered, however, the entire series, from the first labiate to the last crucifer has a common character; and Tragus realized this character and allowed it to influence his classifying. The whole line, or group, is one made up of herbs either aromatic or pungent, or both. Let us accentuate this fact by denominating the mint-lavender division of the series the spicy plants, and the peppergrass-buttercup end of it the peppery plants. Now if one should catalogue and enumerate all the aromatic-scented and all the pungent-tasting herbs that are, the aggregate of them all would be but a fraction of the whole number of herbaceous plants. Such constitute, I suppose, not much more than a tenth of all the herbs known to Tragus. But his thought is that such a qualitative thread as this, pervading many species, may be used to line them up by, even so as to include within the line here and there a few which in the particulars of their morphology are not at agreement with the others.

Now this first series of seven crucifers is made up of species the tender stems and leaves of which were eaten raw as salads, or else the crushed seeds were used as condiments. They are particularly pungent, or peppery crucifers. Even the name nasturtium, which half the species bore, the etymologists derive from that irritation of the nasal passages experienced during mastication of these things. It must here be stated that, after the interpolation of the buttercups, the resumption of the line of crucifers is made at the genus of the mustards, plants the ground seeds of which had been employed in medicine as counter-irritants from time immemorial. And there will be readers to whom the information will be new that the seeds of the buttercups are as pungent as the seeds of mustard and were long used for the same purpose of raising blisters on the skin. Yet this acridly pungent quality of them is expressed in the very names by which the commonest species are known in botany, that is to say *Ranunculus sceleratus* and *Ranunculus acris*. And Tragus was so familiar with all this, as to have been constrained to locate the Ranunculus species in the midst of the counter-irritants; for his whole volume was indited to a great extent in the interest of those who practiced, even rudely and primitively, the healing art. These would expect to find remedial equivalents treated of in contiguous chapters, and he was willing to meet their expectations.

The resumption of the line of the crucifers has brought us to the limit of the one hundred sample pages which we were to examine somewhat thoroughly in quest of the man's mind and method. And now, perhaps in no way may one more easily arrive at a still fuller comprehension of his method than by following him in his further treatment of the cruciferous plants. The list of seven species which, set in line, precede the buttercups, together with the line of five species that succeed the buttercups, amount to about half the number of this family which he figures and describes. The other twelve or fifteen are discussed in two groups, each widely removed from the present series and also from one another. This is because the author must conform to the ancient usage of treating ornamental plants all by themselves in one place, and the edible plants of the kitchen garden also apart from all others. Under these divisions, however, we find the crucifers in each well kept together. The ornamental kinds, to give them by later names, are *Cheiranthus*, two species of *Matthiola*, and *Hesperis matronalis*.[1] The last is a little separated from the line; two other plants, both remote from the crucifers, but popularly called violets, being intruded. I entertain no doubt about Tragus' having perceived the real consanguinity subsisting between these wallflower-gilliflower ornamental plants—all known as violets—and the other crucifers; for in describing the wallflower he remarks that it belongs to the four-leaved—that is, the four-petalled—group of the violets, rather than to the five-leaved sort. Then again under *Hesperis* he describes the seeds of it as being enclosed in elongated and terete siliques like those of the cabbage.

For a glance at his final series of crucifers we must pass to the Second Book, where the topic is that of culinary herbs and roots in general. The series begins with cabbage, which is at once followed by the kales, plants the herbage of which and not the roots is the useful part. At the opposite end of the line occur in order the turnips, the radish, and lastly horseradish[2]; all these being so called root crops, the sum of the members of the family assembled at this point being seven; the line being divided according to nature of the roots as fibrous or fleshy. More than that, the cruciferous series is here again slightly interrupted; for just after the cabbage-kale series, and before that of the real turnip-radish series, two campanulaceous plants are intruded; both of them with fleshy roots so turnip-like in form, and in such frequent use in cookery as substitutes for turnips, that people call them wild turnips, or little turnips, so that this vernacular name became turned into Latin as *Rapunculus*,[3] the earliest Latin name for the genus now long

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[1] *Stirp. Comm.*, pp. 560–567.

[2] *Ibid.*, pp. 716–735.

[3] The Latin for turnip being Rapum.

known as *Campanula*. There are three crucifers described and figured, each of which stands isolated from all the others. One is *Bursa pastoris*.[1] Its having been located so far from its allies of the peppergrass kind was but accidental. At the time of the printing of those, the shepherd's purse does not appear to have been well known to the author, and his first expression concerning it is that its rightful place is next to what he has called *Thlaspidium*,[2] *i.e. Lepidium ruderale*.

Another of the isolated members is *Camelina*.[3] In certain districts it is common, he says, in fields, especially among flax, to which he likens the plant, except as to its having small yellowish flowers. He reports that its seeds ground with grain impart a certain sweetness to bread; also that the oil expressed from this seed is, in his opinion, of a flavor superior to that of olive oil. From many a passage in Tragus it is evident he was accustomed to identify plants as of the cress and mustard alliance by a pungent flavor of the seeds. Possibly in *Camelina* they lack this quality. Possibly also our author, variously misled, never tested them in this regard, or thought of such an experiment; for he nowhere intimates that the plant is of that alliance.

The genus *Isatis*[4] is a second member of the crucifers whose relationship Tragus shows no sign of having recognized. Possibly he did not know the plant but by hearsay. His draftsman copied Fuchsius' plate of it, and in so doing made the mistake of representing most of the flowers as either five-petalled or six-petalled. It is also to be noted that the fruits of *Isatis* at first sight are sadly bewildering; pendulous like the samaras of the ash tree, which they also much resemble. In the next generation after Tragus, and by one of the most illustrious of all botanical systematists, *Isatis* was indeed referred to the crucifers; though even a century after that its right to a place there was disputed. Tragus will, then, be excused for not having guessed this thing to be a cress-mustard ally; nevertheless in describing the fruit he proved that a German father of the sixteenth century in his going "straight to nature," might well have taken with him the old Greek father Dioscorides more often than he did; for Tragus, having described the pendulous pouches or bags that succeed the flowers avers, that "this pouch is the seed of the plant." Dioscorides in describing the

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[1] *Stirp. Comm.*, p. 214.

[2] *Ibid.*, p. 82.

[3] *Ibid.*, p. 655.

[4] *Ibid.*, p. 255.

fruit of *Isatis* had said that "within the pouch there is a seed";[1] and here Tragus' powerful rival, Fuchsius, by plagiarizing Dioscorides' whole account of *Isatis* had been right about its fruit and its one seed.[2] It seems not improbable that a man as keen of botanical vision as Tragus, had been relying on other people's statements when he wrote that pouch and seed are here one and the same.

With the exception of these two, *Camelina* and *Isatis*, Tragus' comprehension of the group of crucifers appears to have been complete; and this will become still more manifest by an item of his taxonomic procedure still to be adduced. Fuchsius, as was related in the preceding chapter, guided by superficial resemblances in purely vegetative characters, and wholly inattentive to their small flowers and fruits, had received a hedge mustard and a small flowered vervain as members of one genus which he called *Verbena*. To Tragus this misplaced plant is so plainly of the mustard alliance that he becomes impatient of his rival's blunder. "This thing is about as much like a verbena as a nettle is like a rosemary bush. I could wish that none should be displeased with me for saying this; but I am aware there are some who will take it much to heart that I have transferred their *Verbena fœmina* to the category of the mustards, and judge me rashly for having done so. But it was reason that compelled me to this course, when I perceived the plant by its whole substance and flavor to be at accord with *Sinapis*."[3] Though he names the texture and flavor of the herbage as the reason, that is because it is the one which will appeal to most people, those into whose minds anthological considerations do not enter. It is none the less presumable that the diminutively mustard-like flowers, along with the pods and seeds so concordant therewith, first led Tragus to investigate the qualities of the plant.

Some twenty-six species of crucifers all told are described by Tragus. That is probably three times as many as may be found in any author earlier than he, except Fuchsius, who allows them to be scattered about according to the alphabetic order of their Greek names, and nowhere gives expression to a thought about their affinities. And Tragus, inasmuch as he plainly discloses his recognition of their consanguinity, except in the case of two species both of them anomalous, ought to be accredited as the first discoverer, so to speak, of this important and taxonomically interesting

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[1] *Diosc.*, Book ii, ch. 180.

[2] Fuchs, *Hist. Stirp.*, p. 330.

[3] *Stirp. Comm.*, p. 104.

family. He did not, indeed, name the family. He was not, like the philosophic Theophrastus of old, given to using significant names for such groups of genera. But let us not here perpetrate a fallacy too common with historians, of attributing the discovery of a thing to the man who did but name it, after it had been discovered by another.

There are to be noted in Tragus not a few other instances of decided movement in the direction of a better grouping of genera; but only two or three more may here be allowed even a passing mention.

In our study of Fuchsius we had observed that, despite the alphabetic artificiality of his arrangement, by dint of stretching to the uttermost the application of the Greek generic name *Strychnos* (=*Solanum*), he had brought almost all solanaceous plants into one line[1]; but that while a thing as anomalous as *Datura* had thus gained admission to the company of its cognates, *Capsicum* had not been at all apprehended by him as a member of that group. He does not appear to have seen in it any likeness thereto. Tragus, while also doubtless like Fuchsius finding the peppery properties of all parts of the plant too foreign to those of other solanaceous genera, nevertheless observes that as to the form of its leaves, and especially of its flowers, it recalls *Solanum*.[2] This was giving that serviceable hint by which later taxonomists were to be led to place *Capsicum* within the lines of the *Solanaceæ*.

The borrages are a group all the then known members of which are first brought together in unbroken line by Tragus[3]; and he has seven genera of them, embracing something like twice that number of species. All much alike in habit and inflorescence, but differing one genus from another very notably as to nature of the pubescence, and still more so as to form of the "flower," they again come to almost one and the same thing as to the calyx and its . . . three or four naked seed-like nutlets; and all these peculiarities of the flowers, together with the aspect and character of the fructification, Tragus is the first botanist to describe; and he describes them for each genus. In all except the naming of it he is the founder of the family of the *Borraginaceæ*.

While there is evidence enough that this man's perceptions of plant affinity were keener than those with which any earlier author had been endowed, yet there was never with him any such thought

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[1] Page 209 preceding.

[2] *Stirp. Comm.*, p. 928.

[3] *Ibid.*, pp. 229–241.

as that natural groups of genera are very numerous; still farther from his mind was such an idea as that all genera may be reduced to a line of natural families. The time was not ripe for the engendering of that thought; nor was it to find expression until three generations later. The list of genera which Tragus can not systematize, as he had done those of the labiates, the crucifers, and the borrages, is a long one. But he must needs bring them all into some kind of grouping, or succession; and the principles upon which he collocates monotypic genera are various. We must take note of several of them.

There is one quite extended series made up of the following: *Convolvulus*, two species, *Nummularia*, *Cuscuta*, *Humulus*, *Smilax*, *Dulcamara*, *Clematis*, *Bryonia* and *Lonicera*.[1] There is one character, at least a negative one, by whch all these are connectible. Not one of them has an upright stem. All are in some manner climbing; and twining and prehensile plants are much more exceptional, at least in cool-temperate latitudes where Tragus botanized, than one would suppose. Outside of this series now in hand not many were known to him; for the series does not end with *Lonicera*. To that there immediately succeed all the genera of cucurbits that are rough-leaved and are grown in gardens.[2] The smooth-leaved and unvigorous or delicate genera *Bryonia* and *Momordica* had not yet gained recognition as members of the *Cucurbitaceæ*. And if neither the twining leguminous plants nor the tendril-bearing are placed in this succession, it is for the reason that in their case, and ages befor Tagus, the principle of stem-posture had been subordinated to the higher one of their agreement with the upright and bushy kinds in a peculiar and distinctive morphology of flower and fruit. Tragus was perfectly aware of all this; and can not have had so much as a thought of including in this present line the weak-stemmed and prehensile peas and beans.

Sometimes one finds him placing two generic types in juxtaposition for no botanical reason, but only for what may be called a literary motive. The vine, the fig tree, the palm, the olive, and the bay are types not genetically interrelated[3]; neither to Tragus' knowledge was any one of the five related to any other tree. But in ancient history and poetry all had often been associated. They form a group, and that most historic and distinguished, but on the basis not of botany but of literature. And yet, as regards the after

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[1] *Stirp. Comm.*, pp. 804–823.

[2] *Ibid.*, pp. 824–835.

[3] *Ibid.*, pp. 1049–1056.

part of this line, comprising palm, olive, and bay, one would not dare either affirm or deny that these three had been botanically connected in the author's mind by their drupaceous fruits.

Repeatedly does he bring together two types in every way dissimilar for the sole reason that their names are practically the same; this of course in condescension to those who, in looking for a given plant under a certain name may find that and its homonyms all in one place. Thus at the end of the buttercups does he locate the herb called *Coronopus* (= *Plantago Coronopus*). It would be irrational to require of Tragus that he should have referred this to the genus *Plantago*. The floral structure which connects them could never have been seen until after the invention of hand lenses at least. The leaves of this *Coronopus* are cut into narrow and remote pinnated segments and beset with bristly hairs; on the whole as far from plantain leaves in form as imaginable. The form of these leaves had procured for the plant the name coronopus, Greek for crowfoot, and that very anciently. *Ranunculus* had also for a second Latin name *Pescorvi*, the exact equivalent of the Greek coronopus and English crowfoot. Because they had the same name our author placed side by side these different plants. He did the like with the labiate that was usually called *Hedera terrestris*, that is, ground ivy, locating it next the true ivy, *Hedera Helix*, as well aware as any one ever was that there is no consanguinity between them; but this disposal of it would suit the convenience of those untaught in better classification.

Nomenclature. No special attention is given to nomenclature by this author. He follows the usages of antiquity and of his own period, yet in ways of his own by which it comes to pass that he illustrates those usages uncommonly well. We have already observed that such family names as Umbelliferæ, Cichoriaceæ, Carduaceæ, Legumina for the leguminous plants, and Malvæ, for the malvaceous had been in familiar use time out of mind. Tragus essays the addition of a few new terms of that kind to comprehend other groups of genera; but these have not been successful; and the cause of their failure will readily be seen. He proposed the name Serpentariæ[1] for that group of trailing, twining, and climbing herbs referred to above as embracing *Convolvulus*, *Humulus*, *Clematis*, and others; both the outlining and the naming of it being made, curiously enough, just at a time when taxonomists—even Tragus himself foremost among them—began to depend less upon the texture

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[1] *Stirp. Comm.*, p. 798.

and posture of stems, and more upon characteristics of flowers and fruits. And his small group of the Lappæ[1] was equally futile; for while this series might at first view seem to be connected upon a thread of fruit characters, a more attentive inspection brings it out that nothing more significant than the hooked character of the prickles investing the fruits holds the genera together; for they are *Lappa*, *Xanthium*, *Trapa*, *Caucalis*, and *Agrimonia*. Thus Tragus' two new family names, *Serpentariæ* and *Lappæ*, were both destined to suppression, because the grouping in either case was little better than fanciful; being based on agreement as to certain peculiarities that are of no general taxonomic value.

Upon the then settled principles of generic nomenclature—principles approved by all antiquity—Tragus attempts no inroads. It does not enter his thought to question the perfection of the established methods in naming things. A generic name of two words, noun and adjective, suits him as well as one of a single term and that substantive; perhaps even better, as signifying somewhat more; for there is more of meaning conveyed by a noun qualified by an adjective than there is in a noun standing alone; and the time is yet distant when meaningless and cabalistic names will be tolerated. So when he becomes the discoverer of a new and nameless generic type that is an ally of *Cyanus*, the common cornflower, or bluebottle, though not of its genus exactly, he assigns the new genus the compound name *Cyanus silvestris*[2]; and we, well aware that half the generic names in sixteenth-century botany are thus made, must read his whole account of the plant in order to assure ourselves that he does not, after all, mean simply a new species of the genus *Cyanus*.

Fuchsius, convinced that the genus *Plantago aquatica* is identifiable as the *Alisma* of the Greeks, had taken up the latter name[3]; but Tragus shows a preference for the two-worded appellation and restores it; taking pains also to inform the untaught that, although the plant's name is *Plantago aquatica*, it does not belong to the genus *Plantago*.[4]

Even for Fuchsius' new genus aptly named *Digitalis*[5] Tragus thinks that such a two-worded name as *Campanula silvestris* would be better; and he formally proposes this as a substitute, writing

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[1] *Stirp. Comm.*, pp. 836–844.

[2] *Ibid.*, pp. 218, 219.

[3] Fuchs, *Hist. Stirp.*, p. 43.

[4] *Stirp. Comm.*, p. 227.

[5] Fuchs, *Hist. Stirp.*, p. 892.

Digitalis down as a synonym. His remarks at this point illustrate well the idea then prevailing, that the nomenclature of newly proposed genera should be freely open to amendment and improvement. "Let any one name this plant what he will. We, in consideration of the form of the flower, shall name it *Campanula silvestris*, at least for the time being, and until a still more appropriate name shall arise. There are those who call it *Digitalis*."[1]

While Tragus, like others both before him and long after, leaves the representatives of monotypic genera without specific names, yet up and down the margins of a great majority of his 1100 pages are the binary names of species. If many of these seem to consist of three terms, it is usually because two of them constitute the generic name. Occasionally the third word indicates that what is in hand is a mere variety of the species preceding; and now and then it will be seen that a fourth word is introduced to indicate the variety. In case the generic name itself is binary, the fourth term becomes needful for the indicating of a named variety. Still there is no trace in this author of those phrase names that became a burden upon the botany of the seventeenth and eighteenth centuries. The marginal placing of these binary names is as universal with Tragus as one finds it in Linnæus; but these same names are often repeated as headings to the chapters, and again over the plates. The author employs less freely the numeral adjectives for specific names, and has not many that are geographic. The personal names for species are less rare; though most of these are of earlier mediæval origin, commemorating saints of the Roman calendar; and he is perhaps the first of botanists to have dedicated a new species to himself.[2]

Ecology, Phenology. Tragus is far from emulating in any general way the endeavors of Theophrastus to indicate groups of plants ecologically considered; but there is one piece of such work that ought not to be allowed to pass unnoticed. The Third Book of his volume is to be devoted to the trees and to other lesser but strictly woody growths. Accordingly in the first chapter at its beginning there is introduced the figure of a large tree, a spruce tree, as we are able to determine from a branch or two of small dimensions which are all that remain alive; for the tree is moribund. All up and down its trunk there are fungi and lichens of several kinds; then upon the ground beneath are as many more. The text of the whole chapter, and it is a long one, covering seven pages, relates

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[1] *Stirp. Comm.*, p. 889.

[2] *Quinquefolium Tragi*, Tragus, *Stirp. Comm.*, p. 587.

exclusively to these plants, and a considerable number of them of various genera, embracing an aggregate of some thirty species. The next chapter treats of mosses; the third, of mistletoe. Thus the whole assemblage of German saprophytes and tree parasites is made one ecologic group, as of things growing together, many of them upon trees, most of the others upon the ground beneath trees.

Ecology, however, forms an item and a very distinct one in the account of almost every wild plant which he describes; a fact that will be best impressed by a few citations.

"Asarum affects shady places where the soil is rather moist, and is usually found under thickets of hazel, but sometimes also in deep damp woods."[1]

"Alliaria is an elegant plant which in the month of April is found in certain waste places, under walls, along the bases of hills, by hedges and in cavernous places which are the abode of lizards and other vermin."[2] The chickweed is located thus: "This most common of herbs is found throughout the whole year in gardens and vineyards; and the richer the soil, the more large and tender the herbage."[3]

Ranunculus sceleratus "grows in low swamps, especially if the soil be sandy, and preferably where there are frogs; but occasionally in very rainy years it will be found in wet lands that are more elevated."[4]

"Fumaria grows in gardens, fields of rye, and also among flax, onions, and cabbages, where it flowers in May, and again in autumn it reappears in turnip fields."[5]

The almost omnipresent knotgrass, *Polygonum aviculare*, he thus descants upon: "Polygonum among common plants is the very commonest of all, at the same time a useful one also. What part of the country is there where one does not meet with it? What roadside is there where it does not abound? What fields (for in cultivated fields it particularly delights), what hedgerows, and what by paths are not covered with it?"[6]

"Aquilegia, mostly a garden plant with us, also grows wild in elevated woodlands, on rocky hills, and sometimes in the crevices of precipitous rocks."[7]

[1] *Stirp. Comm.*, p. 65.

[2] *Ibid.*, p. 85.

[3] *Ibid.*, p. 384.

[4] *Ibid.*, p. 93, under the name *Apium aquaticum*.

[5] *Ibid.*, p. 111.

[6] *Ibid.*, p. 390.

[7] *Ibid.*, p. 136.

In phrases like these does Tragus almost always particularize about the habitat, the soil, the exposure of the wild plants of Germany which he describes; and it would not be difficult to gather out of these antiquated and yet living pages definite outlines of the plant associations of every part of the country with which he was familiar.

The fungi, lichens, and mosses, already alluded to under this heading, are not the only plants in connection with which he permits ecological considerations to influence his taxonomy. He collocates in an unbroken series, as plants nearly allied, broad-leaved houseleeks, and sedum species the leaves of which are small and terete. Such a series is of course a faultless one in the judgment of modern botany, because the structure of flower and fruit is the same in all; but the case was otherwise four hundred years ago, when anthology was hardly yet in embryo, and even leaves were more generally received as furnishing the criteria of affinity. Tragus had to defend the position he had taken when placing certain small plants regarded as leafless in the same line with live-forever and houseleek as their next of kin. He himself could not claim that *Sedum acre* and its cognates had leaves at all. They exhibited, he said, in what seemed to be the places for leaves, grain-like things which he preferred to call acini; and an acinus may be a seed, a grain, a germ, or even a berry. He has but one argument to offer in defense of this line of broad-leaved things and things green though leafless, as being a natural series, and that argument is purely ecological. All of them inhabit peculiarly the roofs of buildings, and thrive there much better than elsewhere. Even such of the species as now and then establish themselves on the ground are never seen but in the most open exposures. All of them everywhere avoid all protection from extremes of temperature, retaining their fresh verdure unimpaired under the rigors of the severest winter.[1] Such ecologic groupings are of course traditional, having come down from earlier times; and under such defense as Tragus makes of this one, his contemporaries would perhaps admit its validity despite the great diversity among the members of it as to foliage.

Another instance of this kind of procedure to which I wish to call attention is the reverse of the above as to the result attained. German species of the rather ample genus *Veronica* are placed in widely sundered groups on principles as purely ecologic. In one

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[1] *Stirp. Comm.*, pp. 373–380.

place, under the generic name *Sium non odoratum*, he describes what is now *Veronica Beccabunga*, appending to his excellent diagnosis the following: "It grows around springs which never freeze, or in such ditches as are equally immune from frost during the whole winter."[1] Then the habitat of a second species of the genus——*Veronica Anagallis aquatica* is its Linnæan name—is given in similar terms: "Throughout the whole winter season this keeps its verdure quite untouched by frost, growing as it does in the water of warm springs." Now six chapters away from this which treats of the two aquatic veronicas, and with more than as many plants not allied to *Veronica* intervening, he describes the dry land members of this same genus, but under the generic names *Chamædrys* and *Teucrium*.[2] These have retained in more recent botany those generic names as specific under *Veronica*.

Thus do we find that our familiar genus *Veronica* was all unrecognized as a whole by Tragus, its members being ranged in two rather widely separated groups, bearing different generic names; and all this in deference to mere ecology, as it were; for, if those of the aquatic group have a tender subsucculent and glabrous herbage holding its freshness all winter, whereas those of the dry land are thin-leaved, soft-hairy, and die down to the ground in autumn, and if these differences may have helped to keep the groups apart, yet are they anatomical differences rather than morphological. And the case can not fail to convince us of the weight which ecological considerations carried in sixteenth-century classifyings. Neither Tragus, however, nor any of his contemporaries had invented these ecologic distinctions. They were already an old, old story. Contemplate the mere name for those aquatic speedwells, *Sium non odoratum*. It is a generic name, because there are two very clearly distinct species of it. There is somewhat of early botanical history concentrated in that very name. It implies beyond mistake the existence of a genus named *Sium odoratum*. Still further it suggests as almost certain that the name *Sium odoratum* is less ancient than the other. Searching old records now, we shall find that things happened exactly after the manner which the name *Sium non odoratum* seemed to indicate. *Sium odoratum*, the original of all siums, was at first *Sium*, simply, that is, a monotypic genus. More than a thousand years before Tragus, and maybe two thousand or three, the Greeks had known, and had used medicinally, an aquatic of springs and spring runs

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[1] *Stirp. Comm.*, p. 187.

[2] *Ibid.*, pp. 203–209.

that never freeze. It had the foliage and the pleasantly odorous quality of certain umbellifers, as Dioscorides had intimated[1]; and this was *Sium*[2]; and centuries later when up in middle Europe men versed in the materia medica looked in wild springy places for the *Sium* of Dioscorides, and more often found there those different things, unknown to the ancients, which though green in midwinter were odorless, and therefore not the real thing, and named them *Sium non odoratum*, they were proceeding upon the principle that both, and all such plants, being generated as most of them believed, by spring water and the earth at the bottom, were naturally allied, and might all be named so many kinds of *Sium*. Furthermore, the establishment of such a name as *Sium non odoratum* rendered it needful that original *Sium* should be invested with a cognomen in order to avoid misunderstanding and confusion. Hence its later generic name *Sium odoratum*.

That this sium, constant inhabitant of springs and warm drainage ditches, is classed not ecologically but morphologically by Tragus argues no inconsistency. It would be one thing for a sixteenth-century botanist to fail to recognize by morphologic marks the membership of the *Scrophulariaceæ*, and quite another thing to miss the family characters of any umbellifer. Sium at first glance, as well as by its properties, is unmistakably an umbellifer; and the time is not to be found in the annals of botany when this family had not obtained general recognition, marked as it is both morphologically and qualitatively. The family, so-called, to which the veronicas belong is not so. The *Scrophulariaceæ* have never yet been circumscribed otherwise than most arbitrarily and unsatisfactorily. Tragus understood well the superiority of morphologic over ecologic criteria, and that the latter are to cede to the former when the former are manifest. The anthologic harmony between hardy undying water veronicas and the tender perishable kinds of dry meadows and uplands it was not given him to see; nor, indeed, to any one until long after Tragus' day. And yet, an umbellifer to him was an umbellifer whether hydrophilous or xerophilous. But in the arranging of his umbelliferous genera it will be observed that the two aquatic genera *Sium* and *Apium* are placed side by side.[3]

To the botanist of the fields, the plains, the marshes and the

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[1] Diosc., Book ii, ch. 120.

[2] The plant is *Sium angustifolium* Linn., type of the genus, though now called *Berula angustifolia*.

[3] *Stirp. Comm.*, pp. 464, 465.

mountains it is easy to recall the special habitat of almost any plant, but not so the average time of its flowering year by year. To be able to say that this shrub will be found in bloom about the middle of May, that tree in the early part of May, this flower appears late in March, the other in the last days of April, is not likely to be a matter of unaided memory. To know the average time of flowering for everything that grows involves the keeping of written notes through years. Tragus tells this time of the annual flowering of things in almost every chapter of his book; and he is the first of botanical authors to have done this.

Transmutation. Though much given to diversifying his botanical pages by bits of invective against superstitions that are of theologic type, Tragus has never doubted the easy transmutability of wheat and rye into chess. In a long chapter he demonstrates to his readers how this may and does come to pass, under various conditions. And here some experimentations of his own are recorded: "That it is possible for seeds of one species to degenerate and become so changed as to come up as another species is something which I have learned by experience; for from very old cabbage seed sown by my own hands I have raised a crop of turnips."[1] At another place he has the following upon the same subject: "There are those who think that a sowing of turnip seed upon very dry and sandy ground, especially if the seed be very old, will come up as wild mustard; or at least in that which is as much of the nature of mustard as of that of turnip. In the same fashion cabbage seed very commonly changes into that of a poor and stunted kind of turnip, as I myself have often proven by experiment."[2]

Again in his dissertation upon wheat he reports a certain dark-grained kind as apt to appear intermixed with the other in the low moist parts of the fields; so dark—blackish is his word—as to render flour and bread from such admixture dark-colored. He in perfect confidence accepts this admixture of dark-grained as another instance of transmutation; has never a suspicion that it is another variety of wheat, the seed of which was mixed by chance, in the sowing, with the other.

The case is one entirely apart from that of the melampyrum, or black wheat, of a totally different plant alliance, the seed of which, accidentally harvested with the grain and ground with it also

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[1] *Stirp. Comm.*, p. 668.

[2] *Ibid.*, pp. 101, 102.

caused a darkening of the bread; for on this weed he has a chapter apart,[1] as well as another upon smut[2] in grains.

From our view point of four centuries later it may not be easy to understand how such childish fancies could hold their places firmly in gifted minds which, after all, were seriously bent on cold scientific enquiry into all nature's mysteries and were often successful; but of the fact itself there is abundant proof.

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[1] *Stirp. Comm.*, p. 662.

[2] *Ibid.*, p. 666.

CHAPTER VIII

EURICIUS CORDUS, 1486–1535

One of the most gifted and scholarly men among all who figured in German botany in the early sixteenth century is Euicius rCordus who, though a cultivator of plants, and also a zealous field botanist, wrote no great book, and is chiefly interesting here as having been the father and the educator of that most brilliant of early German botanists, Valerius Cordus.

Life. Henricus—the name was altered by himself in his school days to Euricius—was the thirteenth child of a pair of honest and worthy Hessian peasants, and was born at Siemershausen in the year 1486. His parents died when he was a child, and in some way he became for a time the inmate of a collegiate school at Frankenberg. Here he formed a strong and lasting attachment to a youth, his junior by two years, who afterwards under the adopted name of Helius Eobanus Hessus became celebrated as a philologist and as one of the most elegant Latin writers of the period. On account of a treatise upon dietetics favoring vegetable foods,[1] which in its day was well received and passed through several editions, Haller has enrolled the name of this Eobanus Hessus in his list of botanical authors. What influence he had upon botany was more indirect. It was evidently by virtue of this strong attachment between Hessus and Cordus that the latter was brought finally to devote himself to intellectual pursuits.

After those first school days at Frankenberg, and while Cordus was very young, he married and was settled at his native Siemershausen; in what occupation no records tell; but when in the year 1515 a son was born to him, the event appears to have stimulated him to renewed endeavors to attain distinction in scholarship; for before the son was two years old Euricius Cordus had won the Master's degree at the university of Erfurth; his special studies

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[1] "Præcepta bonæ valetudinis conservandæ" is the title of this treatise according to Haller, *Bibliotheca Botanica*, vol. i, p. 260.

having been the ancient languages and philology, the same which his particular friend Eob nus Hessus had been all the while pursuing. The year following, *i.e.* 1517, we find him a student at Leipzig, where also he gives lectures on pastoral poems in Latin of which he is himself the author. Here also he makes a lasting friendship with the young Joachim Camerarius—father of the botanist of that same name—a much younger man than Cordus, and at the time a student at Leipzig, and who subsequently became distinguished in philology. Camerarius soon removed to Erfurth, and Cordus returned thither with him. Eobanus Hessus was still there, and the three determined to open there a select school of their own. That Cordus' lectures and poems had already earned for him a reputation is evinced by this, that his opening a seminary of learning brought him a letter of congratulation and good counsel from so great a celebrity as Desiderius Erasmus.

The time at which this new school enterprise was undertaken proved inopportune; a time when, in Germany, even the oldest and most renowned seats of learning were realizing the influences of that ecclesiastical and civil upheaval commonly called the Reformation; and Cordus and his companions closed their school in the year 1521. And now, as if in hope of thereby gaining a better, or at least a surer, living for himself and his family, he entered the medical profession. Without the means of journeying to Italy and maintaining a year's residence at the most celebrated school of medicine in Europe, that of Ferrara, a physician at Erfurth, one Doctor Sturtius,[1] offered him financial aid; and at Ferrara, in 1522, Cordus received the Doctorate in Medicine at the hands of the venerable Leonicenus then 94 years of age and still active in the discharge of his professorial duties.

Returning now to Erfurth, Cordus practiced medicine during four or five years, and then in 1527 accepted an appointment to the chair of medicine in the newly founded Protestant university at Marburg; from which movement the historians infer, and not unreasonably, that Cordus had abandoned the Catholic faith and become a Lutheran In this new position he found leisure for study and authorship, for he translated into Latin verse both the *Alexipharmaca* and the *Theriaca* of Nicander. both published by Egenolph of Frankfort, in 1532. Here also his own Latin pastoral poems were published, but without date, or name of publisher. Here also he wrote and gave to the public his one botanical work, the

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[1] Winckler, *Geschichte der Botanik*, p. 76.

Botanologicon, which closed his career as an author. This was published at Cologne in 1534. In this he complains of oppositions and persecutions which he has had to endure at Marburg, such as had compelled him to accept a proffered appointment as City Physician at Bremen, to the Senate and Citizens of which city the book is dedicated. He died at Bremen in 1535, at the age of 49 years.

The Botanologicon.[1] The book is in the form of a colloquy between Cordus and a few friends of his, most of them away back in their younger years fellow students at Erfurth, all now men of middle age, physicians, pharmacists, or men otherwise interested in plants, at least the medicinal. As having been university students of medicine every one of them is assumed to be somewhat familiar with all the ancient line of Greek and Roman authors who had written on the materia medica, and whose books were still the standards of study and reference.

Euricius Cordus, even while young, and as yet aiming at nothing else but distinction in languages and philology, had been a great lover and cultivator of plants, training his child Valerius from infancy to know and love them. Then when through mad religious partizanship the universities of Germany began to suffer disruption and depletion,[2] and Cordus with a family on his hands was obliged to prepare for a remunerative calling, he was trebly prepared to make a mark in botany. He was a genius. He was intensely a lover of plants. He was uncommonly well skilled in those ancient languages in which the old standards of the materia medica had been written.

The useful purposes which the *Botanologicon* has in view are several, and are essentially reformatory. Prominent among them is that of demonstrating that, through sheer ignorance, a considerable proportion of the jars and drawers and packets in the drug shops are falsely labelled. They are marked with the names of Diocoridean and Galenian roots and herbs, while commonly filled with things which can not be the same as those which the ancients knew and made much of under those names. If this was really the case it would follow that the lives of those in illness calling for a certain powerful remedy, were apt to be endangered by the administration, either of some drug wholly inert, or else with pro-

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[1] Euricii Cordi Simesusii Medici Botanologicon. Coloniæ, apud Joannem Gymnicum, Anno 1534.

[2] *The Botanologicon* abounds in expressions deploring the adverse influence of the religious dissensions of the time upon the universities of Germany.

perties aggravating rather than remedial to that ailment. It was a strong arraignment of the whole united company of the doctors and pharmacists of the time; and Euricius Cordus complains bitterly of the oppositions and persecutions that had followed him and driven him from place to place, while lecturing and writing in endeavors to correct this ignorance. He was not philosopher enough to comprehend that just this abuse and this feigned contempt are the very highest encomiums: the only attestations of his learning and genius which the envious horde of the criticized and the offended know how to pay.

In this colloquy, the *Botanologicon*, there are given many particular instances of errors on the part of the physicians and druggists as to plants. We have already noted, in our study of Brunfelsius, how that author mistook German species of *Corydalis* for the classic *Aristolochia*, thus, at once agreeing with, and confirming in their ignorance, the whole array of the German doctors and druggists of his time.[1] Euricius Cordus takes up this case as one which easily establishes that for which he contends. One member of his party reads from Dioscorides that *Aristolochia* has leaves like the bush ivy, *i.e.* well rounded and entire.[2] This which the Germans call by that name has leaves dissected like those of rue. The leaves and even the flowers of ancient aristolochia were described as having an odor somewhat sharply aromatic; a quality of which there is no trace in these fumariaceous herbs. The root of these, it is confessed by all the party, are rounded and turnip-like, as Dioscorides and all the others of olden time had described those of *Aristolochia rotunda;* but that was the only point at which the *Corydalis* answered to the *Aristolochia* description. The fact was plain that people in comparing the plant with the ancient diagnosis of aristolochia, finding that the root agreed, became at once blind to all the points of disagreement. Among the many instances of this kind of error the author presents that of the druggists having mistaken the common wild plum of German woodland borders for the acacia of the ancients.[1] The acacia had been described as a thorny tree, yielding a mild gum. In these two points the wild plum was at agreement with the old acacia description. They gathered this native German gum and made the accustomed uses of it, believing all the while that in this thorny tree they had the real gum-bearing acacia. Cordus invites

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[1] See page 173 preceding.

[2] *Botanologicon*, p. 96.

[1] *Ibid.*, pp. 77, 78.

their attention to an important part of that classic diagnosis to which they all the while have been blind; that which says that acacia produces its seeds in a pod, after the manner of the lupine; and Cordus asks his party how this gum-bearing thorny tree that yields plums instead of legumes can possibly be believed to be the acacia? It would be easy to mutiply by twenty the examples of this kind which find mention and full demonstration in the *Botanologicon;* but the above must suffice. They are representative.

A most every one who has written a few chapters of botanical history has made record of the fact that at first the botanists of middle Europe wrongly expected to find, and as wrongly believed themselves to have found there most of the plants that had been described by ancient Greek and Arabian authors. The discovery of the error and the correction of it have usually been credited to botanists of later centuries. But Euricius Cordus is the man who at the very outset, and himself a German, saw the magnitude of this mistake, and so clearly exposed it, that despite the rage of an incensed multitude of doctors and apothecaries, the reform began at once. If Fuchsius in his great folio of pictures had in many an instance corrected Brunfelsian errors as to the identity of plants, it was largely if not altogether due to his having studied and been guided by the *Botanologicon.* He pays full tribute to the importance of this work in his Preface, which was not written until after the demise of Euricius Cordus. In this Fuchsius says: "In this work of restoring botany Brunfelsius was succeeded by Euricius Cordus, a man of high integrity, great industry, distinguished as a poet, and a man of varied learning. What he accomplished for the elevation and advancement of botany so abundantly appears in his *Botanologicon* as to need no further commendation from us. But this I wish to say; that one so singularly qualified seemed worthy of a longer life, in which to have contributed to medical botany much more matter of the same high import."

At the time when the *Botanologicon* appeared, the illustrated folios of Brunfelsius were quite new, and the work is often referred to. On one of the earlier pages of this botanical colloquy there is such a record of contemporary opinion about the merits of Brunfelsius' book as can not fail to be interesting, and for the sake of which I shall attempt a reproduction of the whole passage.

MEGOBACCHUS. Since Gallus desires it, may we not all go out and botanize a while?

RALLA. Please grant us this favor.

Cordus. I am willing, and the more so as hoping to learn something from all of you.

Niger. *You* learn from *us?*

Cordus. Why not? I have sometimes learned botany from illiterate women and peasants. I am not ashamed of gaining information from a child even, much less from you, men of learning. So whenever it pleases you let us go forth. I will not keep you back; nevertheless I, just as if none of you were here, shall follow my usual practice of taking along a little book or two. I take the greatest delight in these sallyings into the country, where I can have before me fresh and growing those herbs which I have read about at home, and may compare them with the pictures of others which I carry in memory; also taking such note of their names and reputed virtues, as I may gather such from old women whom I meet upon the way. By the use of all these means I am the better able to arrive at a sound conclusion, or at least a more probable opinion, about the identity of a thing.

Gallus. I wish that Brunfelsius had followed your course; for concerning a good part of his plants it will have to be said that he named them not according to their descriptions, but after the opinions of the ignorant vulgus.

Cordus. How do you know that?

Gallus. I have read his two volumes, and have compared them with Dioscorides.

Cordus. That was advisedly done; and if there are errors, we must overlook them, and stand by the things that are well and rightly said.

Gallus. What if, in the meantime, by virtue of that authority which a new and plausible work must carry with the unlearned, those old errors which ought to have been eradicated, are only made to strike root still more deeply? I do not think this a matter to keep silence about.

Cordus. Speak out, then; and if you wish to, cry aloud.

Gallus. This should be your province rather than mine.

Cordus. Cordus himself is too unlearned to undertake that piece of criticism. Let him address himself to the task who may choose to; I shall only indicate, with all candor and open-heartedness, some things which I do know and stand by; yet all the while ready at any moment, and without a blush, to be taught by any of you who may know better than I.

Ralla. What are we waiting for?

Cordus. Young man, lay aside that genteel cloak, and carry this little volume of Dioscorides.

NIGER. You advise well; this author alone suffices.

GALLUS. I have the two volumes of Brunfelsius.

CORDUS. If it please you, we will first enter this my little garden by the house.[1]

The conversation carried on between the five friends while on this botanical excursion constitutes the body of this rare booklet of 185 pages, and gives a clearer insight into the state of medical botany in middle Europe in the time of Brunfels, Fuchs, and Tragus, than could be gathered from the most exhaustive study of those author's folios themselves.

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[1] *Botanologicon*, pp. 26, 27.

CHAPTER IX

VALERIUS CORDUS, 1515–154

Hitherto Valerius Cordus remains almost unknown except by name. Not one of the four of his own countrymen who wrote botanical history within the nineteenth century ever looked into Cordus' writings—all of them published after his early death—far enough to see whether he had been least or greatest among German botanists of the sixteenth century.

Sprengel says that "Valerius Cordus, son of Euricius, if he had lived longer, might have given to his works a certain maturity which is conspicuously wanting to them";[1] after which anything but laudatory opinion he proceeds to give the young man full credit for having travelled widely in many parts of Germany, and for having discovered and published a goodly number of new plants.

Of a very different tenor is the language of Ernst Meyer: "A splendid and all too transitory phenomenon was Euricius Cordus' son Valerius."[2] While in my view this opinion of Valerius Cordus' merits is not extravagant, still Meyer, as it seems to me, fails to show reason for such high praise. He does much better than Sprengel; yet I am obliged to infer that he borrowed the opinions of sixteenth- and seventeenth-century authorities on Cordus, rather than as having made the young man's writings a subject of careful examination. The estimate of the best botanists of two generations after Cordus was an exalted one, as to his merits; and that outside of Germany.

Sachs in his volume of history seems to have found it easy to adopt Sprengel's tone of indifference to this youth of rare genius. "For the present," he says, "we pass by Valerius Cordus, Conrad Gesner, Matthioli, and several others of no importance,"[3] etc., etc.; and this is the only reference to him which I have been able to find

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[1] *Hist. Rei. Herb.*, vol. i, p. 346.

[2] *Geschichte der Botanik*, vol. iv, p 317.

[3] Sachs, *Geschichte*, p. 31.

save mention of the fact of his having observed the sensitivity of certain leaves.[1]

Such conflict of opinion on the part of nineteenth-century writers has seemed to make it incumbent on me to determine if possible, out of Cordus' writings themselves, why it was that for nearly two centuries before the nineteenth he was held in such esteem by accomplished botanists of every nationality in Europe.

Life. Valerius Cordus was born in the year 1515 at Siemershausen, where also his father Euricius Cordus had been born. There is a beauty and a certain pathos in the story of how the destined father while a boy had made fine progress on the road to higher learning, and then by an early marriage, evidently in poverty, seemed to have extinguished all hope of a scholar's career. Yet when this son was born, the young father's zeal for learning returned; for to the child there must be given every advantage of high education; and he himself would be the educator of his Valerius. In some way he managed, as we have already seen, to go to the university of Erfurth, where he very soon obtained his first academic degree. Thenceforward he supported his family by teaching and lecturing until other degrees had been gained, and he was settled in the profession of medicine; meanwhile training his boy carefully in the ancient languages, philosophy, and the sciences, among them botany in particular. We have a biographer's testimony to this. "Valerius Cordus was imbued with an incredible zeal for learning thoroughly not only medicine, but also the right recognition of plants, to which latter his father Euricius, a physician and also an illustrious poet, urged him by both precept and example; for he had reared the child even from the cradle in the midst of herbs and flowers."[2]

Cordus took his degree in medicine at the university of Wittemberg, and there shortly after gave regular lectures on Dioscorides, and with such marked acceptance that to the audience of medical students certain professors joined themselves.[3] It must have been earlier than this that he prepared his *Dispensatorium*, or manner of preparing all medicines, the only work of his that was printed in his lifetime, and which was a great success. He had not even intended it for publication, but appears to have written it as a pas, time while with his uncle, Joachim Ralla, an apothecary at Leipzig.

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[1] Sachs, *Geschichte*, p. 579.

[2] Walter Riffius in Preface to first edition of Cordus *Annotations on Dioscorides*, Frankfurt, 1549; also to Strasburg ed. of Cordus' works, 1561.

[3] Meyer, *Geschichte*, vol. iv, p. 317.

But the uncle thought so highly of the manuscript that he placed it before the magistracy of the city of Nuremberg, and they ordered it printed.[1] It was first published in 1535, was often reprinted during the next 150 years, and was even translated out of the original Latin into French.[2]

The *Annotations on Dioscorides*, being a kind of abstract of his Wittemberg lectures, were not published until five years after his death, and were never by Cordus himself prepared for the press; perhaps not even so much as once written down by him at any time; for the printer's copy, when it came to the printing, consisted of the notes of a student who had been his auditor, whose note book was found available.[3]

In this kind of work young Cordus is before us in but the ordinary rôle of the early sixteenth-century botanical scholar, a master of the ancient languages, delving deeply into the medico-botanical works of Greek and Roman antiquity, and laboring to correct, amend, and in some degree perhaps augment the ancient pharmacopeia. To have been able to accomplish so much in this direction, and that while yet hardly having attained to manhood, was in itself a proof of genius. To understand the exalted character of this genius it is only necessary to canvass what the youth had also attained to along other and different lines at the same time.

In field work in Germany, for botany alone—not to speak of geology and mineralogy, in both of which he was, for his time, an expert—he had wrought out more results that had his older contemporaries, Brunfelsius, Tragus, and Fuchsius combined. In his repeated journeyings to the great forests and wildest mountain districts, it is estimated that he discovered several hundred new plants.[4] Sprengel has given the Linnæan names of some twenty-five of these new discoveries of Cordus; and that is perhaps double or treble the number of novelties gathered in by the whole three above named; and they both were men of longer life and more or less extensive travel. At the time when Cordus' field studies of German botany were in progress nothing had ever been published bearing on Germany in particular as to its plants. The investigator of the botany of its forests, fields, and mountains had no other descriptive resources than the folios of Theophrastus, Dioscorides, and Pliny. Even Brunfelsius' book, which had appeared when

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[1] Meyer, *Geschichte*, p. 318.

[2] Haller, *Bibliotheca Botanica*, vol. i, p. 282.

[3] Meyer, *Geschichte*, vol. iv, p. 318.

[4] Haller, *Bibliotheca Botanica*, vol. i, p. 281.

Valerius Cordus was sixteen or seventeen years old was, as to its descriptive text, nothing but the reiteration of ancient botany. Soon after the publication of that work Euricius Cordus had publicly cautioned men against expecting to find all the trees, herbs, and flowers of Germany described in the botanies of the ancients, who had known but the plants of the very different region of the Mediterranean Sea. This was nothing like an intimation that the books of the ancient scould not be serviceable to students of German botany, and might therefore well be closed and laid aside. That would have been the proposition of an ignorant man and a charlatan; never of one of reason and erudition. The fields, the gardens, culinary and ornamental, the orchards, waysides, and hedgerows abounded in plants cultivated or naturalized which, in part purposely and in part fortuitously, had been brought into Germany from the South and from the East; and the discussion of just these formed no small part of the phytography of antiquity. All this had been clear to the elder Cordus, and was as easily comprehensible to the younger. But there must now be conveyed a better notion than we have yet gained of the rare subjective equipment wherewith young Valerius went forth to the botanical conquest of the great German forests and unexplored mountain districts. On this we have the following from a contemporary once before quoted.

"To the best possible education of an intellect naturally keen, there was united in him that happy temperament to which nothing is impossible, or even difficult of attainment. To these gifts he added a truly marvellous industry and assiduity in research; and above all a most wonderfully retentive memory for everything he either saw in nature or read in books. In this he so greatly excelled as to be able to carry in mind in their entirety descriptions of things which he had not seen but was looking to find; thus having the descriptions always available whenever occasion called for the use of them."[1]

Probably it is not unusual in modern botany for one who is afield to carry in mind, as gathered out of books, the essential characteristics of certain plants not yet met with, so that he is able to recognize such the instant he first sees them; but it will come as a revelation to most botanists of the present, that just this thing was being done almost four centuries ago, by a German boy in his teens, and while as yet the only plant descriptions extant for him

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[1] Riffius, in Preface to Cordus' *Annotations on Dioscorides.*

to memorize were those of ancient Greek and Roman physicians, philosophers, poets, and historians.

In the year 1540, when Cordus was twenty-five years of age, he had in manuscript among other things four books which he had entitled *Historia Plantarum.* The four books contain an aggregate of 446 chapters, each devoted to one species; so that in this manuscript he had described that number of species. The work embraces a part of the results of his travels at home in Germany. Finished, as to these four books, as early as 1540, the work was not published until 1561, or seventeen years after Cordus' death. We shall get an idea of the wealth of these pages in botanical matter entirely new if we but glance at the contents of Book I. The first four types described are, in modern nomenclature, *Drosera*, *Gratiola*, *Sagittaria*, and *Bistorta*. Every one of them, at the time when Cordus here described them, was new to science. Bistorta had been figured by Brunfels, though guessed by him to be one of the dracontiums of Dioscorides, and not described.

In the year 1542 Cordus went to Italy, dividing his time for nearly two years between the universities of Padua, Ferrara, and Bologna, where he made the acquaintance of Luca Ghini, reputed the most accomplished botanist of his time, but of whose greatness only the tradition remains, because he published nothing; thence he proceeded to Florence, Pisa, Lucca, and late in the summer of 1544, to Rome. The misdirected and ungoverned zeal with which he prosecuted this summer journey in a southern climate cost the young man his life. In the company of two friends and a servant, while tending Romeward, he ranged everywhere from the cool mountains down to the heated and malarious marshes of the seaboard. Almost immediately on reaching Rome, Cordus fell ill of a fever and died there in September, 1544, at the age of 29 years. His body found its resting place there in the Church of S. Maria de Anima, where there is a long Latin epitaph, ending with the lines[1]:

Ingenio superest Cordus; mens ipsa recepta est
Cælo; quod terræ est, maxima Roma tenet.

The botanical outcome of these Italian journeyings was a Book V of the *Historia Plantarum*, consisting of descriptions of 25 plants which he had studied in that country, as not having been met with by him in Germany. This work was first published at Strasburg in 1563; then again reprinted at late as 1751, in the large folio entitled *Opera Botanica Conradi Gesneri.*

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[1] *Gesneri Opera Botanica*, vol. i, p. 20 (1751).

Cordus left no dissertations on the philosophy of plants; but only his descriptions of some 500 species; and it is out of these fragments, all posthumously published, that we gather proofs of his resplendent genius.

Phytography. When some years after Cordus' death the manuscript of the *Historia Plantarum* had been sent to the erudite Conrad Gesner at Zurich, in hope that he would approve the work and procure its publication, this worthy, in his letter of acknowledgment—subsequently printed—says that the four books are "truly extraordinary because of the accuracy with which the plants are described."[1]

Almost a century and a half later Tournefort named Valerius Cordus as having been "the first of all men to excel in plant description."[2] Then coming down to the time of Linnæus we shall find the very learned botanist and historian Haller still more pointedly crediting Valerius Cordus with having been "first to teach men to cease from dependence on the poor descriptions of the ancients, and to describe plants anew from nature."[3]

This, then, appears to be Cordus' title to special distinction among German botanists of the sixteenth century. He is the inventor of the art of phytography. This is saying very much, and the warrant for it must be shown. In our study of Tragus we observed that he, writing in German, and for popular reading, also without thought that his writing would ever have the helpful accompaniment of pictures, used an originality and a minuteness of detail in his descriptions of many plants that were quite new in botanical writing. It is one thing to write popular plant descriptions for every class of readers, and quite another to institute a set form of describing them, and that in the common language of the world of learning, and as if for learned botanists only. Just this is what Valerius Cordus did, thereby actually creating a phytography of a new type. And this new phytography had in view the philosophic end of doing away with the need of pictured illustrations. A leading purpose of Cordus was to demonstrate that every species could be so characterized in words as to be identifiable by description alone. It is, indeed, the only reason there ever was at any time in botanical history for describing plants; and the remotest ancients, when one of them undertook to describe a plant at all did it not so badly, but often very well. The trouble they made

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[1] Gesner, in letter to Hieronymus Heroldus, Cordus' works (1561), p. 85.

[2] Tournef., *Inst. Rei Herb.* vol. i, p. 26.

[3] Haller, *Bibliotheca Botanica*, vol. i, p. 282.

was through giving but the names and medical properties in the majority of instances. Euricius Cordus had expressed himself in print as to the unreasonableness of hoping to find the names of all German plants in Dioscorides and Pliny; and the logical sequence was, that German types unknown to the ancients ought to begin to be named and described. Knowing the intense devotion of the father to the son, and recognizing the zeal and ability of the latter, it is not possible to think of Valerius Cordus' work of describing German plants as having had other than this origin. It was likewise of deliberate purpose that the help of the engraver's art was to be dispensed with, as being unnecessary where the verbal descriptions are what they ought to be, except to the untaught, to whom descriptions are useless; for whom, however, Cordus did not pretend to write.

That which I here affirm is a fact which became obscured, and was in effect contradicted, by the editor and the publisher of Cordus' posthumous works; for the folio appeared almost throughout bedizened with woodcuts of plants, to the number of some 280 figures; a condition of things which Cordus could not have dreamed of as possible, and to which, living, it is most improbable that he would have consented. It was by urgent demand of the printer and publisher that figures were inserted. He evidently considered them to be indispensable to the financial success of the undertaking; and most probably he was right in that. The proposal to publish Cordus' works came at the time when the new iconographic movement that had been inaugurated by Brunfels thirty years before was at its high tide of public favor; for Fuchsius' larger and more specious volume with doubly numerous plates had followed, and even Tragus had at last come out in an edition with 567 figures. It was not a time when the publisher would look for the success of a volume of plant descriptions in Latin unaccompanied by figures. The Strassburg printer, Rihelius, prospective publisher of Cordus' *Historia*, was in possession of the plates of Tragus' work and desired Gesner as editor to make use of them in as far as possible to illustrate the text. Gesner acceded to the proposition, and did as well as he could, yet not altogether very well; for there were some of Cordus' plant discoveries that were quite unknown to Gesner; moreover, the latter sometimes erred rather sadly in his interpretation of Cordus' diagnoses. Altogether, the attempt to illustrate by those old woodcuts the beautiful texts of Cordus has led to much misunderstanding and many errors in the interpretation of his chapters; the errors being in the main such as have arisen from incautiously concluding the identity of a given plant from the figure which Gesner by mistake associated with the description, rather than from the description itself.

This is not the place for indicating severally the errors made, as editor, by Gesner; but it may be well just here to call attention to the serious mistake made by Tournefort when, justly lauding Cordus' merit as a phytographer, he said also that he "did not disdain to make use of plates."[1] He had not read Gesner's letter prefatory to the History, with its apology for the introdution of the plates; and many another since—even Linnæus among them—has been chargeable with the same oversight, to his own humiliation.

I suppose Valerius Cordus is the first in all history to have formulated for himself a definite plan or model of botanical description. There is a plan which he follows with such uniformity as to leave not the least room for questioning that he had studied it out for himself; and he presents it by example only, without formal announcement, without explanation, defense or apology, and on its obvious merits. In this plan of his we have the first foundation, and the actual beginning of modern phytography; therefore we must analyze it carefully.

1. There must be a plant before him, a living one; for, while in his day herbarium specimens, especially of uncommon plants, were in the possession of some botanists and pharmacists for purposes of identification, Cordus would not have had the temerity to offer the diagnosis of a dead fragment, or even of a more complete dead specimen, for a plant description. That innovation on phytography was not attempted until two centuries later.

2. The subject must be mature, or at least in flower; the fruit to be waited for if it must be, and described later; for Cordus describes flowers, fruits, and seeds invariably if at all available.

3. He begins with those parts of the plant that are most obvious as it stands living before him. If the foliage is most conspicuous, and the stem insignificant, as in a dandelion or a sundew, he begins with the foliage, proceeding thence to the stem; otherwise the stem is first described, then the foliage.

4. The flower is taken up next in order, the actual diagnosis of it being proceded by a mention of the time of year when the flowering occurs. As to the floral organs, while neither calyx nor corolla has its name as a separate part, he manages to describe both, and always very accurately.

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[1] Tournef., *Inst. Rei Herb.*, vol. i, p. 26.

5. Fruits and seeds are described with great precision. In the case of capsular fruits if the cells are several he tells the number of them and notes the lines of dehiscence, often giving not only the color and the form of the seeds, but the number of rows they form within the capsule.

6. The root is always the last part of the plant which Cordus describes. This, as has been remarked in an earlier chapter, is the inconspicuous or at least the hidden organ, the one that has to be unearthed often with difficulty; and if physiologically and biologically speaking the root is the first of plant organs—and Cordus must have known this, or at least might have learned it from Theophrastus—phytographically it is perhaps well enough regarded as the last; for it is perfectly natural that a man in describing a plant should begin with those parts that are obviously before his eyes, and then proceed in a search for those that are hidden away.

7. In describing herbaceous plants Cordus never fails to state the natural duration of the species if he knows it. Everything with him is annual, or biennial, or perennial. He is far more careful about this than most botanists of the present.

8. To the morphology of things he adds faithful reports of the odors and flavors, whether of foliage, flowers, or roots; and he is so distinctively phytographic as to make the briefest possible mention of medical qualities. This all the more clearly reveals in him a purpose to separate between descriptive botany and economic botany; for as a young physician he was particularly distinguished in pharmacy.

The boldest innovation that was made by any botanist of the whole sixteenth century, in whatever part of Europe, was that of Valerius Cordus when he proceeded to describe anew, and according to his own phytographic scheme, some of the best known and even best described plants of Dioscorides. And yet the ultimate success of the innovation might have been foreseen, doubtless was foreseen, by the young author. The reader must here be given one example of Cordus' new description of a very old medicinal plant; contrasting his with that of Dioscorides.

Dioscorides, Book II., Ch. 162. "Arum, called Lupha by the Syrians, sends up leaves like those of Dracunculus, but larger and less spotted; stem purplish, nine inches high, bearing something like a pestle, upon which the red seeds grow; root like that of Dracunculus. white."

Valerius Cordus, *Hist.* Book I., Ch. 50. "Arum in early spring sends up its leaves each rolled together like a cloak and the roll slenderly pointed; these gradually expand and assume the outline of an ivy leaf, though they are much larger, sometimes attaining the length of nine inches, ending in a point, but widening below, yet receding into a sinus where joined to the petiole. In certain localities the leaves are purple spotted. At the same time of the year it sends up another rolled up cloak[1] which rests at the summit of a short upright stalk, and which about the middle of May opens to something like the form of a rabbit's or a donkey's ear, and shows within that which may be likened to the pestle of a mortar, is of about the length of the little finger, erect, of a dull purple or ashy color, and rests on a kind of roughish tubercle, beneath which there is another tuberculation of the same size, but paler as to color. This last-named tuberculation, after the one above it and the pestle have withered, grows to the size of a walnut and takes on the aspect of a bunch of red berries, each berry containing a seed or two a little smaller than a lentile. This thing ripens at about the summer solstice, and the knot of shining berries and its stalk are all that remain visible of the plant at that time; and when these have fallen away everything disappears. The plant is from a bulbous perennial root of the size of the first joint of the thumb, white, delicate, which is found in a shrunken and withering state under the growing herb, yet after the withering of the herbage is found increased in size and firm. It sends out many eyes or tubercles by means of which the plant is propagated. Every part of the herbage exhales a heavy odor, and is so acrid in flavor as to affect the tongue and palate of him who tastes it as if he had swallowed thistles or briers. The plant inhabits shady places in deep woods, or old and shaded drains and ditches, or along hedges. Some cultivate it in gardens."

Cordus' descriptions of new types discovered by himself do not differ in plan from the above. Some such are longer, others shorter, according to the requirements of the plant itself as examined by him in minute detail. One sees that in them all the same attention is given to the morphology, and also to the life history of the plant in as far as this is known to him. In his practice of describing each species both morphologically and biologically, he is a herald of our late nineteenth and early twentieth century writers who, now that we have the microscope, give life histories with minuteness of detail before impossible.

It will also be observed that Cordus' descriptions of plants

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[1] Cordus' Latin word here is *involucrum*.

are lengthy. The writing them over again in the descriptive terminology of eighteenth- and nineteenth-century phytography would reduce the number of words by at least one half, if not by two thirds, yet would not greatly improve them as to their definitive quality. But the same thing may be said of hundreds of more than equally lengthy descriptions of species published very recently.

Vegetative Organs. One meets with a new term or two in Cordus relating to vegetative organs; and new terms, with a man of his mentality, mean new thoughts. Foremost among such new terms of his is *coliculus*, *i.e.*, cauliculus, or caulicle, diminutive of caulis, a stem. This caulicle appears as the very first word in the descriptive account of an herbaceous plant newly discovered in Cordus' day, now known as *Calla palustris*. The only organ this plant has which Cordus could possibly have identified as any kind of stem is that part now for some time known as its rootstock, or rhizome. The account given of these caulicles is, that they are spread about over the ground, are about a foot long, of the thickness of one's little finger, are green, glabrous, and jointed; that from each joint white fibrous roots descend into the ground, and broad-bladed leaves arise above them,[1] etc., etc. Every other botanical writer all through the long ages had called every such thing a root. Theophrastus alone, and that seventeen centuries earlier, had gone so far as to register a doubt about the propriety of classing them with roots. Cordus is the first to publish a decision that they are primarily of the nature of stems. Note also that the name he assigns this organ is one that accentuates its stem characteristics, veiling those conditions which had led to its having been denominated a root. Such prudence in the selection of a name for the organ evidences a philosophic mind. He might have named the thing as rootstock or a rhizome; thereby, however, veiling those stem characteristics which he wished to impress, and accentuating the very things which had led to their being mistaken for roots; and so the adverse critic would have made light of the whole affair; pointing out that one says it is a stem, yet names it by a name which seems to say that after all it is a kind of root. It is very possible by the injudicious naming of a scientific discovery to retard the acceptance of the discovery itself.

Cordus must not be thought of as having regularly defined these horizontal root-like stems, or as having even recognized them in all their phases as being of the nature of stems. Their distinctly

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[1] *Hist. Pl.*, p. 95.

articulated and leaf-bearing character is in many cases far from being obvious. Such plants as iris and acorus are credited by him, as they always of old had been, with having thick fleshy horizontal roots. But he is none the less first to name as stem any form or phase of rootstock or rhizome, and all the merit of such fine organologic discovery belongs to him.

The occurrence of adventitious roots at the lower joints of large culms Cordus remarks upon admiringly. He names them *fulcra*, not perceiving that at their very earliest starting forth from aerial joints they are roots. He makes the subjoined comment on them as he has studied them in the Indian millet: "The plant has many obliquely descending and quite firm roots, but is not content with these; for when the culm obtains its growth and begins to be top heavy with leaves and the growing spike, it sends down from its inferior joints certain braces which, when they reach the ground put forth roots and fibres. Through Divine Providence, by means of these braces the plants more securely maintain themselves erect against the force of winds."[1] The same is stated more succinctly and briefly in his account of Indian corn.[2]

There are intimations that Cordus is not content with the notion that leaves may spring from roots immediately, but that leaf bearing should be the prerogative of stems, or of that which represents them. When having in view a plant the leaves of which form a rosette hardly raised above the level of the ground, he seems purposely to avoid writing them down as radical leaves, or root leaves, and is wont to describe them as radiating "around" the root. It is an evasion, certainly; but it subserves its purpose; for he thereby escapes the necessity of saying that they grow from the root. But again, in describing some garden biennials, like the carrot, the part to which the leaves are attached is visibly distinct from the fusiform root, though it is extremely short, too short to be called a stem, and he denominates it the caput,[3] the head above the root. Only tardily has botany come to approve, formally, this one of the improvements in organography which it owes to Cordus. It is now taught that such apparently radical leaves grow from what is called a crown, and is understood to be but a shortened and thickened stem; but the teaching is still ineffectual to altogether prevent our occasional speaking and writing about stemless plants and root leaves.

[1] *Hist. Pl.*, p. 143.

[2] *Ibid.*, p. 112.

[3] *Ibid.*, p. 103.

To the fundamentals of leaf morphology I have not found this author contributing very much that is new; but his descriptions show him always carefully discriminating between the equally and the unequally pinnate in compound leaves. Not that these leaves have yet been formally designated as "compound." They have not been, nor will they be so named until after the lapse of more than a century after Cordus' time. The epoch is one of discovery, not of always naming the thing discovered.

But Cordus is the man who first reaches the conclusion that an organ need neither be green colored nor horizontally explanate in order to be a leaf. It seems to have fallen to his lot to describe but two orobanchaceous types. In the case of the first of them it is evident he had not yet seen that the colorless scales investing the base of the stem have the nature of foliage; for he says "the plant is destitute of leaves."[1] Some years after this, travelling in Italy, he becomes acquainted with another and much more scaly member of this alliance. His opinion respecting what the scales are now undergoes a change. "The dense investiture of scales upon the stems of this plant, all of them pointing vertically, are to be interpreted as being its leaves."[2]

Anthology. Among botanical authors of his time Cordus alone gives some attention to inflorescence. He is the first after Theophrastus to have noted the distinction of the centripetal and centrifugal in anthesis; or, to state it otherwise, of the indeterminate and determinate in inflorescence; and every historian of botany appears to have overlooked this. Meyer writing on this topic little more than a half century since says that, in as far as he is aware, all the way from Theophrastus down to the times of Link and Robert Brown, no mention was made of these distinctions.[3] Cordus' writings antedate those of the worthies last named by almost three centuries; and Meyer can not have taken the time to read them; for it is a very common thing with this German youth, in describing plants spicate or racemose as to their flowering, to say that the expanding of the individual flowers proceeds from base to summit of the axis, and that thus the succession of the flowering is prolonged indeterminately as it were, and may continue indefinitely, through a long season. In the case of loose inflorescences such as the corymb and the umbel he does not make note of such differences. But in the crowded, though quite spherical flower-cluster

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[1] *Hist. Pl.*, p. 89.

[2] *Ibid.*, Book v, p. 5.

[3] Meyer, *Geschichte der Botanik*, vol. i, p. 166.

of *Echinops sphaerocephalus*, he observes and records it that the first flowers to open are those at the summit of the cluster and that the succession is from that point downward to where the globose head is joined to its peduncle.[1]

It will be taken for granted that Cordus had received from Theophrastus the suggestion of the centripetal and centrifugal in inflorescence; but there is one excellent definition of a particular kind of inflorescence which modern botany receives from this German youth of the sixteenth century; that of the word umbel. The word itself as a botanical term is as old as botany, and was in the first place suggested by the mechanism of an umbrella or sunshade, which in more than one way certain inflorescences recall. There are two things essential to an umbrella, and these not equally conspicuous. The more obvious part is the rounded and expanded externally more or less convex surface; and there is also the frame work beneath supporting it in expansion. Now while the modern botanist is taught to look not at the expanded surface, but at the structure of the framework beneath for the evidence that a flattish topped inflorescence is an umbel, it was quite otherwise with the botanists of antiquity, and with all of them before Valerius Cordus. Brunfels, Fuchsius, Tragus, and this young botanist's wisest contemporaries held the broad flat clusters of the elder and the viburnum to be umbels equally with those of carrot and caraway, parsnip and fennel; no heed being given to the complexity of the supporting framework in the elder, or to the simplicity of it in caraway and fennel. Even the small heads of those composites the yarrow and the tansy were said to be arranged in umbels. It is in the midst of his own new and improved description of the common yarrow that Cordus suggests the need of distinguishing between umbel and corymb; and that nothing should be called an umbel the stalklets of which do not all arise from one and the same point.[2] This is done with the utmost modesty, the revolutionizing proposition being enclosed in a parenthesis. He sees the important distinction between umbel and corymb, names briefly the characteristic of the original and true umbel, and quietly passes on, leaving it to be inferred that other flattish-topped inflorescences not answering to this clear definition of the umbel may be called the corymb; and his practice proves this to have been his purpose. We shall be interested in observing later how long after Cordus botanists in general saw the need of distinguishing between umbel and corymb.

[1] *Hist. Pl.*, p. 87.

[2] *Ibid.*, p. 139.

Just here, however, it may be observed that when the vast family of the composites first gained clear recognition, *Corymbiferæ* became at once the family name, and remained steadily in use for a century and more; and Cordus made the first beginning of calling the inflorescences of many commonest composites corymbs.

Those modified leaves now known as bracts, and collectively as an involucre subtending inflorescences and flowers, first obtained mention and description by Cordus. In *Daucus* they so conspicuously embrace the umbel, and are so very unlike the proper foliage that he is careful to bring out in writing all their peculiarities[1]; the first step in the direction of giving taxonomic significance to the involucre in the family of umbellifers. Recalling that that comparatively modern and well advanced anthologist Tournefort wrote down the involucre of every araceous plant as its "petal,"[2] it is interesting to note that this German youth more than a hundred and fifty years before Tournefort denominated it an "involucrum,"[3] as do we of to-day; for the spathe is but one type of involucre.

The calyx under Cordus makes noteworthy though by no means great progress toward gaining recognition as a part of the flower. Quite in conformity to the definition which we found in Fuchsius' vocabulary Cordus calls that a "calyculus" which is synsepalous and either becomes a part of the fruit, or at least a protection to the growing and mature seeds. All labiates and borrageworts will readily be accredited as having calyxes. But how will it fare in this regard with the solanaceæ. In the some half-dozen genera of this family then known in German gardens there was not one, *Hyoscyamus* excepted, which could exhibit an organ answering to the then accepted definition of a calyx; and, curiously enough Tragus, and even Fuchsius, when they name the "calyx" of *Hyoscymus*, mean nothing but the operculate, and therefore literally chalice-shaped, capsule.[4] The real calyx goes with Tragus for a "vasculum," while Fuchsius makes no reference to it whatever. One can not but regret that Cordus did not describe *Hyoscyamus;* it would have been so interesting to have seen what he with his perfect originality of view would have made of the anthology and carpology of a type so marked. But the youth was much engaged in bringing from German meadows, river banks, and woods representatives of genera unknown to the ancients, and describing these.

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[1] *Hist. Pl.*, p. 90.

[2] Tournef. *Inst.*, p. 158.

[3] Cordus, *Hist. Pl.*, p. 102.

[4] Tragus, *Stirp. Comm.*, p. 132; Fuchs, *Hist. Stirp.*, p. 832.

The presentation of his views about calyxes in solanaceous plants will be helped by a list of the genera which at that period, over and above *Hyoscyamus*, were in German gardens. They were *Solanum*, *Atropa*, *Mandragora*, *Melongena*, *Physalis*, *Capsicum* and *Stramonium*, or *Datura*. The last had been credited with a calyx by Tragus, though not technically, or as being an organ regularly so named, but only as a cup-shaped green thing forth from which "the flower proceeds."[1] The corresponding part of the plant *Physalis* Fuchsius had described collectively as "rounded pouches resembling bladders,"[2] and Tragus calls them simply "*bladders*."[3] These I think are all the references made by Fuchs and Bock to anything like a calyx in their eight or nine solanaceous genera. Of course the small, flat, hardly more than disk-like calyxes of proper solanum, mandragora, capsicum and their like, would not be noticed by them in any way. They were no part of the flower, nor had any significancy. With Valerius Cordus every one of them—the tubiform cup of *Datura*, the globiform-inflated and closed bladder of *Physalis*, the prickly saucer-shaped thing holding the mere base of a melongena fruit, and the almost flat green disk visible at the basal end of a pepper pod—is a "calix" or "caliculus." This was a most significant innovation; the employment of an old term in such wise as to make of it a strictly scientific term. Hitherto "calyx" had been used in botany from the remotest times as signifying any organ that happened to be cup-shaped, the ovary of a pear or quince or pomegranate in their early stages, a cup-shaped corolla, the cup-shaped corona within the perianth of a jonquil or daffodil, or even an operculate dry seed-vessel. Any one of these was a "calyx" in pre-Cordian terminology. Now, according to this use of the term, which makes its first appearance in Cordus' descriptions of solanaceous plants, calyx is not calyx by reason of its shape, but because it holds always one and the same place-relation to "flower" and to the fruit that succeeds the flower This principle of the location of an organ, the place which it visibly occupies as next some other organ, rather than its form or coloring or texture, is one which, long after Cordus' day came to be received a absolutely fundamental in anthology. Morphologically considered the whole doctrine of the flower, as almost every reader will know, rests on this basis, and most securely. Therefore Cordus' mere application of this principle to the identifying of calyx in all its extremes of form

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[1] Tragus, p. 896.

[2] Fuchs, p.881.

[3] Tragus, p. 304.

throughout the one family of the nightshades must rank among the most important contributions ever made to anthology; for it was the first hint ever given of a truly scientific classification of floral organs.

The truthfulness of this last statement will remain unimpeached notwithstanding that Cordus did not name the calyx as forming a part of the flower. It was too early for the expression of anything so iconoclastic as that. From the earliest dawn of botany down through uncounted ages, a circle of green-colored leaves, no matter what their size or form, or where placed, had been a circle of leaves, and a whorl of leaf-like organs colored otherwise than green had been a flower. Even in the mind of young Cordus this appears to have been a prejudice too deeplyseated to fade away even before the light of his own brilliant discoveries. He never admitted the calyx to the rank of a floral organ; though he seeems upon the verge of doing so. In describing the "flower" of the white water lily he actually sets apart, as too different from the others, those four outer members which are green externally and which also, as he says, completely enfold all the others, and he gives to them collectively a name by which they must be distinguished from the many and narrower white "flower leaves"; the name is not calyx but "tunica."[1] He does not, however, look on them as partaking so much of the nature of a calyx as of ordinary flower leaves. He observes that they are not altogether of that green color which at first glance they seem to be, but that at summit and marginally they are of the same texture and whiteness as the others. They impress him as being modified flower leaves, whereas the green things at the back of buttercups and others like them are but reduced and modified ordinary leaves. The terms calyx and caliculus I have failed to find Cordus making use of at all except for such as are synsepalous. If such a circle is quite chorisepalous, or even approximately so, he calls it a circle of leaves simply. He does not overlook the fact that such chorisepalous circle has a particular function, and that in immediate relation to the flower; and here again he seems as on the verge of extending the use of the term flower so as to make it include the calyx; but he never quite does that. His manner of expressing himself in such cases is exemplified in his description of what he calls *Hepatica alba*, which is his name for a new genus of his discovery, its equivalent in modern nomenclature being *Parnassia;* the type, *P. palustris*. Here is his

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[1] *Hist. Pl.*, p. 99.

account of what is now called the calyx in that species. "Beneath the flower itself there are five very small leaves within which the flower before it opens is enclosed."[1] Certainly that which envelops a flower before its expansion can be no part of that flower. There is here a more than whispered call for a new extension of the use of the word flower; a new definition of that organ, by the terms of which the calyx shall be recognized as a part of it. All this will come, but by no means speedily.

There is one use of the term calyx frequent with Cordus which practically implies chorisepaly. I refer to his habitual writing of the involucres subtending the flower heads of all cichoriaceous plants and some of the true composites as the calyx. This was a complying with the terms of the then accepted definition of a calyx as that which enfolded at first the flower, after that the fruit. As a somewhat special application of the term this, perhaps introduced by Cordus, seems strongly to have commended itself to future generations; for, long after the "flower" of chicory and its cognates had been seen to be an inflorescence, this term calyx remained in use instead of involucre. With Tournefort in 1700 it was still a calyx. Linnæus a half century later modified the expression in so far as to write it "calyx communis"; but this needful modification was afterwards ruled out. A number of prominent botanists, even down to the middle of the nineteenth century, wrote down the involucre even of the sunflowers as a calyx, and the bracts composing it, as sepals.

To the morphology of the corolla—if one may use that term in writing about a time which far antedates the term's invention—Cordus adds a few items of high import to phytography. Botany has now not many expressions which it could as hardly do without as the terms papilionaceous and bilabiate; for they at once recall, and respectively designate, two large and important families of plants. Fuchsius in one instance speaks of a certain "flower" as having the form of a butterfly.[2] In as far as I have been able to discover the idea of comparing the pea blossom to a butterfly originated with Fuchsius' brilliant contemporary Conrad Gesner.[3] Neither of these appears to have used the expression "flore papilionis forma" in connection with any more than the single species *Pisum sativum;* but this evidently suggested to Cordus the possibility of something better than the antique usage of describing a vetch blossom as

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[1] *Hist. Pl.*, p. 153.

[2] *Hist. Stirp.*, p. 628, under *Pisum* (1542).

[3] *Historia Plantarum*, p. 102 (1541).

being like that of a lupine, and the lupine flower in turn being compared to that of a pea. And his appears to have been the botanical eye that was first to perceive in the flowers of the whole long line of beans, peas, and lupines, and of broom and cytisus, and laburnum tree, only more or less marked deviations from—mere modifications of — that pea-blossom type which Gesner had compared to the insect called a butterfly; and by and by the adjective term papilionaceous was coined by him. Such readers as may not have access to Cordus' works may be interested in following the gradual genesis of that useful adjective in the young master's mind; and this can be shown by citing his expressions in precisely the order in which they occur in the succession of his pages. I therefore give these in the footnote.[1]

Of equal service to all botany was his invention of a term that should at once indicate and describe the corolla of the Labiatæ. His term is "flos hians," the gaping flower, or corolla as we now say. Nor does it require but a moment's reflection to become convinced that the expression gaping, or yawning corolla is more perfectly and exactly descriptive of the most common and typical corollas of labiates than is the term bilabiate; for certainly that which one sees clearly in the form of such is, not a pair of lips, but a wide-open yawning mouth, exposing even the throat itself. Terminology certainly lost something of the accurately definitive when later authority displaced Valerius Cordus' "yawning" flower and substituted the less fitly chosen bilabiate.

It was yet far from the time when any taxonomic use would be made of the different ways in which members of floral circles are enfolded in the bud. Cordus was first to observe some such differences, and to name them in his plant descriptions. In describing *Iris* he notes that the parts of the flower are convolute in the bud.[2]

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[1] "Flores parvi, * * * papilionum figura." (*Hist. Plant.*, 99^2).

"Flores * * figura papilionibus similes." (*Ibid.*, p. 100).

"Flores * * * papilionum figura." (*Ibid.*, p. 101).

"Flores papilionibus similes." (*Ibid.*, p. 127^2).

"Flores * * * * figura papilionum." (*Ibid.*, p. 137).

"Pediculus * * parvis oblongis papilionaceis floribus circumdatus. (*Ibid.*, p. 164^2).

"Flores forma papilionacei et oblongi." (*Ibid.*, p. 166^2).

"Flores * * producit * * papilionaceos, quales in omnibus leguminibus est videre(!)." (*Ibid.*, p. 187^2).

The reader must not fail to note that, when once the term *papilionacea* has presented itself to Cordus' mind he thenceforward employs it constantly.

[2] *Hist. Pl.*, p. 133.

Similarly he describes the corolla of *Datura* as being laid in folds before expansion.[1] It will not be assumed that in first writing these things down he had any thought of their important bearing upon affinities. He may have had; but whether so or no the terms he chose are so perfectly correct for the kinds of prefloration which they indicate, that no reformer of terminology has sought to displace them, and they remain in common use to-day.

The word petal was still unheard and unwritten in our science. It will be proposed by an Italian botanist two generations later. It is evident Cordus has realized to some degree the desirability of some term by which flower leaves shall be distinguished from ordinary foliage; for where flowers are choripetalous he uses the diminutive foliolum (leaflet) instead of folium; this, however, not as an inviolable rule, nor in such wise as to preclude the application of the term to ordinary green leaves that are of very small size. Also such elongated and more or less strap-shaped flower leaves as radiately encircle the middle of a flower, or head of flowers, Cordus repeatedly describes as resembling rays, "radii"; the earliest adumbration of the term now long in use for this kind of corolla.

The disk-corollas in the composites that have rays were still undiscovered. They were seen in the mass only and were always written of as the "stamina," therefore quite undistinguished from the central parts of a buttercup or anemone blossom. Cordus alone I find in one instance so writing of the "stamina" of one particular composite as to prove that he had seen the individual "stamen" as he could have called it, and had found its form peculiar. The plant is *Tussilago;* and having described the rays, he says that the stamina in the midst of them are "in form like minute lilies."[2] No matter what he calls the thing, it is plain that he is the discoverer of the disk-corolla in composites.

If both stamens and pistils are numerous, small, and slender in the same flower Cordus makes no distinction between them in name. In this case all are "stamina"; thus in *Pulsatilla* all the threads in the midst of the flower are stamina; but those occupying the very center he observes as "changing into a tuft of long hairs at the base of each of which there is a seed as in clematis."[3] If, however, in flowers at once many-stamened and many-pistilled the pistils, being without styles, have no capillary aspect in the aggregate, then the two sets are not confused and the inner ones are not

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[1] *Hist. Pl.*, p. 90.

[2] *Ibid.*, p. 93.

[3] *Ibid.*, p. 121.

"stamina." This is apparent in the account which he gives of thalictrum flowers. He says they are "very small, and consist of pendulous stamens only; but after the falling away of these, very small oblong black seeds remain."[1] In the case of larger flowers, where the stamens are fewer and easily counted, it becomes clear that by "stamen" Cordus means primarily the filament; this doubtless partly because in very many instances it was all he could find. There are no "apices" to the inner "stamina" of pulsatilla or clematis; none to the forked styles of the many cichoriaceæ and compositæ which he examined, and he always calls these the stamina. Even in the solanum type of floral structure, where five stamens, almost all anther, form a central column forth from whose summit one slender style-thread protrudes, Cordus, seeing them all, denominated all six indiscriminately "stamina."[2] Evidently his mind was exercised by these small things, in the morphology of which he saw enough to prevent him at least from calling them indiscriminately stamina and apices as others of his time were doing. In the large and therefore convenient flowers of lilies he saw and took note of the transverse position of the anthers, but would not name the things by any name at all. The stalks which these rested on were what he called stamina. The term apex he seems to have wished to transfer to the style and stigma and to have it apply to that part of the flower only.[3] He takes particular notice of dust—*rubiginosus pulviusculus*—which the lily anthers shed before collapsing. Again in describing the anthers of *Paris* he sees this same kind of dust, and there proceeds to assign it a name; even the name which it has always since borne; for he describes these parts as being "luteo *polline* conspersa."[4]

Since the most ancient times what they knew as a fruit they had recognized in its germinal state resting at the bottom of the flower, or else below it; but in every stage from the tender germinal condition forward to its maturity they had called it the "fruit" simply. Cordus makes the first inroad upon this time-honored usage. That which we know as the ovary he uniformly declines to write of as the fruit; as if perceiving an absurdity in making this small and tender mere promise of fruit, identical with the future actual fruit, even in name. He does not, however, formally propose a

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[1] *Hist. Pl.*, p. 97^2.

[2] *Ibid.*, p. 90^2.

[3] *Ibid.*, p. 139^2. Also in the flower of *Paris*, p. 152, he calls the stigmas, not the anthers, apices.

[4] *Ibid.*, p. 152.

universal name for this hitherto really nameless thing. Where, as in the water lilies and in some others, it is rather large and conspicuous within the flower, he applies to it that already much and variously used term *capitulum;* but in the more numerous cases of smaller flowers, where it appears as only a small protuberance in the bottom of the blossom, he writes of it as the *tuberculum.* The ovary, then, like the pollen, is one of Cordus' discoveries, so to speak, in anthology. Furthermore, this tuberculum, as he calls it, is openly recognized as an integral part of the flower as a whole. The proof that he so received it is found in the language he employs in describing the pæonia. Having given the characteristics of the flower leaves, he passes from these to the two large ovaries occupying the center. These he calls a pair of horns, *cornicula,* mentioning that they are hedged about by the yellow stamens, then proceeding to state that these two cornicula "remain after the other parts of the flower have fallen," then grow to be an inch and a half long,[1] etc., etc. The idea that corolla, stamen, and pistils collectively constitute a flower could not be expressed more unmistakably; and it is the earliest record I have met with of such a proposition. Theophrastus, as we have seen, first classified flowers as leafy and capillary, so that a mere tuft of stamens only, unattended by leaves of any kind, was a flower. Then, with a wild rose blossom before him he construed the yellow stamens as being, not a circle of floral organs, but a capillary flower within a leafy one. Thus the rose had two flowers; the very central axis, globosely enlarged below, was the "fruit" even then, and no part of the flower. This Theophrastan anthology of two flowers in one, rather than one flower made up of two sets of organs, was everywhere accepted in Cordus' time. Tragus had reproduced it, though with augmentations, even to calling the petals "rose leaves," and the stamens the "rose flower."[2] Now in Cordus' procedure we have an illustration of how the making of one little distinction, and the invention of a word that accentuates that distinction, may revolutionize a science. The man had seen an inaccuracy in the practice of calling by the one name of fruit both the little tubercle lying at the bottom of a cherry blossom and the subsequent ripe cherry. That mere floral sign and promise of a fruit he determined to name a tubercle. Logically a fruit is no part of any flower; as logically this "tubercle" *is* a part of it. In rose and pæony there are not three flowers one within another, but one flower made up of three different kinds of parts. We shall not be

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[1] *Hist. Pl.*, p. 135 .

[2] Tragus, *Stirp. Comm.*, p. 988.

likely to meet with any other one botanist in the whole history of our science who so greatly advanced the morphology of the flower as Valerius Cordus. To the finishing of morphologic anthology only two things remained to be done. The calyx was to be added to the category of floral organs, and the "flower leaves" awaited a distinctive name; for the "foliolum" proposed and used by Cordus did not satisfactorily meet the demand. It was not sufficiently different from folium. The discovery of the functions of certain floral parts, though of the very highest importance, awaited not the coming of a more astute botanist, but the invention of mechanical aids to natural vision.

Among a number of Cordus' signal new discoveries in anthology, one must not omit to mention the flowers of the genus *Ficus*. Every botanical authority from Theophrastus forward had averred that fig trees have no flowers, and that the fruits are only fruits and nothing more from their first small appearing to their ripening. Cordus says: "When the figs are half grown they develop in their interior (what you may be surprised to know) what appear to be crowded stamens, of a pale purplish color, standing forth from the fleshy part, and all pointing toward the central hollow, to each of which there suceeds a flattened yellowish seed."[1] It is most remarkable that, two hundred years after this clear description of fig flowers, Linnæus should have placed *Ficus* among the genera of cryptogams.

Fruit and Seed. Not Brunfels, nor Fuchs, nor Tragus shows sign of ever having read Theophrastus' scientific definition of a fruit. As far as my careful reading has gone the Greek's term pericarp first reappears in Cordus; and he uses it frequently. Then, in his full descriptions of various plants it comes out that he has made sections, longitudinal and transverse, of many pericarps, so as to be able to record the number of the seeds when they are not too numerous, or the number of rows in which they arrange themselves when the number of the seeds is too great to be told. While the elder German authors have quite minutely and well described the curious and beautiful seeds of cardiospermum, this youth, as if belonging to a later generation and to a later century than the

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[1] *Hist. Pl.*, p. 184. In my copy of Cordus, and presumably in the whole edition, a word has been omitted by the printer. As printed, the first line about fig flowers reads: "Flores, ut omnes tradiderunt, fert, sed statim parva fructuum rudimenta." This is wholly unintelligible, as a sentence, until you supply the negative non before fert, so that the reading shall be, "non fert." Cordus' manuscript was not printed until seventeen years after his death.

sixteenth, makes note of the fact that these same seeds are attached to the central part of their pericarp[1]; thus suggesting long before the botanical world had apprehended its taxonomic usefulness, the several modes of placentation.

He is accustomed to peer into and take note of every aspect of the various dry dehiscent fruits that he meets with in the gardens or in the wilds. In describing species with capsular fruits he tells whether its capsule is one-celled or several-celled, naming the lines of dehiscence, the commissures, the partitions, if there be any, the septa, and the compartments themselves the loculamenta. Then the seeds are reported on, not only as to the method of arrangement but as to every item of their form, the color and texture of their testa, and the color and flavor of the nucleus—so he names it—when they are large enough to be tested by the sense of taste.

Cordus has not, like Tragus, followed up the suggestions of Theophrastus about the cotyledons; for that belongs to the garden student, who plants seeds and watches their germination and first appearing above ground; and Cordus is more zealously devoted to wild botany. To him, however, must be conceded priority in the matter of distinguishing between spores and seeds on the one hand, and between spores and pollen on the other; this also without his ever having seen either an individual spore or an individual pollen grain. For the clearer understanding of Cordus in this particular field of enquiry we must recall Tragus' having so studiously and so laboriously gathered what he and others believed to be the seeds of the fern osmunda.[2] That he called them seeds implies the belief on his part that young ferns could be produced from them. But then, the superstitious Tragus seems also to have believed that trees could be reproduced both by their proper seeds, and also by that flower-dust which Cordus afterwards named pollen.[3] Among

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[1] *Hist. Pl.*, p. 89.

[2] See page 238 preceding.

[3] Tragus at page 1073 has the following as to the reproduction of junipers: "Maio mense tenuissimus ac luteus pulvis è juniperis in auras avolare conspicitur, quod semen illius esse animadverti. Post hunc quem diximus pulverem baccæ prorumpunt exiguæ, virides, quæ altero demum anno antumno appetente, quod illi tempor maturitatis est, cœruleo tinguntur colore, etc., etc. È nucleis lapidosis, qui in hisce baccis continentur, novæ Juniperi plantulæ fruticomt." If in this passage semen is used botanically the meaning can be no other than that junipers may grow from pollen. If, what is improbable, he employs it in a zoological sense, as meaning that the dust which he says sails away upon the air is needful to the fertilization of the seed within the juniper berry, then he is the first to proclaim the modern doc-

nature students of four hundred years ago I know not who else is so far from accepting things on other people's guess or hearsay as Valerius Cordus; in whom I have not yet read a line that savors of the fabulous or superstitious; and that, for the period, is much to say of any author. Concerning the propagation of scolopendrium he says: "Phyllitis has no stem, or flower, or seed; nevertheless, from the vermiform patches on the back of the leaf, when these are resolved into powder and are scattered abroad, it is propagated."[1] Substantially the same proposition is repeated with emphasis at the end of his description of *Aspidium filix mas:* "Although it has neither stem nor flowers nor seeds, it nevertheless propagates itself by means of the yellow and hairy powder that is produced on the back of the leaf and is blown away by the wind."[2] Again, of trichomanes it is said: "It grows copiously on moist shaded rocks, although it produces no stem, or flower or seed. But it reproduces itself by means of the dust that is developed on the back of the leaves, as do all kinds of ferns; and let this statement of the fact once for all suffice."[3] From this point forth he proceeds to describe a half dozen other ferns in close succession, carefully bringing out the form and arrangement of their fruit-dots or lines not omitting even the indusium, but not again mentioning their seedlessness or their means of propagation.

This positive and reiterated assertion that ferns have no seeds, yet propagate by organs so infinitesimal that he has never seen one,[4] implies that what ferns shed from the back of their fronds is understood to be of a different structure from that of seeds. It is an easy matter for one frequenting the native haunts of certain ferns to see their prothallia both with and without the first diminutive fern leaf; and it can not reasonably be doubted—indeed Cordus by his strong language compels us to think—that he had seen these things, and had assured himself that the germination of ferns is most different from that of seed plants; thence inferring to a certainty that those particles, invisible except in mass, are things different and distinct from seeds.

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trine of bisexuality in plants. He who knows Tragus' belief in the reproductibility of even seed plants apart from any kind of seed or germ, will not very readily accept this latter interpretation of his language.

[1] *Hist. Pl.*, p. 113.

[2] *Ibid.*, p. 169^2.

[3] *Ibid.*, p. 170.

[4] The individual spores of ferns were first seen some seventy years after Cordus' demise.

Taxonomy. Cordus' Books I and II are devoted to herbaceous plants, Book III to woody growths great and small. He accepts, then, and without objection, these anciently established first Grand Divisions of the plant world. The primary division of the herbaceous plants is not made from the point of view of affinities, but is ordered historically rather, as one may say; for while the heading of Book I is "Concerning Divers Herbs," that of Book II reads, "Plants whose History was either inexactly transmitted by the Ancients, or else altogether omitted." Such headings do not seem to promise much of taxonomic doctrine, or of the tacit expression of it by grouping. Members of the same natural family, and species of the same genus will almost inevitably be distributed partly to Book I and partly to Book II.

Despite these seeming obstacles to ready expression, and while there has never yet been any attempt to relegate all genera to families, or even formally to characterize any of those several families that have always been recognized, still Cordus advances well beyond all his predecessors in this significant part of botanical systematizing. When, as sometimes happens, his general plan has led to the placing of some type away from its real cognates, he is apt to give the hint that such genus thus isolated in his book belongs to a certain family. An example of this occurs in connection with his new description of the old genus *Lupinus*. His first word is "Lupinus is a leguminous plant."[1] He seems to be offering this as a piece of taxonomic information that is needed. He is not presenting his readers with an empty platitude. When writing of *Faba*[2] and *Cicer*,[3] and *Phaseolus*[4] he does not tell that either one is a leguminous plant. All the world knows that these are, and have been so classed immemorially. The family of leguminosæ of antiquity consisted of such papilionaceæ as yielded edible seeds and were therefore food plants. Important though they were, they could not be harvested and threshed after the manner of harvesting the frumenta, or cerealia. The individual pods had to be collected by hand; hence the very name legumina. Lupinus was not one of the leguminous plants with the ancients. Its seeds were bitter, but endowed with active medicinal qualities. It was by virtue of botanical principles quite new in Cordus' day that he dared to say Lupinus is a leguminous plant. The family was now receiving

[1] *Hist. Pl.*, p. 137.

[2] *Ibid.*, p. 166.

[3] *Ibid.*, p. 99.

[4] *Ibid.*, p. 127².

new accessions, not of plants newly discovered, but of such as had been known very anciently but not admitted to the group, but now ready to be received because new and truer criteria of relationship were being exploited, and gaining recognition. Cordus, as we have seen, had been the discoverer of the papilionaceous flower—the expression tells but the truth—and one immediate result of the discovery was, the augmentation of the ancient group of the legumina by the referring thereto of every genus the mere form and plan of whose corolla was at agreement with that of beans and peas. Anthology, dormant if not dead for fifteen centuries, has come to life, is developing with some rapidity, and the ancient taxonomy that had been constructed largely according to vegetative characters, culinary uses, and medicinal properties, is being steadily but quietly displaced. The chief agent of this radical taxonomical reform at this period is Valerius Cordus. We must follow him in his bringing in of one other new accession to the family of the leguminous. The type had been well known to the ancient Greeks. They had given it the name which, as a genus, it has always borne from their day to ours, *Glycyrrhiza*, Sweetroot. No use was known for any part of the plant but its root. Anciently no one would have called it a leguminous plant; and for this very reason Cordus argues the case of its proper admissibility to membership in the family. In the place of a concluding note, supplementing a fine description of the plant in all its parts, in the course of which he has announced having found the flowers, though small and crowded, to be precisely papilionaceous, he says, "The roots have a very sweet flavor, moderately astringent, and a trifle acrid, to which there is added the mere trace of a certain bitterness that belongs to all leguminous plants; for even this plant is as certainly referable to the leguminosæ as the peas and beans themselves; in view of which decision it seems fitting that I should make mention of this one sensible quality that it has in common with all the leguminosæ; for perhaps not every one would prove to have the sense of taste sufficiently cultivated to be able to perceive it and attest its presence."[1]

One is obliged to pause a moment in admiration of the calm, judicious conservatism of this youthful botanical genius, as it reveals itself in the last of those lines. He has discovered, and as modestly as possible he announces the discovery, that by their floral structures alone the family relationships of hosts of plant genera may be determined, and unmistakably. The detection of a

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[1] *Hist. Pl.*, p. 164^2.

principle so manifestly valid, and so surely destined to revolutionize completely all botanical system, can not but have fired with enthusiasm a man only some twenty years old—a year or two more, possibly, and quite as possibly a year or two less—but in his writing how completely does he repress all enthusiasm. Profoundly respectful toward venerated authorities of two thousand years before who had held that agreement as to properties was required in order to establish the fact of interrelationship, he investigates licorice root to find that also by qualitative criteria as well as by floral structure the plant proves itself a member of the leguminosæ. In this repression of excessive enthusiasm for the new, and continuing to respect the old principles, some later celebrities are in unfavorable contrast to Cordus; for they so greatly magnified the value of the new anthology, as to write intolerantly and even in derision of the old ideas that vegetative characters and sensible qualities have taxonomic weight.[1]

We must follow Cordus a few steps further in this path of the discovery of relationships; for he is making distinct and lasting landmarks in the history of plant families.

Tracing backwards the history of the *Cucurbitaceæ*, we reach no point, however ancient, at which gourds, pumpkins, squashes, melons, and cucumbers were not recognized collectively in their status of a family, or larger genus, as such a group was at first named. In their mode of growth, their coarse, rough herbage, and even as to the structure and qualities of their familiar large firm-fleshy fruits, they were in a comprehensive way at one. Any cultivator of them, however untaught, would be botanist enough to see that. Meanwhile there were two or three other types, long and familiarly known, which had never been thought of by even the most skillful botanists, as cognates of gourds and cucumbers. They were the bryonias and the momordicas. According to such signs of consanguinity as availed with the ancients, these could never have been thought of as possible cucurbits. They were not large, coarse, harshly almost hispid plants, but were small, comparatively smooth and delicate in texture; and the bryonias, so far from yielding any fruit the least like gourds and melons, put forth bunches of small, soft, pulpy berries, red or black, more like those of nightshades. In the second place there were no mild qualities in these plants, no parts were edible. They were actively medicinal, and some of them powerfully narcotic-scented. It was no less

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[1] See Tournefort's *Elemens* (1694), also Linnaeus' *Philosophia Botanica* (1751).

than a master stroke of the new taxonomy, Cordus' bringing in of momordica and bryonia to augment the series of genera of the cucurbitaceæ.[1] He had examined their small flowers, dissected the little bryony berries, and compared their plan of structure with that of their robust innocuous neighbors of the fields and gardens, and by these tokens had found them all to be of one lineage.

Singularly the little soft-leaved weed *Lithospermum arvense* had never been thought of as in the least degree akin to the coarse, rough, stinging borrage and bugloss and anchusa; but Cordus describing the plant in every part with a minuteness and accuracy unapproached by any earlier writer, concludes it all with the proposition that its affinities are with the anchusas and echiums. "It is of their kindred; something which the ancients did not apprehend."[2] It had been through a comparison of their inflorescences, and by a recognition of the same floral plan and fruit characters that he had become able confidently to add lithospermum to the family of the borrage-worts; and yet not by these alone; for he avers that as to properties also it is much like the others.

In his delimitation of the genus *Ranunculus* Cordus defers unwontedly to floral characters, and slights those of root, stem, and foliage. This is very interesting as proving that the trend of his mind has been strongly in the direction of what has come to be the established and settled first principle of classification.

Upon this point the subjoined list of his *Ranunculus* species will be instructive. They are given in the order in which he places them. The equivalents of them, that is to say the identification of them, in modern nomenclature is somewhat doubtful in only one or two cases.

	Cordus	Modern
1.	Ranunculus palustris	Ranunculus sceleratus.
2.	Ranunculus Sardous	Ranunculus Sardous.
3.	Ranunculus tertius	Anemone ranunculoides.
4.	Ranunculus quartus	Anemone nemorosa.
5.	Ranunculus quintus	Ranunculus arvensis.
6.	Ranunculus flammula	?Ranunculus repens.
7.	Ranunculus coronarius	R. acris, double-flowered.
8.	Ranunculus octavus	?R. acris, single-flowered.
9.	Ranunculus nonus	Ranunculus bulbosus.
10.	Ranunculus leptophyllus	Pulsatilla vernalis.
11.	Ranunculus undecimus	Anemone silvestris.
12.	Ranunculus platyphyllus	Ranunculus flammula.

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[1] *Hist. Pl.*, pp. 114^2–119.

[2] *Ibid.*, p. 94.

A genus Ranunculus of such composition as that perfectly illustrates taxonomy in its stage of transition from where, as at the first, it was almost wholly dependent on vegetative characters, to where it relies almost as entirely upon those of flower alone; and this at a time when as yet the calyx has received but little attention and has not been admitted to the category of floral organs; and the wholly petaloid sepals of *Anemone*, *Sylvia*, and *Pulsatilla* form as good a "flower," *i.e.*, corolla, as do the flower leaves of a rose or pæonia. It is also a stage at which the fruit is not yet accorded the taxonomic weight that was allowed it fifty or sixty years after Cordus by Cesalpino. In the eyes of a twentieth-century botanist the above is a curious medley to be called a genus Ranunculus; and what is more, no one anterior to Cordus had done as badly as that. The remotest Greeks seem to have admitted to Ranunculus no species not thereto referred by most botanists of the nineteenth century. Here there are added to the genus, and admirably indeed, a considerable list of true buttercups unknown to antiquity; but over and above these a trio of representatives of anemoneous genera that differ much among themselves. This kind of a genus Ranunculus, for that period, explains itself readily. It is plain that Cordus has yielded for once to enthusiasm for the newly rising anthology; that he has attempted the abandonment of the old reliance on vegetative characters, and is putting things together more with reference to the structure of the flower. Throughout this series there is always a five-leaved or six-leaved "flower," a circle of indefinitely numerous stamens within that, and in the middle a compact head of many "seeds." If the time had then arrived for the formal statement of the generic characters of such a group, we know that it would have been on this wise that Cordus' Ranunculus would have been characterized by him; to which morphologic diagnosis it would have been appended that certain acrid properties prevade the entire line of species. When I say that we know he would have done this, I have before me this proof, that the clear mention of just such common characteristics forms a part of the description of each leading species. One may take Cordus' separate diagnoses of any one line of related species and cull from them to a certainty that which to his view is the generic character. Not that this is peculiarly true of Cordus; for with equal certainty does one gather out of Brunfels' and Tragus' groups of square-stemmed opposite-leaved axillary-flowered aromatic herbs—or those otherwise vegetatively marked—the characters of their more crudely conceived genera. What distinguishes Cordus is—we must once more insist—

this one rather steadily maintained appeal to floral organs, while for the time quite too much neglecting the vegetative; and also too little heeding very marked differences in the fruits of things; for he is perfectly aware that one of his ranunculi has "seeds" with long feathery tails, that another has them densely woolly-coated and compacted into the closest kind of a head, and that in a third they are rather few, and more like those of a *Thalictrum* than they are like the seeds of buttercups.

On the whole, and as thus studiously looked into, this Ranunculus of Cordus is one of the most significant chapters of taxonomic history ever written; for herein is illustrated as nowhere else the transition from an old taxonomy to a new one. By its making too little use of fruit characters it calls for the carpologically established genera of a Cesalpino[1]; but by the very pronounced corollism of such a chapter Cordus most clearly presages Tournefort.

We shall fall short of a fair comprehension of all that is in this chapter of taxonomy, unless we as carefully consider his disposal of two other types not referred by Cordus to Ranunculus, but so placed as immediately to succeed that genus. These are:

Cordus	Modern
Chelidonium minus	Ficaria ranunculoides.
Chelidonium palustre	Caltha palustris.

These bring the number of ranunculaceous species in this unbroken line up to fourteen. That, in as far as it goes, is very good; yet there will seem quite a glaring inconsistency in the man's having excluded Ficaria from a genus to which he has already admitted, over and above many good ranunculi, three anemones and a pulsatilla. If the last four could go into Ranunculus, why not—and much more easily—Ficaria? There never is in any age any other so ponderous a dead weight upon scientific progress as so-called "authority," and the prejudices it entails. Those particular anemones and the pulsatilla were plants in Cordus' time newly discovered; northern types of which no ancient Greek or Latin authority had ever heard. They hardly yet had well established Latin names, or fixed places in the new books of botany. It was easy for Cordus to name these what he would, and to place them where he would, without risk of seeming to set himself superior to ancient and revered authority. The case of Ficaria was as different as possible. All antiquity had known this plant. From Dioscorides forward

[1] Cesalpino was but four years younger than Cordus, but lived to old age.

through the ages its name had been *Chelidonium minus*. Cordus, as a university lecturer on Dioscorides, by the acuteness and originality of his genius, had made himself famous at the age of twenty-three years. To have given any Dioscoridean plant a new name would have been venturesome. He had been bold enough when, afterwards, in his own *Historia Plantarum* he had indicated the plants' intimate relationship to Ranunculus rather than to true celandine. This, to my mind, seems the explanation of the inconsistency referred to. The inconsistency is, however, more than condoned by the neat item of constructive taxonomy with which it is intimately connected.

I have never been able to comprehend the view point of the professedly natural systematist who fails to perceive a most intimate relationship as subsisting between Ficaria and the plant called Caltha palustris. With Cordus the type last named was new. The German peasantry had always known the plant, and had their several vernacular names for it; but the botanist became convinced that botanically it was unnamed and undescribed; and then, like an accomplished expert in the detection of affinities—a master in taxonomy—locates it close against *Chelidonium minus*, *i.e.*, *Ficaria*, and to accentuate the expression of this relationship, names the new type *Chelidonium palustre*,[1] this to be understood, I doubt not, as a binary generic name, just as *Chelidonium minus* was understood to be; for no one now thought of this as a mere species of the celandine.

Even as to the conception of a species Cordus is so far in advance of his own time as to appear quite abreast of Tournefort, who flourished a hundred and fifty years later. Among the botanical fables that have passed for history, none is more familiar than that Linnæus was first to clearly recognize varieties; but Cordus seems to realize the difference between species and variety as well as if he had lived in the nineteenth century. The instances of his mentioning varieties are not numerous, but they suffice to show that he discriminated them readily enough. The double-flowered buttercup which he calls *Ranunculus coronarius*[2] he describes with the utmost brevity, remarking that it is not a proper species, but is a "factitious" thing of the gardens.

In giving full account of the *Viburnum Opulus* and its ornamental variety the Snowball Bush which appears so different he is careful to say that there is really no difference between the two save this,

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[1] *Hist. Pl.*, p. 122.

[2] *Ibid.*, p. 120^2.

that the latter, instead of having both kinds of flowers, the fertile and small and the sterile and showy, has but the sterile kind. Plainly, he regards it as but a freak, a variety of the other. It is also worth noting that his is the earliest mention in history of this universal favorite, which he says grows wild along with the other, but is rare, except in gardens and pleasure grounds to which they transferred it from its native wilds.[1]

The question of the degree of relationship subsisting between the two kinds of *Dipsacus*, or teasel, has exercised the minds of successive generations of taxonomists early and late. Cordus appears to have settled it at the outset in the right way; or at least in a manner to satisfy the requirements of the modern evolutionary. He almost fills a folio page with his fine description of the original wild and useless *D. silvestris*, and then disposes of the cultivated and singularly useful *D. sativus* in five lines; even these five relating in the main to the mechanical serviceability of the hard, prickly heads; presenting as the only morphological distinction between this and the former, the harder, firmer texture and convenient curvature of the prickle-like bracts investing the head.[2]

A philosophic botanist, writing for the philosophic, need not more explicitly avow his belief that *Dipsacus sativus* is but a usefully variant offspring of *Dipsacus silvestris;* nor need he more clearly express his understanding that the wild thing, being the type of the species, is the thing for the botanist to describe in full.

Illustrations have been given already of Cordus' superior skill in bringing into line related genera, as if members of a natural family; but these taxonomic notes must not be concluded without allusion to one of the most striking manifestations of his ability to segregate, amend, and improve larger groups. Perhaps the best example of all is one that occurs in his early lectures on Dioscorides. The pharmacists of the time have a group of plants which they know as the Lactariæ, that is, milky-juiced herbs. Those best informed understand the species of the euphorbiaceous genus *Tithymalus* to be meant by this name Lactariæ, so says the lecturer; but then, he adds the suggestion that so many other herbs besides these have the faculty of shedding drops of milky juice when their stems are cut or broken, and the milks of these different herbs are so dissimilar as to their properties—some being innocuous, others poisonous—that the plants ought clearly to be distinguished in groups.

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[1] *Hist. Pl.*, p. 190.

[2] On the history of the *Dipsacus* controversy, and its nomenclature, *cf.* Greene, *Pittonia*, vol. iii, pp. 1–9.

"Let us, then, indicate three groups by their (morphologic) differences. Let the first be the Tithymali, the second the Intybaceæ. Then we shall have a third group that is of neither the Tithymali nor the Intybaceæ. We shall now proceed to demonstrate how these two may be distinguished. Tithymalus is a genus of many species every one of which has elongated leaves devoid of any kind of indentation or division. All intybaceous herbs have their leaves more or less erose or incised all around the edges. The greater proportion of the tithymali have an umbellate inflorescence and all of them, even those not umbellate have each flower and fruit subtended by a pair of opposite leaves. But the flower stalklets of the intybaceæ bear each a flower composed of small leaves compacted together, yellow as to color, *Chondrilla minor* and *Cichorium* excepted, in which they are either blue or white. The flowers of intybaceous plants resolve themselves into a kind of wool called a pappus. Never so the tithymali, for all of them have a three-celled fruit with a single seed in each cell. The seeds of intybaceæ with their pappus sail away on the breeze. The tithymali eject their seeds forcibly and with a sound. The milk of the intybaceæ is bitter at first taste. That of the tithymali is at first taste mild, even not so unlike that of cow's milk, but after that it begins to burn the mouth and throat, and if applied to the skin may blister it; which the mild juices of the intybaceæ never do. Now these herbs, which I have thus distinguished by their proper marks from the Intybaceæ, are called the Tithymali and may be known by that general [*i. e.* family] name. To the Intybaceæ belong all the species of *Lactuca*, both kinds of *Chondrilla*, *Intybus sativus*, and *silvestris*, commonly known as *Cichorium*, also *Hieracium*, and whatever other plants are like them [*i. e.* organologically], and have a milky juice.

"My third assemblage of lactiferous plants can not be distributed among these two groups, neither do they of themselves constitute an alliance; for they are not united by any resemblances which they have in common. But they are not very numerous. *Ficus*, *Erinus*, *Scammonia*, and *Cissampelus* are among them; each seeming to belong to an alliance of its own."[1]

Cesalpino, of the end of the sixteenth century, will be praised in future millenniums for having founded Systematic Botany. But had Valerius Cordus lived to only twice his nine-and-twenty years, it is easy to conceive that the great Italian might have missed his laurels.

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[1] Cordus, *Annot. in Diosc.*, p. 74^2, of Cordus' works, edition of 1561.

Nomenclature. Euricius Cordus having demonstrated that many a plant type native to Germany and well known to Germans was entirely unknown to the Greeks and Latins of antiquity, his son Valerius proceeded to name and describe such types, so that they might take their places—so that each might become the subject of a chapter—in books of botany. Valerius Cordus was the first man in history to establish many new genera of plants. I do not know that any one author between Dioscorides and Valerius Cordus proposed more than one or two new genera. This man all at once proposed so very many that his book is a great landmark in the history of genera. What we are here interested in finding out is whether in the naming of his new genera Cordus either tacitly or openly subscribes to any particular principles of generic nomenclature. From what we know of his antecedents as the carefully instructed favorite son of a celebrated Greek and Latin scholar, we shall reasonably expect to find him constructing his new names after classic models; making no inroads upon ancient usage. Let us make a selection of his new names, that the student of nomenclature may observe the manner of their construction.

Cordus	Modern
Anblatum	Lathræa.
Balsamella	Impatiens.
Coralloides	Dentaria.
Drosion	Alchemilla.
Isophyllon	Bupleurum.
Limnesion	Gratiola.
Lycostaphylos	Opulus.
Millegrana	Herniaria.
Moschatella	Adoxa.
Helianthemum	Helianthemum.
Oxycoccus	Oxycoccus.
Pneumonanthe	Pneumonanthe.
Rorella	Drosera.
Sagitta	Sagittaria.
Thamæcnemum	Vaccaria.

It will be seen that by far the greater proportion of these fifteen generic names is regularly Greek-made; some five are as plainly Latin, while one only, and that the first of the list is very barbarous Latin; for *Anblatum* is said to be the old German name *Ohnblatt* (leafless) written over into Latin and supplied with the convenient terminal. It may be remarked that there is not one in this list of fifteen generic names that ought ever to have been displaced. It will hardly be possible to point out one of them which is not as good as its modern equivalent. Yet *Helianthemum* is the only one of the fifteen that never has been set aside; and the suppression of the rest is mainly to be attributed to Linnæus' recklessness in the matter of priority in nomenclature.

While most of Valerius Cordus' new generic names are of the universally acceptable one-worded type, it is clear from the following list that he did not find the binary sort objectionable.

Cordus	Modern
Chelidonium palustre	Caltha.
Chelidonium phragmites	Corydalis.
Hepatica alba	Parnassia.
Pentaphyllum palustre	Comarum.
Trifolium palustre	Menyanthes.

Here again I apprehend a difficulty on the part of many a reader to see that these two-worded names are purely generic; we are so perfectly accustomed all our life long to read every such name as being half generic, half specific; and so I affirm again that not one of those second and adjective terms is a specific name at all. Colloquially, and in our vernacular, we make use of two-worded generic names most freely without thought of impropriety. When we speak of Rose (=*Rosa*), Christmas Rose (=*Helleborus*), Rock Rose (=*Cistus*) and Guelder Rose (=*Opulus*) we have no such thought as that all these are so many different kinds, or species, of Rose. No more had Valerius Cordus any such thought as that the four types which he knew as Chelidonium majus, Chelidonium minus, Chelidonium palustre, and Chelidonium phragmites were four kinds or species, of Celandine. To his mind they were as clearly four distinct genera as their respective modern equivalents Chelidonium, Ficaria, Caltha, and Corydalis are different genera to the mind of the botanist of to-day.

Nevertheless there were people in Cordus' time and before that,—untaught, unbotanical botanists—to whom such binary generic names were a stumbling block; people who supposed that the above four were but so many kinds of celandine, being misled by the reiteration of the substantive part of the name; and Cordus, while himself creating some such double generic appellations, suppresses several of the older ones in favor of new ones of one term; and this as if to correct in each instance a deep-seated popular error. The labiate plant known now as *Glechoma* had long been known in Latin as *Hedera terrestris*, a name which caused it to be thought of as really a kind of ivy. Cordus proposes that botanists shall call it by a new name *Chamæclema*.[1] Similarly what had always been called *Trifolium acetosum*, or sour clover, seeing they really mistake it for a kind of clover, Cordus proposes shall be known as *Oxys*.[2] The genus, by the way, was so known for two centuries after Cordus had proposed it, and until Linnæus without the shadow of a reason for so doing, changed it to *Oxalis*.

In respect to the construction of new generic names Cordus represents the most rigidly classic type of nomenclator in this, that he creates no meaningless names. Every appellator of this kind that he makes is framed in allusion to some characteristic, either organologic, or ecologic, or qualitative, of the type itself, and is therefore full of meaning. The nomenclature of genera in even the remotest antiquity was not universally so; for they had in ancient times the genera *Artemisia*, *Eupatorium*, *Euphorbia*, *Gentiana*, *Pæonia*, *Valeriana*, and several others which, like these, were named in honor of eminent medical botanists. Not one, however, of Cordus' many new genera is dedicated to any person whether historical or mythical. Even Theophrastus, Nicander, Dioscorides, and Pliny were not to be commemorated in generic nomenclature until after the lapse of more than a century and a half from the date of Cordus' death.

As regards the principle of priority, it is to be observed that he stands by it and contends for it only in the case of names that are of great antiquity. For instance, he finds that the name *Eupatorium* has been displaced from its ancient type so that the plant is everywhere in his time known under the name of *Agrimonia*. He will enter into no compromise with this kind of error. While as he says, "Almost all physicians and pharmacists of to-day call it *Agrimonia*,"[3] he declines to head his chapter with that appellation, and writes *Eupatorium* instead, regardless of the convenience of the erring multitude. It is taking boldly the ground that the scientific man must be true to history in his plant naming; and that it belongs to the doctors and druggists to correct their errors according to the light of history. This is nothing less than the most tenacious adherence to the principle of priority; the restoring of an ancient name, where the whole concourse of those in a business way interested will be opposed to the restoration, and he knows it.

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[1] *Hist. Pl.*, p. 161.

[2] *Ibid.*, p. 173.

[3] *Ibid.*, p. 169.

We have observed Brunfelius suppressing so ancient and classic a generic name as *Chelidonium minus* in favor of a new name, *Ficaria*[1]; this presumably on the ground that the latter is short, one-worded, and free from ambiguity. Cordus, as if intolerant of any change that should relegate to synonymy so old a name, continues the old binary in use, not deigning to cite Brunfelsius' *Ficaria* even as a synonym.[2]

Specific nomenclature, as far as Cordus is concerned, remains in *statu quo*. All specific names are binary. There is not in him the trace of a tendency to combine name and description in one phrase. No botanist of the nineteenth century was any further from that than he. But he is no more careful than his contemporaries to avoid the use of those meaningless things, the numeral adjectives, as specific names. He has an even dozen species of *Ranunculus*, and the names of three are *R. octavus*, *nonus* and *undecimus*. The use of these cabalistic appellations, however, perfectly demonstrates that he had no idea of any other than binary names for species in genera that were more than monotypic.

Anatomy and Physiology. Almost every page of original plant description in Cordus bears evidence of his having been accustomed to examine the interiors of organs. We shall hardly expect him to lay any foundations of a science of plant anatomy; for he knows nothing of any artificial aids to vision. The vegetable cell will not reveal itself to him, nor anything else that is too small to be seen with naked eye. But he makes sections of roots, stems, leaf-stalks, fruits of all kinds, and even of seeds, and records the anatomical aspects of them all. Neither Grew nor Malpighi, had he lived at the time of Valerius Cordus, could have done more.

One meets with these records of anatomic and physiologic observation only as distributed up and down the pages of the whole volume and forming part of the regular descriptions of genera and species; and they are so very numerous that one may here reproduce but a small selection of them.

That stemless aquatic, the common European sagittaria, is a large plant noteworthy on account of small size of the leaf-blades and the flowers, in contrast to the great dimensions of the leaf-stalks. The bulk of the plant as a whole consists of mere leaf-stalk. It is certain that by way of enquiry into this matter Cordus has dissected those leaf-stalks; for the first clause of his description of the plant is this: "*Sagitta* has triquetrous petioles, very thin and

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[1] See pages 182, 185 preceding.

[2] *Hist. Pl.*, p. 122.

spongy as to the inside, a yard high or sometimes longer, or shorter, all according to the depth of the lakes in which it grows."[1] In like manner he has examined the interior of the flower stalk in its turn, ascertaining that while smaller than the petioles it is also of "spongy texture, and of remarkably light weight."

The water lilies are plants large in all their parts, so that viewed in cross section they exhibit clearly their structural characteristics, and he notes them fully. What he takes for the main root of *Nymphæa alba* he reports to be as thick as one's wrist lying horizontally along the mud, knotted on at the places where the leaves of former seasons were inserted, are quite black on the outside, but of a clear white within, and of a very spongy and porous substance. The fibres that descend from these into the ground he reports to be outwardly greenish-white, quite white within and also porous. The stalks that support the flowers and leaves in this species he finds to be terete, and to have open tubes extending throughout their whole length within. The yellow water lily rootstock he finds less knotted on the outside, and white both without and within; otherwise like the former, but with coarser fibrous roots and these still more spongy; the petioles, obtusely angled rather than terete, show smaller tubes in the middle.[2]

There was a coarse rank dry land herb, the teasel, *Dipsacus silvestris*, which lent itself as readily to Cordus' anatomical inspection. He says it is of but biennial duration; that its root, of a finger's thickness, during the first year of its life is fleshy; the second it is of a ligneous hardness, and in that condition dies after the plant has flowered and fruited. The tall stem of the plant he remarks is heavy enough to warrant the inference that it is solid; nevertheless he finds it hollow throughout in the very center. Even the large egg-shaped head that bears the flowers and the seeds on its surface he has cut across in each direction to find that within it is filled with what he calls a woolly pith.[3] And this inspection of the interior of stems is made not only in easy and inviting cases, but everywhere. Seldom is there wanting to any of his plant descriptions the clause that tells of the internal structure of its stem, as being solid, or hollow, or as filled in the center with pith; and the color of the pith is also often given. Manifestly the cutting of stems across is as much a part of his work with herbaceous plants as it is to note the exterior of them as terete or angled, smooth or rough, even-surfaced,

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[1] *Hist. Pl.*, p. 86^2.

[2] *Ibid.*, p. 99.

[3] *Ibid.*, p. 105^2.

or striate, or fluted; and he is particular about mentioning every such characteristic. Of still deeper interest are his dissections of all manner of fruits, fleshy and capsular, and of seeds; but on account of the taxonomic significancy of their results, these have been related under another heading.

The intimations of what seemed like a sense of feeling in some mimosaceous foliage had been one of the wonders of plant phenomena as observed by ancient Egyptians and Greeks. Cordus had seen no mimosa, or sensitive acacia, but in two plants frequent in German gardens he observed the foliage remarkably sensitive to varying meteorological conditions. In his very full and minute description of the licorice plant the first remark about the foliage, after having stated its character as a compound leaf, is that its leaflets exude some substance by which they adhere to one's fingers when handled; then he proceeds to describe the several different degrees of enfoldment or of expansion which the leaflets exhibit according to the altitude of the sun on clear days, and how they keep themselves folded together all day long if the sky be cloudy or the weather wet or cold; concluding with the observation that all this is true of the leaflets only, and that the leaf-stalk itself does not alter its position at any time of the day, and that in all kinds of weather its attitude is the same.[1] This last remark is called for by what he had said in an earlier chapter about nyctotropic movement in the common foenugreek.[2] In this he says the nightly folding together of the leaflets is accompanied by a deflection of the whole leaf, petiole and all.

In every description of a twining species of herb Cordus mentions the direction of the circumnutation, whether as following the course of the sun or as taking the contrary direction. Of these phenomena he is the first of all writers to make record, I think. He was the first to describe the plant called sundew,[3] and its physiology interested him. Its description occupies the first chapter of the *Historia*, and as a new genus he would like to call it *Rorella*.[4] He seems to have perfectly established it, that what seems like dew on the leaves of the plant is really an exudation; for he says that in the very driest weather the plant is still sprinkled all over

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[1] *Hist. Pl.*, p. 164².

[2] *Ibid.*, p. 100.

[3] *Ibid.*, p. 86.

[4] Tragus had known *Drosera rotundifolia*, and has it figured as a species of *Polytrichum*, *P. minus* (*Stirp. Comm.*, p. 528). He said its habitat was "dewy rocks," and had no idea that the "dew" on its leaves was an exudation.

with those minute drops that have the appearance of dew. He has done what was in his power to ascertain at least the qualities of those minute glistening drops. He has tasted, and reports the flavor to be a little bitterish, with also a hint of the acidulous, and slightly acrid.

The plant physiologist of to-day, interested in the functions of the root tubercles of leguminous plants may find in Valerius Cordus the earliest mention of these organs. I do not find him taking note of them except as occurring in the cultivated lupine of Europe. Accustomed to give a full account of every kind of root, even to its medicinal usefulness or uselessness, he says of that of the lupine that it is "slender, woody, white and without useful properties, parted into a few slender fibres upon which there sometimes grow small tubercles."[1]

Ecology. We have already been learning that even from the most primitive times every botanist was an ecologist; at least to the extent of observing and recording the special environment which every kind of wild plant affects, and sometimes to the mentioning of some of its associate species. Valerius Cordus, being well skilled in both chemistry and mineralogy, goes beyond all his predecessors in that he names the petrography of a plants' habitat, or otherwise indicates the constituency of the soil in which it is to be looked for. We can in no other way so well present this, his own new aspect of matters ecological, than by the translation of a few of his passages.

The fern called hart's tongue, best known as *Scolopendrium*, but which Cordus knew as *Phyllitis*, he says, "grows on shaded and rocky declivities of mountains; loves a rich soil, though not springing from the soil directly, but from the moss that covers the rocks, especially limestone."[2]

To *Saxifraga Aizoon* he attributes the habitat of "Limestone cliffs, especially where they are wet and overgrown with moss."[3]

Describing two species of *Sanguisorba*, that which he calls *S. major* "inhabits low clayey pasture lands that are subject to inundation from rivers," while *S. minor* also "grows in clayey soil, or even gravelly, but on open sunny slopes and along roadsides."[4]

He seems to take a special interest in the ecology of such plants as he has himself first discovered and described as new. The

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[1] *Hist. Pl.*, p. 137.

[2] *Ibid.*, p. 113.

[3] *Ibid.*, p. 92.

[4] *Ibid.*, p. 144.

cranberry is one of these. He says he found it growing "in wild and very wet and soft mossy bogs; often with the sundew not far away."[1] Then, under his description of the sundew itself—also new—we learn that he found that, though as a neighbor to the cranberry, yet in soil distinctly of another character, that is, "in very wet sandy places."[2]

In the case of that new generic type which he denominates *Moschatella* (=*Adoxa*, Linn.) he mentions its most interesting associates. "It grows in shady places, under trees, in soil very rich, along with the fumariaceous kind of Celandine (=*Corydalis cava*) and also the ranunculaceous Celandine (=*Ficaria ranunculoides*[3])."

Cordus knows at least one specific type which, as he observes, has a way of establishing itself upon some diversity of soils, and adapting itself to several different kinds of locality; and the phases which it assumes according to its altered environment so much interest him that he gives a particular account of them. The subject is the shrub *Spartium scoparium*.[4] He says: "It inhabits rough places on mountains in a hard reddish soil somewhat sandy, as in Hesse, etc.; but sometimes occurs on the lowest plains in mere sand and gravel, as about Nuremberg, etc. Nor should it be left unmentioned that while on sandy plains it is a low bush seldom exceeding a yard in height; on the mountains, where the soil is better, it approaches the dimensions of a tree, with a trunk from seven to nine feet high and so thick that one can not span around it, supporting a head of virgate branches so dense as to intercept and hold all the snow of a considerable storm, so that the traveller, passing through such a wood in winter, may walk on almost bare ground under arches of snow overhead."

There is no indication that the author would distinguish even as varieties these rather strikingly different phases of the shrub. He regards them as the natural products of different conditions as affecting a simple species. It is the well skilled botanist's view, whether of the sixteenth century or of the twentieth.

Pomology. The recognition of marked varieties in the same species of cultivated fruits—varieties originating under cultivation—is so very ancient that there is no hope of one's ever tracing it to

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[1] *Hist. Pl.*, p. 140.

[2] *Ibid.*, p. 86.

[3] *Ibid.*, p. 172^b.

[4] *Ibid.*, p. 189, as *Genista angulosa;* where the editor, Gesner, made the inexcusable error of inserting Tragus' wood cut of the extremely different *Genista sagittalis*.

its beginnings. Also before the dawn of history men had learned that valued varieties of fig, olive, grape, and other fruits could not be depended on to come true to seed. Seedlings of these were apt to prove degenerate, as they called it; and the propagating of them by layers, and especially by grafting, had been invented as the sure means of preserving and perpetually reproducing choice varieties.

They who wrote of fruit culture two thousand years ago and more mention by name great numbers of varieties, not only of fig, olive, and grape, but also of peaches, cherries, and other fruits; sometimes favoring the reader with a few hints of the differences subsisting between two varieties; but I have met with nothing like descriptive lists of the varieties of even such common and variable fruits as figs, olives, and grapes, in the writers of antiquity; nothing that was written for the purpose of enabling the reader to identify the varieties. I can not discover that any one anterior to Valerius Cordus, engaged in this kind of an enterprise.

There are long chapters in Cordus' book which so read as to make it certain that in the course of his botanical expeditions to many parts of Germany, as well as at home, he made everywhere very special studies of the different varieties of apple and pear which were under cultivation in the orchards of the time, and that he wrote a careful description of each on the spot, and that so full that the properly qualified reader would be able to indentify the different kinds by the description alone. I say the qualified reader, meaning of course the educated; for every line of Cordus' pomologic writing, like all the rest of his botany, is in Latin; and a knowledge of the Latin terminology of descriptive botany is essential to the full understanding of him here.

With the intention, then, of interesting the botanical fraternity in this diversity of cultivated varieties, he describes as many as fifty named varieties of pears, and thirty-one of apples, all of which he has found in one part or another of Germany.[1] The original German names are always given, then the name is turned into Latin as if for the convenience of the botanists, all of whom in the time of Cordus, like the other educated people, find Latin the only adequate medium of scientific converse. The excellency of these pomologic diagnoses can more readily be seen than described, and I therefore present English translations of two or three of them; and since it is his practice to describe one variety partly by comparison with another, I shall take up three descriptions that are consecutive.

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[1] *Hist. Pl.*, vol. iii, pp. 176–182.

Also inasmuch as most of the pears which one sees in the fruit marts of to-day are quite pear-shaped, I shall begin with one which Cordus knew that was globose as an apple.

"KAULBIRN, that is, Globe Pear, is almost as round as a globe, except that at base it ends in a blunt and scarcely manifest protuberance. It is hardly two inches long, seldom at all exceeding that, and the diameter is a trifle greater than the length. The color is altogether pale green; the flesh very tender, melting in the mouth, and of a mild delicious flavor, and by its abundant juiciness satisfying thirst, delightfully fragrant when pared. This matures at the beginning of autumn, and is very perishable. It is cultivated about Eisleben.

"HAUFFBIRN, that is, Hemp Pear, is similar to the Globe, but a little larger, in color green, with darker spots and dots; in flavor answering to the Globe, but the flesh not as tender and juicy; matures at the same time and soon decays. This also is grown at Eisleben.

"GLOCKENBIRN, that is, Bell Pear, is abruptly narrowed below into a narrow neck, this again at the very base widening into a blunt head-like protuberance, the fruit as a whole having the configuration of a bell,[1] whence it has its name. The color is yellow, well specked with green, the length a trifle less than three inches, the diameter not more than two inches. Rather fragrant when pared: the flavor that of the Hemp Pear; matures at the same time with that and is perishable. Grown in quantities at Eisleben.

"KÜNIGSBIRN, that is, Royal Pear. Large and ventricose, sometimes four inches long, the diameter somewhat less; color blue-green, but on the side exposed to the sun faintly reddening. Flavor a trifle astringent, flesh somewhat juicy and vinous, assuaging thirst. Matures at the end of September and is not very perishable."

It is perhaps less difficult to describe apples well, than pears; and all those familiar to Cordus are very vividly depicted. We present two or three:

"HARTLINGE WEISS, that is, White Harding. Somewhat depressed-globose, the height about two inches, the diameter two and a half or somewhat more. Colored reddish-yellow on the sunward side, elsewhere greenish-white, dotted with specks that seem to lie beneath the transparent epiderm. Flesh tender though firm, juicy, of an acid-vinous and excellent flavor; the fruit fragrant

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[1] That is to say, a hand bell, with its handle.

for a considerable period after having been stored. Ripens in early autumn and keeps until the end of winter.

"Grown almost throughout Germany.

"SAFFRANECKE. Saffron Apple. In quality scarcely second to any apple, this is also nearly globose and but an inch and a half in diameter, the breadth not rarely a little exceeding the length. Color something between green, yellow, and pale saffron; in warm and dry seasons adorned with dots and narrow streakings of scarlet and orange; in moist and cloudy summers devoid of any such markings. Flesh tender, sweet, slightly acidulous, luscious, and with a kind of spicy pungency which commends it to any palate. Moreover, it is delightfully fragrant. Begins to ripen in early autumn, and keeps until the vernal equinox, and frequently even longer.

"Cultivated at Hildesheim, particularly in the orchards of the monks at the monastery in Sulta, located outside the city.

"HARTLINGE ROT. Red Harding. Either much flattened at the ends and orbicular, or now and then quite globose; in size also as variable, but on the average perhaps about two inches high, and two and a half in breadth. Color rose-red, marked with long blood-red stripes. Flesh rather solid and dense, yet very tender, juicy, acidulous, and with a peculiarly luscious flavor. Rather pleasantly scented. Ripening in early autumn, and the most enduring of all apples, sometimes keeping until the middle of the next summer, and, according to the statements of some, even longer than that.

"It is found in almost all orchards of nearly every part of Germany."

I apprehend that a skillful pomologic artist should be able to draw and color these apples and pears so vividly word-pictured by this perhaps the very first of descriptive pomologists, and for the history of pomology it may be doubted that there are extant any more important chapters than these written by Valerius Cordus three hundred and seventy years ago.

Commemorating Valerius Cordus, Plumier established the genus *Cordia* in the year 1703.

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DEVELOPMENT OF THE BRAIN OF THE AMERICAN ALLIGATOR:

THE PARAPHYSIS AND HYPOPHYSIS

WITH FIVE PLATES

BY

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DEVELOPMENT OF THE BRAIN OF THE AMERICAN ALLIGATOR: THE PARAPHYSIS AND HYPOPHYSIS[1]

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The Paraphysis

The literature of the pineal region of vertebrates is remarkably extensive. A considerable portion of this literature deals with the much-debated relation of the epiphysis to the pineal eye or parietal organ. It is not the purpose of this paper to enter into that discussion, for the simple reason that these structures are not present in the alligator.

O. Hertwig (22)[2] says that, except in Amphioxus, the pineal gland is not wanting in vertebrates; and Wiedersheim (54) says: "The pineal apparatus consists of the epiphysis or pineal organ proper, which persists in a more or less rudimentary condition in all vertebrates, and a more anterior outgrowth which may be called the parietal organ."

It is not surprising, then, that various authors should have described the similarly situated structure in the alligator as the epiphysis.

Parker and Haswell, in their "Text-book of Zoology," figure 947 (from Wiedersheim), show, in a dorsal view of the brain of the alligator, a structure which they call the epiphysis.

C. L. Herrick (20) mentioned, though he did not describe in detail, the epiphysis of the brain of the alligator.

The present writer (42), in his previous paper dealing with the

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[1] The present paper is one of the results of special researches in continuation of my paper on "The Development of the American Alligator," published in 1908 in Smithsonian Miscellaneous Collections, Vol. 51, 66 pages and 23 plates, in which there was given a general outline of the whole process of development of the American alligator. There was also published in the same series, Vol. 48, pp. 381-387, an article on "The breeding habits of the Florida alligator."

[2] The numeral citations in this paper are to bibliographical references at the end of the paper.

general features in the development of the alligator, described what he then supposed to be the epiphysis.

Voelzkow (51), whose paper the writer had not seen at the time of the publication of his above-mentioned paper, quotes Sorensen in the statement that, in the alligator, the paraphysis had been wrongly taken for the epiphysis. In this paper Voelzkow says that the epiphysis is absent in both the Madagascar Crocodile and the Caiman. His figures show conditions very similar to those to be described in the present paper.

For obvious reasons sagittal sections were chiefly used in studying the development of the paraphysis and hypophysis, though sections cut in other directions were used to some extent.

The earliest stage in which any sign of the paraphysis is seen is shown in figure 1, a sagittal section of an embryo of about 7 mm. length, though the exact size, owing to the marked body flexure, could not be determined. At this stage there is not much difference in the thickness of the walls of the brain, in the different regions, except for the marked thinness of the roof of the hind-brain. A ventral depression, *v.*, of the dorsal wall of the fore-brain is the beginning of the velum; a wide arch, *p. a.*, just anterior to this marks the position of the paraphysis, and may be called the paraphysal arch. Posterior to the velum the roof of the fore-brain is slightly arched to form the beginning of what may be called the post-velar arch, *v. a.* (Minot), the dorsal sac or zirbelpolster of other writers. An indistinct thickening, *p. c.*, of the dorsal wall marks the posterior limit of this arch, and also the boundary between the fore- and mid-brains; it is the future posterior commissure.

Ventral to the brain are seen the anlage of the hypophysis, *h.*, to be described later, and the vacuolated notochord, *ch.* At this stage of development the brain cavity is very wide, dorso-ventrally, in proportion to its antero-posterior length.

Figure 2 represents the anterior end of an embryo of about 10 mm. length. It will be noticed that the brain is much longer, in proportion to its depth, than in the preceding figure, and that the cranial flexure is more marked. The section passes almost exactly through the median plane of the roof of the brain, but a little to one side of this plane in the region of the hind-brain, so that the notochord is not shown.

The fore-brain, *f. b.*, is beginning to push forward to form the cerebral hemispheres, *c.*, and a marked fold, the cerebellum, *cb.*, is seen between the mid- and hind-brains. The hypophysis, *h.*, cut slightly to one side of the median plane, has increased in size and complexity; it will be described later.

The posterior commissure, *p. c.*, is somewhat more distinct than in the preceding stage, and the post-velar arch, *v. a.*, is distinctly differentiated, partly because of the increasing thinness of its wall and partly because of the marked increase in the size of the velum, *v.*, which now projects into the cavity of the fore-brain as a heavy transverse ridge. As seen in this plane, the velum has two lower angles, a more acute one projecting ventrad and caudad, and a thicker, more obtuse angle, projecting forward under the paraphysis.

Instead of the wide, flat arch, seen in the preceding stage, the paraphysis is here a distinct, saccular diverticulum of the fore-brain, with whose cavity it connects by a wide opening.

A section cut laterad to the paraphysal opening is shown in figure 2a. Here the paraphysis is seen as a separate, circular cavity, *p.*, lying in the mesoblast between the wall of the fore-brain and the superficial ectoderm. It is not strange that, when seen in such a section, the paraphysis should have been taken for the epiphysis, because the marked forward projection of the post-velar arch, *v. a.*, gives the impression that it and not the arch anterior to the velum is connected with this paraphysal vesicle.

In this figure the paraphysis has thick walls, with a denser layer of nuclei towards the central cavity. The velum, *v.*, has here but one angle, that projecting caudad, the other angle is now continuous with the roof of the fore-brain, beneath the paraphysis.

The infundibulum is seen as a wide, shallow depression, *in.*, in the floor of the fore-brain. The plane of the section, being more nearly median in the posterior region, cuts the side of the notochord, *ch.*, at its extreme anterior end.

Figure 3 represents an embryo somewhat older than the one represented in figures 2 and 2a. The brain is considerably more elongated than in the preceding stage, and the cranial flexure is more marked.

In the region of the hind-brain the section is exactly median, so that the notochord, *ch.*, and hypophysis, *h.*, are cut through the median plane. In the region of the paraphysis, *p.*, the section is to one side of the median plane, and the opening of that structure into the fore-brain is not seen.

The velum and paraphysis are about the same as in the preceding figure, except that the latter, in its median region, has thinner walls than in the earlier stage. The greatest change is in the post-velar arch, *v. a.*, which is now much more sharply defined, mainly because its dorsal wall is much reduced in thickness. This thin roof of the post-velar arch gradually increases in thickness as it passes into the velum, but its transition into the posterior commissure, *p. c.*, is now very sudden, so that the latter structure is more distinct than in the preceding figure.

Figure 4 represents an embryo 2 or 3 mm. longer than the one just described. The section, which was drawn under somewhat lower magnification than was figure 3, is almost exactly median in position, and hence does not pass through the lateral ventricles.

The cerebellum, *cb.*, and posterior commissure, *p. c.*, are more distinct in outline, the distinctness of the latter being due to the upward curvature of its anterior region, and to its now sharp differentiation from the thin roof of the post-velar arch.

The roof of the post-velar arch is thinner than in the preceding stage, and its posterior portion, where it becomes continuous with the posterior commissure, is, by the above-mentioned upbending of the anterior edge of the commissure, carried suddenly upward as a transverse furrow, seen in this figure just anterior to the posterior commissure.

The velum, *v.*, is thinner in an antero-posterior direction than before, and contains, in this region, two or three small vesicles, lined with distinct cuboidal epithelium. Each cell of this epithelium contains a large, spherical nucleus.

The paraphysis, *p.*, is seen as a large, thin-walled sac, connected by a very wide opening with the fore-brain.

A plane twenty-four section laterad to the one shown in figure 4 is shown in figure 4a. Here the posterior commissure, *p. c.*, and post-velar arch, *v. a.*, have about the same appearance as in figure 4; but the paraphysis, *p.*, which is cut laterad to its opening, is seen as a small circular cavity with thicker walls than were shown in the median section. The velum, *v.*, shows the most marked change over the earlier stages, and projects forward as an irregular mass into the lateral ventricle.

The paraphysis, at this stage, is a large, thin-walled vesicle, opening by a wide mouth into the fore-brain just in front of the velum. It is somewhat compressed, laterally, and, in some cases, lies more on one side of the median line than the other. The width of its opening is usually about one-third of the width of the entire vesicle, while the antero-posterior diameter is at least one-half the antero-posterior diameter of the entire vesicle.

Figure 5 represents a sagittal section of a much older embryo than the one shown in figures 4 and 4a, though the condition of the paraphysis is not very different from the description just given. The greatest diameter of the head, from the tip of the snout to the roof of the mid-brain, is about 9 mm.

The outline of the head is beginning to assume the reptilian form, largely because of the growth of the mandible, *md.* The nasal cavity, *n.*, opens to the pharynx through the posterior nares, *p. n.*, though this connection is not shown in the figure. The cerebral hemispheres, *c.*, are large, thick-walled structures, while the infundibulum is now relatively smaller than in the preceding stage.

The posterior commissure, cerebellum, etc., are not very different from what has been described.

The post-velar arch, *v. a.*, has changed considerably; it is now higher and less wide, in an antero-posterior direction, than in figure 4. Although the length of the head has increased 50 per cent over the preceding stage, the actual diameter of the arch, from the velum to the posterior commissure, is less than is shown in figure 4.

The paraphysis, *p.*, also shows signs of having been compressed in an antero-posterior direction. The lateral diameter of its opening is still about one-third of the width of the entire structure, while the antero-posterior diameter of the opening is only one-fifth of the antero-posterior diameter. The walls of the paraphysis, as well as the roof of the post-velar arch, are somewhat thicker than they were in the preceding stage, and consist, apparently, of simple columnar epithelium, though the exact character of the cells could not be determined because of poor fixation. The paraphysis, which is now more tubular than spherical in outline, is slightly inclined caudad instead of having a slight inclination cephalad, as in the preceding stage. As will be seen, this inclination of the paraphysis away from the cerebral hemispheres becomes more marked in later development.

The velum, *v.*, has undergone marked development. In the median plane, as shown in figure 5, it is seen as a somewhat arched band of tissue forming the posterior border of the paraphysis. In this plane are seen in the velum the two or three vesicles, *vs.*, noted in the preceding figure. These vesicles are not connected with either paraphysis or fore-brain. When followed laterally the velum is seen to expand to form, on each side, a much branched structure, the plexus of the lateral ventricle, figures 5a and 5b, *c. p.*

Figure 5a represents a section laterad to the one just described. It passes through the median edge of the choroid plexus, *c. p.*, just described, and through the lateral portion of the paraphysis, *p.*, whose walls appear thick merely because cut tangentially.

Figure 5b is a section of a somewhat older embryo than the one shown in figures 5 and 5a. It is about, though not exactly, in the plane a-b of figure 5. It shows the laterally compressed paraphysis, *p.*, and in each lateral ventricle, *c.*, the much convoluted plexus, *c.p.*

Figure 6 shows the paraphysal region of an embryo of about 7 cm.

length. This embryo is several times the length of the one represented by figure 5, and has practically the external form of the adult, even the pigmentation of the skin being partially formed. In spite of this great advance in general development, about the only change in the paraphysis, *p.*, is a slight increase in length. Its walls are still practically smooth and unconvoluted; they are composed of a simple columnar epithelium.

The velum, *v.*, has, in this median plane, nearly the same appearance as in figure 5; in it are seen four or five of the same small vesicles, *vs.*, lined with cuboidal cells, that were seen in figures 4 and 5. The prolongations of the velum into the lateral ventricles, the choroid plexuses, as seen in more lateral sections, are now large and much convoluted.

The post-velar arch, *v. a.*, is much reduced in extent, so that the distance between the posterior commissure, *p. c.*, and the velum, *v.*, even in this much larger embryo, is only about two-thirds of the corresponding distance in figure 5. A greater part of the roof of the post-velar arch is considerably thickened and is somewhat convoluted.

The brain cavities are all relatively and actually smaller than in the preceding stage, and their diminution in size is made more evident by the increased thickness of the brain wall.

The oldest embryo studied was one of about 13 cm. length, nearly twice the length of the one shown in figure 6. This embryo has practically the adult form, and is fully pigmented, so that it is likely that the paraphysis has here approximated more or less closely its adult condition.

In spite of the great increase in the size of the brain over the preceding stage, there is very little change in either the size or form of the paraphysis; it is slightly longer, but it curves back over the post-velar arch in the same way as is shown in figure 6. Its walls are of the same character and are neither more nor less wrinkled or folded than in this figure. Owing either to its increase in length or to a change in the relative positions of parts of the head the tip of the paraphysis is somewhat nearer the dorsal surface of the head; it is, in fact, in contact with a dense layer of connective tissue which will form the roof of the skull.

The velum is somewhat longer than in the preceding stage and has rather more numerous vesicles than were there seen. It is, perhaps, partly to this increase in length of the velum that the increase in the length of the paraphysis is due.

The post-velar arch is of about the same size and outline as in figure 6; its dorsal wall is, perhaps, slightly more deeply wrinkled.

The Hypophysis

The hypophysis is first seen in the alligator in embryos of about 7 mm. length. In this animal, at any rate, there is no reason for doubt as to whether it be derived from the ectoderm or the entoderm. As may be seen in figure 1, *h.*, the hypophysis originates as an invagination of ectoderm close under the floor of the fore-brain. The superficial ectoderm gradually thickens as it passes under the fore-brain, and in the region of the invagination it is several times as thick as in other regions of the body. Followed towards the pharynx it gradually thins out again as it becomes continuous with the entoderm of the fore-gut.

Gaupp (17) says that in lizards and perhaps in all reptiles the hypophysis has a three-fold beginning: A large, round middle part, and two lateral parts, pushed in from the epithelium of the mouth cavity. In the alligator this three-fold origin cannot be made out. The conical invagination seen in figure 1 gradually diminishes in depth and width as it is followed laterad, until it disappears, but no lateral pouches are seen.

The general ectoderm of the body is composed of a single layer of cuboidal or even flattened cells, while the walls of the hypophysal invagination are made up of a single layer of long, narrow, columnar cells with distinct nuclei.

Just caudad to the hypophysis is seen in figure 1 an indefinite and smaller invagination of the thickened epithelium, *p. s.;* this may represent what has been called, in other forms, the pharyngeal sac. Bawden (2) says that in the duck the pharyngeal sac appears earlier than the hypophysis, reaches its maximum development in five days, and soon disappears. It lies between the hypoblast and the notochord, with the latter of which it is connected.

In the present figure no connection between the pharyngeal sac and the notochord, *ch.*, is to be seen, unless it be represented by the slight condensation of the mesoblast that extends from the tip of the notochord to the inner surface of the sac.

Figures 2 and 2a represent the condition of the hypophysis in an embryo of about 10 mm. length. The general topography of the head has been described in speaking of the paraphysis of this stage.

Figure 2 is laterad to the exact median plane. It shows the main invagination of the hypophysis at the end of the reference line, *h.*, caudad to which are several wrinkles of the thickened epithelium. Between the invagination just mentioned and the floor of the infundibulum, *in.*, are two small, oval, compact masses of cells which are lateral branches of the main invagination or stalk, as it may be called. These lateral branches are nearly solid, at this stage, though the cavity of the hypophysal stalk may be traced, in sections mediad to this, for a short distance into them, as a very narrow slit.

Figure 2a is through a more nearly median region of the hypophysis, as may be seen by the section of the notochord, *ch.,* that is shown. The region of thickened epithelium is more extensive than in figure 1. The hypophysal stalk is at the end of the reference line, *h.,* caudad to which are seen three or four wrinkles in the epithelium. The most posterior of these wrinkles, *p. s.,* is much deeper and more distinct than the others, and probably represents the pharyngeal sac, though no connection whatever may be seen between it and the notochord.

In figure 3 is represented a section through the median region of the hypophysis, *h.*, now considerably more developed than in the preceding stage. The actual depth of this median invagination or stalk is about 0.3 mm., or about one-twelfth of the greatest length of the head. The greatest width of the hypophysis is about equal to the depth of the stalk, 0.3 mm. The lateral, inside diameter of the stalk is about 0.2 mm.; the antero-posterior diameter is about 0.12 mm.

On each side of the stalk are two lateral diverticula, seen better as a horizontal section, to be described later, figure 5b. Of these the pair nearer the notochord are the larger and are directly continuous with the inner end of the stalk; the other and smaller pair open into the outer, lateral angles of the stalk near the opening of the latter structure.

Just caudad to the stalk of the hypophysis is a distinct invagination of the epithelium, *p. s.,* the pharyngeal sac, which shows no connection whatever with the notochord.

The walls of the hypophysis have become so much thicker, and the basal wall of the infundibulum has become so much thinner that now the former wall is, in places, thicker than the latter.

Figure 4b is a sagittal section of an embryo of 13 mm., "crown-rump" measurement, if such a term be here permissible. The hypophysis has made considerable progress in development and has increased somewhat in size over the last stage; its greatest lateral diameter is slightly less than 0.5 mm.; its greatest antero-posterior diameter is practically the same.

The shape of the head has changed in several particulars. The infundibulum, *in.,* is a more definitely outlined depression in the floor of the fore-brain, and the roof of the mouth or pharynx, whichever it may be called, extends back of the stalk of the hypophysis instead of ending in the hypophysal invagination as in the last stage.

The most important change in the hypophysis itself is the complete closure of the wide opening to the exterior, seen in figure 3. The walls of the original opening are still distinct from each other, though they are in close contact (fig. 4, *h. s.*), and form a sort of solid stalk, the hypophysal stalk. The oral epithelium is still continuous with the hypophysal stalk, but it is thickened for only a short distance on either side of it.

The closure of the hypophysal opening has taken place in such a way that the stalk is at right angles to the long axis of the hypophysis proper. As seen in the present figure, the hypophysis is a hollow body of irregular outline, with thick, dense walls. The cavity consists of a central region (fig. 4b, *h.*), and three outgrowths, the largest, *o.*, extending back till it nearly reaches the notochord, *ch.*; the second, *o'.*, extending in the same direction from the base of the hypophysal stalk; and the third, *o''.*, extending towards the floor of the infundibulum, *in.* As the sections are followed laterad these three outgrowths are found to extend for some distance on each side of the body of the hypophysis. The outgrowth *o'.* is the largest and remains unbranched; it is hollow throughout. The outgrowth *o''.* is also hollow and unbranched; it is the shortest of the three. The outgrowth *o.* extends laterad for some distance, and then divides into three nearly solid outgrowths which extend nearly as far as the outgrowth *o'.*

Figure 4c represents a section of an embryo of approximately the same size as the one under discussion. The plane of the section is nearly that of the broken line in figure 4b.

The section shows one of the outgrowths, *o.*, on each side of the median cavity, *h.*, the one on the left being cut tangentially so that its cavity does not show. The larger outgrowth, *o'.*, is seen on either side of the median cavity, just above the hypophysal stalk, *h. s.*

The hypophysis at this stage, then, is a completely closed vesicle of irregular shape, consisting of a central cavity with three main diverticula on each side, and a solid stalk connecting it with the oral epithelium.

The pharyngeal wall, back of the hypophysis, exhibits several well-marked wrinkles; one of these, shown at *p. s.*, in figure 4b, may represent the pharyngeal sac, though it differs very little from any other of the wrinkles. In this sac the epithelium, which is somewhat thickened, almost touches the mesoblast that surrounds the notochord, *ch.* No connection whatever can be seen between this invagination and the notochord.

Figure 5, as was stated in connection with the paraphysis, represents a much later stage of development than the preceding. The hypophysis is here cut laterad to its median plane, so that its stalk is not shown. The lateral outgrowths from the median body are now more numerous, more than a dozen being cut by the plane of this section. Some of these outgrowths still exhibit a small, circular lumen, while others are solid. The outgrowths lying next to the infundibular wall are crowded very close together, while those nearer the pharynx are separated by considerable connective tissue.

No sign of the pharyngeal sac is seen in this section.

Figure 5c is through the median plane of the hypophysis. The hypophysal stalk, *h. s.*, is here cut throughout its entire length; its connection with the oral epithelium is still evident and complete, but its lumen has entirely disappeared. The stalk, though narrower than in the preceding stage, is more than twice as long, and is slightly bent towards an S shape. A considerable cavity, *h.*, is still seen in the body of the hypophysis, and two of the outgrowths, *o.* and *o′.*, are evident. The position of the outgrowth *o″.* is seen as a small, nearly solid projection close under the wall of the infundibulum. Just to the right and below (as seen in the figure) the outgrowth *o″.* is seen a larger mass, *o‴.*, elongated in outline and with a narrow lumen (a small, round hole in this section). Although in close contact with the front wall of the body of the hypophysis, this thick-walled vesicle seems to have no direct connection, at this stage, with the rest of the hypophysis.

The greatest length of the body of the hypophysis is now about 0.75 mm., an increase of 0.25 mm. over the preceding stage. Owing to a failure to record the thickness of the sections of this series the width of the hypophysis could not be determined.

A well-marked invagination, *p. s.*, of the pharyngeal wall, a short distance back of the hypophysal stalk, may represent the pharyngeal sac.

From the floor of the infundibulum, *in.*, a deep, narrow pit, *in′.*, projects down into the body of the hypophysis. The bottom wall of this pit is in close contact with the cells of the hypophysis, perhaps continuous with them, but no opening from infundibulum to hypophysis can be made out.

Figure 6a shows the condition of the hypophysis in an embryo of about 6 cm. length. The stalk, *h. s.*, is very long and still more slender and curved than in the preceding stage; it connects with the surface at the base of a backwardly projecting fold of skin, the eustachian valve, *e.*. The stalk not only shows no sign of a lumen, but is actually discontinuous at a point near its middle region.

The body of the hypophysis is little if any larger than in the preceding stage; its cavity has almost completely disappeared, being seen only as a narrow slit in one or two regions.

The number of lobes or branches of the hypophysis has increased considerably, and, under the low magnification used in the present figure, they appear as a collection of irregular, granular masses, separated by narrow lines of connective tissue. Higher magnification shows these masses to be composed of small, densely stained cells resembling lymphoid tissue.

The infundibular pit, *in'.*, does not show such intimate connection with the cells of the hypophysis as was noted in the preceding stage. It is, however, more distinctly separated from the infundibulum proper by a lateral narrowing of its mouth, so that, in sections on either side of the one represented in figure 6a, the pit is seen as a separate, circular vesicle, distinct from the infundibular wall and lying among the lobes of the hypophysis.

No sign whatever of the pharyngeal sac is to be seen in this embryo.

Figure 7 represents the hypophysis of the 13-cm. embryo described in connection with the paraphysis. The hypophysal stalk, *h. s.*, is here reduced to a slightly curved stump in connection with the main body of the hypophysis; its connection with the mouth is completely lost.

The body of the hypophysis still shows a narrow, slit-like lumen, *l.*, on the side below the infundibular pit, *in'*.

The mass of lobules making up the hypophysis is more compact, in this median region, than in the preceding stage, so that the connective tissue septa are almost invisible, and the lobules seem to have fused together to form larger lobes. The appearance of the hypophysis under higher magnification is not different from what was seen in the preceding stage.

The infundibular pit, *in'.*, has, in this figure, about the same appearance as in figure 6a, but, in sections cut laterad to the present one, the pit is found to have a branched lobe on each side. Each of these lobes is seen in parasagittal sections as two circular, thick-walled vesicles lying among the lobules of the hypophysis, entirely distinct from the ventral wall of the infundibulum proper.

SUMMARY

The paraphysis in the alligator has long been mistaken for the epiphysis, the latter structure being entirely absent.

The paraphysis is first seen in embryos of 7 mm. length, as a wide evagination of the roof of the fore-brain, just cephalad to a transverse fold, the velum. This evagination early becomes partially constricted off from the brain and forms a rounded, hollow mass connected with the diencephalon by a wide stalk. As growth proceeds the paraphysis becomes elongated until, in embryos of 7 cm. length, it is seen as a tubular structure, with nearly smooth walls, slightly curved away from the cerebral hemispheres and over the top of the diencephalon. In embryos of 13 cm. the paraphysis has practically the same structure as in the 7 cm. embryo.

The velum grows forward into each lateral ventricle to form its choroid plexus.

The hypophysis in the alligator begins, at about the same stage as does the paraphysis, as a single, median evagination of the roof of the mouth, just beneath the floor of the infundibulum. The original evagination becomes the stalk of a considerably branched, hollow structure which, by the lengthening of the stalk, recedes to some distance from the roof of the mouth. The stalk becomes solid and finally loses all connection with the oral epithelium. The body of the hypophysis also becomes almost completely solid, in an embryo of 13 cm., and is seen as a lobulated mass of lymphoid tissue lying close under the floor of the infundibulum.

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EXPLANATION OF FIGURES 1-7, PLATES 1-5.

All figures were drawn under a camera lucida.

Fig. 1. A sagittal section through the head of a 7 mm. embryo, to show the beginning of the paraphysis and hypophysis. Objective ⅔ inch; ocular 2 inch.

Figs. 2, 2a. Sagittal sections of the anterior region of a 10 mm. embryo. In Fig. 2 the section passes through the median plane of the paraphysis, but slightly laterad to the median region of the hypophysis. In Fig. 2a the section cuts the hypophysis medially, and the paraphysis to one side of the median plane. Objective 2 inch; ocular 1 inch.

Fig. 3. A sagittal section of an embryo somewhat older than the one represented in Figs. 2 and 2a. The hypophysis is cut medially, while the paraphysis is cut somewhat to one side of its median plane so that its connection with the fore-brain is not shown. Objective 2 inch; ocular 1 inch.

Fig. 4. A sagittal section of the anterior region of an embryo of about 13 mm. length. The paraphysis is cut sagittally, while the hypophysis is cut through its lateral edge. Objective 2 inch; ocular 2 inch.

Fig. 4a. A parasagittal section through the dorsal part of the head of the same embryo represented in the preceding figure. The paraphysis is seen as a distinct vesicle lying between the fore-brain and the superficial ectoderm. The forward growth of the velum into one of the lateral ventricles is also shown. Objective 2 inch; ocular 2 inch.

Fig. 4b. A sagittal section through the hypophysal region of an embryo of the same stage as the one shown in Figs. 4 and 4a. Objective ⅔ inch; ocular 1 inch.

Fig. 4c. A horizontal section through the hypophysal region of an embryo of about the same age as the ones shown in Figs. 4, 4a, and 4b. The approximate plane of this section is indicated by the broken line in Fig. 4b. Objective ⅔ inch; ocular 1 inch.

Fig. 5. A sagittal section of a much older embryo than the ones shown in Figs. 4 to 4c. The paraphysis is cut almost exactly medially, while the hypophysis is cut laterad to its median plane. Objective 2 inch; ocular 2 inch.

Fig. 5a. A sagittal section of the paraphysal region of the same embryo shown in Fig. 5. Objective ⅔ inch; ocular 2 inch.

Fig. 5b. A horizontal section through the paraphysal region of an embryo of the stage represented by Figs. 5 and 5a. The approximate plane of the section is indicated by the broken line a-b, in Fig. 5. Objective 2 inch; ocular 2 inch.

Fig. 5c. A sagittal section through the hypophysal region of the same embryo shown in Figs. 5 and 5a. Objective ⅔ inch; ocular 2 inch.

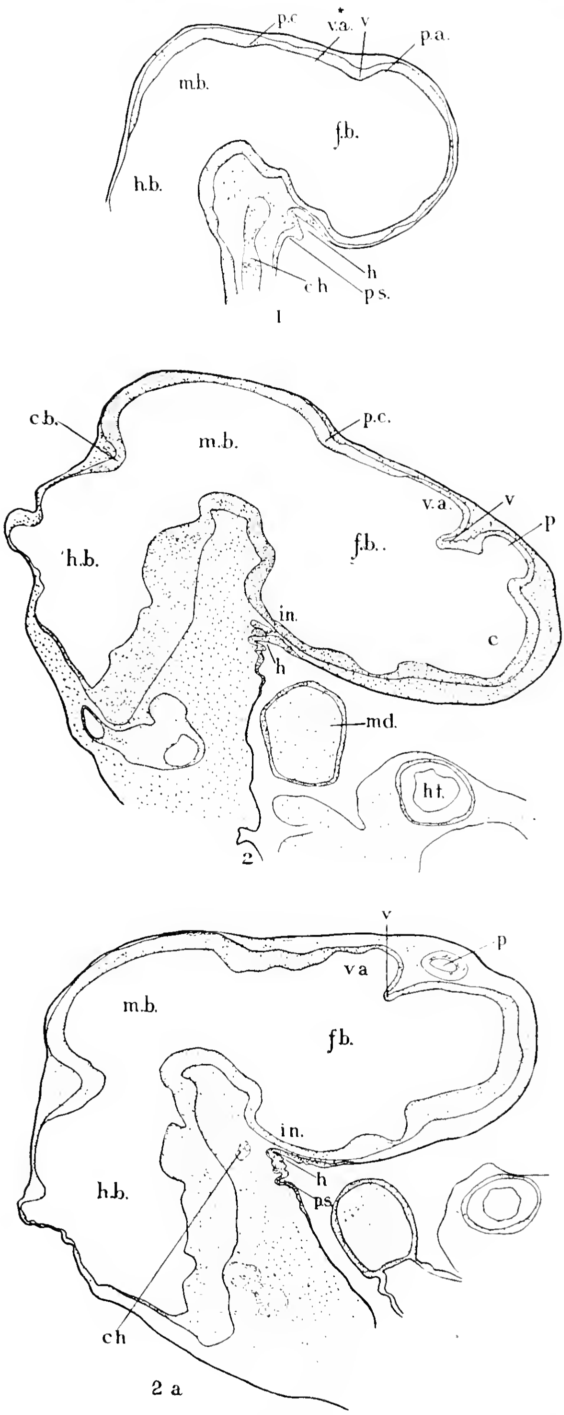
Fig. 6. A sagittal section through the paraphysal region of a 7 cm. embryo. Objective 2 inch; ocular 2 inch.

Fig. 6a. A sagittal section through the hypophysal region of the same embryo. Objective ⅔ inch; ocular 2 inch.

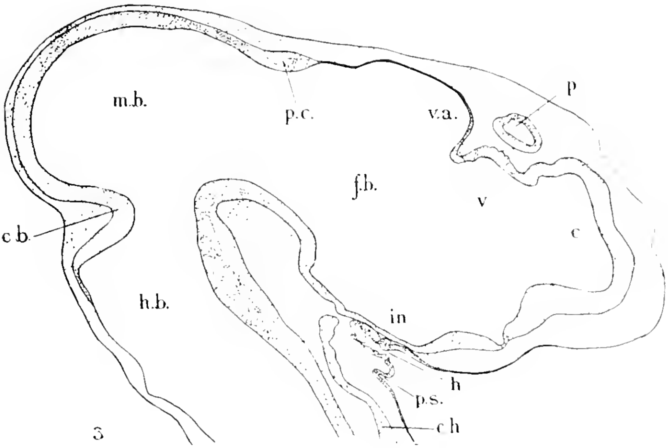
Fig. 7. A sagittal section through the hypophysal region of a 13 cm. embryo. Objective ⅔ inch; ocular 2 inch.

Lettering.

b. v., blood vessel.
c., cerebral hemispheres.
cb., cerebellum.
ch., notochord.
c. p., choroid plexus.
e., eustachian valve.
f. b., fore-brain.
h., hypophysis.
h. b., hind-brain.
ht., heart.
h. s., hypophysal stalk.
in., infundibulum.
in'., infundibular pit.
l., slit-like lumen of hypophysis.
m. b., mid-brain.
md., mandible.
n., nasal cavity.
o.-o'''., lateral diverticula of hypophysis.
p., paraphysis.
p. a., paraphysal arch.
p. c., posterior commissure.
ph., pharynx.
p. n., posterior nares.
p. s., pharyngeal sac.
v., velum.
v. a., post-velar arch (dorsal sac).
vs., velar vesicles.



DEVELOPMENT OF BRAIN OF ALLIGATOR



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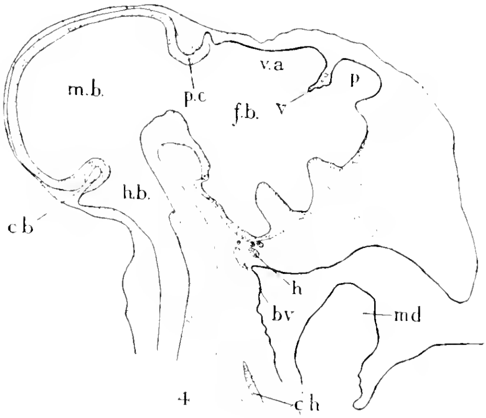

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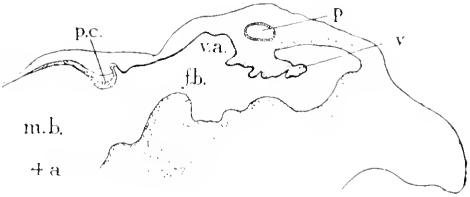

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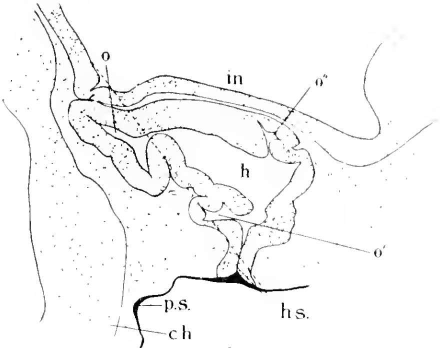

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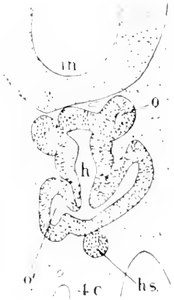

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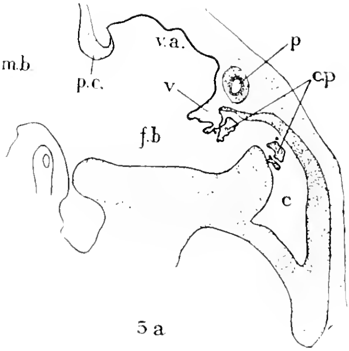

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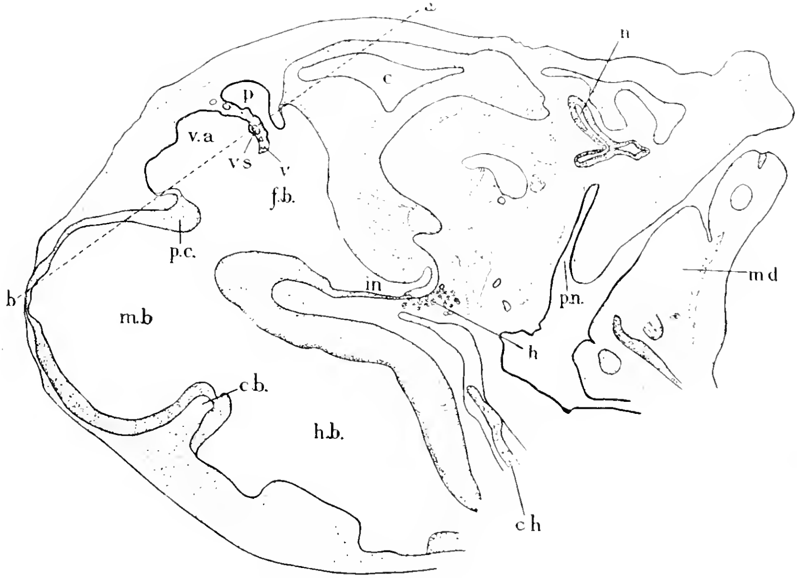

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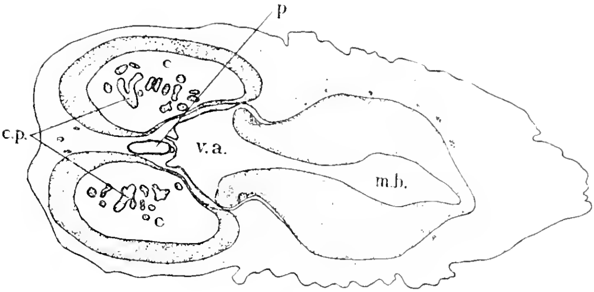

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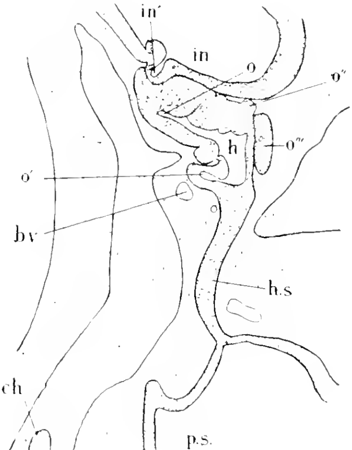

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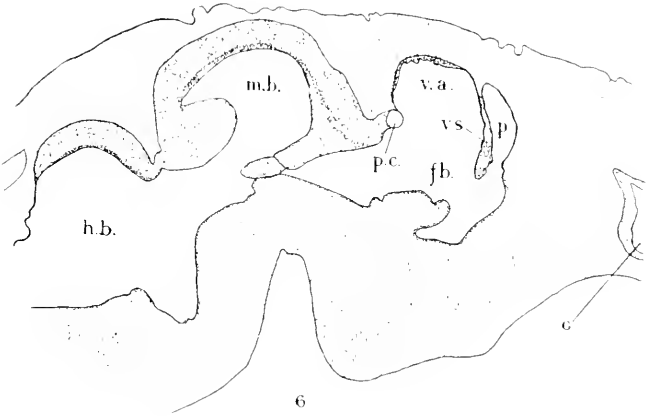

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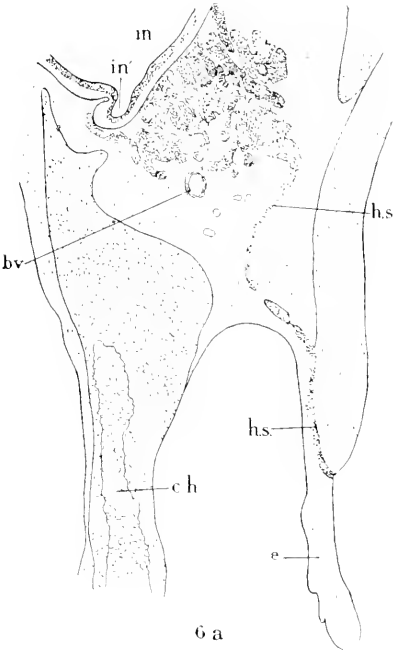

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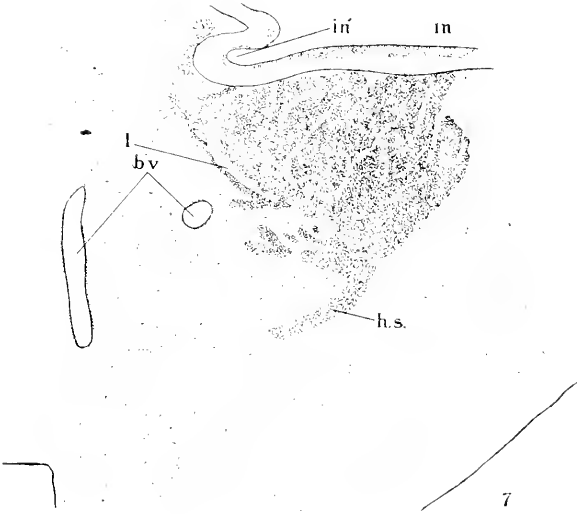

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DEVELOPMENT OF BRAIN OF ALLIGATOR

SMITHSONIAN MISCELLANEOUS COLLECTIONS
VOLUME 54, NUMBER 3

THE CONSTANTS OF NATURE

PART V

A RECALCULATION
OF
THE ATOMIC WEIGHTS

THIRD EDITION, REVISED AND ENLARGED

BY
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A RECALCULATION OF THE ATOMIC WEIGHTS.

(Third Edition, Revised and Enlarged.)

By FRANK WIGGLESWORTH CLARKE.

INTRODUCTION.

In the autumn of 1877 the writer began collecting data relative to determinations of atomic weight, with the purpose of preparing a complete résumé of the entire subject, and of recalculating all the estimations. The work was fairly under way, the material was collected and partly discussed, when I received from the Smithsonian Institution a manuscript by Professor George F. Becker, entitled "Atomic Weight Determinations: a Digest of the Investigations Published since 1814." This manuscript, which has since been issued[1] as Part IV of the "Constants of Nature," covered much of the ground contemplated in my own undertaking. It brought together all the evidence, presenting it clearly and thoroughly in compact form; in short, that portion of the task could not well be improved upon. Accordingly, I decided to limit my own labors to a critical recalculation of the data; to combine all the figures upon a common mathematical basis, and to omit everything which could as well be found in Professor Becker's "Digest."

In due time my work was completed, and early in 1882 it was published.[2] About a year later Meyer and Seubert's recalculation appeared, to be followed later still by the less elaborate discussions of Sebelien and of Ostwald. All of these works differed from one another in various essential particulars, presenting the subject from different points of view, and with different methods of calculation. Each one, therefore, has its own special points of merit, and, in a sense, reinforces the others. At the same time, the scientific activity which they represent shows how widespread was the interest in the subject of atomic weights, and how fundamentally important these constants undoubtedly are.

The immediate effect of all these publications was to render manifest the imperfections of many of the data, and to point out most emphatically in what directions new work needed to be done. This led to an extraordinary activity in the determination of atomic weights, and so much

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[1] Smithsonian Miscellaneous Collections, Vol. 27, Serial No. 358, pp. 152.
[2] Smithsonian Miscellaneous Collections, Vol. 27, Serial No. 441, pp. 279.

new material accumulated that in 1897[1] a new edition of this work, revised to date, became necessary. Since then, much more has been done, with great improvements in technique, especially by Richards and his colleagues at Harvard University, by Edgar F. Smith in Philadelphia, and by Guye at Geneva, not to mention many other workers of high merit. The assimilation of this new material, and its combination with the older data, is the object of the present volume.

At the very beginning of my work, a fundamental question confronted me. Should I treat the investigations of different individuals separately, or should I combine similar data together in a manner irrespective of persons? For example, ought I, in estimating the atomic weight of silver, to take Stas' work by itself, Marignac's work by itself, and so on, and then average the results together; or should I rather combine all series of figures relating to the composition of potassium chlorate into one mean value, and all the data concerning the composition of silver chloride into another mean, and, finally, compute from such general means the constant sought to be established? The latter plan was finally adopted; in fact, it was rendered necessary by the method of least squares, which, in a special, limited form, was chosen as the best method of dealing with the problem.

The mode of discussion and combination of results was briefly as follows. The formulæ employed are given in another place. Beginning with the ratio between oxygen and hydrogen, each series of experiments was taken by itself, its arithmetical mean was determined, and the probable error of that mean was computed. Then the several means were combined according to the appropriate formula, each one receiving a weight dependent upon its probable error. The general mean thus established was taken as the most probable value for the ratio, and at the same time its probable error was mathematically assigned. In the former editions of this work it was used to give the atomic weight of oxygen referred to hydrogen as unity. In the present edition the oxygen standard is assumed, and the atomic weight of hydrogen is determined. This is in accordance with the decisions of the International Committee on Atomic Weights; although my personal preference, on theoretical grounds, is for the hydrogen standard. The subsequent computations, however, are rendered simpler by assuming that $O = 16$, and that is a principal reason for my change of policy.

Next in order came a number of elements which were best considered together; namely, silver, chlorine, bromine, iodine, potassium, sodium, nitrogen, sulphur and carbon. Their atomic weights, with those of hydrogen and oxygen, form a fundamental group, by means of which

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[1] Smithsonian Miscellaneous Collections, Vol. 38, Serial No. 1075, pp. vi, 370.

other atomic weights are determined. Direct comparisons with oxygen or hydrogen are relatively few; indirect determinations with the aid of silver and the halogens are many. For the elements in question there were data from many experimenters. All similar figures, that is, the figures for each ratio, were first reduced to a common standard, and then the individual means were combined into general means. Thus all the data were condensed into fifty-five ratios, from which a number of values for each atomic weight could be computed. The ratios represent the actual experimental work; the atomic weights are inferential. Finally, the several values for each atomic weight are treated as if they were means of the usual type, and combined by the method of least squares into a general mean, which is supposed to represent the most probable value for each constant. The fundamental values having been determined, they are next applied to the calculation of what may be called the secondary atomic weights, and in this work the probable error of each term in each ratio is taken into account. This will appear more clearly evident in the subsequent actual calculations.

But although the discussion of atomic weights is ostensibly mathematical, it cannot be purely so. Chemical considerations are necessarily involved at every turn. In assigning weights to mean values I have been, for the most part, rigidly guided by mathematical rules; but in some cases I have been compelled to reject altogether series of data which were mathematically excellent, but chemically worthless because of constant errors. In certain instances there were grave doubts as to whether particular figures should be included or rejected in the calculation of means, there having been legitimate reasons for either procedure. Probably many chemists would differ with me upon such points of judgment. In fact, it is doubtful whether any two chemists, working independently, would handle all the data in precisely the same way, or combine them so as to produce exactly the same final results. Neither would any two mathematicians follow identical rules or reach identical conclusions. In calculating the atomic weight of any element those values are assigned to other elements which have been determined in previous chapters. Hence a variation in the order of discussion might lead to slight differences in the final results.

As a matter of course the data herein combined are of very unequal value. In many series of experiments the weighings have been reduced to a vacuum standard; but in other cases chemists have neglected this correction altogether. In a majority of instances the errors thus introduced are slight; nevertheless they exist, and interfere more or less with all attempts at a theoretical consideration of the results.[1]

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[1] For a discussion of these vacuum corrections see Guye and Zachariades, Compt. Rend., 149, 593. The errors in reductions to a vacuum are larger than has been commonly supposed.

Necessarily, this work omits many details relative to experimental methods, and particulars as to the arrangement of special forms of apparatus. For such details original memoirs must be consulted. Their inclusion here would have rendered the work unwarrantably bulky. There is such a thing as over-exhaustiveness of treatment, which is equally objectionable with under-thoroughness.

Of course, none of the results reached in this revision can be considered as final. Every one of them is liable to repeated corrections. To my mind the real value of the work, great or little, lies in another direction. The data have been brought together and reduced to common standards, and for each series of figures the probable error has been determined. Thus far, however much my methods of combination may be criticised, I feel that my labors will have been useful. The ground is cleared, in a measure, for future experimenters; it is possible to see more distinctly what remains to be done; some clues are furnished as to the relative merits of different series of results.

On the mathematical side my method of recalculation has obvious deficiencies. It is special, rather than general, and at some future time, when a sufficiently large mass of evidence has accumulated, it must give way to a more thorough mode of treatment. For example, the ratio $Ag_2 : BaBr_2$ has been used for computing the atomic weight of barium, the atomic weights of silver and bromine being supposed to be known. But these atomic weights are subject to small errors, and they are superimposed upon that of the ratio itself in the process of calculation. Obviously, the ratio should contribute to our knowledge of all three of the atomic weights involved in it, its error being distributed into three parts instead of appearing in one only. The errors may be in part compensatory; but that is not certainly known.

Suppose now that for every element we had a goodly number of atomic weight ratios, connecting it with at least a dozen other elements, and all measured with reasonable accuracy. These hundreds of ratios could then be treated as equations of observation, reduced to linear form, and combined by the general method of least squares into normal equations. All errors would thus be distributed, never becoming cumulative; and the normal equations, solved once for all, would give the atomic weights of all the elements simultaneously. The process would be laborious but the result would be the closest possible approach to accuracy. The data as yet are inadequate, although some small groups of ratios may be handled in that way; but in time the method is sure to be applied, and indeed to be the only general method applicable. Even if every ratio was subject to some small constant error, this, balanced against the similar errors of other ratios, would become accidental or unsystematic with reference to the entire mass of material, and would practically vanish from the final means.

Concerning this subject of constant and accidental errors, a word may be said here. My own method of discussion eliminates the latter, which are, in great part at least, removable by ordinary averaging; but the constant errors, vicious and untractable, remain, at least partially. Still, where many ratios are considered, even the systematic errors may in part compensate each other, and do less harm than might be expected. They have, moreover, a peculiarity which deserves some attention.

In the discussion of instrumental observations, the systematic errors are commonly constant, both as to direction and as to magnitude. They are therefore independent of the accidental errors, and computation of means leaves them untouched. But in the measurement of chemical ratios the constant errors are most frequently due to an impurity in one of the materials investigated. If different samples of a substance are studied, although all may contain the same impurity, they are not likely to contain it in the same amount; and so the values found for the ratio will vary. In other words, such errors may be constant in direction but variable in magnitude. That variation appears in the probable error computed for the series of observations, diminishes its weight when combined with other series, and so, in part, corrects itself. It is not removed from the result, but it is self-mitigated. The constant errors familiar to the physicist and astronomer are obviously of a different order.

That all methods of averaging are open to objections, I am of course perfectly aware. I also know the doubts which attach to all questions of probable error, and to all combinations of data which depend upon them. I have, however, preferred to face these objections and to recognize these doubts rather than to adopt any arbitrary scheme which permits of a loose selection of data. After all, the use of probable error as a means of weighting is only a means of weighting, and perhaps more justifiable than any other method of attaining the same result. When observations are weighted empirically—that is, by individual judgment—far greater dangers arise. Almost unconsciously, the work of a famous man is given greater weight than that of some obscure chemist, although the latter may ultimately prove to be the best. But the probable error of a series of measurements is not affected by the glamor of great names; and the weight which it assigns to the observations is at least as good as any other. In the long run, I believe it assigns weight more accurately, and therefore I have trusted to its indications, not as if it were a mathematical fetish, but regarding it as a safe guide, even though sometimes fallible.

One possibly weak point in the method adopted, deserves to be mentioned. Its fairness depends in part upon the fairness of the experimenter. One chemist, making a series of measurements, gives all of his determinations. Another chemist selects those which are most concordant, and suppresses others which seem to him less trustworthy. The latter series, therefore, is likely to receive higher weight than belongs to it; while the former series will be underweighted. The rejection of data, even by the man who is most familiar with them, is always a dangerous proceeding, and one which should be discouraged.

The other and more usual method of adjusting the atomic weights, that of selecting determinations in accordance with their apparent chemical merit, has recently been followed by Brauner. In his excellent and critical discussion of the subject, now appearing in Abegg's Handbuch der anorganischen Chemie, he gives all the determinations for each element, and then assigns preference to those which most appeal to his judgment. In most instances his findings agree with mine, and therefore our conclusions reinforce each other. Sometimes we differ, and in such cases it would seem that new determinations are desirable. When values derived from different sources, and computed by different methods are concordant, they may be regarded as probably well established; but even then certainty is not attained. The history of atomic weight determinations bears abundant witness to this assertion.

For example: Until within very recent years the work of Stas, emphasized by that of Marignac, was regarded as almost final. Now, however, some of the ratios measured by these chemists are found to be out of harmony with the best modern investigations, and there is a tendency towards rejecting the older work altogether. But the researches of Stas give a homogeneous and concordant group of atomic weights, which cannot be entirely thrown aside without much more evidence against them than as yet exists. It is probable that the silver used by Stas contained occluded oxygen, as was pointed out by Dumas[1]; and this would account for some, but not all of the variations from recent revisions of the ratios. It is also probable, as Richards has shown, that Stas underrated the solubility of silver chloride. How large these errors may be in Stas' work, assuming them to exist, is uncertain; and to assign zero weight to his determinations would be too extreme a procedure. His data and Marignac's are therefore retained in the present recalculation, with the proper mathematical weight; and the final results seem to be satisfactory. Indeed, the Stas values for silver, chlorine and bromine, applied to the determinations of other atomic weights, sometimes give more concordant results than the modern figures. This is especially true in the cases of cæsium, barium and magnesium, although the discrepancies are not large.

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[1] Ann. Chim. Phys. (5), 14, 289. See also the Appendix to the first edition of this Recalculation, in which the influence of a correction for occluded oxygen is considered.

The data of Richards and his collaborators for the chlorides of these metals give a ratio between silver and chlorine in agreement with the measurements by Stas, and lower than that which Richards and Wells have established. If, therefore, the work of Stas is in error, the same error inheres in the atomic weights of the three metals above mentioned, and the latter, even if the uncertainty is small, ought to be revised. The sharp concordance found when the atomic weights were computed with Stas' figures is either illusive, or else the modern data for silver and chlorine are wrong. The first of these alternatives is the more probable. In spite of the discordance now evident, the determinations for cæsium, barium and magnesium are by far the best we have, and their uncertainties need not be regarded as serious.

In Meyer and Seubert's recalculation, weights are assigned in quite a novel manner. In each series of experiments the maximum and minimum results are given, but instead of the mean there is a value deduced from the sum of the weighings—that is, each experiment is weighted proportionally to the mass of the material handled in it. For this method I am unable to find any complete justification. Of course, the errors due to the operations of weighing become proportionally smaller as the quantity of material increases, but these errors, with modern apparatus, are relatively unimportant. The real errors in atomic weight determinations are much larger than these, and due to different causes. Hence an experiment upon ten grammes of material may be a little better than one made upon five grammes, but it is by no means necessarily twice as good. The ordinary mean of a series of observations, with its measure of concordance, the probable error, is a better value than one obtained in the manner just described. If only errors of weighing were to be considered, Meyer and Seubert's summation method would be valid, but in the presence of other and greater errors it seems to have but little real pertinency to the problem at hand.

In addition to the usual periodicals, the following works have been freely used by me in the preparation of this volume:

BERZELIUS, J. J. Lehrbuch der Chemie. 5 Auflage. Dritter Band. SS. 1147-1231. 1845.

VAN GEUNS, W. A. J. Prœve eener Geschiedenis van de Æquivalentgetallen der Scheikundige Grondstoffen en van hare Soortelijke Gewigten in Gasvorm, voornamelijk in Betrekking tot de vier Grondstoffen der Bewerktuigde Natuur. Amsterdam, 1853.

MULDER, E. Historisch-Kritisch Overzigt van de Bepalingen der Æquivalent-Gewigten van 13 Eenvoudige Ligchamen. Utrecht, 1853.

MULDER, L. Historisch-Kritisch Overzigt van de Bepalingen der Æquivalent-Gewigten van 24 Metalen. Utrecht, 1853.

OUDEMANS, A. C., JR. Historisch-Kritisch Overzigt van de Bepaling der Equivalent-Gewigten van Twee en Twintig Metalen. Leiden, 1853.

BECKER, G. F. Atomic Weight Determinations: a Digest of the Investigations Published since 1814. Smithsonian Miscellaneous Collections, Vol. 27, No. 358. Washington, 1880.

STAS, J. S. Untersuchungen über die Gesetze der Chemischen Proportionen über die Atomgewichte und ihre gegenseitigen Verhältnisse. Uebersetzt von Dr. L. Aronstein. Leipzig, 1867.
See also his " Oeuvres Complètes," 3 vols., published at Bruxelles in 1894.[1]

MEYER, L., and SEUBERT, K. Die Atomgewichte der Elemente, aus den Originalzahlen neu berechnet. Leipzig, 1883.

SEBELIEN, J. Beiträge zur Geschichte der Atomgewichte. Braunschweig, 1884.

OSTWALD, W. Lehrbuch der allgemeinen Chemie. Zweite Aufl. I Band. SS. 18-138. Leipzig, 1891.

MARIGNAC, J. C. G. DE. Oeuvres Complètes. 2 vols. Geneva, 1902.

RICHARDS, T. W. Experimentelle Untersuchungen ueber Atomgewichte. Hamburg and Leipzig, 1909.

Abegg's Handbuch, containing Brauner's recalculation, has already been mentioned. Its value is very great. The four Dutch monographs above cited are also especially valuable. They represent a revision of all atomic weight data down to 1853, as divided between four writers.

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[1] The citations used in the present Recalculation are all from the Oeuvres Complètes.

FORMULÆ FOR THE CALCULATION OF PROBABLE ERROR.

The formula for the probable error of an arithmetical mean, familiar to all physicists, is as follows:

(1.) $$e=0.6745\sqrt{\frac{S}{n(n-1)}}$$

Here n represents the number of observations or experiments in the series, and S the sum of the squares of the variations of the individual results from the mean.

In combining several arithmetical means, representing several series, into one general mean, each receives a weight inversely proportional to the square of its probable error. Let A, B, C, etc., be such means, and a, b, c their probable errors respectively. Then the general mean is determined by the formula:

(2.) $$M=\frac{\frac{A}{a^2}+\frac{B}{b^2}+\frac{C}{c^2}\ldots\ldots}{\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\ldots\ldots}$$

For the probable error of this general mean we have:

(3.) $$e=\frac{1}{\sqrt{\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\ldots\ldots}}$$

In the calculation of atomic and molecular weights the following formulæ are used: Taking, as before, capital letters to represent known quantities, and small letters for their probable errors respectively, we have for the probable error of the sum or difference of two quantities, A and B:

(4.) $$e=\sqrt{a^2+b^2}$$

For the product of A multiplied by B the probable error is

(5.) $$e=\sqrt{(Ab)^2+(Ba)^2}$$

For the product of three quantities, ABC:

(6.) $$e=\sqrt{(BCa)^2+(ACb)^2+(ABc)^2}$$

For a quotient, $\frac{B}{A}$, the probable error becomes

(7.) $$e=\frac{\sqrt{\left(\frac{Ba}{A}\right)^2+b^2}}{A}$$

Given a proportion, A : B : : C : x, the probable error of the fourth term is as follows:

$$(8.)\qquad e = \frac{\sqrt{\left(\frac{\mathrm{BC}a}{\mathrm{A}}\right)^2 + (\mathrm{C}b)^2 + (\mathrm{B}c)^2}}{\mathrm{A}}$$

This formula is used in nearly every atomic weight calculation, and is, therefore, exceptionally important. Rarely a more complicated case arises in a proportion of this kind:

$$\mathrm{A} : \mathrm{B} : : \mathrm{C} + x : \mathrm{D} + x$$

In this proportion the unknown quantity occurs in two terms. Its probable error is found by this expression, and is commonly large:

$$(9.)\qquad e = \sqrt{\frac{(\mathrm{C}-\mathrm{D})^2}{(\mathrm{A}-\mathrm{B})^4}(\mathrm{B}^2a^2 + \mathrm{A}^2b^2) + \frac{\mathrm{B}^2c^2 + \mathrm{A}^2d^2}{(\mathrm{A}-\mathrm{B})^2}}$$

When several independent values have been calculated for an atomic weight they are treated like means, and combined according to formulæ (2) and (3). Each final result is, therefore, to be regarded as the general or weighted mean of all trustworthy determinations. This method of combination is not theoretically perfect, but it seems to be the one most available in practice.

THE FUNDAMENTAL RATIOS.

In the determination of atomic weights, a small number of values are to be regarded as fundamental. They are the standards of reference; and by comparison with them all the other atomic weights are established. Two of these values, the atomic weights of hydrogen and oxygen, are primary; that is, one or the other of them is the basis of the entire system; hydrogen as unity in the older arrangements; oxygen equal to sixteen in the more modern scheme. Over the relative merits of these two ultimate standards there has been much controversy; but with discussions of that sort the present work has nothing to do. The oxygen standard is now recognized by international agreement, and will therefore be accepted here.

Comparatively few of the atomic weights, however, are fixed by direct comparison with either oxygen or hydrogen. In most cases other values intervene, and especially the atomic weights of silver, chlorine, bromine, iodine, nitrogen, carbon, sulphur, potassium and sodium. These constants are first to be determined, and their establishment may be compared to a primary triangulation, of which the hydrogen-oxygen ratio is the base line. The ratios connecting these eleven elements with one another are to be discussed in the following pages.

THE OXYGEN-HYDROGEN RATIO.

Leaving out of account the earliest researches, which now have only historical interest, the first determinations of this ratio worth considering are those by Dulong and Berzelius,[1] who, like some of their successors, effected the synthesis of water over heated oxide of copper. The essential features of the method are in all cases the same. Hydrogen gas is passed over the hot oxide, and the water thus formed is collected and weighed. From this weight and the loss of weight which the oxide undergoes, the composition of water is readily calculated. Dulong and Berzelius made but three experiments, which gave the following percentages of oxygen and hydrogen in water:

O.	*H.*
88.942	11.058
88.809	11.191
88.954	11.046

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[1] Thomson's Annals of Philosophy, July, 1821, p. 50.

From these figures the ratio H : O becomes—

16.124
15.863
16.106

Mean, 16.031, ± .057

As the weighings were not reduced to a vacuum, this correction was afterwards applied by Clark,[1] who showed that these syntheses really make O = 15.894; or, in Berzelian terms, if O = 100, H = 12.583. The value 15.894, ± .057 we may therefore take as the true result of Dulong and Berzelius' experiments, a figure curiously close to that reached in the latest and best researches.

In 1842 Dumas[2] published his elaborate investigation upon the composition of water. The first point was to get pure hydrogen. This gas, evolved from zinc and sulphuric acid, might contain oxides of nitrogen, sulphur dioxide, hydrosulphuric acid, and arsenic hydride. These impurities were removed in a series of wash bottles; the H_2S by a solution of lead nitrate, the H_3As by silver sulphate, and the others by caustic potash. Finally, the gas was dried by passing through sulphuric acid, or, in some of the experiments, over phosphorus pentoxide. The copper oxide was thoroughly dried, and the bulb containing it was weighed. By a current of dry hydrogen all the air was expelled from the apparatus, and then, for ten or twelve hours, the oxide of copper was heated to dull redness in a constant stream of the gas. The reduced copper was allowed to cool in an atmosphere of hydrogen. The weighings were made with the bulbs exhausted of air. The following table gives the results:

Column A contains the symbol of the drying substance; B gives the weight of the bulb and copper oxide; C, the weight of bulb and reduced copper; D, the weight of the vessel used for collecting the water; E, the same, plus the water; F, the weight of oxygen; G, the weight of water formed; H, the crude equivalent of H when O = 10,000; I, the equivalent of H, corrected for the air contained in the sulphuric acid employed. This correction is not explained, and seems to be questionable.

A.	*B.*	*C.*	*D.*	*E.*	*F.*	*G.*	*H.*	*I.*
H_2SO_4	291.985	278.806	480.807	495.634	13.179	14.827	1250.5	1249.6
"	344.548	324.186	488.227	511.132	20.362	22.905	1249.0	1248.0
"	316.671	296.175	439.711	462.764	20.495	23.053	1248.1	1247.2
P_2O_5	625.829	568.825	884.190	948.323	57.004	64.044	1250.6	1249.0
H_2SO_4	804.546	728.182	887.331	973.291	76.364	85.960	1256.2	1254.6
"	533.726	490.155	867.159	916.206	43.571	49.047	1256.3	1255.0
"	661.915	627.104	839.304	878.482	34.811	39.178	1254.6	1253.3

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[1] Phil. Mag. (3), 20, 341.

[2] Compt. Rend., 14, 537.

P_2O_5	612.625	566.738	824.624	876.244	45.887	51.623	1250.0	1249.0
"	904.643	844.612	822.660	890.246	60.031	67.586	1258.3	1255.1
H_2SO_4	642.325	590.487	741.095	799.417	51.838	58.320	1250.4	1248.9
P_2O_5	587.645	535.137	874.832	933.910	52.508	59.078	1251.2	1249.0
"	673.280	613.492	931.487	998.700	59.789	67.282	1253.3	1250.8
H_2SO_4	660.855	598.765	682.374	752.273	62.090	69.899	1257.7	1254.8
"	642.325	590.487	741.097	799.455	51.838	58.360	1258.1	1256.2
"	937.845	881.362	1064.762	1128.319	56.483	63.577	1255.8	1252.2
P_2O_5	756.352	719.563	878.640	920.030	36.789	41.390	1250.6	1249.1
"	754.162	720.000	887.817	926.275	34.162	38.458	1257.3	1255.1
"	759.762	727.632	888.662	924.837	32.133	36.175	1257.5	1254.7
"	747.652	716.825	877.862	912.539	30.827	34.677	1248.8	1248.0
						Means......	1253.3	1251.5

In the sum total of these nineteen experiments, 840.161 grammes of oxygen form 945.439 grammes of water. This gives, in percentages, for the composition of water—oxygen, 88.864; hydrogen, 11.136. Hence the ratio H : O, calculated in mass, is 1 : 15.9608. In the following column the values are deduced from the individual data given under the headings F and G:

15.994
16.014
16.024
15.992
15.916
15.916
15.943
16.000
15.892
15.995
15.984
15.958
15.902
15.987
15.926
15.992
15.904
15.900
16.015

Mean, 15.9607, ± .0070

In calculating the above column several discrepancies were noted, probably due to misprints in the original memoir. On comparing columns B and C with F, or D and E with G, these anomalies chiefly appear. They were detected and carefully considered in the course of my own calculations; and, I believe, eliminated from the final result.

The investigation of Erdmann and Marchand[1] followed closely after that of Dumas. The method of procedure was essentially that of the latter chemist, differing from it only in points of detail. The hydrogen used was prepared from zinc and sulphuric acid, and the zinc, which contained traces of carbon, was proved to be free from arsenic and sulphur. The copper oxide was made partly from copper turnings and partly by the ignition of the nitrate. The results obtained are given in two series, in one of which the weighings were not actually made in vacuo, but were, nevertheless, reduced to a vacuum standard. In the second series the copper oxide and copper were weighed in vacuo. The following table contains the corrected weights of water obtained and of the oxygen in it, with the value found for the ratio in a third column. The weights are given in grammes.

First Series.

Wt. Water.	*Wt. O.*	*Ratio.*
62.980	55.950	15.917
95.612	84.924	15.891
94.523	84.607	15.977
35.401	31.461	15.970
		Mean, 15.939, ± .014

Second Series.

Wt. Water.	*Wt. O.*	*Ratio.*
41.664	37.034	15.996
44.089	39.195	16.018
53.232	47.321	16.011
55.636	49.460	16.017
		Mean, 16.010, ± .0036

The effect of discussing these two series separately is somewhat startling. It gives to the four experiments in Erdmann and Marchand's second group a weight vastly greater than their other four and Dumas' nineteen taken together. For so great a superiority as this there is no adequate reason; and it is highly probable that it is due almost entirely to fortunate coincidences, rather than to greater accuracy of work. We will, therefore, treat Erdmann and Marchand's experiments as one series, giving all equal weight, the mean now becoming $O = 15.975, \pm .0113$. If we take the sum of the eight experiments, 483.137 grammes water and 429.352 grammes oxygen, and compute from these figures, then $O = 15.966$.

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[1] Journ. prakt. Chem., 26, 461. 1842.

It would be easy to point out the sources of error in the foregoing sets of determinations, but it is hardly worth while to do so in detail. A few leading suggestions are enough for present purposes. First, there is an insignificant error due to the occlusion of hydrogen by metallic copper, rendering the apparent weight of the latter a trifle too high. Secondly, as shown by Dittmar and Henderson, hydrogen dried by passage through sulphuric acid becomes perceptibly contaminated with sulphur dioxide. In the third place, Morley[1] has found that hydrogen prepared from zinc always contains carbon compounds not removable by absorption and washing. Erdmann and Marchand themselves note that their zinc contained traces of carbon. Finally, copper oxide, especially when prepared by the ignition of the nitrate, is very apt to contain gaseous impurities, and particularly occluded nitrogen.[2] Any or all of these sources of error may have vitiated the three investigations so far considered, but it would be useless to speculate as to the extent of their influence. They amply account, however, for the differences between the older and the later determinations of the constant under discussion.

Leaving out of account all measurements of the relative densities of hydrogen and oxygen, to be considered separately later, the next determination to be noted is that published by J. Thomsen in 1870.[3] Unfortunately this chemist has not published the details of his work, but only the end results. Partly by the oxidation of hydrogen over heated copper oxide, and partly by its direct union with oxygen, Thomsen finds that at the latitude of Copenhagen, and at sea level, one litre of dry hydrogen at 0° and 760 mm. pressure will form .8041 gramme of water. According to Regnault, at this latitude, level, temperature, and pressure, a litre of hydrogen weighs .08954 gramme. From these data, $O=15.9605$. It will be seen at once that Thomsen's work depends in great part upon that of Regnault, and is therefore subject to the corrections recently applied by Crafts and others to the latter. These corrections, which will be discussed further on, reduce the value of O from 15.9605 to 15.91. In order to combine this value with others, it is necessary to assign it weight arbitrarily, and as Thomsen made eight experiments, which are said to be concordant, it may be fair to rank his determination with that of Erdmann and Marchand, and to assume for it the same probable error. The value 15.91, ± .0113 will therefore be taken as the outcome of Thomsen's research.

In 1887 Cooke and Richards published the results of their elaborate investigation.[4] These chemists weighed hydrogen, burned it over copper

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[1] Amer. Chem. Journ., 12, 460. 1890.

[2] See Richards' work cited in the chapter on copper.

[3] Ber. Deutsch. chem. Ges., 3, 928. 1870.

[4] Proc. Amer. Acad., 23, 149. Amer. Chem. Journ., 10, 81.

oxide, and weighed the water produced. The copper oxide was prepared from absolutely pure electrolytic copper, and the hydrogen was obtained from three distinct sources, as follows: First, from pure zinc and hydrochloric acid; second, by electrolysis, in a generator containing dilute hydrochloric acid and zinc-mercury amalgam; third, by the action of caustic potash solution upon sheet aluminum. The gas was dried and purified by passage through a system of tubes and towers containing potash, calcium chloride, glass beads drenched with sulphuric acid, and phosphorus pentoxide. No impurity could be discovered in it, and even nitrogen was sought for spectroscopically without being found.

The hydrogen was weighed in a glass globe holding nearly five litres and weighing 570.5 grammes, which was counterpoised by a second globe of exactly the same external volume. Before filling, the globe was exhausted to within 1 mm. of mercury and weighed. It was then filled with hydrogen and weighed again. The difference between the two weights gives the weight of hydrogen taken.

In burning, the hydrogen was swept from the globe into the combustion furnace by means of a stream of air which had previously been passed over hot reduced copper and hot cupric oxide, then through potash bulbs, and finally through a system of driers containing successively calcium chloride, sulphuric acid, and phosphorus pentoxide. The water formed by the combustion was collected in a condensing tube connected with a U tube containing phosphorus pentoxide. The latter was followed by a safety tube containing either calcium chloride or phosphorus pentoxide, added to the apparatus to prevent reflex diffusion. Full details as to the arrangement and construction of the apparatus are given. The final results appear in three series, representing the three sources from which the hydrogen was obtained. All weights are corrected to a vacuum.

First Series.—Hydrogen from Zinc and Acid.

Wt. of H.	*Wt. H_2O.*	*Ratio H:O.*
.4233	3.8048	15.977
.4136	3.7094	15.937
.4213	3.7834	15.960
.4163	3.7345	15.941
.4131	3.7085	15.954
		Mean, 15.954, ± .0048

Second Series.—Electrolytic Hydrogen.

.4112	3.6930	15.962
.4089	3.6709	15.955
.4261	3.8253	15.955
.4197	3.7651	15.942
.4144	3.7197	15.953
		Mean, 15.953, ± .0022

Third Series.—Hydrogen from Caustic Potash.

.42205	3.7865	15.943
.4284	3.8436	15.944
.4205	3.7776	15.967
.43205	3.8748	15.937
.4153	3.7281	15.954
.4167	3.7435	15.967

Mean, 15.952, ± .0035

Mean of all as one series, 15.953, ± .0020

Shortly after the appearance of this paper by Cooke and Richards Lord Rayleigh pointed out the fact, already noted by Agamennone, that a glass globe when exhausted is sensibly condensed by the pressure of the surrounding atmosphere. This fact involves a correction to the foregoing data, due to a change in the tare of the globe used, and this correction was promptly determined and applied by the authors.[1] By a careful series of measurements they found that the correction amounted to an average increase of 1.98 milligrammes to the weight of hydrogen taken in each experiment. Hence O equals not 15.953, but 15.869, the probable error remaining unchanged. The final result of Cooke and Richards' investigation, therefore, is

$$O = 15.869, \pm .0020$$

Keiser's determinations of the ratio were published almost simultaneously with those of Cooke and Richards. He burned hydrogen occluded by palladium, and weighed the water so formed. In a preliminary paper[2] the following results are given:

Wt. of H.	*Wt. of H_2O.*	*Ratio H:O.*
.65100	5.81777	15.873
.60517	5.41540	15.897
.33733	3.00655	15.822

Mean, 15.864, ± .015

Not long after the publication of the foregoing data Keiser's full paper appeared.[3] Palladium foil, warmed to a temperature of 250°, was saturated with hydrogen prepared from dilute sulphuric acid and zinc free from arsenic. From 100 to 140 grammes of palladium were taken, and it was first proved that the metal did not absorb other gases which might contaminate the hydrogen. Before charging, the foil was heated to bright redness in vacuo. After charging, the tube containing the palladium

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[1] Proc. Amer. Acad., 23, 182. Am. Chem. Journ., 10, 191.

[2] Berichte, 20, 2323. 1887.

[3] Amer. Chem. Journ., 10, 249. 1888.

hydride was exhausted by means of a Geissler pump to remove any nitrogen which might have been present. In the preliminary investigation cited above, the latter precaution was neglected, which may account for the low results.

Between the palladium tube and the combustion tube a U tube was interposed, containing phosphorus pentoxide. This was to determine the amount of moisture in the hydrogen. The combustion tube was filled with granular copper oxide, prepared by reducing the commercial oxide in hydrogen, heating the metal so obtained to bright redness in a vacuum, and then reoxidizing with pure oxygen.

Upon warming the palladium tube, which was first carefully weighed, hydrogen was given off and allowed to pass into the combustion tube. When the greater part of it had been burned, the tube was cut off by means of a stopcock and allowed to cool. Meanwhile a stream of nitrogen was passed through the combustion tube, sweeping hydrogen before it. This was followed by a current of oxygen, reoxidizing the reduced copper; and the copper oxide was finally cooled in a stream of dry air. The water produced by the combustion was collected in a weighed bulb tube, followed by a weighed U tube containing phosphorus pentoxide. A second phosphorus pentoxide tube served to prevent the sucking back of moisture from the external air. The loss in weight of the palladium tube, corrected by the gain in weight of the first phosphorus pentoxide, gave the weight of hydrogen taken. The gain in weight of the two collecting tubes gave the weight of water formed. All weights in the following table of results are reduced to a vacuum:

Wt. of H.	*Wt. H_2O.*	*Ratio H:O.*
.34145	3.06338	15.943
.68394	6.14000	15.955
.65529	5.88200	15.952
.65295	5.86206	15.954
.66664	5.98116	15.944
.66647	5.98341	15.955
.57967	5.20493	15.958
.66254	5.94758	15.952
.87770	7.86775	15.950
.77215	6.93036	15.951
		Mean, 15.9514, ± .0011

In sum, 6.55880 grammes of hydrogen gave 52.30383 of water, whence O = 15.9492.

In March, 1889, Lord Rayleigh[1] published a few determinations of the ratio obtained by still a new method. Pure hydrogen and pure oxygen

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[1] Proc. Roy. Soc., 45, 425.

were both weighed in glass globes. From these they passed into a mixing chamber, and thence into a eudiometer, where they were gradually exploded by a series of electric sparks. After explosion the residual gas remaining in the eudiometer was determined and measured. The results, given without weighings or explicit details, are as follows:

15.93
15.98
15.98
15.93
15.92

Mean, 15.948, ± .009

Correcting this result for shrinkage of the globes and consequent change of tare, it becomes $O = 15.89, \pm .009$.

In the same month that Lord Rayleigh's paper appeared, W. A. Noyes[1] published his first series of determinations. His plan was to pass hydrogen into an apparatus containing hot copper oxide, condensing the water formed in the same apparatus, and from the gain in weight of the latter getting the weight of the hydrogen absorbed. The apparatus devised for this purpose consisted essentially of a glass bulb of 30 to 50 cc. capacity, with a stopcock tube on one side and a sealed condensing tube on the other. In weighing, it was counterpoised by another apparatus of nearly the same volume but somewhat less weight, in order to obviate reductions to a vacuum. After filling the bulb with commercial copper oxide (90 to 150 grammes), the apparatus was heated in an airbath, exhausted by means of a Sprengel pump, cooled, and weighed. It was next replaced in the airbath, again heated, and connected with an apparatus delivering purified hydrogen. When a suitable amount of the latter had been admitted, the stopcock was closed, and the heating continued long enough to convert all gaseous hydrogen within it into water. The apparatus was then cooled and weighed, after which it was connected with a Sprengel pump, in order to extract the small quantity of nitrogen which was always present. The latter was pumped out into a eudiometer, where it was measured and examined. The gain in weight of the apparatus, less the weight of this very slight impurity, gave the weight of hydrogen oxidized.

The next step in the process consisted in heating the apparatus to expel water, and weighing again. After this, pure oxygen was admitted and the heating was resumed, so as to oxidize the traces of hydrogen which had been retained by the copper. Again the apparatus was cooled and weighed, and then reheated, when the water formed was received in a

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[1] Amer. Chem. Journ., 11, 155. 1889.

bulb filled with phosphorus pentoxide, and the gaseous contents were collected in a eudiometer. On cooling and weighing the apparatus, the loss of weight, less the weight of gases pumped out, gave the amount of water produced by the traces of residual hydrogen under consideration. This weight, added to the loss of weight when the original water was expelled, gives the weight of oxygen taken away from the copper oxide. Having thus the weight of hydrogen and the weight of oxygen, the ratio sought for follows. Six results are given, but as they are repeated, with corrections, in Noyes' second paper, they need not be considered now.

Noyes' methods were almost immediately criticised by Johnson,[1] who suggested several sources of error. This chemist had already shown in an earlier paper[2] that copper reduced in hydrogen persistently retains traces of the latter, and also that when the reduction is effected below 700°, water is retained too. The possible presence of sulphur in the copper oxide was furthermore mentioned. Errors from these sources would tend to make the apparent atomic weight of oxygen (referred to hydrogen as unity) too low.

In his second paper[3] Noyes replies to the foregoing criticisms, and shows that they carry no weight, at least so far as his work is concerned. He also describes a number of experiments in which oxides other than copper oxide were tried, but without distinct success, and he gives fuller details as to manipulations and materials. His final results are in four series, as follows:

First Series.—Hydrogen from Zinc and Hydrochloric Acid.

Wt. of H.	*Wt. of O.*	*Ratio H:O.*
.9443	7.5000	15.885
.6744	5.3555	15.882
.7866	6.2569	15.909
.5521	4.3903	15.904
.4274	3.3997	15.909
.8265	6.5686	15.895
		Mean, 15.8973, ± .0032

This series appeared in the earlier paper, but with an error which is here corrected.

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[1] Chem. News, 59, 272.
[2] Journ. Chem. Soc., May, 1879.
[3] Amer. Chem. Journ., 12, 441. 1890.

Second Series.—Electrolytic Hydrogen, Dried by Phosphorus Pentoxide.

Wt. of H.	*Wt. of O.*	*Ratio H:O.*
.5044	4.0095	15.898
.6325	5.0385	15.932
.6349	5.0517	15.913
.5564	4.4175	15.879
.7335	5.8224	15.876
.6696	5.3181	15.885
		Mean, 15.8971, ± .0064

Third Series.—Electrolytic Hydrogen, Dried by Passage Through a Tube Packed with Sodium Wire.

Wt. of H.	*Wt. of O.*	*Ratio H:O.*
.9323	7.4077	15.891
.9952	7.9045	15.885
.3268	2.5977	15.898
.7907	6.2798	15.884
.7762	6.1671	15.891
1.1221	8.9131	15.887
		Mean, 15.8893, ± .0014

At the end of this series it was found that the hydrogen contained a trace of water, estimated to be equivalent to an excess of three milligrammes in the total hydrogen of the six experiments. Correcting for this, the mean becomes O = 15.899.

Fourth Series.—Electrolytic Hydrogen, Dried over Freshly Sublimed Phosphorus Pentoxide.

Wt. of H.	*Wt. of O.*	*Ratio H:O.*
1.0444	8.3017	15.898
.7704	6.1233	15.896
.8231	6.5421	15.896
.8872	7.0490	15.890
.9993	7.9403	15.892
1.1910	9.4595	15.885
		Mean, 15.8929, ± .0013

The mean of all the twenty-four determinations, taken as one series, with the correction to the third series included, is O = 15.8966, ± .0017. In sum, there were consumed 18.5983 grammes of hydrogen and 147.8145 of oxygen; whence O = 15.8955.

Dittmar and Henderson,[1] who effected the synthesis of water over copper oxide by what was essentially the old method, begin their memoir

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[1] Proc. Roy. Soc. Glasgow, 22, 33. Communicated Dec. 17, 1890.

with an exhaustive criticism of the work done by Dumas and by Erdmann and Marchand. They show, as I have already mentioned, that hydrogen dried by sulphuric acid becomes contaminated with sulphur dioxide, and also that a gas passed over calcium chloride may still retain as much as one milligramme of water per litre. Fused caustic potash they found to dry a gas quite completely.

In their first series of syntheses, Dittmar and Henderson generated their hydrogen from zinc and acid, sometimes hydrochloric and sometimes sulphuric, and dried it by passage, first through cotton wool, then through vitrioled pumice, then over red-hot metallic copper to remove oxygen. In later experiments it first traversed a column of fragments of caustic soda to remove antimony derived from the zinc. The oxide of copper used was prepared by heating chemically pure copper clippings in a muffle, and was practically free from sulphur. In weighing the several portions of apparatus it was tared with somewhat lighter similar pieces of as nearly as possible the same displacement. The results of this series of experiments, which are vitiated by the presence, unsuspected at first, of sulphur dioxide in the hydrogen, are stated in values of H when O=16, but in the following table have been recalculated to conformity with the earlier determinations:

Wt. of Water.	*Wt. of O.*	*Ratio H:O.*
4.7980	4.26195	15.901
7.55025	6.71315	16.039
6.2372	5.53935	15.875
11.29325	10.03585	15.963
11.6728	10.3715	15.940
11.8433	10.5256	15.976
11.7317	10.4243	15.947
19.2404	17.0926	15.916
20.83435	18.5234	16.031
17.40235	15.4598	15.917
19.2631	17.11485	15.934
		Mean, 15.949, ± .0103

Reducing to a vacuum, this becomes 15.843, while a correction for the sulphur dioxide estimated to be present in the hydrogen brings the value up again to 15.865. Still another correction is suggested, namely, that as the reduced copper in the combustion tube, before weighing, was exposed to a long-continued current of dry air, it may have taken up traces of oxygen chemically, thereby increasing its weight. As this correction, however, is quantitatively uncertain, it may be neglected here, and the result of this series will be taken as O=15.865, ±.0103. Its weight, relatively to some other series of experiments, is evidently small.

In their second and final series Dittmar and Henderson dried their hydrogen, after deoxidation by red-hot copper, over caustic potash and subsequently phosphorus pentoxide. The copper oxide and copper of the combustion tube were both weighed in vacuo. The results were as follows, vacuum weights being given:

Wt. of Water.	*Wt. of O.*	*Ratio H:O.*
19.2057	17.0530	15.843
19.5211	17.3342	[15.853]
19.4672	17.2882	15.868
22.9272	20.3540	15.820
23.0080	20.4421	[15.934]
23.4951	20.8639	15.859
23.5612	20.9226	[15.859]
23.7542	21.0957	15.870
23.6568	21.8994	15.884
23.6179	21.8593	15.848
24.6021	21.8499	15.878
24.3047	21.5788	15.832
23.6172	20.9709	15.849
		Mean, 15.861, ± .0052

The authors reject the three bracketed determinations, because of irregularities in the course of the experiments. The mean of the ten remaining determinations is 15.855, ±.0044. Both means, however, have to be corrected for the minute trace of hydrogen occluded by the reduced copper. This correction, experimentally measured, amounts to +.006. Hence the mean of all the experiments in the series becomes 15.867, ±.0052, and of the ten accepted experiments, 15.861, ±.0044. The authors themselves select out seven experiments, giving a corrected mean of 15.866, which they regard as the best value. Taking all their evidence, their two series combine thus:

First series	15.865,	± .0103
Second series	15.867,	± .0052
General mean	15.8667,	± .0046

Leduc,[1] who also effected the synthesis of water over copper oxide, following Dumas' method with slight modifications, gives the results of two experiments, as follows:

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[1] Compt. Rend., 115, 41. 1892. See also the complete memoir in Ann. Chim. Phys. (7), 15, 48. 1898. In the latter Leduc gave a preliminary determination which made O = 15.860.

Wt. Water.	*Wt. O.*	*Ratio H:O.*
22.1632	19.6844	15.882
19.7403	17.5323	15.880
		Mean, 15.881

These experiments we may arbitrarily assign equal weight with two in Dittmar and Henderson's later series, when the result becomes 15.881, ±.0132, the value to be accepted. Leduc states that his copper oxide, which was reduced at as low a temperature as possible, was prepared by heating clippings of electrolytic copper in a stream of oxygen.

To E. W. Morley[1] we owe the first complete quantitative syntheses of water, in which both gases were weighed separately, and afterwards in combination. The hydrogen was weighed in palladium, as was done by Keiser, and the oxygen was weighed in compensated globes, after the manner of Regnault. The globes were contained in an artificial "cave," to protect them from moisture and from changes of temperature; being so arranged that they could be weighed by the method of reversals without opening either the "cave" or the balance case. For each weighing of hydrogen about 600 grammes of palladium were employed. After weighing, the gases were burned by means of electric sparks in a suitable apparatus, from which the unburned residue could be withdrawn for examination. Finally, the apparatus containing the water produced was closed by fusion and also weighed. Rubber joints were avoided in the construction of the apparatus, and the connections were continuous throughout. The weights and derived ratios are as follows:

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
3.2645	25.9176	29.1788	15.878	17.877
3.2559	25.8531	29.1052	15.881	17.878
3.8193	30.3210	34.1389	15.878	17.873
3.8450	30.5294	lost	15.880	.....
3.8382	30.4700	34.3151	15.877	17.881
3.8523	30.5818	34.4327	15.877	17.876
3.8298	30.4013	34.2284	15.877	17.875
3.8286	30.3966	34.2261	15.878	17.879
3.8225	30.3497	34.1742	15.879	17.881
3.8220	30.3479	34.1743	15.881	17.883
3.7637	29.8865	33.6540	15.881	17.883
3.8211	30.3429	34.1559	15.882	17.878
		Mean,	15.8792, ± .00032	17.8785, ± .00066

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[1] "On the Density of Oxygen and Hydrogen, and on the Ratio of their Atomic Weights," by Edward W. Morley. Smithsonian Contributions to Knowledge, 29, 1895, 4to, xi + 117 pp., 40 cuts. Abstract in Am. Chem. Journ., 17, 267 (gravimetric), and Ztschr. phys. Chem., 17, 87 (gaseous densities); also note in Am. Chem. Journ., 17, 396. Preliminary notice in Proc. Amer. Association, 1891, p. 185. See also a discussion by Morley of all the earlier determinations, in the Western Reserve University Bulletin, for April, 1895.

Combined, these data give:

From ratio H:O	15.8792, ± .00032
From ratio $H:H_2O$	15.8785, ± .00066
General mean	15.8790, ± .00028

For details Morley's original memoir must be consulted. No abstract can do full justice to it.

Two other series of determinations, by Julius Thomsen, are radically different in method from all the previous work. In the first series[1] he determined the ratio between HCl and NH_3; and thence, using Stas' values for Cl and N, fixed by reference to O=16, computed the ratio H:O. This method was so indirect as to be of little importance, and gave for the atomic weight of oxygen approximately the round number 16. I shall use the data farther on for another purpose. The paper has been sufficiently criticised by Meyer and Seubert,[2] who have discussed its sources of error.

In Thomsen's later memoir[3] a method of determination is described which is, like the preceding, quite novel, but more direct. First, aluminum, in weighed quantities, was dissolved in caustic potash solution. In one set of experiments the apparatus was so constructed that the hydrogen evolved was dried and then expelled. The loss of weight of the apparatus gave the weight of the hydrogen so liberated. In the second set of experiments the hydrogen passed into a combustion chamber in which it was burned with oxygen, the water being retained. The increase in weight of this apparatus gave the weight of oxygen so taken up. The two series, reduced to the standard of a unit weight of aluminum, gave the ratio between oxygen and hydrogen.

The results of the two series, reduced to a vacuum and stated as ratios, are as follows:

First.	*Second.*
Weight of H Weight of Al	Weight of O Weight of Al
0.11180	0.88788
0.11175	0.88799
0.11194	0.88774
0.11205	0.88779
0.11189	0.88785
0.11200	0.88789
0.11194	0.88798
0.11175	0.88787
0.11190	0.88773

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[1] Zeitsch. physikal Chem., 13, 398. 1894.

[2] Ber. Deutsch. chem. Ges., 27, 2770.

[3] Zeitsch. anorg. Chem., 11, 14. 1895.

0.11182	0.88798
0.11204	0.88785
0.11202	
0.11204	0.88787, ± 0.000018
0.11179	
0.11178	
0.11202	
0.11188	
0.11186	
0.11185	
0.11190	
0.11187	
0.11190, ± 0.000015	

Dividing the mean of the second column by the mean of the first, we have for the equivalent of oxygen:

$$\frac{0.88787, \pm 0.000018}{0.11190, \pm 0.000015} = 7.9345, \pm 0.0011$$

Hence $O = 15.8690, \pm 0.0022$.

The details of the investigation are somewhat complicated, and involve various corrections which need not be considered here. The result as finally stated includes all corrections and is evidently good.

The syntheses of water reported by Keiser[1] in 1898, involved the direct oxidation of hydrogen occluded in palladium, with subsequent weighing of the water so produced in the vessel in which it was generated. That vessel was tubular in form, and divided into two compartments; one containing phosphorus pentoxide, to absorb the water, the other holding the palladium hydride. Each determination required five weighings, as follows: First, of the vessel, containing only the drying agent, and exhausted of air. Second, the same as the first, plus the palladium. Third, the gain in weight was measured after saturating the palladium with hydrogen. Fourth, the entire apparatus after complete oxidation of the hydrogen to water. The gain in weight gave the oxygen absorbed. Fifth, like the fourth, but with the palladium removed. The difference between the first and fifth weighings gave the amount of water formed. All the operations were thus performed in a single piece of apparatus, and troublesome corrections were avoided. The data obtained were as follows, with weights not reduced to a vacuum standard:

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
.27549	2.18249	2.45975	15.8444	17.8573
.27936	2.21896	2.49923	15.8860	17.8925
.27091	2.15077	2.42355	15.8781	17.8919
.26845	2.13270	2.40269	15.8890	17.9005
		Mean,	15.8744	15.8855

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[1] Amer. Chem. Journ., 20, 733. 1898.

Taken as one series, the two sets of values, eight determinations in all, give for the ratio H : O the number 15.8799, ±.0046. This figure is slightly higher than Morley's average, but below his maximum.

Late in 1907, an elaborate investigation by Noyes[1] was published, covering five series of syntheses. The first series of twenty experiments, however, was found to be affected by a small constant error, and it was therefore rejected. The other series gave the subjoined results, with all corrections, including the reduction to a vacuum, applied.

Second Series. Electrolytic hydrogen, from sulphuric acid, was weighed in palladium, and again in the copper oxide tube in which it was oxidized to water. The apparatus was similar to that used in his former research, and so, too, but with differences in detail, was the procedure.

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
3.72565	29.57891	33.30408	15.8785	17.8783
3.80318	30.18400	33.98748	15.8730	17.8732
3.75873	29.83358	33.59127	15.8743	17.8737
2.96328	23.5197	26.48379	15.8742	17.8746
2.11395	lost	18.89214	......	17.8734
3.53136	28.02910	31.56024	15.8744	17.8743
3.53959	28.09619	31.63554	15.8754	17.8753
		Mean,	15.8750,	17.8747,
			± .00052	± .00045

Third Series. Hydrogen from sulphuric acid was passed directly into the copper oxide bulb and there converted into water.

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
2.44279	19.39757	21.84042	15.8813	17.8815
2.18739	17.36305	19.55117	15.8756	17.8763
2.75129	21.84345	24.59389	15.8787	17.8781
4.00062	....	35.75083	......	17.8726
4.04057	32.07689	36.11762	15.8774	17.8775
		Mean,	15.8782,	17.8772,
			± .00091	± .00097

Fourth Series. Hydrogen and oxygen, both obtained by electrolysis of sulphuric acid, were directly combined by means of palladium, somewhat as in Keiser's determinations. The use of copper oxide was thus avoided.

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[1] Amer. Chem. Journ., 29, 1718. 1907. In Vol. 30, p. 4, 1908, Noyes discusses all determinations of the atomic weight of hydrogen, and proposes criteria for the rejection of doubtful data.

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
2.27916	18.08455	20.36128	15.8695	17.8674
4.12734	32.76527	36.89043	15.8772	17.8761
4.17556	33.13449	37.30787	15.8707	17.8696
4.19346	33.27384	37.46453	15.8694	17.8681
2.30746	18.30863	20.61357	15.8691	17.8669
4.59692	36.48543	41.08162	15.8739	17.8735
4.63625	36.79354	41.42905	15.8721	17.8718
4.57274	36.28696	40.85834	15.8710	17.8704
		Mean,	15.8716, ± .00066	17.8705, ± .00074

Fifth Series. Essentially like series four, except that the two gases were prepared by electrolysis of barium hydroxide.

H taken.	*O taken.*	*H_2O formed.*	*H:O.*	*$H:H_2O$.*
4.61180	36.60909	41.22105	15.8763	17.8763
4.62358	36.69575	41.31647	15.8733	17.8721
4.59853	36.50484	41.10212	15.8768	17.8762
4.55832	36.17887	40.73904	15.8738	17.8746
4.20399	33.37000	37.57336	15.8754	17.8751
		Mean,	15.8751, ± .00046	17.8749, ± .00051

Since these series of measurements represent different methods, and are evidently of unequal value, it is best to combine them mathematically, giving each mean a weight inversely proportional to the square of its probable error. Noyes computed all of the experiments on the basis of the oxygen standard, giving for each one its expression as the atomic weight of hydrogen. I have chosen the present form for simplicity of calculation, and for greater ease of combination with previous determinations. In the following table I give Noyes' deductions in an additional column[1]:

		Ratio H:O.	*Atomic weight H.*
Series 2.	H:O	15.8750, ± .00052	1.00787
	$H:H_2O$	15.8747, ± .00045	1.00789
Series 3.	H:O	15.8782, ± .00091	1.00767
	$H:H_2O$	15.8772, ± .00097	1.00774
Series 4.	H:O	15.8716, ± .00066	1.00809
	$H:H_2O$	15.8705, ± .00074	1.00815
Series 5.	H:O	15.8751, ± .00046	1.00786
	$H:H_2O$	15.8749, ± .00051	1.00788
	General mean..	15.8745, ± .00021	1.00783, ± .000013

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[1] See Noyes' memoir for the results of combining his data in different ways. The 48 experiments, taken as one series, give H = 1.00793, ±.00002.

Referring to his determinations published in 1890, Noyes points out a constant error in them, the elimination of which reduces the value of the ratio to 15.879, in agreement with the measurements by Morley.

The details of Noyes' investigation are too voluminous for repetition here. It goes almost without saying that every precaution was taken which his own previous experience and the experience of others could suggest, and that his materials were of the highest degree of purity. The true value of the ratio must lie somewhere within the range of variation shown by his individual determinations, which it may be observed, overlap those of Morley.

We have now before us, for combination, fifteen sets of determinations of the hydrogen-oxygen ratio. I have arranged them in the order of descending magnitude, and computed their general mean as follows:

	Ratio.	*Atomic weight H.*
1. Erdmann and Marchand	15.975, ± .0113	1.00156
2. Dumas	15.9607, ± .0070	1.00246
3. Keiser, 1888	15.9514, ± .0011	1.00305
4. Thomsen, 1870	15.91, ± .0113	1.00565
5. Noyes, 1890, uncorrected	15.8966, ± .0017	1.00650
6. Dulong and Berzelius	15.894, ± .0570	1.00667
7. Rayleigh	15.89, ± .0090	1.00692
8. Leduc	15.881, ± .0132	1.00750
9. Keiser, 1898	15.8799, ± .0046	1.00756
10. Morley	15.8790, ± .00028	1.00762
11. Noyes, 1907	15.8745, ± .00021	1.00783
12. Cooke and Richards	15.8690, ± .0020	1.00825
13. Thomsen, 1895	15.8690, ± .0022	1.00825
14. Dittmar and Henderson	15.8677, ± .0046	1.00834
15. Keiser, 1887	15.864, ± .0150	1.00857
General mean	15.8779, ± .00016	1.00769, ± .00001

In this combination, which includes all the syntheses, good or bad, the general mean lies between the values found by Noyes and Morley. It is, therefore, not far from the truth. If we reject the high values, Nos. 1 to 7, the general mean becomes 15.8760, ± .00017, and H = 1.00781, ± .00001. Values 10 and 11, combined, give 15.8761, ± .00017, and H = 1.00780, ± .00001. That is, the Morley and Noyes determinations control all the others, and practically eliminate them. The high and low figures tend to balance one another, and so to disappear from the final combination.

In discussing the relative densities of oxygen and hydrogen gases we need consider only the more modern determinations, beginning with those of Dumas and Boussingault. As the older work has some historical value, I may in passing just cite its results. For the density of hydrogen we have .0769, Lavoisier; .0693, Thomson; .092, Cavendish; .0732, Biot and Arago; .0688, Dulong and Berzelius. For oxygen there are the following determinations: 1.087, Fourcroy, Vauquelin, and Séguin; 1.103, Kirwan; 1.128, Davy; 1.088, Allen and Pepys; 1.1036, Biot and Arago; 1.1117, Thomson; 1.1056, De Saussure; 1.1026, Dulong and Berzelius; 1.106, Buff; 1.1052, Wrede.[1]

In 1841 Dumas and Boussingault[2] published their determinations of gaseous densities. For hydrogen they obtained values ranging from .0691 to .0695; but beyond this mere statement they give no details. For oxygen three determinations were made, with the following results:

1.1055
1.1058
1.1057

Mean, 1.10567, ± .00006

If we take the two extreme values given above for hydrogen, and regard them as the entire series, they give us a mean of .0693, ± .00013. This mean hydrogen value, combined with the mean for oxygen, gives for the latter, when H=1, the density ratio 15.9538, ± .031.

Regnault's researches, published four years later,[3] were much more elaborately executed. Indeed, they long stood among the classics of physical science, and it is only recently that they have been supplanted by other measurements.

For hydrogen three determinations of density gave the following results:

.06923
.06932
.06924

Mean, .069263, ± .000019

For oxygen four determinations were made, but in the first one the gas was contaminated by traces of hydrogen, and the value obtained, 1.10525, was, therefore, rejected by Regnault as too low. The other three are as follows:

1.10561
1.10564
1.10565

Mean, 1.105633, ± .000008

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[1] For Wrede's work, see Berzelius' Jahresbericht for 1843. For Dulong and Berzelius, see the paper already cited. All the other determinations are taken from Gmelin's Handbook, Cavendish edition, v. 1, p. 279.

[2] Compt. Rend., 12, 1005. Compare also with Dumas, Compt. Rend., 14, 537.

[3] Compt. Rend., 20, 975.

Now, combining the hydrogen and oxygen series, we have the ratio H : O :: 1 : 15.9628, ±.0044. According to Le Conte,[1] Regnault's reductions contain slight numerical errors, which, corrected, give for the density of oxygen, 1.105612, and for hydrogen, .069269. Ratio, 1 : 15.9611.

A much weightier correction to Regnault's data has already been indictated in the discussion of Cooke and Richards' work. He assumed that the globes in which the gases were weighed underwent no changes of volume, but Agamennone,[2] and after him, but independently,[3] Lord Rayleigh showed that an exhausted vessel was perceptibly compressed by atmospheric pressure. Hence its volume when empty was less than its volume when filled with gas. Crafts, having access to Regnault's original apparatus, has determined the magnitude of the correction indicated.[4] Unfortunately, the globe actually used by Regnault had been destroyed, but another globe of the same lot was available. With this the amount of shrinkage during exhaustion was measured, and Regnault's densities were thereby changed to 1.10562 for oxygen, and .06949 for hydrogen. Corrected ratio, 1 : 15.9105. Doubtless Dumas and Boussingault's data are subject to a similar correction, and if we assume that it is proportionally the same in amount, the ratio derived from their experiments becomes 1 : 15.9015.

In the same paper, that which contained the discovery of this correction, Lord Rayleigh gives a short series of measurements of his own. His hydrogen was prepared from zinc and sulphuric acid, and was purified by passage over liquid potash, then through powdered mercuric chloride, and pulverized solid potash successively. It was dried by means of phosphorus pentoxide. His oxygen was derived partly from potassium chlorate, and partly from the mixed chlorates of sodium and potassium. Equal volumes of the two gases weighed as follows:

	H.	*O.*
	.15811	2.5186, ± .00061[5]
	.15807	
	.15798	
	.15792	
Mean,	.15802, ± .000029	

Corrected for shrinkage of the exhausted globe these become—H, 0.15860; O, 2.5192. Hence the ratio 1 : 15.884, ±.0048.

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[1] Private communication. See also Phil. Mag. (4), 27, 29, 1864, and Smithsonian Report, 1878, p. 428.

[2] Atti Rendiconti Acad. Lincei, 1885.

[3] Proc. Roy. Soc., 43, 356. Feb., 1888.

[4] Compt. Rend., 106, 1662.

[5] Arbitrarily assigned the probable error of a single experiment in Rayleigh's paper of 1892.

In 1892 Rayleigh published a much more elaborate determination of this ratio.[1] The gases were prepared electrolytically from caustic potash, and dried by means of solid potash and phosphorus pentoxide. The hydrogen was previously passed over hot copper. The experiments, stated like the previous series, are in five groups; two for oxygen and three for hydrogen; but for present purposes the similar sets may be regarded as equal in weight, and so discussable together. The weights of equal volumes are as follows:

	H.	*O.*	
First set Mean, .15808	.15807	2.5182	First set Mean, 2.51785
	.15816	2.5173	
	.15811	2.5172	
	.15803	2.5193	
	.15801	2.5174	
	.15809	2.5177	
Second set Mean, .15797	.15800	2.5183	Second set Mean, 2.5172
	.15820	2.5168	
	.15792	2.5172	
	.15788	2.5181	
	.15783	2.5156	
Third set Mean, .15804	.15807		
	.15801		
	.15817		
	.15790		
	.15810		
	.15798		
	.15802		
	.15807		
	Mean, .15804, ± .000019	Mean, 2.5176, ± .00019	

These weights with various corrections relative to temperatures and pressures, and also for the compression of the exhausted globe, ultimately become for H, .158531; and for O, 2.51777. Hence the ratio 1 : 15.882, ±.0023. For details relative to corrections the original memoir should be consulted.

In his paper " On a new method of determining gas densities,"[2] Cooke gives three measurements for hydrogen, referred to air as unity. They are:

.06957
.06951
.06966

Mean, .06958, ± .000029

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[1] Proc. Roy. Soc., 50, 448, Feb. 18, 1892.

[2] Proc. Amer. Acad., 24, 202. 1889. Also Am. Chem. Journ., 11, 509.

Combining this with Regnault's density for oxygen, as corrected by Crafts, 1.10562, ±.000008, we get the ratio H : O :: 1 : 15.890, ±.0067.

Leduc, working by Regnault's method, somewhat modified, and correcting for shrinkage of exhausted globes, gives the following densities[1]:

H.	*O.*
.06947	1.10501
.06949	1.10516
.06947	
Mean, .06948, ± .00006745	

The two oxygen measurements are the extremes of three, the mean being 1.10506, ±.0000337. Hence the ratio 1 : 15.905, ±.0154.

In a later memoir Leduc[2] gives two more measurements of the density of oxygen. They are 1.10527 and 1.10521. If we include these in series with the other values the mean becomes 1.10514, ±.0000321. The use of this figure in subsequent combinations of data has an insignificant effect upon the computations. It raises O from 15.905 to 15.906.

The first two hydrogen determinations were made with gas produced by the electrolysis of caustic potash, while the third sample was derived from zinc and sulphuric acid. The oxygen was electrolytic. Both gases were passed over red-hot platinum sponge, and dried by phosphorus pentoxide.

Much more elaborate determinations of the two gaseous densities are those made by Morley.[3] For oxygen he gives three series of data: two with oxygen from potassium chlorate, and one with gas partly from the same source and partly electrolytic. In the first series, temperature and pressure were measured with a mercurial thermometer and a mano-barometer. In the second series they were not determined for each experiment, but were fixed by comparison with a standard volume of hydrogen by means of a differential manometer. In the third series the gas was kept at the temperature of melting ice, and the mano-barometer alone was read. The results for the weight in grammes, at latitude 45°, of one litre of oxygen are as follows:

First Series.	*Second Series.*	*Third Series.*
1.42864	1.42952	1.42920
1.42849	1.42900	1.42860
1.42838	1.42863	1.42906
1.42900	1.42853	1.42957
1.42907	1.42858	1.42910

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[1] Compt. Rend., 113, 186. 1891.

[2] Ann. Chim. Phys. (7), 15, 29. 1898. In C. R., 148, 42, Leduc claims that the probable error of his H is only ± .00001.

[3] Paper already cited, in the gravimetric portion of this chapter.

1.42887	1.42873	1.42930
1.42871	1.42913	1.42945
1.42872	1.42905	1.42932
1.42883	1.42896	1.42908
	1.42880	1.42910
Mean, 1.42875, ± .000051	1.42874	1.42951
Corrected,[1] 1.42879, ± .000051	1.42878	1.42933
	1.42872	1.42905
	1.42859	1.42914
	1.42851	1.42849
		1.42894
	Mean, 1.42882, ± .000048	1.42886
	Corrected, 1.42887, ± .000048	
		Mean, 1.42912, ± .000048
		Corrected, 1.42917, ± .000048

General mean of all three series, 1.42896, ± .000028.

Morley himself, for experimental reasons, prefers the last series, and gives it double weight, getting a mean density of 1.42900. The difference between this mean and that given above is insignificant with reference to the atomic weight problem.

In the case of hydrogen, Morley's determinations fall into two groups, but in both the gas was prepared by the electrolysis of pure dilute sulphuric acid, and was most elaborately purified. In the first group there are two series of measurements. Of these, the first involved the reading of temperature and pressure by means of a mercurial thermometer and mano-barometer. In the second series, the gas was delivered into the weighing globes after occlusion in palladium; it was then kept at the temperature of melting ice, and only the syphon barometer was read. In this group the hydrogen was possibly contaminated with mercurial vapor, and the results are discarded by Morley in his final summing up. For present purposes, however, it is unnecessary to reject them, for they have confirmatory value, and do not appreciably affect the final mean. The weight of one litre of hydrogen at 45° latitude, as found in these two sets of determinations, is as follows:

First Series.	*Second Series.*
.089904	.089977
.089936	.089894
.089945	.089987
.089993	.089948
.089974	.089951
.089941	.089960
.089979	.090018

---

[1] Correction applied by Morley to all his series, for a slight error, $\frac{1}{30000}$, in the length of his standard metre bar.

.089936	.089909
.089904	.089953
.089863	.089974
.089878	.089922
.089920	.090093
.089990	.090007
.089926	.089899
.089928	.089974
	.089900
	.089869
	.090144
	.089984
Mean, .089934, ± .000007	Mean, .089967, ± .000011
Corrected, .089938, ± .000007	Corrected, .089970, ± .000011

In the second group of experiments, the hydrogen was weighed in palladium before transfer to the calibrated globe; and in weighing, the palladium tube was tared by a similar apparatus of nearly equal volume and weight. After transfer, which was affected without the intervention of stopcocks, the volume and pressure of the gas were taken at the temperature of melting ice. A preliminary set of measurements was made, followed by three regular series; of these, the first and second were with the same apparatus, and are different only in point of time, a vacation falling between them. The last series was with a different apparatus. The data are as follows, with the means as usual:

Preliminary.	*Third Series.*	*Fourth Series.*	*Fifth Series.*
.089946	.089874	.089972	.089861
.089915	.089891	.089877	.089877
.089881	.089886	.089867	.089870
.089901	.089866	.089916	.089867
.089945	.089911	.089770	.089839
	.089856	.089846	.089874
	.089912		.089864
	.089872		.089883
			.089830
			.089877
			.089851
Mean, .089918, ± .0000271	Mean, .089883, ± .0000049	Mean, .089875, ± .0000187	Mean, .089863, ± .0000034
Corrected, .089921	Corrected, .089886	Corrected, .089880	Corrected, .089866

Now, rejecting nothing, we may combine all the series into a general mean, giving the weight of one litre of hydrogen as follows:

First series	.089938, ± .000007
Second series	.089970, ± .000011
Preliminary series, second method	.089921, ± .0000271
Third series	.089886, ± .0000049
Fourth series	.089880, ± .0000187
Fifth series	.089866, ± .0000034
General mean	.089897, ± .0000025
Rejecting the first three	.089872, ± .0000028

This last mean value for hydrogen will be used in succeeding chapters of this work for reducing volumes of the gas to weights. Combining the general mean of all with the value found for the weight of a litre of oxygen, 1.42896, ±.000028, we get for the ratio H : O,

$$O = 15.8955, \pm .0005$$

If we take only the second mean for H, excluding the first three series, we have—

$$O = 15.9001, \pm .0005$$

This value is undoubtedly nearest the truth, and is preferable to all other determinations of the density ratio. Its probable error, however, is given too low; for some of the oxygen weighings involved reductions for temperature and pressure. These reductions involve, again, the coefficient of expansion of the gas, and its probable error should be included. Since, however, that factor has been disregarded elsewhere, it would be an over-refinement of calculation to include it here. Other corrections, of a mathematical character, have been recently applied to Morley's data by Guye and Mallet.[1] They find, for the normal weight of one litre of each gas, O = 1.42886, and H = 0.089875. The difference between these figures and those given by Morley is so small as to be negligible.

Still more recently, by a novel method, J. Thomsen has measured the two densities in question.[2] In his gravimetric research, already cited, he ascertained the weights of hydrogen and of oxygen equivalent to a unit weight of aluminum. In his later paper he describes a method of measuring the corresponding volumes of both gases during the same reactions. Then, having already the weights of the gases, the volume-weight ratio, or density, is in each case easily computable. From 1.0171 to 2.3932 grammes of aluminum were used in each experiment. Omitting details, the volume of hydrogen in litres, equivalent to one gramme of the metal, is as follows:

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[1] Compt. Rend., 138, 1034. 1904.

[2] Zeitsch. anorg. Chem., 12, 4. 1896.

1.24297
1.24303
1.24286
1.24271
1.24283
1.24260
1.24314
1.24294

Mean, 1.24289, ± .00004

The weight of hydrogen evolved from one gramme of aluminum was found in Thomsen's gravimetric research to be 0.11190, ±.000015. Hence the weight of one litre at 0°, 760 mm., and 10.6 meters above sea level at Copenhagen is:

.090032, ± .000012

or at sea level in latitude 45°,

.089947, ± .000012 gramme

The data for oxygen are given in somewhat different form, namely. for the volume of one gramme of the gas at 0°, 760, and at Copenhagen. The values are, in litres:

.69902
.69923
.69912
.69917
.69903
.69900
.69901
.69921
.69901
.69922

Mean, .69910, ± .00002
At sea level in latitude 45°, .69976, ± .00002

Hence one litre weighs 1.42906, ±.00004 grammes.

Dividing this by the weight found for hydrogen, 0.089947, ±.000012 we have for the ratio H : O,

15.8878, ± .0022

The determinations, by Jaquerod and Pintza,[1] of the weight of a litre of oxygen, can hardly be utilized here. They give, as the mean of five observations, the value 1.4292 grammes, but without the individual figures, and with no corresponding data for hydrogen. The ratio now under consideration, therefore, is not directly given by their work.

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[1] Compt. Rend., 139, 129. 1904. Compare also Jaquerod and Scheuer, *ibid.*, 140, 1384. 1905.

The density ratios, H : O, now combine as follows:

Dumas and Boussingault, corrected	15.9015, ± .031
Regnault, corrected	15.9105, ± .0044
Rayleigh, 1888	15.884, ± .0048
Rayleigh, 1892	15.882, ± .0023
Cooke	15.890, ± .0067
Leduc	15.906, ± .0154
Morley, including all the data	15.8955, ± .0005
Thomsen	15.8878, ± .0022
General mean	15.8948, ± .00048

If we reject all of Morley's data for the density of hydrogen except his third, fourth and fifth series, the mean becomes

15.8991, ± .00048

In either case Morley's data vastly outweigh all others.

If oxygen and hydrogen were perfect gases, uniting by volume exactly in the ratio of one to two, then their relative densities would also indicate their relative molecular weights. But, in fact, the two gases vary from Boyle's law in opposite directions, and the true composition of water by volume diverges from the theoretical ratio to a measurable extent. Hence, in order to deduce the atomic weight of hydrogen from its density, or that of oxygen, if the hydrogen scale is preferred, a small correction must be applied which depends upon the amount of the divergence. Until modern times our knowledge of the volumetric composition of water rested entirely upon the determinations made by Humboldt and Gay Lussac[1] early in the last century, which gave a ratio between H and O of a little less than 2 : 1, but their data need no farther consideration here.

In 1887 Scott[2] published his first series of experiments, 21 in number, finding as the most probable result a value for the ratio of 1.994 : 1. In March, 1888,[3] he gave four more determinations, ranging from 1.9962 to 1.998 : 1; and later in the same year[4] another four, with values from 1.995 to 2.001. In 1893,[5] however, by the use of improved apparatus, he was able to show that his previous work was vitiated by errors, and to give a series of measurements of far greater value. Of these, twelve were especially good, being made with hydrogen from palladium hydride, and with oxygen from silver oxide. In mean the value found is 2.00245, ±.00007, with a range from 2.0017 to 2.0030.

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[1] Journ. de Phys., 60, 129.

[2] Proc. Roy. Soc., 42, 396.

[3] Nature, 37, 439.

[4] British Assoc. Report, 1888, 631.

[5] Proc. Roy. Soc., 53, 130. In full in Philosophical Transactions, 184, 543. 1893.

In 1891 an elaborate paper by Morley[1] appeared, in which twenty concordant determinations of the volumetric ratio gave a mean value of 2.00023, ±.000015. These measurements were made in eudiometer tubes, and were afterwards practically discarded by the author. In his later and larger paper,[2] however, he redetermined the ratio from the density of the mixed electrolytic gases, and found it to be, after applying all corrections, 2.00274. The probable error, roughly estimated, is .00005. Morley also reduces Scott's determinations, which were made at the temperature of the laboratory, to 0°, when the value becomes 2.00285. The mean value of both series may therefore be put at 2.0028, ±.00004, with sufficient accuracy for present purposes. Leduc's[3] single determination, based upon the density of the mixed gases obtained by the electrolysis of water, gave 2.0037; but Morley shows that some corrections were neglected. This determination, therefore, may be left out of account.

There is also a corroborative measurement by Rayleigh,[4] who assigns to the ratio the value 2.0026. This agrees well with the figures given by Scott and Morley. Rayleigh also gives measurements of gaseous densities at very low pressures, and obtains molecular ratios differing considerably from those ordinarily found. At atmospheric pressure, for example, H=1.0075; and at very low pressures its atomic weight becomes 1.0088.

Now, including all available data, we have as a mean value for the density ratio:

(A.) H:O::1:15.8948, ± .00048

or, omittting Morley's rejected series,

(B.) H:O::1:15.8991, ± .00048

Correcting these by the volume ratio, 2.0028, ±.00004, the final result for the atomic weight of oxygen, in terms of the hydrogen unit, and as computed from the gaseous densities becomes—

From A .... O=15.8726, ± .00058
From B .... O=15.8769, ± .00058

Combining these figures with the values deduced from the syntheses of water, rejecting nothing, we have—

By syntheses of water.......... O=15.8779, ± .00016
By gaseous densities............ O=15.8726, ± .00058

General mean .... O=15.8775, ± .00015

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[1] Amer. Journ. Sci. (3), 46, 220 and 276.

[2] Already cited with reference to syntheses of water.

[3] Compt. Rend., 175, 311. 1892. In a later, more complete memoir, Ann. Chim. Phys. (7), 15, 49, Leduc gives the figure 2.0034. He also criticizes Morley's deductions.

[4] Proc. Roy. Soc., 73, 153. 1904.

Hence, on the oxygen scale, H = 1.00772 ± .00001.

If we reject the seven highest values under the first heading, and omit Morley's defective hydrogen series under the second, we get—

By syntheses of water........	O = 15.8760, ± .00017, or	H = 1.00781, ± .00001
By gaseous densities..........	O = 15.8769, ± .00058, or	H = 1.00775, ± .000035
General mean	O = 15.8762, ± .00016, or	H = 1.00779, ± .00001

The two component values of the last mean are remarkably concordant, differing by only one part in 17640. For practical purposes the last decimal of the hydrogen value may be rounded off, giving

H = 1.0078, ± .00001

as the atomic weight under consideration. The actual uncertainty of this value, however, is greater than the so-called "probable error." The latter, it must be borne in mind, is a mathematical expression which should not be used in a colloquial sense. For computations of this kind the probable error is essentially a *coefficient of concordance*, which merely indicates the relative value or weight assignable to a given series of observations in comparison or combination with others.

THE NITROGEN-OXYGEN RATIO.

The direct ratio between nitrogen and oxygen has been determined by analyses of nitrous and nitric oxides, and by measurements of gaseous densities. The different methods may be considered in regular order.

The exact analysis of nitrous oxide, with reference to the atomic weight of nitrogen, was effected by Guye and Bogdan.[1] The gas itself was condensed in carefully purified charcoal, and so weighed; it was then passed slowly through a tube containing a spiral of iron wire, which was heated to redness by an electric current. The iron was oxidized, and its gain in weight gave the amount of oxygen in the N_2O. The results obtained were as follows:

Weight N_2O	*Weight O.*	*At. Wt. O.*
1.1670	.4242	14.0085
.9498	.3453	14.0052
.8652	.3145	14.0083
1.2247	.4455	13.9924
1.4202	.5159	14.0229
		Mean, 14.0075, ± .0033

For the complete gravimetric analysis of nitric oxide we have the elaborate data furnished by R. W. Gray.[2] The gas was weighed, and

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[1] Compt. Rend., 138, 1494. 1904. Journ. Chim. Phys., 3, 537. 1905.

[2] Journ. Chem. Soc., 87, 1601. 1905.

then reduced by electric heating over finely divided metallic nickel. The gain in weight of the nickel represented the amount of oxygen absorbed. In some of the experiments the liberated nitrogen was condensed, at the temperature of liquid air, in cocoa-nut charcoal, and its weight also was determined. Two series of determinations were made, on nickel from different sources, but for present purposes these may be treated as one. For three of the measurements corrections are given for the nitrogen occluded by the mixed nickel and nickel oxide, which corrections I have applied in the following table of Gray's results:

Weight NO.	*Weight O.*	*Weight N.*
.31384	.16729	.....
.64304	.34300	.30010
.50672	.27025	.....
.54829	.29221	.....
.61862	.32981	.28885
.62622	.33401	.29234
.62128	.33111	.....
.54469	.29029	.25432
.52001	.27715	.24270
.62103	.33103	.28998

From these weights the subjoined values for N are derived.

	$NO:O_2$.	$N_2:O_2$.	$NO:N_2$.
	14.0164	......	......
	13.9960	13.9988	13.9996
	14.0001	......	......
	14.0217	......	......
	14.0110	14.0129	14.0146
	13.9997	14.0039	14.0094
	14.0217	......	......
	14.0218	14.0174	14.0136
	14.0204	14.0112	14.0031
	14.0169	14.0159	14.0151
Mean,	14.0126, ± .0022	14.0100, ± .0020	14.0092, ± .0018

The general mean of the three series is

$$N = 14.0104, \pm .0011$$

The accurate calculation of molecular and atomic weights from gaseous densities is really an affair of very recent times. The gases, as measured, show divergencies from Avogadro's law, and the crude density ratios therefore require correction, as we have already seen in reference to the atomic weight of hydrogen. It seems best, however, to assemble the actual measurements first, and to apply the corrections to the entire mass of data afterwards.

For nitrogen there are abundant measurements made upon the element itself, and also good data for nitrous oxide, nitric oxide and ammonia.

The earlier determinations of the density of nitrogen were all made upon nitrogen derived from the atmosphere. But the supposed nitrogen contained, as we now know, the heavier argon, and the value obtained was therefore incorrect. It is, however, worth while to examine the data, and to see whether a correction for argon may not be advantageously made. The very early work of Biot and Arago, Thomson, Dulong and Berzelius, Lavoisier and others can be neglected, and, as in the case of oxygen, we need consider only the results obtained by Dumas and Boussingault, Regnault, and several more recent investigators.

Taking air as unity, Dumas and Boussingault[1] found the density of atmospheric nitrogen to be—

.970
.972
.974

Mean, .972, ± .00078

For oxygen, as was seen in our discussion of the O : H ratio, the same investigators found a mean of 1.10567, ± .00006. The ratio between this and the nitrogen figure is 16 : 14.0657, ± .0113.

By Regnault[2] much closer work was done. He found the density of atmospheric nitrogen to be as follows:

.97148
.97148
.97154
.97155
.97108
.97108

Mean, .97137, ± .000062

For oxygen, Regnault's mean value is 1.105633, ± .000008. Hence, combining as before, N = 14.057, ± .0009.

Both of the preceding values are affected by a correction for the difference in volume between the weighing globes when full and when empty. This correction, in the case of Regnault's data, was measured by Crafts,[3] who gives 1.10562 for the density of oxygen, and 0.97138 for that of nitrogen. The changes are so small that the ratio remains practically unaltered. The correction in this particular instance, is negligible.

Von Jolly,[4] working with electrolytic oxygen and with nitrogen pre-

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[1] Compt. Rend., 12, 1005. 1841.

[2] Compt. Rend., 20, 975. 1845.

[3] Compt. Rend., 106, 1664.

[4] Annalen der Physik. (2), 6, 529. 1879.

pared by passing air over hot copper, compared the weights of equal volumes of the two gases, with results as follows:

Oxygen.	*Nitrogen.*
1.442470	1.269609
1.442579	1.269389
1.442489	1.269307
1.442570	1.269449
1.442571	1.269515
1.442562	1.269443
1.442478	1.269478
Mean, 1.442545, ± .000013	Mean, 1.269455, ± .000024

The ratio, when O=16, is N=14.0802, ±.0003. Corrected by Rayleigh, the ratio between the weights becomes 14.0805.

The next determination in order of time is Leduc's.[1] He made nine measurements of the density of atmospheric nitrogen, giving a mean of .97203, with extremes of .9719 and .9721; but he neglected to cite the intermediate values. Taking the three figures given as representative, and assuming a fair distribution of the other values between the indicated limits, the probable error of the mean is not far from 0.00002. For oxygen he found 1.10514, ±.000032. The ratio between the two densities is 16 : 14.0729, ±.0005.

Lord Rayleigh,[2] who prepared nitrogen from the atmosphere by several methods, and weighed it in a standard globe in direct comparison with oxygen, obtained the following weights:

Oxygen.	*Nitrogen.*
2.6272	2.31035
2.6271	2.31026
2.6269	2.31024
2.6269	2.31012
2.6271	2.31027
Mean, 2.62704, ± .00004	2.31025, ± .000025

In a later paper[3] Rayleigh gives the following additional weights for atmospheric nitrogen, which are directly comparable with the foregoing series.

2.31017
2.30986
2.31010
2.31001
2.31024
2.31010
2.31028
2.31163
2.30956

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[1] Compt. Rend., 113, 186. 1891.

[2] Proc. Roy. Soc., 53, 134. 1893.

[3] Proc. Roy. Soc., 55, 340. 1894.

Including these figures with those of the first series, the mean of all becomes 2.31023, ±.00008. Correcting these data for the compression of the empty globes, the mean weights become, for oxygen, 2.6276, ±.00004, and for nitrogen, 2.31079, ±.00008. The ratio between them is 16 : 14.0704, ±.0005.

The combination of these determinations is as follows:

Dumas and Boussingault	14.0657, ± .0113
Regnault	14.0570, ± .0009
Von Jolly	14.0805, ± .0003
Leduc	14.0729, ± .0005
Rayleigh	14.0704, ± .0005
General mean	14.0758, ± .00022

Now, to correct this mean for the argon contained in the nitrogen. Good measurements have shown that normal air contains, by volume, 0.937 per cent of argon, and 78.122 of nitrogen. The density of argon, referred to the oxygen standard, is 19.940. Applying these values, the final figure for nitrogen, derived from air, becomes 14.0052, ±.00022, a result which is in harmony with others to be considered presently.

In Rayleigh's investigation of the density of nitrogen it was found that nitrogen from chemical sources was lighter than that extracted from the atmosphere. This led to the discovery of argon, to which reference has already been made. In two of his memoirs[1] Rayleigh has given determinations of the density of "chemical nitrogen" obtained from nitrous oxide, nitric oxide, ammonium nitrite, urea and magnesium nitride, and the gas from all these sources is precisely the same. His weights, given now as one series, and representing the same volume as those previously cited, are as follows:

2.30143	From nitric oxide
2.29890	" " "
2.29816	" " "
2.30182	" " "
2.29869	From nitrous oxide
2.29940	" " "
2.30074	" " "
2.30054	" " "
2.29849	From ammonium nitrite
2.29889	" " "
2.29870	" " "
2.29850	From urea
2.29918	From magnesium nitride

Mean, 2.29949, ± .00024

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[1] Proc. Roy. Soc., 55, 340, 1894, and 57, 266, 1895.

Corrected for the compression of the empty globe, this mean becomes 2.30005. The weight of an equal volume of oxygen was found to be 2.62760, ±.00004. Hence the ratio is 16 : 14.0055, ±.0015.

Two determinations of density for "chemical nitrogen" are given by Leduc.[1] In mean, the value found, referred to air as unity, is 0.96717, ±.00008. This, combined with the figure already cited for oxygen gives, as the value for the ratio under discussion, 16 : 14.0025, ±.0012. There are also two determinations by Gray,[2] whose comparative weights at 0° and 760 mm., are as follows:

Nitrogen.	*Oxygen.*
.32286	.36889
.32275	.36879
Mean, .322805, ± .00004	Mean, .36884, ± .00003

Hence the ratio 16 : 14.0030, ±.0021.

It is evident here that the data given by Leduc and Gray are overvalued in comparison with Rayleigh's much larger series of determinations. The general mean, however, as shown in the following combination, cannot be far from the truth:

Rayleigh	14.0055, ± .0015
Leduc	14.0025, ± .0012
Gray	14.0030, ± .0021
General mean	14.0036, ± .00085

Hence the normal litre of nitrogen weighs 1.25066 grammes.

For the density of nitrous oxide there are several series of measurements. Leduc[3] gives three figures, as follows, referred to air as unity:

1.5304
1.5298
1.5301

Mean, 1.5301, ± .00012

Combined with Leduc's value for oxygen, this gives the density ratio $O_2 : N_2O :: 32 : 44.3050, \pm .0037$.

By Rayleigh there are two series of determinations,[4] made at different times. In the earlier series the gas was possibly contaminated by traces of nitrogen, in the second series the nitrous oxide was purified by condensation at the temperature of liquid air. The weights of nitrous oxide filling his standard globe are subjoined.

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[1] Ann. Chim. Phys. (7), 15, 33. 1898.

[2] Journ. Chem. Soc., 87, 1601. 1905.

[3] Ann. Chim. Phys. (7), 15, 35. 1898.

[4] Proc. Roy. Soc., 62, 204, 1897, and 74, 181, 1904.

1897 series.	*1904 series.*
3.6359	3.6368
3.6354	3.6360
3.6364	3.6362
3.6358	3.6363
3.6360	3.6367
	3.6366
Mean, 3.6359	3.6354
	Mean, 3.6363

These are so nearly together that I venture to treat them as one series, in mean 3.6361, ±.000093. The weight of the same volume of oxygen was 2.6276, ±.00004. The value of the ratio, therefore, is 32 : 44.2819, ±.0037.

The measurements by Guye and Pintza[1] are stated so as to show the weight of a normal litre of nitrous oxide. The figures are, in grammes—

1.97762
1.97707
1.97760

Mean, 1.97743, ± .00015

The weight of a litre of oxygen, according to Morley, is 1.42896, ± .000028. Combining this with Guye and Pintza's figure the ratio becomes 32 : 44.2824, ±.0035.

The three independent values for the density ratio $O_2 : N_2O$, combine as follows:

Leduc	44.3050, ± .0037
Rayleigh	44.2819, ± .0037
Guye and Pintza	44.2824, ± .0035
General mean	44.2895, ± .0021

This mean corresponds to a normal litre-weight for nitrous oxide of 1.97775 grammes.

It is convenient at this point to consider the volumetric analysis of nitrous oxide made by Jaquerod and Bogdan.[2] A measured volume of the gas was decomposed by an electrically heated spiral of iron wire, and the volume of the residual nitrogen was measured afterwards. Then, with the known densities of the two gases, the ratio between them was easily calculable. Reduced to uniform conditions, one litre of nitrous oxide gave the following volumes of nitrogen:

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[1] Compt. Rend., 139, 677. 1904. Corrected in C. R., 141, 51. 1905. The corrected figures are used here.

[2] Journ. Chim. Phys., 3, 562. 1905.

1.00737
1.00698
1.00714
1.00718

Mean, 1.00717, ± .000054

To this value, however, a correction is yet to be applied; namely, for the increase in volume of the iron wire consequent upon oxidation. This demands a deduction of 0.00030, which reduces the mean to 1.00687. That is, one litre of nitrous oxide, decomposed, yields 1.00687 litres of nitrogen. Hence the following calculation:

1 litre N_2O weighs.......................... 1.97775 gramme.[1]
1.00687 N_2 weighs........1.25066 × 1.00687 = 1.25925 "

Oxygen in N_2O, 0.71850 "

From these data, $O : N_2 :: 0.7185 : 1.25925, = 28.0417$, and $N = 14.0208$, ±.0030. The probable error is computed from the figures already given relative to the densities of the gases.

For the density of nitric oxide there are two modern investigations. First, by Gray;[2] second, by Guye and Davila.[3] Gray gives two series of weights, in which nitric oxide is directly compared with an equal volume of oxygen. Two supplementary determinations are cited as additions to series 2.

Oxygen.	*NO, I.*	*NO, II.*
.38230	.35845	.35851
.38229	.35852	.35848
.38227	.35851	.35852
.38225	.35849	.35850
.38226	.35859	.35848
.38230	.35856	.35855
Mean, .38228, ± .0000058	Mean of all, .35851, ± .0000076	

From these weights the crude density ratio is

$$O_2 : NO :: 32 : 30.0102, \pm .0007$$

Guye and Davila prepared their nitric oxide by three distinct methods, and obtained the following figures for the normal litre-weight.

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[1] Jaquerod and Bogdan assume, for the litre-weights of N_2 and N_2O, 1.25045 and 1.97772, respectively. I here use the weights previously computed in this chapter. Jaquerod and Bogdan find N = 14.015.

[2] Journ. Chem. Soc., 87, 1601. 1905.

[3] Compt. Rend., 141, 826. 1905.

I.	*II.*	*III.*
1.3406	1.3403	1.3399
1.3402	1.3398	1.3403
1.3401	1.3400	
1.3407	1.3408	
1.3398	1.3402	
1.3402	1.3402	

The mean of the 14 determinations, taken as one series, is 1.3402, ± .000056. With Morley's value for oxygen, 1.42896, ± .000028, the density ratio becomes—

32:30.0124, ± .00168

Combined with Gray's determination, the weighted mean is

32:30.0106, ± .00065

The density of gaseous ammonia, according to Leduc,[1] is 0.5971, referred to air as unity. But this figure represents only a single determination, with material of doubtful purity, and need not be considered further. Guye and Pintza,[2] with carefully purified ammonia, made five determinations of density, which gave the subjoined results for the weight of the normal litre:

.77080
.77069
.77073
.77099
.77076

Mean, .77079, ± .000035

Perman and Davies[3] made three series of determinations, by two methods; but the first series, with commercial ammonia, is to be rejected. The other series gave the following figures for the weight of one litre of the gas:

I.	*II.*
.7709	.77094
.7711	.77094
.7712	.77090
.7713	.77088
.7711	.77091
.7713	
	Mean, .77094
Mean, .77115	

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[1] Ann. Chim. Phys. (7), 15, 39. 1898.

[2] Compt. Rend., 141, 51. 1905.

[3] Proc. Roy. Soc., 78A, 28. 1906. Perman and Davies give still other density determinations for different temperatures and pressures.

Corrected, by reduction to latitude 45°, etc., these two series become nearly identical with each other, and with Guye and Pintza's average; namely, 0.77085 and 0.77086; in mean, as one series, 0.770855, ±.000034. With Guye and Pintza's figure, the general mean becomes 0.77083, ± .000024. Hence, with Morley's weight for a litre of oxygen, the crude density ratio is

$$O_2:NH_3::32:17.2619, \pm .00063$$

The law of Avogadro, that equal volumes of gases contain equal numbers of molecules, is rigorously true only for ideally perfect gases. For gases as they actually occur it is approximately true, but with varying degrees of divergence. The approximation is close for the so-called permanent gases, while those which are easily liquefiable conform less nearly to the law. In order, therefore, to compute molecular weights from observed gaseous densities, it is necessary to apply corrections to the experimental data, or else to employ methods of determination of great manipulative difficulty. By measuring densities at very low pressures, quite close approximations to the truth may be obtained, and observations at high temperatures are also nearly valid. For example, Rayleigh[1] from gaseous densities at very small pressures, obtained the following value for nitrogen, as compared with the standard, oxygen:

$$N_2 = 28.018, \text{ and } N = 14.009$$

On the other hand, by measuring the density of nitrogen at 1067.4°, Jaquerod and Perrot[2] found

$$N_2 = 28.0155, \text{ and } N = 14.0077$$

These values are probably not far from the truth, and are obviously well in accord. At low pressures and at high temperatures gases are more nearly in agreement with Avogadro's law than they are under ordinary conditions.

In the case of the oxygen-hydrogen ratio, the density corrections were determined by actual measurement of the volumes in which the two gases combined, a method which is not always applicable, or at least not conveniently so. It is easier to compute the corrections from physical data, and for this purpose various methods have been proposed.[3]

The following formulæ, based upon the celebrated gas equation of Van der Waals, are, according to Guye,[4] available for the reduction of gaseous densities to true molecular weights:

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[1] Proc. Roy. Soc., 73, 153. 1904.

[2] Compt. Rend., 140, 1542. 1905.

[3] See D. Berthelot, Journ. Physique (3), 8, 263. 1899. Leduc, Ann. Chim. Phys. (7), 15, 1. 1898. Guye and Friderich, Arch. Sci. Phys. Nat. (4), 9, 505, and 13, 559. Guye, Journ. Chim. Phys., 3, 321, and 5, 203, also Compt. Rend., 138, 1213, and 140, 241. There is a copious literature upon this subject.

[4] Journ. Chim. Phys., 3, 321. 1905.

$$\text{I} \qquad M = \frac{(R \times mT)L}{(1+a)(1-b)} \qquad\qquad \text{II} \qquad M = \frac{RL}{(1+a_0)(1-b_0)}$$

Equation I applies to the permanent gases, that is, to those which are liquefiable only below 0°. Equation II applies to the easily liquefiable gases. R is the gas constant, and according to Berthelot[1] its value is 22.412. The constant m, of equation I, is given by Guye the value 0.0000623. T represents the critical temperature, on the absolute scale; L is the weight of one litre of gas at 0°, 760 mm., sea level, and latitude 45°; and M is the molecular weight. The symbols a and b are the constants of the Van der Waals equation, which vary for different gases, and in II are brought to the standard temperature and pressure.

In any given case the use of these formulæ requires a knowledge of the constants a and b. These can be deduced from the compressibilities and coefficients of expansion of a gas, or from the critical constants. The latter method is the one adopted by Guye, and with one exception it will be followed here. Guye gives the required data in form ready for use,[2] and they yield results which appear to be trustworthy. Applied to the densities given in the preceding pages they give the following reductions:

Nitrogen, Chemical. L=1.25066. T=127.5°. $(1+a)(1-b)=1.00100$. Hence N=14.0058, ±.00085. From the figures given for atmospheric nitrogen, N=14.0074, ±.00022. The weighted mean is N=14.0073, ±.0002.

Nitrous Oxide. L=1.97775. $(1+a_0)(1-b_0)=1.00733$. Hence $N_2O=44.0028$, ±.0021. The crude density ratio gives 44.2895, ±.0021, showing that the correction is large. This reduced value combines with other values for N_2O as follows:

From density of gas............$N_2O = 44.0028$, ± .0021
From gravimetric analysis..... " $= 44.0150$, ± .0066
From volumetric analysis ..... " $= 44.0417$, ± .0060

General mean ....$N_2O = 44.0074$, ± .0019

Nitric Oxide. L=1.34012. T=179.5°. $a=0.00257$. $b=0.00115$. Hence NO=30.0073, ±.00065. Gray's analyses of the gas gave NO=30.0104, ±.0011. The general mean is 30.0083, ±.00055.

Ammonia. The crude density ratio gave $NH_3=17.2619$, ±.00065. This has been reduced by means of compressibility data. Perman and Davies, who measured the compressibility, give the multiplying factor

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[1] Zeitsch. Elektrochem., 1904, 621. In Journ. Physique (3), 8, 527, Berthelot gives values of a and b for several gases.

[2] When Guye gives two or more figures for $(1+a)(1-b)$ I take the average.

0.9867, whence $NH_3=17.0323$. Jaquerod and Scheuer,[1] by a different formula, and using only the density determinations of Guye and Davila, find $NH_3=17.0148$. If $H=1.0078$, $N=14.0089$, Perman and Davies' method, or 13.9914 by Jaquerod and Scheuer. The first value is apparently the best and will be adopted here.

There are now four independent values for N, as follows:

From N_2 .... $N=14.0073$, ± .00020
From N_2O .... " $=14.0037$, ± .00095
From NO .... " $=14.0083$, ± .00055
From NH_3 .... " $=14.0089$, ± .00065

General mean .... $N=14.0074$, ± .00018

From compressibility data Rayleigh[2] found from N_2, $N=14.008$, and from N_2O, $N=13.998$. His low pressure value, as previously cited, was $N=14.009$, and Jaquerod and Perrot, at high temperatures, found $N=14.0077$. To include these values in the general mean would change the final result inappreciably, if at all, and they may therefore be disregarded. They have, however, confirmatory significance.

Some of the determinations utilized in the foregoing combination are evidently overvalued, especially the figure derived from atmospheric nitrogen. The "probable errors," scrutinized in detail, merely show that the density measurements are much more concordant than the gravimetric analyses. Moreover, the errors of the critical constants have not been taken into account, for they can hardly be estimated correctly. Allowances for these uncertainties might be made, but their effect upon the final combination would be trifling. The "probable error" here assigned to N, simply indicates the weight which it should receive in calculating other atomic ratios.[3]

As a check upon the other determinations of the atomic weight of nitrogen, Guye and Pintza[4] have determined the composition of ammonia by volume. The gas was decomposed by a spiral of platinum wire heated to redness, and from the density of the mixed gases, N_2+3H_2, compared with the known densities of nitrogen and hydrogen, the required datum was calculated. For the weight, in grammes, of a normal litre of the gaseous mixture, the following figures were obtained:

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[1] Compt. Rend., 140, 1384. 1905. From NO Jaquerod and Scheuer found N = 14.005.

[2] Proc. Roy. Soc., 74, 446. 1904.

[3] For a general discussion of the atomic weight of N, see Guye's lecture delivered before the Chemical Society of Paris, June 10, 1905. Also Compt. Rend., 140, 1386, and 144, 1360; and Ber., 39, 1470. Two papers by Berthelot, of a controversial nature, are in Compt. Rend., 144, 76 and 269, and one by Leduc in Compt. Rend., 146, 399.

[4] Compt. Rend., 147, 925. 1908.

0.38044
0.38055
0.38046

Mean, 0.38048, ± .000024

Corrected for traces of SO_2 and SO_3, this becomes 0.37989. If the weights of the normal litres of N_2 and H_2 are 1.2507 and 0.08987, respectively, the two gases in ammonia are combined in the ratio 1 : 3.00172. Applying this datum to the densities of nitrogen and hydrogen, and assuming $H = 1.0078$, $N = 11.017$, with a probable error, not exactly calculable, greater than ±.0017. To combine this figure with the value already found would change the latter inappreciably. Indeed, Guye and Pintza regard their determinations as inferior to those made by other methods, and publish their results only as a confirmation of the low value for N, as compared with the value 14.04 which had been in general acceptance for many years.

THE CARBON-OXYGEN RATIO.

The ratio between carbon and oxygen, or in other words, the atomic weight of carbon, has been directly determined by several methods. It has also been indirectly computed from analyses of silver salts, such as the acetate; but that group of ratios will be considered under another heading. The early attempts to estimate it from analyses of hydrocarbons, have now only historic value, and can be omitted from the present discussion. The direct measurements of the ratio represent three distinct processes:

First, by the combustion of carbon itself.

Second, by the combustion of carbon monoxide.

Third, by determining the density of gaseous compounds of carbon.

The first of these methods was used by Dumas and Stas[1] in 1840, and a year later by Erdmann and Marchand.[2] In both investigations weighed quantities of diamond, of natural graphite, and of artificial graphite were burned in oxygen, and the amount of dioxide produced was determined by the usual methods. The graphite employed was purified with extreme care by treatment with strong nitric acid and by fusion with caustic alkali. I have reduced all the published weighings to a common standard, so as to show in the third column the amount of oxygen which combines with a unit weight (say one gramme) of carbon. Taking Dumas and Stas' results first in order, we have from natural graphite:

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[1] Compt. Rend., 11, 991. Ann. Chim. Phys. (3), 1, 1.
[2] Journ. prakt. Chem., 23, 159.

1.000	grm. C gave	3.671	grm. CO$_2$.	2.6710
.998	"	3.660	"	2.6673
.994	"	3.645	"	2.6670
1.216	"	4.461	"	2.6686
1.471	"	5.395	"	2.6676

Mean, 2.6683, ± .0005

With artificial graphite:

.992	grm. C gave	3.642	grm. CO$_2$.	2.6714
.998	"	3.662	"	2.6693
1.660	"	6.085	"	2.6657
1.465	"	5.365	"	2.6621

Mean, 2.6671, ± .0014

And with diamond:

.708	grm. C gave	2.598	grm. CO$_2$.	2.6695
.864	"	3.1675	"	2.6661
1.219	"	4.465	"	2.6628
1.232	"	4.517	"	2.6664
1.375	"	5.041	"	2.6662

Mean, 2.6662, ± .0009

Erdmann and Marchand's figures for natural graphite give the following results:

1.5376	grm. gave	5.6367	grm. CO$_2$.	2.6659
1.6494	"	6.0384	"	2.6609
1.4505	"	5.31575	"	2.6647

In one experiment 1.8935 grm. of artificial graphite gave 6.9355 grm. CO$_2$. Ratio for O, 2.6628. This, combined with the foregoing series, gives a mean of 2.6636, ± .0007.

With the diamond they found:

.8052	grm. gave	2.9467	grm. CO$_2$.	2.6596
1.0858	"	3.9875	"	2.6632
1.3557	"	4.9659	"	2.6629
1.6305	"	5.97945	"	2.6673
.7500	"	2.7490	"	2.6653

Mean, 2.6637, ± .0009

In more recent years the ratio under consideration has been carefully redetermined by Roscoe, by Friedel, and by Van der Plaats. Roscoe[1] made use of transparent Cape diamonds, and in a sixth experiment he

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[1] Ann. Chim. Phys. (5), 26, 136. Zeit. anal. Chem., 22, 306. 1883. Compt. Rend., 94, 1180. 1882.

burned carbonado. The combustions were effected in a platinum boat, contained in a tube of glazed Berlin porcelain; and in each case the ash was weighed and its weight deducted from that of the diamond. The results were as follows, with the ratios stated as in the preceding series:

1.2820	grm. C gave	4.7006	CO_2.	2.6666
1.1254	"	4.1245	"	2.6649
1.5287	"	5.6050	"	2.6665
.7112	"	2.6070	"	2.6656
1.3842	"	5.0765	"	2.6675
.4091	"	1.4978	"	2.6612

Mean, 2.6654, ± .0006

Friedel's work,[1] also upon Cape diamond, was in all essential particulars like Roscoe's. The data, after deduction of ash, were as follows:

.4698	grm. C gave	1.7208	CO_2.	2.6628
.8616	"	3.1577	"	2.6649

Mean, 2.6638, ± .0004

By Van der Plaats[2] we have six experiments, numbers one to three on graphite, numbers four and five on sugar charcoal, and number six on charcoal made from purified filter paper. Each variety of carbon was submitted to elaborate processes of purification, and all weights were reduced to a vacuum standard. The data, with ash deducted, are subjoined:

1.	5.1217	grm. C gave	18.7780	CO_2.	2.6664
2.	9.0532	"	33.1931	"	2.6664
3.	13.0285	"	47.7661	"	2.6663
4.	11.7352	"	43.0210	"	2.6660
5.	19.1335	"	70.1336	"	2.6655
6.	4.4017	"	16.1352	"	2.6657

Mean, 2.6660, ± .0001

This combines with the previous series thus:

	Ratio.	*Atomic weight C.*
Dumas and Stas, first set.........	2.6683, ± .0005	11.9926
Dumas and Stas, second set.......	2.6671, ± .0014	11.9981
Dumas and Stas, third set.........	2.6662, ± .0009	12.0031
Erdmann and Marchand, first set...	2.6636, ± .0007	12.0138
Erdmann and Marchand, second set.	2.6637, ± .0009	12.0134
Roscoe	2.6654, ± .0006	12.0057
Friedel	2.6638, ± .0007	12.0129
Van der Plaats....................	2.6660, ± .0001	12.0030
General mean	2.6660, ± .0001	12.0030, ± .0005

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[1] Bull. Soc. Chim., 41, 100. 1884.

[2] Compt. Rend., 100, 52. 1885.

The effect of this combination is to give the work of Van der Plaats overwhelming weight, to which it is perhaps not entitled. The other determinations practically vanish.

According to Scott[1] all of the foregoing determinations are subject to an important correction, namely, a reduction to weight in vacuo. This correction was applied by Van der Plaats, at least partially; but Scott lays emphasis upon the change in volume of the potash solution in which the carbon dioxide was absorbed and weighed. The corrections, as applied by Scott, are given in the following table, in which the total reduced weights of carbon and dioxide are used instead of the individual weights of the separate experiments:

	Total C.	*Total CO_2.*	*Ratio.*	*Atomic weight.*
Dumas and Stas..........	16.1994	59.4201	2.66804	11.9938
Erdmann and Marchand..	12.1636	44.58537	2.66547	12.0054
Roscoe	6.4428	23.6275	2.66727	11.9973
Friedel	1.33185	4.8818	2.66543	12.0056
Van der Plaats...........	62.5115	229.1836	2.66630	12.0017

If to these figures we assign the relative weights given in the previous combination, the final mean will be identical with that of Van der Plaats as before, and C = 12.0017, ± .0005. Scott adopted the unweighted average of the five series given above, and made C = 12.0008.

The second method for determining the atomic weight of carbon was employed by Stas[2] in 1849. Carefully purified carbon monoxide was passed over a known weight of copper oxide at a red heat, and both the residual metal and the carbon dioxide formed were weighed. The weighings were reduced to a vacuum standard, and in each experiment a quantity of copper oxide was taken representing from eight to twenty-four grammes of oxygen. The method, as will at once be seen, is in all essential features similar to that usually employed for determining the composition of water. The figures in the third column, deduced from the weights given by Stas, represent the quantity of carbon monoxide corresponding to one gramme of oxygen:

9.265	grm. O =	25.483	CO_2.	1.75046
8.327	"	22.900	"	1.75010
13.9438	"	38.351	"	1.75040
11.6124	"	31.935	"	1.75008
18.763	"	51.6055	"	1.75039
19.581	"	53.8465	"	1.74994
22.515	"	61.926	"	1.75043
24.360	"	67.003	"	1.75053

Mean, 1.75029, ± .00005

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[1] Journ. Chem. Soc., 71, 550. 1897.

[2] Bull. Acad. Belg., 1849 (1), 31. Oeuvres Complètes, 1, 287.

Hence CO = 28.0046, and C = 12.0046; ± .0008.

This work of Stas was also criticised by Scott,[1] in connection with the determinations by the first method. The process employed is subject to several possible errors, two of them being especially serious. First, the carbon monoxide may have contained hydrogen or hydrocarbons. Secondly, the copper oxide, which was prepared by calcining copper nitrate, almost certainly contained occluded nitrogen. The value found for C, however, is probably not very far from the truth, and it is not unlikely that errors in opposite directions tended to compensate one another.

For the density of carbon monoxide there are available determinations by Leduc[2] and Rayleigh.[3] Leduc used a globe which had a capacity of 2.9440 grammes of air. Filled with CO it held the following weights, giving the accompanying densities:

Weight CO.	*Density.*
2.8470	.96705
2.8468	.96698
2.8469	.96702
	.96702, ± .000015

This density, combined with Leduc's determination of the density of oxygen, 1.10514, ± .000032, gives the crude ratio—

$$O_2 : CO :: 32 : 28.0007, \pm .0010$$

Rayleigh's determinations may be stated in the following form: A globe which held 2.62760, ± .00004 grammes of oxygen, held of carbon monoxide—

2.29843
2.29852
2.29854

Mean, 2.29850, ± .000024

Corrected for the compression of the globe when empty this becomes 2.29906, ± .000024. From these data the crude value for CO is 27.9989, ± .0012. Combining this with Leduc's determination, the general mean becomes—

$$CO = 28.0000, \pm .00077$$

Rayleigh, it must be observed, prepared his three samples of carbonic oxide by three distinct methods, and the concordance in their weights gives strong assurance of their purity.

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[1] *Loc. cit.* See also Richards, Amer. Chem. Journ., 20, 701. 1898.

[2] Compt. Rend., 115, 1072. 1893.

[3] Proc. Roy. Soc., 62, 204. 1897.

For the calculation of the true molecular weight of carbon monoxide from this crude density ratio, the critical data cited by Guye[1] are available. The mean of two sets of critical constants gives $(1+a)(1-b)=1.00109$, and $T=132.7°$. Applying these figures by the formula given under nitrogen, the molecular weight becomes

$$CO=12.0031, \pm .00077.$$

The density of carbon dioxide has been determined by many investigators,[2] but the earliest measurements have now only historical interest. In 1845 Regnault[3] published five determinations of the density, referred to air as unity, and they were the first to be worth consideration now His figures are as follows:

1.52915
1.52900
1.52915
1.52906
1.52915

Mean, 1.52910, ± .000032

Corrected by Crafts,[4] for compression of the empty globe, this becomes 1.52897, ± .000032. For the density of oxygen, Regnault's corrected value is 1.10562, ± .000008.

Hence $O_2 : CO_2 :: 32 : 44.2530, \pm .00098$.

In three concordant measurements, which are not given separately, Leduc[5] found for CO_2 the density 1.52874. This figure, combined with his value for oxygen, already cited, gives for CO_2 the density ratio 32 : 44.2667. Rayleigh[6] gives a single figure for the density of CO_2, namely, 1.52909. For oxygen he found 1.10535. Hence the ratio 32 : 44.2673.

The three determinations by Guye and Pintza[7] are stated in the form of normal litre-weights, as follows:

1.97684
1.97676
1.97681

Mean, 1.97680, ± .0000176

With Morley's figure for the weight of a litre of oxygen, 1.42896, ± .000028, the ratio becomes

$$O_2 : CO_2 :: 32 : 44.2683, \pm .00097.$$

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[1] Journ. Chim. Phys., 3, 342. 1905.

[2] The early determinations are well summarized in Van Geun's monograph.

[3] Compt. Rend., 20, 993. 1845.

[4] Compt. Rend., 106, 1664.

[5] Ann. Chim. Phys. (7), 15, 34. 1898.

[6] Proc. Roy. Soc., 62, 204. 1897.

[7] Compt. Rend., 141, 51. 1905.

To the values deduced from Leduc's and Rayleigh's data we may arbitrarily assign equal weight with the mean of Regnault's series. The four determinations then combine thus:

Regnault	44.2530, ± .00098
Leduc	44.2667, ± .00098
Rayleigh	44.2673, ± .00098
Guye and Pintza	44.2683, ± .00097
General mean	44.2638, ± .00050

Regnault's figure is probably too low. Its omission would raise the general mean to 44.2674; but such a procedure is questionable. I prefer therefore to leave the combination unchanged, except for the necessary reduction by means of the critical constants. For these, based on the mean of determinations by Amagat and Keesom, Guye[1] deduces $(1+a_0)(1-b_0)=1.00687$. Applying this value we have for the molecular weight under consideration,

$$CO_2=43.9972, \pm .0005.$$

The four independent values for carbon now combine as follows:

By combustion of C, corrected	C = 12.0017, ± .0005
By combustion of CO	C = 12.0046, ± .0008
From density of CO	C = 12.0031, ± .0008
From density of CO_2	C = 11.9972, ± .0005
General mean	C = 12.0007, ± .0003

In short, the oxygen-carbon ratio may be written

$$O:C::16:12$$

within the limits of experimental uncertainty.

There are a few other data relative to carbon yet to be considered. Rayleigh[2] has compared the density of carbon monoxide at atmospheric pressure with its density at pressures between 75 and 150 millimetres of mercury. The molecular weights deduced are, for normal pressure, CO = 28.000, for low pressures, 28.006, when O = 16. Hence C = 12.006.

A comparison of the gases at high temperatures has been made by Jaquerod and Perrot.[3] They measured the expansion of the two carbon oxides up to 1067.4°, applied their results to the mean densities found by Leduc and Rayleigh, and obtained the following molecular values:

$$CO = 28.009, \text{ and } C = 12.009.$$
$$CO_2 = 43.992, \text{ and } C = 11.992.$$

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[1] Journ. Chim. Phys., 3, 337. 1905.
[2] Proc. Roy. Soc., 73, 153. 1904.
[3] Compt. Rend., 140, 1542. 1905.

These figures are interesting for comparison with those previously discussed, but can hardly be used in a general combination.

Another group of data from which the carbon-oxygen ratio can be deduced is found in the density determinations of certain organic compounds. The older measurements need not be considered, but two recent investigations have some real value.

First, the density of methyl oxide $(CH_3)_2O$, as determined by Baume.[1] Two series are given, with the subjoined values for the weight of a normal litre:

I.	*II.*
2.10912	2.10925
2.10886	2.10941
2.11045	2.11026
2.10920	2.10936
2.10948	2.11005
2.11003	2.10977
2.10947	
Mean, 2.10951, ± .00014.	Mean, 2.10968, ± .00011

A small correction raises these means by 0.00001. Combined, the final value is 2.10961, ± .000084. With the critical data given by Baume, $a_0 = 0.03111$, and $b_0 = 0.00382$. Applying these figures by means of the formula already cited, and assigning to the weight of oxygen the probable error found from Morley's observations, the molecular weight of methyl oxide becomes 46.0306, ± .0021. Hence, with H = 1.0078,

$$C = 11.9919, \pm .0010$$

a value which is almost certainly too low.

For the weight of a normal litre of methane, CH_4, Baume and Perrot[2] find the following values:

0.71690
0.71657
0.71633
0.71669
0.71751
0.71636
0.71672
0.71678
0.71725

Mean, 0.71689, ± .000098

---

[1] Journ. Chim. Phys., 6, 46. 1908. Baume also gives data for methyl chloride, but they are not available for a good determination of molecular weight.

[2] Compt. Rend., 148, 39. 1909.

Reducing this with the critical constants determined by Guye, CH_4 = 16.034, ±.0022.

Hence

$$C = 12.0028, \pm .0022$$

Combining this with the value from methyl oxide, the weighted mean becomes

$$C = 11.9937, \pm .00091$$

From the oxide ratios C = 12.0007, ±.0003. The two values combined give C = 12.0000, ±.00029.

In this combination the actual variation from the whole number 12 is only 4 in the sixth decimal place; a variation quite without significance. Later, in the discussion of all the fundamental ratios, the value for carbon is modified by other values derived from silver compounds; but the change is not very large.

From the density of toluene, Leduc[1] has recently deduced the value 12.003, which is notably higher than that computed here. The determination, however, is not sufficiently explicit in detail to admit of its use for present purposes. Another value is calculable from Parson's glucinum ratios;[2] namely, C = 12.007.

Addenda. The determinations by Baume and Perrot[3] of the density of ethane appeared too late for use in the general discussion of the fundamental ratios. Two series of measurements were made, giving the subjoined figures for the weight of the normal litre:

I.	*II.*
1.35671	1.35600
1.35679	1.35610
1.35671	1.35653
1.35652	1.35640
1.35700	1.35590
1.35640	1.35640

Mean of all as one series, 1.356455, ±.000065. Reducing their data by means of the critical constants, the authors find C_2H_6 = 30.119, and C = 12.036. This value is evidently too high.

There is also a preliminary note, by Scott,[4] which gives, without details, the results of combustions of naphthalene and cinnamic acid. In six analyses, 17.6175 grammes of naphthalene gave 60.5355 of CO_2.

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[1] Compt. Rend., 148, 832. 1909.

[2] See section on glucinum, later.

[3] Journ. Chim. Phys., 7, 369. 1909.

[4] Proc. Chem. Soc., 25, 310.

Hence C=11.999. In two analyses, 8.6153 grammes of cinnamic acid gave 23.0413 of CO_2. Hence 12.0015.

SYNTHESES AND DENSITY OF HYDROCHLORIC ACID.

The quantitative synthesis of hydrochloric acid, with reference to the atomic weight of chlorine, was first effected by Dixon and Edgar.[1] Chlorine, prepared by the electrolysis of fused silver chloride, was weighed in liquid form. Hydrogen, obtained electrolytically from barium hydrate, was occluded by palladium, and so weighed. A combustion globe was filled with the chlorine, and the hydrogen, ignited by a spark, was burned in it. The excess of chlorine was determined by absorption in potassium iodide, and subsequent titration of the liberated iodine with thiosulphate solution. With corrected weights the following results were obtained:

Weight H.	*Weight Cl.*	*Ratio Cl:H.*
0.9993	35.1666	35.191
1.0218	35.9621	35.195
.9960	35.0662	35.207
1.0243	36.0403	35.185
1.0060	35.4144	35.203
.9887	34.8005	35.198
1.0159	35.7639	35.204
1.1134	39.1736	35.184
1.0132	35.6527	35.188
		Mean, 35.195, ± .0019

The determinations by Noyes and Weber[2] were differently conducted. The hydrogen was weighed in palladium; but the chlorine was taken in the form of potassium chloroplatinate. That salt was heated in a stream of hydrogen, and the loss in weight gave the weight of chlorine taken. The hydrochloric acid produced was also collected and weighed.

Two series of experiments were made, differing in detail. In series I, the hydrogen was prepared by electrolysis of sulphuric acid, and in series II from barium hydroxide. The hydrochloric acid of series I was collected in water directly; but in series II it was first condensed to a solid by cooling with liquid air. The two series, however, were concordant, and may therefore be treated here as one. The data obtained, reduced to a vacuum basis, were as follows:

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[1] Phil. Trans., 205A, 169. 1905.

[2] Journ. Amer. Chem. Soc., 30, 13. 1908.

H.	*Cl.*	*HCl.*	*Cl:H.*	*HCl:H.*
0.25394	8.93293	9.18695	35.177	36.178
.28004	9.85590	10.13259	35.195	36.183
.51821	18.23468	18.75359	35.188	36.189
.67631	23.79587	24.47123	35.185	36.183
.58225	20.48158	....	35.177	.....
.47989	16.88423	17.36310	35.184	36.181
.64132	22.55816	23.20054	35.175	36.176
.81608	28.71691	29.53167	35.188	36.187
.83194	29.28055	30.11207	35.195	36.195
.39074	13.74926	14.14078	35.188	36.188
.75560	26.58427	27.33926	35.183	36.182
.77518	27.26746	28.04110	35.176	36.174
		Mean,	35.1843, ± .0014	36.1835, ± .0013

Edgar's[1] syntheses of hydrochloric acid resembled those of Dixon and Edgar, so far as the preparation and weighing of the initial substances were concerned. The chlorine was then burned in the hydrogen, at the end of a quartz tip, and the hydrochloric acid so produced was condensed to solid form by means of liquid air. It was afterwards allowed to evaporate, and passed through a quartz tube filled with mercury vapor, which removed any free chlorine. The purified hydrochloric acid was finally condensed, either in a steel bomb or by absorption in water, and weighed. The corrected weights and ratios are subjoined:

H.	*Cl.*	*HCl.*	*Cl:H.*	*HCl:H.*
2.1452	75.5026	77.6469	35.196	36.196
2.0387	71.7504	73.7880	35.194	36.194
1.7762	62.5004	......	35.188	.....
1.9935	70.1638	72.1565	35.196	36.196
1.6469	57.9671	......	35.198	.....
2.1016	73.9662	......	35.195	.....
1.7254	60.7162	62.4401	35.190	36.189
2.0885	73.4991	75.5859	35.192	36.191
		Mean,	35.194, ± .0008	36.193, ± .0009

Upon reducing the HCl:H ratios to the Cl:H form the five sets of determinations combine thus:

Dixon and Edgar, H:Cl	35.195,	± .0019
Noyes and Weber, H:Cl	35.1843,	± .0014
Noyes and Weber, H:HCl	35.1835,	± .0013
Edgar, H:Cl	35.194,	± .0008
Edgar, H:HCl	35.193,	± .0009
General mean,	35.1911,	± .00049

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[1] Proc. Roy. Soc., 81A, 216. 1908.

That is, the atomic weight of chlorine, when H = 1, is 35.1911. If O = 16, then Cl = 35.4652, ± .0005.

For the weight of a normal litre of hydrochloric acid, Guye and Ter Gazarian[1] give the subjoined figures:

	1.6404
	1.6397
	1.6389
	1.6401
Mean,	1.6398, ± .00007

Reducing these by the method of critical constants, in which the term $(1+a_0)(1-b_0) = 1.00773$, the molecular weight of HCl becomes 36.4693, ± .0015. Hence, if H = 1.0078, Cl = 35.4615, ± .0015.

In a preliminary note Gray and Burt[2] have given the results of their investigation upon the density and composition by volume of hydrochloric acid. For the weight of the normal litre of the gas, as a mean of twenty experiments, the value 1.63885 grammes was found, ± .00004. By passage over heated aluminum the volume of hydrogen liberated from two volumes of HCl was found to be 1.00790, ± .00002; the mean of eight experiments. From these data, with H = 1.0078, and with Morley's value and probable error for the density of hydrogen, HCl = 36.4672, ± .0009, and Cl = 35.4594, ± .0009.

The several values for Cl now combine thus:

By syntheses of HCl	35.4652, ± .0005
From density of HCl	35.4615, ± .0015
From volumetric composition of HCl	35.4594, ± .0009
General mean	35.4637, ± .00042

This value is still to be modified by the analyses of nitrosyl chloride, as given in the next section of this work.

Addenda. Since the foregoing pages on the chlorine-hydrogen ratio were written, and after the final mean had been utilized in a large number of other calculations, the complete work of Gray and Burt[3] has appeared. First, three series of determinations of the density of HCl are given, with the weight of one litre of the gas at 0°, 760 mm., and at London, as follows:

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[1] Compt. Rend., 143, 1233. 1906.

[2] Proc. Chem. Soc., 24, 215. 1908. For changes and corrections see *addenda* to this section.

[3] Journ. Chem. Soc., 95, 1633. 1909.

	I.	*II.*	*III.*
	1.64053	1.64022	1.63950
	1.64004	1.63999	1.64013
	1.64020	1.63976	1.63984
	1.63986	1.64083	1.64069
		1.64030	1.64031
		1.64021	1.64017
		1.64027	1.64050
			1.64051
			1.63992
			1.64001
Mean,	1.64016,	1.64023,	1.64016,
	± .00010	± .00008	± .00008

The three series represent hydrochloric acid prepared by three distinct methods. Series I and II are to be corrected by −0.00013 gramme, which represents gas adsorbed by the walls of the containing glass bulb. The corrected mean becomes 1.64011, ±.00005 grammes, which, at sea level and latitude 45° is equivalent to 1.63915 grammes, the weight of the normal litre. If we reduce this with the critical constants used by Guye and Ter Gazarian,[1] it gives HCl=36.4548, and with H=1.0078, Cl=35.4470, ±.0011, a very low value.

In order to ascertain the atomic weight of chlorine, Gray and Burt have measured the compressibility of the HCl, and also determined its composition by volume. In the latter case the gas was decomposed by heated aluminum, and the volume of hydrogen liberated from two volumes of hydrochloric acid was measured. The volumes thus found were as follows:

1.00797
1.00795
1.00790
1.00790
1.00781
1.00779
1.00787
1.00798

Mean, 1.00790, ± .000017

Using Morley's value for the weight of a litre of hydrogen, 0.089872, ±.0000028 gramme, and with the atomic weight H=1.00779, ±.00001, the molecular weight of HCl is given by the following equation:

$$\frac{1.63915}{0.089872} \times \frac{2}{1.00790} \times 1.00779 = 36.4735$$

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[1] See *ante*. Gray and Burt do not make this calculation. It is useful, however, for purposes of comparison.

and Cl = 35.4657, ± .0013. Gray and Burt, calculating with H = 1.00762 (Morley's value), find Cl = 35.459.

The data given by Gray and Burt for the compressibilities of oxygen and hydrochloric acid are too complex to admit of detailed reproduction here. The normal litre of oxygen, weighing 1.42900 grammes, gave a limiting density of 1.42762 grammes. That of HCl, 1.63915 grammes, gives a limiting density of 1.62698 grammes. The ratio between these limiting densities is the true ratio between the molecular weights according to Avogadro's law. Hence,

$$\frac{1.62698 \times 32}{1.42762} = \text{Mol. Weight HCl} = 36.4687$$

Hence, assuming the probable errors 0.00005 for HCl, and 0.0000028 for O_2, Cl = 35.4609, ± .0011.

The three values derived from Gray and Burt's determinations, now give the subjoined values for the molecular weight of HCl, when H = 1.00779.

By critical constants...........HCl =	36.4548, ± .0011
By volumetric analysis...............	36.4735, ± .0013
By compressibility measurements....	36.4687, ± .0011
General mean ....HCl =	36.4647, ± .0007

Hence Cl = 35.4569, ± .0007. Gray and Burt, from their two methods alone, find Cl = 35.460. The difference between their figure and the foregoing combination is only one part in 11440, which is less than the actual uncertainty.

Determinations of the density of HCl have also been made, very recently, by Scheuer.[1] By simultaneous weighings of the gas in six globes, 28 values were obtained for the weight of the normal litre, as follows:

1.63935	1.63983	1.63943	1.63941
1.63959	1.63932	1.63943	1.63944
1.63939	1.63887	1.63933	1.63895
1.63940	1.63977	1.63933	1.63928
1.63968	1.63951	1.63931	1.63962
1.63945	1.63892	1.63942	1.63932
1.63987	1.63938	1.63968	1.63928

Mean of all, as one series, 1.63941, ± .000031.

Reducing with the critical constants, as given by Guye and Ter Gazarian, Cl = 35.4528, ± .0007.

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[1] Compt. Rend., 149, 599. 1909. Scheuer's complete memoir (Zeitsch. phys. Chem., 68, 575) was received after this work had gone to the printer. In it he discusses his own measurements, in connection with those of Gray and Burt, and finally concludes that Cl = 35.466.

The several values for chlorine, as derived from hydrochloric acid and also from the analyses of nitrosyl chloride, as cited in the next section of this work, now combine thus:

By syntheses of HCl	Cl = 35.4652, ± .0005
Guye and Ter Gazarian	35.4615, ± .0015
Gray and Burt, revised	35.4569, ± .0007
Scheuer	35.4528, ± .0007
From NOCl, Guye and Fluss	35.4680, ± .0010
General mean	Cl = 35.4630, ± .00032

This varies from the value adopted in the previous discussion, Cl = 35.4647, by one part in 21,000. Its introduction into the final reduction of the fundamental atomic weights would change the latter inappreciably.

ANALYSES OF NITROSYL CHLORIDE.

The analyses of nitrosyl chloride, NOCl, by Guye and Fluss,[1] are of special interest, because they give direct ratios between the three component elements. The carefully purified chloride was first weighed, and then distilled over heated silver, which absorbed the chlorine. The weight of the latter was given by the gain in weight of the silver. It was next passed over heated copper, which retained oxygen, and finally over metallic calcium to fix the nitrogen. The sum of the three components was generally a little less than that of the nitrosyl chloride, but whether the loss represents undetermined impurity, or failure to collect all the products of decomposition, seems to be uncertain. The weights obtained were as follows:

NOCl.	*Cl.*	*O.*	*N.*	*Loss.*
.5341	.2893	.1305	.1142	.0001
.4284	.2319	.1046	.0916	.0003
.7995	.4331	.1954	.1710	.0000
.5639	.3048	.1375	.1204	.0012
.5121	.2773	.1251	.1095	.0002

From these figures, with O = 16, the atomic weights of N and Cl are directly calculable, by comparison with O = 16. In a third column I also give the value of the ratio Cl : N :: 100 : x, computed from columns 2 and 4.

	N.	*Cl.*	*Cl:N.*
	14.001	35.470	39.475
	14.011	35.472	39.500
	14.002	35.464	39.483
	14.010	35.468	39.501
	14.005	35.466	39.488
Mean,	14.006, ± .0017	35.468, ± .0010	39.489, ± .0033

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[1] Journ. Chim. Phys., 6, 732. 1908.

Several other ratios are calculable from the data given, and, indeed, were computed by Guye and Fluss; but they are not needed here. They involve to a greater extent the uncertainties due to the losses from the initial substance. The values found in this series of analyses may now be combined with those obtained in the preceding sections of this work, as follows:

N.

Previously found ....N = 14.0074, ± .00018
From NOCl ....N = 14.006, ± .0017

General mean ......N = 14.0074, ± .00018

Cl.

Previously found[1] ......Cl = 35.4637, ± .00042
From NOCl ....Cl = 35.468, ± .0010

General mean ......Cl = 35.4643, ± .00039

THE RATIO $HCl : NH_3$.

Julius Thomsen,[2] for the purpose of fixing indirectly the ratio H : O, has made a series of determinations of the ratio $HCl : NH_3$, which may properly be used toward establishing the atomic weight of nitrogen. First, pure, dry, gaseous hydrochloric acid is passed into a weighed absorption apparatus containing pure distilled water. After noting the increase in weight, pure ammonia gas is passed in until a very slight excess is present, and the apparatus is weighed again. The excess of NH_3, which is always minute, is measured by titration with standard hydrochloric acid. In weighing, the apparatus is tared by one of similar form, and containing about the same amount of water. Three series of determinations were made, differing only in the size of the absorption apparatus; so that for present purposes the three may be taken as one. Thomsen considers them separately, and so gives greatest weight to the experiments involving the largest masses of material. I give his weighings, and also, as computed by him, the ratio $\frac{HCl}{NH_3}$.

	HCl.	*NH_3.*	*Ratio.*
First series	5.1624	2.4120	2.1403
	3.9425	1.8409	2.1416
	4.6544	2.1739	2.1411
	3.9840	1.8609	2.1409
	5.3295	2.4898	2.1406
	4.2517	1.9863	2.1405

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[1] Not including addenda.

[2] Zeitsch. physikal. Chem., 13, 398. 1894. For a criticism of Thomsen's work, see Acree and Brunel, Amer. Chem. Journ., 36, 117. The ratio, as determined, is of small value.

	4.8287	2.2550	2.1414
	6.4377	3.0068	2.1411
	4.1804	1.9528	2.1407
	5.0363	2.3523	2.1410
	4.6408	2.1685	2.1411
Second series	11.8418	5.5302	2.14130
	14.3018	6.6808	2.14073
	12.1502	5.6759	2.14067
	11.5443	5.3927	2.14073
	12.3617	5.7733	2.14118
Third series	19.3455	9.0360	2.14094
	19.4578	9.0890	2.14081

Mean of all, 2.14093, ± .000053
Reduced to vacuo, 2.1394

From the sums of the weights Thomsen finds the ratio to be 2.14087, or 2.13934 in vacuo.

ANALYSES OF CHLORATES.

Until recent times the fundamental values for the atomic weights of silver, chlorine and potassium, were best determined by analyses of chlorates. Modern, direct determinations of the chlorine-hydrogen and nitrogen-oxygen ratios have in great measure supplanted the chlorate work, which, however, must still be taken into account, and may even regain some of the lost ground.

The first good series of analyses of potassium chlorate was made by Berzelius.[1] All the earlier estimations were vitiated by the fact that when potassium chlorate is ignited under ordinary circumstances a little solid material is mechanically carried away with the oxygen gas. Minute portions of the substance may even be actually volatilized. These sources of loss were avoided by Berzelius, who devised means for collecting and weighing this trace of potassium chloride. All the successors of Berzelius in this work have benefited by his example, although for the methods by which loss has been prevented we must refer to the original papers of the several investigators. In short, then, Berzelius ignited potassium chlorate, and determined the percentage of chloride which remained. Four experiments gave the following results:

60.854
60.850
60.850
60.851

Mean, 60.851, ± .0006

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[1] Poggend. Annalen, 8, 1. 1826.

The next series was made by Penny,[1] who worked after a somewhat different method. He treated potassium chlorate with strong hydrochloric acid in a weighed flask, evaporated to dryness over a sand bath, and then found the weight of the chloride thus obtained. His results are as follows, in six trials:

60.825
60.822
60.815
60.820
60.823
60.830

Mean, 60.8225, ± .0014

In 1842 Pelouze[2] made three estimations by the ignition of the chlorate, with these results:

60.843
60.857
60.830

Mean, 60.843, ± .0053

Marignac, in 1842,[3] worked with several different recrystallizations of the commercial chlorate. He ignited the salt, with the usual precautions for collecting the material carried off mechanically, and also examined the gas which was evolved. He found that the oxygen from 50 grammes of chlorate contained chlorine enough to form .003 gramme of silver chloride. Here are the percentages found by Marignac:

In chlorate once crystallized...........	60.845
In chlorate once crystallized...........	60.835
In chlorate twice crystallized..........	60.833
In chlorate twice crystallized..........	60.844
In chlorate three times crystallized....	60.839
In chlorate four times crystallized.....	60.839
	Mean, 60.8392, ± .0013

In the same paper Marignac describes a similar series of experiments made upon potassium perchlorate, $KClO_4$. In three experiments it was found that the salt was not quite free from chlorate, and in three more it contained traces of iron. A single determination upon very pure material gave 46.187 per cent. of oxygen and 53.813 of residue.

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[1] Phil. Trans., 1839, p. 20.
[2] Compt. Rend., 15, 959.
[3] Ann. Chem. Pharm., 44, 18. Oeuvres Complètes, 1, 57.

In 1845 two series of experiments were published by Gerhardt.[1] The first, made in the usual way, gave these results:

60.871
60.881
60.875

Mean, 60.8757, ± .0020

In the second series the oxygen was passed through a weighed tube containing moist cotton, and another filled with pumice stone and sulphuric acid. Particles were thus collected which in the earlier series escaped. From these experiments we get—

60.947
60.947
60.952

Mean, 60.9487, ± .0011

These last results were afterwards sharply criticised by Marignac,[2] who seriously questioned their value.

The next series, in order of time, is due to Maumené.[3] This chemist supposed that particles of chlorate, mechanically carried away, might continue to exist as chlorate, undecomposed; and hence that all previous series of experiments might give too high a value to the residual chloride. In his determinations, therefore, the ignition tube, after expulsion of the oxygen, was uniformly heated in all its parts. Here are his percentages of residue:

60.788
60.790
60.793
60.791
60.785
60.795
60.795

Mean, 60.791, ± .0009

The question which most naturally arises in connection with these results is, whether portions of chloride may not have been volatilized, and so lost.

Closely following Maumené's paper, there is a short note by Faget,[4] giving certain mean results. According to this chemist, when potassium

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[1] Compt. Rend., 21, 1280.
[2] Suppl. Biblio. Univ. Gèneve, Vol. 1.
[3] Ann. Chim. Phys. (3), 18, 71. 1846.
[4] Ann. Chim. Phys. (3), 18, 80. 1846

chlorate is ignited slowly, we get 60.847 per cent. of residue. When the ignition is rapid, we get 60.942. As no detailed experiments are given, these figures can have no part in our discussion.

Last of all we have two series determined by Stas.[1] In the first series are the results obtained by igniting the chlorate. In the second series the chlorate was reduced by strong hydrochloric acid, after the method followed by Penny:

First Series.

60.8380
60.8395
60.8440
60.8473
60.8450

Mean, 60.84276, ± .0012

Second Series.

60.850
60.853
60.844

Mean, 60.849, ± .0017

In these experiments every conceivable precaution was taken to avoid error and insure accuracy. All weighings were reduced to a vacuum standard; from 70 to 142 grammes of chlorate were used in each experiment; and the chlorine carried away with the oxygen in the first series was absorbed by finely divided silver and estimated.

According to Guye and Ter Gazarian,[2] potassium chlorate tends to retain a constant impurity of chloride. The average amount of chloride, they say, is 2.7 parts in 10,000, but they give no detailed figures in support of their assertion. It can therefore be given only provisional consideration, the existence of the impurity being not fully established. Leaving their correction temporarily out of account, the different series of determinations of KCl from $KClO_3$ combine as follows:

Berzelius	60.851, ± .0006
Penny	60.8225, ± .0014
Pelouze	60.843, ± .0053
Marignac	60.8392, ± .0013
Gerhardt, 1st	60.8757, ± .0020
" 2d	60.9487, ± .0011
Maumené	60.791, ± .0009
Stas, 1st	60.8428, ± .0012
" 2d	60.849, ± .0017
General mean	60.846, ± .00038

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[1] Oeuvres Complètes, 1, 395-405.
[2] Compt. Rend., 143, 411.

Hence, with 3O = 48, KCl = 74.593, ± .00086.

The percentage of oxygen in sodium chlorate has been determined only by Penny,[1] who used the same method which he applied to the potassium salt. Four experiments gave the following results:

45.060
45.075
45.080
45.067

Mean, 45.0705, ± .0029

Hence, NaCl = 54.500, ± .0048.

For the composition of silver chlorate there are analyses by Marignac[2] and by Stas.[3] Marignac's series is as follows:

$AgClO_3$.	$AgCl$.	Per cent. $AgCl$.
24.540	18.363	74.920
25.809	19.336	74.913
30.306	22.709	74.932
28.358	21.247	74.924
28.287	21.185	74.893
57.170	42.840	74.934
		Mean, 74.9193, ± .0041

Corrected to a vacuum this becomes 74.917.

The determinations by Stas are only two in number, giving the subjoined percentages of AgCl in $AgClO_3$:

74.919
74.922

Mean, 74.9205, ± .0010

Combining this with Marignac's figure the general mean becomes 74.9203, ± .0010. Hence AgCl = 143.390, ± .0060.

ANALYSES OF BROMATES AND IODATES.

Accurate analyses of bromates and iodates, available for atomic weight determinations, are few in number, and from a modern point of view, not satisfactory. Potassium bromate was analyzed by Marignac,[4] by

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[1] Phil. Trans., 1839, p. 25.

[2] Oeuvres Complètes, 1, 80.

[3] Oeuvres Complètes, 1, 635.

[4] Oeuvres Complètes, 1, 84. From the sum of his weights Marignac computes that $KBrO_3$ contains 28.723 of oxygen. This calculation gives the fourth analysis excessive weight.

careful calcination, with all the precautions taken to avoid loss. His figures are subjoined:

$KBrO_3$.	KBr.	*Per cent. O.*
6.801	4.849	28.7016
3.480	2.483	28.6494
6.320	4.506	28.7025
23.186	16.521	28.7458
		Mean, 28.6998, ± .0133

Hence KBr = 119.249, ± .0596.

Marignac attempted to analyze silver bromate, but found difficulties in drying the salt. He also made some experiments upon the precipitation of silver bromate by potassium chloride, but published no details of his determinations. He merely states that from 31.32 to 31.47 parts of KCl were needed to precipitate the silver from 100 parts of bromate.

Stas[1] effected the analysis of silver bromate by reduction with sulphurous acid, its content in water having been previously determined. After applying all corrections the subjoined percentages of oxygen were found from the weight of the bromate and that of the residual silver bromide:

20.351
20.347

Mean, 20.349, ± .0014

Hence AgBr = 187.884, ± .0133.

The percentage of oxygen in potassium iodate has been determined by Millon.[2] In three experiments he found:

22.46
22.49
22.47

Mean, 22.473, ± .005

Hence KI = 165.590, ± .0384.

According to Marignac[3] potassium iodate loses iodine when calcined, and is therefore unsuited to atomic weight determinations.

Millon also estimated the oxygen in silver iodate, getting the following percentages:

17.05
17.03
17.06

Mean, 17.047, ± .005

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[1] Oeuvres Complètes, 1, 635.
[2] Ann. Chim. Phys. (3), 9, 400. 1843.
[3] Oeuvres Complètes, 1, 85.

The analysis of silver iodate has also been performed with extreme care by Stas.[1] From 76 to 157 grammes were used in each experiment, the weights being reduced to a vacuum standard. As the salt could not be prepared in an absolutely anhydrous condition, the water expelled in each analysis was accurately estimated and the necessary corrections applied. In two of the experiments the iodate was decomposed by heat, and the oxygen given off was fixed upon a weighed quantity of copper heated to redness. Thus the actual weights, both of the oxygen and the residual iodide, were obtained. In a third experiment the iodate was reduced to iodide by a solution of sulphurous acid, and the oxygen was estimated only by difference. In the three percentages of oxygen given below, the result of this analysis comes last. The figures for oxygen are as follows:

16.976
16.972
16.9761

Mean, 16.9747, ± .0009

This, combined with Millon's series above cited, gives us a general mean of 16.9771, ± .0009.

Hence AgI = 234.734, ± .0126.

THE IODINE PENTOXIDE-SILVER RATIO.

The ratio between iodine pentoxide and silver has been measured by Baxter and Tilley.[2] The oxide was prepared by the careful dehydration of iodic acid, the latter having been made from purified iodine. After weighing, the pentoxide was dissolved in water, and the acid so formed was reduced to hydriodic acid by means of hydrazine. By final titration of the solution with a solution of pure silver, the ratio in question was determined. The ultimate data, with vacuum weights and all corrections applied, are as follows:

Series I. Tilley.

Weight I_2O_5.	*Weight Ag.*	*Ratio.*
6.06570 9.48035	3.92027 6.12611 [3]	64.6234
7.73052	4.99564	64.6223
12.63909	8.16777	64.6231
9.49913	6.13841	64.6208
8.34369	5.39202	64.6239
8.83155	5.70715	64.6223
6.77487	4.37803	64.6216

Mean, 64.6225, ± .0003

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[1] Oeuvres Complètes, 1, 628.

[2] Journ. Amer. Chem. Soc., 31, 201. 1909.

[3] These analyses were inadvertently mixed, and hence are combined in the table.

Series II. Baxter.

Weight I_2O_5.	*Weight Ag.*	*Ratio.*
12.09036	7.81320	64.6234
6.29744	4.00957	64.6226
10.89880	7.04309	64.6226
9.33895	6.03505	64.6222
10.15370	6.56169	64.6236
11.00453	7.11141	64.6226
7.01649	4.53431	64.6236
9.33573	6.03304	64.6231
8.72163	5.63619	64.6231
9.01524	5.82591	64.6229
		Mean, 64.6230, ± .0001

Combining both series, the mean value for the ratio is

I_2O_5:2Ag::100:64.2229, ± .0001

THE SILVER-CHLORINE RATIO.

For the ratio between silver and chlorine there are many series of determinations, some direct and some indirect. As with numerous other ratios, the first work entitled to any consideration was done by Berzelius.[1] He made three estimations, using each time twenty grammes of pure silver. This was dissolved in nitric acid. In the first experiment the silver chloride was precipitated and collected on a filter. In the second and third experiments the solution was mixed with hydrochloric acid in a flask, evaporated to dryness, and the residue then fused and weighed without transfer. One hundred parts of silver formed of chloride:

132.700
132.780
132.790

Mean, 132.757, ± .019

Turner's work[2] closely resembles that of Berzelius. Silver was dissolved in nitric acid and precipitated as chloride. In experiments one, two and three the mixture was evaporated and the residue fused. In experiment four the chloride was collected on a filter. A fifth experiment was made, but has been rejected as worthless.

The results were as follows: In a third column I put the quantity of AgCl proportional to 100 parts of Ag.

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[1] Thomson's Annals of Philosophy, 15, 89. 1820.

[2] Phil. Trans., 1829, 291.

28.407 grains Ag	gave	37.737	AgCl.	132.844
41.917	"	55.678	"	132.829
40.006	"	53.143	"	132.837
30.922	"	41.070	"	132.818

Mean, 132.832, ± .0038

The same general method of dissolving silver in nitric acid, precipitating, evaporating, and fusing without transfer of material was also adopted by Penny.[1] His results for 100 parts of silver are as follows, in parts of chloride:

132.836
132.840
132.830
132.840
132.840
132.830
132.838

Mean, 132.8363, ± .0012

In 1842 Marignac[2] found that 100 parts of silver formed 132.74 of chloride, but gave no available details. Later,[3] in another series of determinations, he was more explicit. Silver was dissolved in nitric acid, and precipitated by hydrochloric acid. The precipitate was washed several times with boiling water, by decantation, and the chloride was finally dried and fused in the same flask in which it had been formed. The figures are as follows:

Ag.	*AgCl.*	*Ratio.*
79.853	106.080	132.844
69.905	92.864	132.843
64.905	86.210	132.825
92.362	122.693	132.839
99.653	132.383	132.844

Mean, 132.839, ± .0024

Corrected for weighing in air the mean becomes 132.854.

The above series all represent the synthesis of silver chloride. Maumené[4] made analyses of the compound, reducing it to metal in a current of hydrogen. His experiments make 100 parts of silver equivalent to chloride:

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[1] Phil. Trans., 1839, 28.
[2] Ann. Chem. Pharm., 44, 21.
[3] Oeuvres Complètes, 1, 79.
[4] Ann. Chim. Phys. (3), 18, 49. 1846.

132.734
132.754
132.724
132.729
132.741

Mean, 132.7364, ± .0077

By Dumas[1] we have the following estimations:

9.954 Ag gave 13.227 AgCl.	Ratio,	132.882
19.976 " 26.542 "		132.869
	Mean,	132.8755, ± .0044

Next in order are seven determinations by Stas.[2] In the first, second and third, silver was heated in chlorine gas, and the synthesis of silver chloride thus effected directly. In the fourth and fifth silver was dissolved in nitric acid, and the chloride thrown down by passing hydrochloric acid gas over the surface of the solution. The whole was then evaporated in the same vessel, and the chloride fused, first in an atmosphere of hydrochloric acid, and then in a stream of air. The sixth synthesis was similar to these, only the nitric solution was precipitated by hydrochloric acid in slight excess, and the chloride thrown down was washed by repeated decantation. All the decanted liquids were afterwards evaporated to dryness, and the trace of chloride thus recovered was estimated in addition to the main mass. The latter was fused in an atmosphere of HCl. The seventh experiment was like the sixth, only ammonium chloride was used instead of hydrochloric acid. From 98.3 to 399.7 grammes of silver were used in each experiment, the operations were performed chiefly in the dark, and all weighings were reduced to vacuum. In every case the chloride obtained was beautifully white. Treating Stas' determinations as a single series, his figures are as follows:

Ag.	*AgCl.*	*Ratio.*
91.462	121.4993	132.841
69.86735	92.8145	132.843
101.519	134.861	132.843
108.549	144.207	132.849
399.651	530.920	132.846
99.9925	132.8382	132.848
98.3140	130.602	132.8417
		Mean, 132.8445, ± .0008

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[1] Ann. Chem. Pharm., 113, 21. 1860.

[2] Oeuvres Complètes, 1, 333-341.

According to Richards and Wells,[1] who made two very careful series of syntheses, the work of Stas on the silver-chlorine ratio was subject to constant errors. His silver probably contained occluded oxygen, and perhaps alkalies also, and his glass vessels were attacked and changed in weight by the acids used in his operations. These errors were avoided by Richards and Wells, who precipitated and fused their silver chloride either in porcelain or quartz vessels, generally the latter, and who employed silver of the highest possible purity. A number of minute corrections were also applied to their determinations, but these cannot be considered in detail now. The results obtained appear in the two following tables:

Preliminary Series.

Ag.	*AgCl.*	*Ratio.*
9.06843	12.04365	132.861
8.39217	11.14985	132.860
5.37429	7.14056	132.865
8.08222	10.73869	132.868
7.08517	9.41362	132.864
7.97715	10.59837	132.859
8.11978	10.78767	132.857
8.53452	11.33907	132.861
6.73284	8.94511	132.858
8.91366	11.84240	132.857
9.72295	12.91769	132.858
8.63961	11.47862	132.860
11.13795	14.79849	132.865

Mean, 132.8610, ± .00065

Final Series.

Ag.	*AgCl.*	*Ratio.*
7.24427	9.62508	132.865
8.30502	11.03484	132.870
7.29058	9.68676	132.867
8.58472	11.40614	132.866
8.01318	11.64648	132.862
9.77160	12.98335	132.868
7.98170	10.60528	132.870
11.49983	15.27964	132.868
6.25318	8.30834	132.866
7.72479	12.26360	132.866

Mean, 132.8668, ± .0005

The foregoing nine series of determinations are to be classed as *direct;* that is, they were made for the express purpose of measuring the ratio

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[1] Publ. Carnegie Inst., Washington, No. 28, 1905.

between silver and chlorine, and were not complicated by other considerations. Arranged in the order of ascending magnitude, and expressed in the form Ag : Cl : : 100 : x, these combine as follows:

Maumené	32.736,	± .0077
Berzelius	32.757,	± .0190
Turner	32.832,	± .0038
Penny	32.836,	± .0012
Stas	32.8445,	± .0008
Marignac	32.854,	± .0024
Richards and Wells, preliminary	32.861,	± .00065
Richards and Wells, final	32.8668,	± .0005
Dumas	32.8755,	± .0044
General mean	32.8582,	± .00042

This general mean falls within, but near the lower limit of Richards and Wells' preliminary series.

A second group of determinations of the silver-chlorine ratio may be termed *incidental*. A chloride is balanced against silver, and the silver chloride produced is also weighed, and this procedure, intended to fix other atomic weights, also gives values for the ratio now under consideration. The following determinations, thus obtained, are all useful. I limit myself, however, to work done by individual authorities, and do not attempt to combine observations, say of RCl : Ag by one chemist, and RCl : AgCl by another, into determinations of the ratio Ag : AgCl. The details of the several investigations will be found in subsequent chapters of this work, in relation to what I may term the several collateral elements.

The first series of this incidental kind to be now considered is due to Lenher,[1] and is derived from his data on the atomic weight of selenium. Silver selenite was converted into silver chloride, and the latter was afterwards reduced to metal by heating in hydrogen. The vacuum weights and the derived ratio appear in the next table.

AgCl.	*Ag.*	*Ratio.*
.21897	.16480	132.870
.48522	.36534	132.813
.58999	.44417	132.830
.67532	50821	132.882
.82232	.61882	132.885
1.08350	.81562	132.844
1.36288	1.02588	132.850
1.67234	1.25884	132.848
		Mean, 132.853, ± .0060

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[1] Journ. Amer. Chem. Soc., 20, 555. 1898.

Similar data are furnished by Ebaugh's[1] analyses of silver arsenate, which were designed to determine the atomic weight of arsenic. The weights are all reduced to a vacuum standard.

AgCl.	*Ag.*	*Ratio.*
.21547	.162175	132.863
.44615	.33583	132.850
.48820	.367525	132.844
.74517	.56099	132.831
.88083	.66318	132.819
.94830	.71400	132.815
.98014	.73771	132.863
		Mean, 132.841, ± .0050

In their memoir upon the atomic weight of cæsium, Richards and Archibald[2] give analyses of cæsium and potassium chloride, balancing each salt against silver and silver chloride. In the following table the first two determinations are derived from the potassium salt, and the others from the cæsium compound. The weights refer to the vacuum standard, as do all the others in this group of determinations.

Ag.	*AgCl.*	*Ratio.*
3.61747	4.80600	132.855
3.62283	4.81325	132.859
2.45600	3.26240	132.834
2.53351	3.36532	132.832
1.45686	1.93555	132.858
1.94244	2.58003	132.824
2.05023	2.72382	132.854
1.50720	2.00253	132.864
1.32251	1.75678	132.837
1.29434	1.71972	132.864
1.13743	1.51093	132.837
1.97590	2.62484	132.835
2.00760	2.66720	132.855
3.24850	4.31570	132.852
		Mean, 132.847, ± .0024

Figures of the same order are given by Archibald in his research upon the atomic weight of rubidium,[3] and again in his memoir upon potassium.[4]

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[1] Doctoral thesis, University of Pennsylvania, 1901.

[2] Proc. Amer. Acad., 38, 443. 1903.

[3] Journ. Chem. Soc., 85, 786. 1904.

[4] Trans. Roy. Soc., Canada, 1904, Sec. III, p. 47.

Rubidium Series.

Ag.	*AgCl.*	*Ratio.*
1.78454	2.37070	132.842
1.84241	2.44778	132.858
2.04710	2.71960	132.851
.97702	1.29796	132.849
1.91316	2.54118	132.826
2.58550	3.43475	132.847
1.96076	2.60452	132.832
1.91462	2.54386	132.865
1.89346	2.51557	132.856
2.01515	2.67685	132.836
1.94594	2.58528	132.855
2.07668	2.75878	132.846
3.56998	4.74233	132.842
2.17233	2.88613	132.862
		Mean, 132.848, ± .0020

Potassium Series.

Ag.	*AgCl.*	*Ratio.*
3.20598	4.25916	132.850
2.88479	3.83250	132.852
4.19557	5.57396	132.853
6.85280	9.10362	132.845
		Mean, 132.850, ± .0012

The analyses of cobalt chloride, by Baxter and Coffin,[1] furnish the subjoined figures:

Ag.	*AgCl.*	*Ratio.*
1.82671	2.42676	132.846
2.45398	3.26095	132.884
6.38081	8.47735	132.857
4.92244	6.54019	132.865
5.78815	7.69084	132.872
5.47410	7.27284	132.859
2.61905	3.48012	132.877
		Mean, 132.8657, ± .0034

From the analyses, by Baxter and Hines,[2] of manganese chloride we have—

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[1] Zeitsch. anorg. Chem., 51, 171. 1906.

[2] Journ. Amer. Chem. Soc., 28, 1560. 1906.

Ag.	*AgCl.*	*Ratio.*
7.93740	10.54641	132.870
6.05041	8.03868	132.862
5.67279	7.53731	132.868
6.11818	8.12932	132.871
5.91637	7.86129	132.873
7.67995	10.20372	132.862
6.72227	8.93140	132.863

Mean, 132.8670, ± .0012

Baxter and Wilson,[1] analyzing lead chloride, obtained the following data:

Ag.	*AgCl.*	*Ratio.*
3.62987	4.82273	132.862
3.21408	4.27016	132.858
3.97568	5.28272	132.876
2.99456	3.97949	132.891
2.40837	3.19909	132.832
3.33407	4.42982	132.865

Mean, 132.864, ± .0054

These incidental series of values for the ratio Ag : Cl :: 100 : x now combine thus:

Ebaugh, As series....................	32.841,	± .0050
Richards and Archibald, Cs series....	32.847,	± .0024
Archibald, Rb series..................	32.848,	± .0020
Archibald, K series..................	32.850,	± .0012
Lenher, Se series....................	32.853,	± .0060
Baxter and Wilson, Pb series.........	32.864,	± .0054
Baxter and Coffin, Co series...........	32.8657,	± .0034
Baxter and Hines, Mn series..........	32.8670,	± .0012
General mean	32.8562,	± .00071

A third group of determinations is to be classed as *indirect*. When two ratios, RCl : Ag and RCl : AgCl have been measured by the same investigator, but independently of each other, the cross ratio, Ag : Cl :: 100 : x is easily calculable from them. The examples to be given presently are almost self-explanatory; but the details of the determinations must be sought for in the later sections of this work, on titanium, magnesium, barium, cadmium, etc.

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[1] Journ. Amer. Chem. Soc., 30, 187. 1908.

In his determinations of the atomic weight of titanium, Thorpe gives data from which the subjoined ratios are derived:

$4Ag:TiCl_4::100:43.999.$ ± .0032
$4AgCl:TiCl_4::100:33.118,$ ± .0019

Hence Ag : Cl : : 100 : 32.855, ± .0092.

From Richards' analyses of barium chloride we have—

$2Ag:BaCl_2::100:96.525,$ ± .0010
$2AgCl:BaCl_2::100:72.653,$ ± .0014

Hence Ag : Cl : : 100 : 32.8575, ± .0029.

Ratios computed from the analyses of magnesium chloride by Richards and Parker:

$2Ag:MgCl_2::100:44.138,$ ± .0003
$2AgCl:MgCl_2::100:33.226,$ ± .0013

Hence Ag : Cl : : 100 : 32.842, ± .0054.

Data for cadmium chloride are given by Baxter and Hines, and also, later, by Baxter, Hines and Frevert. Their series, combined together, give—

$2Ag:CdCl_2::100:84.9677,$ ± .0008
$2AgCl:CdCl_2::100:63.9523,$ ± .0004

Hence Ag : Cl : : 100 : 32.861, ± .0016.

For sodium chloride the analyses of Richards and Wells give the following ratios:

$Ag:NaCl::100:54.1854,$ ± .00015
$AgCl:NaCl::100:40.7797,$ ± .00028

Hence Ag : Cl : : 100 : 32.873, ± .0010.

The potassium chloride ratios of Richards and Staehler are—

$Ag:KCl::100:69.1073,$ ± .00032
$AgCl:KCl::100:52.0118,$ ± .00025

Hence Ag : Cl : : 100 : 32.869, ± .0009.

This group of indirect estimates combines as follows:

Richards and Parker, Mg ratios	32.842,	± .0054
Thorpe, Ti ratios	32.855,	± .0092
Richards, Ba ratios	32.8575,	± .0029
Baxter, Hines and Frevert, Cd ratios	32.861,	± .0016
Richards and Staehler, K ratios	32.869,	± .0009
Richards and Wells, Na ratios	32.873,	± .0010
General mean	32.8684,	± .0006

Combining the three groups of determinations, the final value for the ratio Ag : Cl : : 100 : *x* is obtained.

Direct determinations	32.8582, ± .00042
Incidental determinations	32.8562, ± .00071
Indirect determinations	32.8684, ± .00060
General mean	32.8606, ± .00031

This value is almost identical with that found by Richards and Wells in their preliminary series of determinations, namely, 32.8610.

Addenda. The following indirect determinations of the silver-chlorine ratio appeared too late to be used in the general discussion of the fundamental ratios.

In Archibald's work on the atomic weight of platinum the subjoined ratios appear:

Ag : Pt : : 100 : 180.965, ± .0034
AgCl : Pt : : 100 : 136.203, ± .0031

Hence Ag : Cl : : 100 : 32.864, ± .0039.

The final series of determinations by Richards and Willard of the atomic weight of lithium, give these data:

Ag : LiCl : : 100 : 39.2992, ± .00014
AgCl : LiCl : : 100 : 29.5786, ± .00014

Hence Ag : Cl : : 100 : 32.8637, ± .00077.

From the strontium chloride ratios of Thorpe and Francis I find:

2Ag : $SrCl_2$: : 100 : 73.490, ± .0008
2AgCl : $SrCl_2$: : 100 : 55.311, ± .0009

Hence Ag : Cl : : 100 : 32.867, ± .0026.

THE SILVER-BROMINE RATIO.

The measurements of the silver-bromine ratio resemble those of the ratio between silver and chlorine, and fall into three groups. First in order are the *direct* determinations.

Marignac,[1] to effect the synthesis of silver bromide, dissolved the metal in nitric acid, precipitated the solution with potassium bromide, washed, dried, fused and weighed the product. The ratio Ag : Br : : 100 : *x* is given in the third column:

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[1] Oeuvres Complètes, 1, 83.

Ag.	*AgBr.*	*Ratio.*
25.000	43.518	74.072
20.120	35.020	74.055
15.000	26.110	74.066
		Mean, 74.064, ± .003

Corrected for weighing in air the mean becomes 74.077.

Much more elaborate determinations of this ratio are due to Stas.[1] In one experiment a known weight of silver was converted into nitrate, and precipitated in the same vessel by pure hydrobromic acid. The resulting bromide was washed thoroughly, dried, and weighed. In four other estimations the silver was converted into sulphate. Then a known quantity of pure bromine, as nearly as possible the exact amount necessary to precipitate the silver, was transformed into hydrobromic acid. This was added to the dilute solution of the sulphate, and, after precipitation was complete, the minute trace of an excess of silver in the clear supernatant fluid was determined. All weighings were reduced to a vacuum. The data are as follows:

Ag.	*AgBr.*	*Ratio.*
53.1958	92.6042	74.0830
51.3436	89.3780	74.0790
55.0615	95.8505	74.0795
55.8040	97.1450	74.0805
43.3620	84.1904	74.0830
		Mean, 74.0810, ± .0006

In his paper on the atomic weight of cadmium,[2] Huntington gives three syntheses and three analyses of silver bromide. The data are as follows, with the usual ratio given in the last column:

1.4852	grm. Ag gave	2.5855	AgBr.	74.084
1.4080	"	2.4510	"	74.077
1.4449	"	2.5150	"	74.060
4.1450	grm. AgBr gave	2.3817	Ag.	74.035
1.8172	"	1.0437	"	74.111
4.9601	"	2.8497	"	74.057
				Mean, 74.071, ± .0072

Similar synthetic data are also given by Richards, incidentally to his work on copper.[3] There are two sets of three experiments each, which can here be treated as one series, thus:

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[1] Oeuvres Complètes, 1, 587, 603.

[2] Proc. Amer. Acad., 17, 28. 1881.

[3] Proc. Amer. Acad., 25, 199, 210, 211. 1890.

1.11235	grm. Ag gave	1.93630	AgBr.	74.073
1.57620	“	2.74335	“	74.044
2.16670	“	3.77170	“	74.076
.9664	“	1.68205	“	74.053
.9645	“	1.6789	“	74.069
.9639	“	1.6779	“	74.074

Mean, 74.065, ± .0035

In their research upon the electrochemical equivalents of copper and silver, Richards, Collins and Heimrod[1] give the following syntheses of silver bromide from electrolytic silver:

Ag.	*AgBr.*	*Ratio.*
.71585	1.24567	74.013
5.43807	9.46557	74.061
3.76993	6.56216	74.066
2.29649	3.99820	74.100
2.15701	3.75473	74.071
2.37893	4.14187	74.106
2.97120	5.17218	74.077

Mean, 74.0706, ± .0078

It is only fair to state in this connection that the foregoing series was intended to determine the purity of the silver, and not as an accurate measure of the ratio.

Scott,[2] in his analyses of ammonium bromide, titrated the compound with silver. He afterwards collected and weighed the silver bromide, in order to determine the silver bromide ratio. The subjoined weights refer to the vacuum standard:

Ag.	*AgBr.*	*Ratio.*
6.82315	11.87733	74.074
9.66809	16.82816	74.090*
5.41906	9.43315	74.0735
5.51258	9.59596	74.074
5.70686	9.93346	74.062
5.33191	9.28093	74.064
5.62572	9.79254	74.067

Mean, 74.072, ± .0023

The starred figure is corrected for a trace of impurity.

In his paper on the atomic weight of iron Baxter[3] gives three direct comparisons of silver with silver bromide, with vacuum weights, as follows:

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[1] Proc. Amer. Acad., 35, 139. 1899.

[2] Journ. Chem. Soc., 79, 147. 1901.

[3] Proc. Amer. Acad., 39, 250. 1904.

Ag.	*AgBr.*	*Ratio.*
4.77783	8.31754	74.086
5.87977	10.23533	74.077
4.82995	8.40809	74.082

Mean, 74.082, ± .0018

A much more thorough and conclusive set of syntheses was published by Baxter[1] in 1906. The purest silver was dissolved in nitric acid, and precipitated by ammonium bromide. The silver bromide, before weighing, was fused in an atmosphere containing bromine vapor. With vacuum weights, Baxter's figures are as follows:

Ag.	*AgBr.*	*Ratio.*
4.71853	8.21363	74.072
5.01725	8.73393	74.078
5.96818	10.38932	74.079
5.62992	9.80039	74.077
8.13612	14.16334	74.080
5.07238	8.82997	74.079
4.80711	8.36827	74.081
4.27279	7.43776	74.072
5.86115	10.20299	74.078
7.91425	13.77736	74.083
6.40765	11.15468	74.084
6.38180	11.10930	74.078
6.23696	10.85722	74.079
9.18778	15.99392	74.078
8.01261	13.94826	74.079
10.48638	18.25452	74.078
8.59260	14.95797	74.079
8.97307	15.62022	74.079

Mean, 74.0785, ± .00047

The direct determinations of the ratio Ag : Br combine thus:

Richards	74.065, ± .0035
Richards, Collins and Heimrod	74.0706, ± .0078
Huntington	74.071, ± .0072
Scott	74.072, ± .0023
Marignac	74.077, ± .0030
Baxter, 1906	74.0785, ± .00047
Stas	74.081, ± .0006
Baxter, 1904	74.082, ± .0018
General mean	74.0797, ± .00035

Analyses of various metallic bromides have furnished many incidental determinations of the silver-bromine ratio, like those already described

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[1] Journ. Amer. Chem. Soc., 28, 1322. 1906.

for silver and chlorine. In his work on titanium bromide, intended to determine the atomic weight of titanium, Thorpe[1] gives the following equivalent weights of silver and silver bromide:

Ag.	*AgBr.*	*Ratio.*
3.66122	6.375391	74.133
5.55097	9.663901	74.094
8.17645	14.227716	74.008
7.83493	13.639956	74.092
		Mean, 74.082, ± .0176

Thorpe and Laurie[2] compared gold with silver and silver bromide, and give equivalent weights as follows:

Ag.	*AgBr.*	*Ratio.*
3.38451	5.89199	74.087
2.60896	4.54261	74.113
2.28830	3.98288	74.054
2.26415	3.94309	74.153
1.97147	3.43015	73.989
2.01292	3.50207	73.980
2.50334	4.35736	74.062
2.93608	5.11045	74.057
		Mean, 74.062, ± .0143

In Richards'[3] memoir upon the atomic weight of barium, the subjoined vacuum weights of Ag and AgBr are given as equivalent to each other. Two additional determinations are rejected by Richards as inaccurate:

Ag.	*AgBr.*	*Ratio.*
1.71323	2.98230	74.075
2.13584	3.71809	74.081
1.52921	2.66191	74.071
2.11740	3.68615	74.089
1.72276	2.99868	74.063
1.34175	2.33530	74.049
4.11360	7.16120	74.086
2.56010	4.45670	74.083
2.51415	4.37669	74.082
		Mean, 74.075, ± .0029

From the analyses of nickel bromide, by Richards and Cushman,[4] the following figures are derived. These, and all the subsequent series, represent vacuum weights:

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[1] Journ. Chem. Soc., 47, 126. 1885.

[2] Journ. Chem. Soc., 51, 565. 1887.

[3] Proc. Amer. Acad., 28, 1. 1893.

[4] Proc. Amer. Acad., 33, 97. 1897.

Ag.	*AgBr.*	*Ratio.*
3.23910	5.63892	74.089
2.66636	4.64208	74.098
3.33990	5.81391	74.074
1.31787	2.29435	74.088
1.23482	2.14963	74.085
1.30629	2.27384	74.069
2.21652	3.85805	74.059

Richards and Cushman also give one direct determination of the ratio, in which 2.10289 grammes of silver yielded 3.66066 of bromide. Ratio, 74.078. Including this in the foregoing series, the mean becomes 74.080, ±.0030.

Similar data appear in the memoirs of Richards and Baxter[1] on the atomic weight of cobalt. Their analyses of cobalt bromide gave the following equivalent figures:

Ag.	*AgBr.*	*Ratio.*
1.31702	2.29296	74.102
2.54585	4.43095	74.046
2.80449	4.88135	74.055
1.81170	3.15368	74.073
2.64879	4.61046	74.059
2.84891	4.95943	74.086
2.29593	3.99706	74.093
1.89033	3.29053	74.072

There are also in these two memoirs by Richards and Baxter, three direct determinations of the ratio, as follows:

Ag.	*AgBr.*	*Ratio.*
2.18679	3.80679	74.081
2.91386	5.07226	74.073
2.97097	5.17170	74.074

Taking these with the previous eight determinations as one series, the mean value for the ratio is 74.074, ±.0033.

From the analyses of uranium bromide, by Richards and Merigold,[2] the following figures are obtained:

Ag.	*AgBr.*	*Ratio*
1.39365	2.42588	74.066
.82559	1.43713	74.073
1.43617	2.50009	74.080
		Mean, 74.073, ± .0027

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[1] Proc. Amer. Acad., 33, 115, 1897; and 34, 351, 1899.

[2] Proc. Amer. Acad., 37, 393. 1902.

The following figures are derived from the analyses, by Richards and Archibald,[1] of cæsium bromide:

Ag.	*AgBr.*	*Ratio.*
1.77402	3.08815	74.076
3.14606	5.47673	74.082
3.63740	6.33213	74.084
		Mean, 74.081, ± .0017

Archibald's[2] analyses of rubidium bromide give a similar series of comparisons, as follows:

Ag.	*AgBr.*	*Ratio.*
1.74930	3.04578	74.114
1.35230	2.35401	74.075
1.37061	2.38589	74.076
1.70300	2.96462	74.081
2.50590	4.36215	74.075
2.46502	4.29084	74.069
2.83340	4.93210	74.070
		Mean, 74.080, ± .0040

Baxter, Hines and Frevert,[3] in order to determine the atomic weight of cadmium, analyzed cadmium bromide. Their silver figures are subjoined:

Ag.	*AgBr.*	*Ratio.*
9.08379	15.81319	74.081
5.40724	9.41267	74.075
5.35277	9.31830	74.084
5.61597	9.77649	74.088
4.07226	7.08933	74.088
4.63072	8.06130	74.083
4.68200	8.15070	74.086
4.75259	8.27360	74.086
		Mean, 74.084, ± .0010

Baxter and Hines[4] also analyzed manganese bromide, and give the following equivalent weights of Ag and AgBr:

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[1] Proc. Amer. Acad., 38, 443. 1903.

[2] Journ. Chem. Soc., 85, 776. 1904.

[3] Journ. Amer. Chem. Soc., 28, 770. 1906.

[4] Journ. Amer. Chem. Soc., 28, 1560. 1906.

Ag.	*AgBr.*	*Ratio.*
6.56765	11.43300	74.080
4.83238	8.41206	74.077
4.90354	8.53642	74.087
5.65813	9.85008	74.087
5.82600	10.14206	74.083
3.61478	6.29271	74.083
5.18711	9.02959	74.077
3.94042	6.85968	74.085
4.51250	7.85571	74.088
3.61736	6.29740	74.088
4.79620	8.34915	74.078
3.59319	6.25569	74.098
5.72641	9.96840	74.078
		Mean, 74.084, ± .0011

The incidental determinations of the silver-bromine ratio now combine thus:

Thorpe and Laurie, Au series	74.062, ± .0143
Richards and Marigold, U series	74.073, ± .0027
Richards and Baxter, Co series	74.074, ± .0033
Richards, Ba series	74.075, ± .0029
Richards and Cushman, Ni series	74.080, ± .0030
Archibald, Rb series	74.080, ± .0040
Richards and Archibald, Cs series	74.081, ± .0017
Thorpe, Ti series	74.082, ± .0176
Baxter, Hines and Frevert, Cd series	74.084, ± .0010
Baxter and Hines, Mn series	74.084, ± .0011
General mean	74.082, ± .0006

Several indirect determinations of the silver bromine ratio, as in the case of the chlorides, are deducible from analyses of metallic bromides.[1]

In Cooke's determinations of the atomic weight of antimony, the ratios are as follows:

$3Ag:SbBr_3::100:111.114$, ± .0014
$3AgBr:SbBr_3::100:63.830$, ± .008

Hence Ag: Br:: 100: 74.078, ± .0219.

From Huntington's analyses of cadmium bromide we have—

$2Ag:CdBr_2::100:126.076$, ± .0052
$2AgBr:CdBr_2::100:72.4216$, ± .0028

Hence Ag: Br:: 100: 74.086, ± .0098.

The work of Richards on strontium bromide gives—

$2Ag:SrBr_2::100:114.689$, ± .0012
$2AgBr:SrBr_2::100:65.884$, ± .0006

Hence Ag: Br:: 100: 74.077, ± .0024.

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[1] For details, see later sections of this work, on Sb, Cd, Sr, Zn, Cd, Fe, etc.

The ratios deduced from analyses of zinc bromide by Richards and Rogers are—

$2Ag:ZnBr_2::100:104.380$, ± .0007
$2AgBr:ZnBr_2::100:59.962$, ± .0004

Hence Ag : Br : : 100 : 74.077, ± .0016.

Analyses by Baxter of ferrous bromide yield the following ratios:

$2Ag:FeBr_2::100:99.960$, ± .0027
$2AgBr:FeBr_2::100:57.4195$, ± .00044

Hence Ag : Br : : 100 : 74.087, ± .0049.

Richards and Mueller studied potassium bromide with the subjoined results:

$Ag:KBr::100:110.319$, ± .0004
$AgBr:KBr::100:63.3727$, ± .0003

Hence Ag : Br : : 100 : 74.081, ± .0012.

The combination of all these estimates is as follows:

Richards, Sr series	74.077,	± .0024
Richards and Rogers, Zn series	74.077,	± .0016
Cooke, Sb series	74.078,	± .0219
Richards and Mueller, K series	74.081,	± .0012
Huntington, Cd series	74.086,	± .0098
Baxter, Fe series	74.087,	± .0049
General mean	74.0795,	± .00098

Finally, combining the three groups of figures for the ratio Ag : Br : : 100 : x we have—

Direct determinations	74.0797,	± .00035
Incidental determinations	74.082,	± .0006
Indirect determinations	74.0795,	± .00098
General mean	74.0802,	± .00029

Addenda. The determinations by Archibald of the atomic weight of platinum give the following ratios:

$Ag:Pt::100:180.965$, ± .0034
$AgBr:Pt::100:103.955$, ± .0037

Hence Ag : Br : : 100 : 74.080, ± .0070.

From the work of Thorpe and Francis on strontium bromide we have—

$2Ag:SrBr_2::100:114.703$, ± .0040
$2AgBr:SrBr_2::100:65.892$, ± .0011

Hence Ag : Br : : 100 : 74.077, ± .0067.

These figures were received too late to be used in the final reductions of the fundamental ratios.

THE SILVER-IODINE RATIO.

The composition of silver iodide, first thoroughly investigated by Marignac and Stas, has recently been the subject of elaborate researches.

Marignac[1] dissolved weighed quantities of silver in nitric acid, and precipitated the silver iodide with a solution of potassium iodide. He gives the following weights, and the ratio of AgI to 100 parts of Ag:

Ag.	*AgI.*	*Ratio.*
15.000	32.625	217.500
14.790	32.170	217.512
18.545	40.339	217.520
		Mean, 217.511, ± .0036

Corrected for weighing in air this becomes 217.5335.

Three series of determinations are given by Stas,[2] all with weights corrected to a vacuum standard.

In the first series of experiments Stas converted a known weight of silver into nitrate, and then precipitated with pure hydriodic acid. The iodide thus thrown down was washed, dried and weighed without transfer. His figures are as follows:

Ag.	*AgI.*	*Ratio.*
97.5915	212.2905	217.529
43.5281	94.6984	217.536
		Mean, 217.5325, ± .0024

In the second series a complete synthesis of silver iodide from known weights of iodine and metal was effected. The iodine was dissolved in a solution of ammonium sulphite, and thus converted into ammonium iodide. The silver was transformed into sulphate and the two solutions were mixed. When the precipitate of silver iodide was completely deposited the supernatant liquid was titrated for the trifling excess of iodine which it always contained. As the two elements were weighed out in the ratio of 127 to 108, while the atomic weight of iodine is probably a little under 127, this excess is easily explained. From these experiments two sets of values were deduced; one from the weights of silver and

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[1] Oeuvres Complètes, 1, 86.

[2] Oeuvres Complètes, 1, 548-583.

iodine actually employed, the other from the quantity of iodide of silver collected. From the first set we have of iodine for 100 parts of silver:

117.5390
117.5380
117.5318
117.5430
117.5420
117.5300

Mean, 117.5373, ± .0015

From the weight of silver iodide actually collected the following figures are given for the ratio Ag : I. The third experiment in the foregoing column has no equivalent here:

117.529
117.531
117.539
117.538
117.530

Mean, 117.5334, ± .0014

These determinations, by Marignac and Stas, are remarkably concordant, and yet, as shown by later investigations, they are affected by constant errors. Silver iodide, precipitated from nitrate solutions, occludes silver nitrate, a fact which must be taken into account in two of the preceding series. The concordance between the second and third series of Stas, however, remains unexplained, if we suppose them to be in error also. That the errors in four sets of determinations, by two observers and four methods, should be so exactly alike in direction and magnitude, is difficult to understand.

With Ag = 107.93, the Marignac-Stas determinations of this ratio make I = 126.85. This value was accepted for many years, until Ladenburg, in 1902, showed that it was about one-tenth of a unit too low. Ladenburg[1] depended upon the ratio Ag : AgCl to establish this conclusion, but he also gave one measurement of the ratio now under consideration, as follows: 50.3147 grammes Ag gave 109.4608 AgI, whence the ratio Ag : AgI = 217.552; a figure higher than those in the foregoing tables, but not strikingly so.

Soon after the publication of Ladenburg's memoir, Scott[2] announced two syntheses of silver iodide, as follows, with weights corrected to a vacuum:

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[1] Ber. Deutsch. chem. Ges., 35, 2275. 1902.

[2] Proc. Chem. Soc., 18, 112. 1902.

Ag.	*AgI.*	*Ratio.*
4.6240	10.0634	117.6340
6.39978	13.92913	117.6502
		Mean, 117.6421, ± .0054

Koethner and Aeuer,[1] who also studied what might be called the Ladenburg ratio, succeeded in proving the occlusion of silver nitrate by silver iodide, to which allusion has already been made. They effected two syntheses of silver iodide, however, avoiding this error, and by two methods. First, silver iodide was precipitated from solution with pure hydriodic acid. Secondly, silver was directly combined with iodine, by heating in a stream of iodine vapor. The two syntheses are subjoined, with the ratio stated in the form Ag : AgI, and the weights corrected to a vacuum:

Ag.	*AgI.*	*Ratio.*
34.51789	75.12752	217.6347
11.37544	24.75691	217.6480
		Mean, 217.6413, ± .0045

The very thorough and careful experiments by Baxter[2] fall into several series, and represent several distinct methods of procedure. First, pure silver was converted into nitrate, and precipitated by a solution of ammonium iodide in presence of an excess of ammonia. All weighings in Baxter's experiments were reduced to a vacuum standard, and various minor corrections were applied, concerning which the original memoirs must be consulted. Two series of determinations are given, as follows:

Preliminary Series.

Ag.	*AgI.*	*Ratio.*
5.23123	11.38531	217.6411
3.57039	7.77033	217.6325
4.60798	10.02804	217.6233
4.52467	9.84822	217.6561
4.66256	10.14591	217.6039
		Mean, 217.6314, ± .0059

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[1] Liebig's Annalen, 337, 123. 1904.

[2] Two memoirs. First, Proc. Amer. Acad., 40, 419. 1904. Second, *ibid.*, 41, 73. 1905.

Final Series.[1]

Ag.	*AgI.*	*Ratio.*
4.77244	10.38698	217.645
4.82882	10.50981	217.648
4.04262	8.79755	217.620
1.64711	3.58515	217.663
4.85804*	10.57318	217.643
4.83482	10.52241	217.638
4.97120	10.81800	217.613
3.53858	7.70136	217.640
3.89693	8.48187	217.655
5.33031	11.60111	217.644
5.08748	11.07259	217.644

Mean, 217.6412, ± .0029

Mean, rejecting the seventh value, 217.6440, ± .0024

Secondly, pure iodine was weighed, converted into hydriodic acid by means of sulphurous acid, and then transformed into ammonium iodide with pure ammonia. As nearly as possible the exact equivalent of silver was dissolved in nitric acid, and added to the iodine solution. The trifling excess of silver or iodine was finally determined by titration. The following results were thus obtained:

Ag.	*I.*	*Ratio.*
5.54444	6.52288	117.6473
6.27838	7.38647	117.6493
4.57992	5.38814	117.6470

Mean, 117.6479, ± .0005

In Baxter's second memoir the ratio just given was redetermined with variations in the process. Iodine was converted into hydriodic acid, and precipitated by a solution of silver, taking care to avoid an excess of the latter. The final adjustment was effected by titration, as before. In five of the experiments the silver iodide so produced from a known weight of iodine was collected and weighed. In the following table both ratios are expressed in the form Ag : I :: 100 : x; A representing the direct comparisons, and B the silver iodide syntheses.

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[1] The starred figure is erroneously given in the original. The corrected figure was kindly furnished me by Professor Baxter. The seventh experiment in the series Baxter rejects.

I.	*Ag.*	*AgI.*	*Ratio A.*	*Ratio B.*
3.29308	2.79897	......	117.6533	....
3.70132	3.14584	......	117.6576	....
3.75641	3.19258	6.94913	117.6607	117.6555
3.24954	2.76186	6.01137	117.6576	117.6589
4.12541	3.50639	7.63204	117.6543	117.6460
3.53166	3.00165	6.53351	117.6573	117.6494
2.99835	2.54842	5.54682	117.6552	117.6529
2.00015	1.69991	......	117.6621	....
		Mean,	117.6573, ± .0007	117.6525, ± .0015

In a final series of experiments, based upon the silver used in the preceding set, the ratio Ag : AgI was redetermined, as follows:

Ag.	*AgI.*	*Ratio.*
3.19249	6.94877	217.6600
2.76175	6.01110	217.6555
3.00189	6.53399	217.6628
2.54833	5.54659	217.6558
		Mean, 217.6585, ± .0012

The determinations of the silver-iodine ratio by Gallo,[1] although numerous, are not as concordant as the foregoing series. Silver was deposited electrolytically, and in the same circuit iodine was liberated from a solution of potassium iodide, and determined afterwards by thiosulphate titration. With vacuum weights the results obtained are as follows:

Ag.	*I.*	*Ratio.*
.18054	.21230	117.5917
.21360	.251309	117.6541
.23103	.27181	117.6513
.24005	.28213	117.5291
.15454	.18167	117.5553
.2597	.30515	117.5010
.16229	.19080	117.5673
.300988	.35411	117.6490
.26819	.31528	117.5584
.25877	.30425	117.5755
.24422	.28703	117.5293
.20838	.24516	117.6505
.25047	.29445	117.5599
.20266	.23826	117.5664
.18316	.21533	117.5639
.37278	.43809	117.5197
.28221	.33207	117.6677

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[1] Atti Accad. Lincei (5), 15, 24. 1906. Gazz. Chim. Ital., 36, 116.

.2582	.30356	117.5677
.33963	.39923	117.5485
.33461	.39345	117.5846
.3360	.39502	117.5655
.37025	.43526	117.5584
.30824	.36233	117.5480
.36390	.42789	117.5845
		Mean, 117.5770, ± .0074

Neglecting the single determination by Ladenburg, and reducing all the series to the common form of Ag : I :: 100 : x, the various means combine thus:

Marignac	117.5335, ± .0036
Stas, first	117.5325, ± .0024
Stas, second	117.5373, ± .0015
Stas, third	117.5334, ± .0014
Scott	117.6421, ± .0054
Koethner and Aeuer	117.6413, ± .0045
Baxter, 1904, preliminary	117.6314, ± .0059
Baxter, 1904, Ag:AgI	117.6412, ± .0029
Baxter, 1904, Ag:I	117.6479, ± .0005
Baxter, 1905, Ag:I	117.6573, ± .0007
Baxter, 1905, I:AgI	117.6525, ± .0015
Baxter, 1905, Ag:AgI	117.6585, ± .0012
Gallo	117.5770, ± .0074
General mean	117.6351, ± .00034

If we reject the determinations of Marignac, Stas and Gallo the general mean becomes 117.6515, ± .00037. The 1905 determinations by Baxter are probably the best, but they are not absolute and not entitled to exclusive consideration. The two general means correspond to a difference of 0.018 in the atomic weight of iodine.

RATIOS CONNECTING THE SILVER HALIDES.

The three ratios between the silver halides, AgCl : AgBr, AgCl : AgI, and AgBr : AgI, have all been measured with a high degree of accuracy, and by essentially the same process.

When silver bromide is heated in chlorine gas, silver chloride is formed. In 1860 Dumas[1] employed this method for estimating the atomic weight of bromine. His results are as follows. In the third column I give the weight of AgBr equivalent to 100 parts of AgCl:

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[1] Ann. Chem. Pharm., 113, 20.

AgBr.	*AgCl.*	*Ratio.*
2.028	1.547	131.092
4.237	3.235	130.974
5.769	4.403	131.024
		Mean, 131.030, ± .023

The two series of determinations by Baxter[1] are much more elaborate, and far more conclusive. Before being weighed, the silver bromide was fused in a current of air saturated with bromine. The figures given below are for vacuum weights, which is true for all of Baxter's data as cited in this section.

1905 Series.

AgBr.	*AgCl.*	*Ratio.*
10.92091	8.33538	131.019
13.88062	10.59457	131.016
8.21484	6.27006	131.017
7.87887	6.01352	131.020
6.90106	5.26735	131.016
9.53704	7.27926	131.017
		Mean, 131.0175, ± .00045

1906 Series.

AgBr.	*AgCl.*	*Ratio.*
8.03979	6.13642	131.0176
8.57738	6.54677	131.0170
13.15698	10.04221	131.0168
12.71403	9.70413	131.0167
13.96784	10.66116	131.0162
13.08168	9.98469	131 0174
12.52604	9.56059	131.0175
11.11984	8.48733	131.0170
8.82272	6.73402	131.0172
11.93192	9.10721	131.0162
12.53547	9.56767	131.0190
17.15021	13.09009	131.0167
10.31852	7.87572	131.0168
		Mean, 131.0171, ± .00013

The three series combine as follows:

Dumas	131.030, ± .023
Baxter, 1905	131.0175, ± .00045
Baxter, 1906	131.0171, ± .00013
General mean	131.0172, ± .00012

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[1] Proc. Amer. Acad., 41, 82. 1905. *Ibid.*, 42, 201. 1906.

Dumas' figures might be rejected altogether without changing the final mean.

Silver iodide, heated in chlorine, is similarly converted into chloride. This ratio has been repeatedly investigated, first by Berzelius.[1] His figures are subjoined, with the ratio AgCl : AgI :: 100 : x in the last column:

AgI.	*AgCl.*	*Ratio.*
5.000	3.062	163.292
12.212	7.4755	163.360
		Mean, 163.326, ± .023

There are also two early experiments by Dumas,[2] as follows:

AgI.	*AgCl.*	*Ratio.*
3.520	2.149	163.793
7.011	4.281	163.770
		Mean, 163.782, ± .008

The modern work upon this ratio began with an investigation by Ladenburg[3] in 1902, which showed that the previously accepted value for the atomic weight of iodine was at least a tenth of a unit too low. Ladenburg made two series of determinations, with vacuum weights; one preliminary, the other conducted with greater care. His figures are as follows:

Preliminary Series.

AgI.	*AgCl.*	*Ratio.*
31.2558	19.0817	163.800
33.7357	20.5930	163.821
49.88229	30.4525	163.804
47.8830	29.2262	163.836
60.1435	36.7154	163.810
41.3649	25.2448	163.855
50.8916	31.0664	163.816
41.3233	25.2200	163.851
80.8139	49.3181	163.863
89.5071	54.6367	163.822
		Mean, 163.8278, ± .0048

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[1] Ann. Chim. Phys. (2), 40, 430. 1829.

[2] Ann. Chem. Pharm., 113, 28. 1860.

[3] Ber. Deutsch. chem. Ges., 35, 2275. 1902.

Final Series.

AgI.	*AgCl.*	*Ratio.*
62.6658	38.2526	163.821
63.8402	38.9687	163.824
74.7576	45.6324	163.826
		Mean, 163.8237, ± .00103

Ladenburg was followed, in the measurement of this ratio, by Koethner and Aeuer;[1] whose observations are of the highest significance. In a number of preliminary experiments they found that silver iodide, precipitated from solutions of silver nitrate, was liable to contain occlusions of the latter salt; a fact which accounts for the low values for iodine found by Marignac and Stas. Their final determinations of the chloride-iodide ratio are as follows, with vacuum corrections:

AgI.	*AgCl.*	*Ratio.*
24.88066	15.18914	163.8056
10.24699	6.25564	163.8036
12.57020	7.67389	163.8048
25.18868	15.37678	163.8098
9.62006	5.87285	163.8057
12.26770	7.48901	163.8093
22.60660	13.80056	163.8093
20.98601	12.81160	163.8048
22.47667	13.72119	163.8099
		Mean, 163.8070, ± .00057

Two series of measurements of this ratio are due to Baxter,[2] who verified the occlusion of silver nitrate by silver iodide. This source of error he obviated by fusing the iodide in an atmosphere containing iodine. In one series, the silver iodide was first converted into bromide and afterwards into chloride; in the other series the conversion was direct. Baxter's determinations appear in the two following tables:

Bromide Series.

AgI.	*AgCl.*	*Ratio*
13.65457	8.33538	163.815
17.35528	10.59457	163.813
10.27105	6.27006	163.811
8.62870	5.26735	163.815
11.92405	7.27926	163.809
		Mean, 163.8126, ± .00079

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[1] Liebig's Annalen, 337, 123 and 367. 1904. *Ibid.*, 338, 362. See also Ladenburg, *ibid.*, 338, 259. A preliminary paper by Koethner and Aeuer is in Ber., 37, 2536. 1904.

[2] Proc. Amer. Acad., 40, 431. 1904. *Ibid.*, 41, 73. 1905. Journ. Amer. Chem. Soc., 26, 1593, and 27, 879.

Direct Series.

AgI.	*AgCl.*	*Ratio.*
9.26860	5.65787	163.818
6.72061	4.10259	163.814
11.31825	6.90912	163.816
10.07029	6.14754	163.810
13.49229	8.23649	163.811
		Mean, 163.8138, ± .00101

The seven series for the ratio AgCl : AgI, arranged in the order of ascending magnitude, now combine thus:

Berzelius	163.326, ± .023
Dumas	163.782, ± .008
Koethner and Aeuer	163.8070, ± .00057
Baxter, bromide series	163.8126, ± .00079
Baxter, direct	163.8138, ± .00101
Ladenburg, final	163.8237, ± .00103
Ladenburg, preliminary	163.8278, ± .0048
General mean	163.8118, ± .00038

For the ratio AgI : AgBr :: 100 : x there is one set of determinations by Baxter.[1] Silver iodide was converted into bromide by heating in bromine vapor. The data are as follows:

AgI.	*AgBr.*	*Ratio.*
13.65457	10.92091	79.9799
17.35528	13.88062	79.9792
9.70100	7.75896	79.9812
10.27105	8.21484	79.9805
9.85688	7.88351	79.9798
8.62870	6.90106	79.9780
11.92405	9.53704	79.9816
7.56933	6.05389	79.9792
		Mean, 79.9799, ± .00028

THE POTASSIUM CHLORIDE-SILVER RATIOS.

The ratios between silver, potassium chloride and silver chloride have been repeatedly measured. First, let us consider the ratio Ag : KCl :: 100 : x. Marignac[2] dissolved pure silver in nitric acid, and determined the ratio by titration with a solution of potassium chloride. The data are as follows:

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[1] Proc. Amer. Acad., 41, 73. Journ. Amer. Chem. Soc., 27, 878. 1905.

[2] Oeuvres Complètes, 1, 77.

Ag.	*KCl.*	*Ratio.*
4.7238	3.2626	69.067
21.725	15.001	69.050
21.759	15.028	69.066
21.909	15.131	69.063
22.032	15.216	69.063
25.122	17.350	69.063
		Mean, 69.062, ± .0017

Corrected for weighing in air this becomes 69.098, ± .0017.

The work of Stas falls into several series, widely separated in point of time. His earlier experiments[1] upon this ratio may be divided into two sets, as follows: In the first set the silver was slightly impure, but the impurity was of known quantity, and corrections could therefore be applied. In the second series pure silver was employed. The potassium chloride was from several different sources, and in every case was purified with the utmost care. From 10.8 to 32.4 grammes of silver were taken in each experiment, and the weighings were reduced to vacuum. The method of operation was, in brief, as follows: A definite weight of potassium chloride was taken, and the exact quantity of silver necessary, according to Prout's hypothesis, to balance it was also weighed out. The metal, with suitable precautions, was dissolved in nitric acid, and the solution mixed with that of the chloride. After double decomposition the trifling excess of silver remaining in the liquid was determined by titration with a normal solution of potassium chloride.

First series.

69.105
69.104
69.103
69.104
69.102

Mean, 69.1036, ± .0003

Second series.

69.105
69.099
69.107
69.103
69.103
69.105
69.104
69.099

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[1] Oeuvres Complètes, 1, 363, 364.

69.1034
69.104
69.103
69.102
69.104
69.104
69.105
69.103
69.101
69.105
69.103

Mean, 69.1033, ± .0003

In these determinations Stas did not take into account the slight solubility of precipitated silver chloride in the menstrua employed in the experiments. Accordingly, in 1882,[1] he published a new series, in which by two methods he remeasured the ratio, guarding against the indicated error, and finding the following values:

69.1198
69.11965
69.121
69.123

Mean, 69.1209, ± .0003

Corrected for a minute trace of silica contained in the potassium chloride, this mean becomes

69.11903, ± .0003 [2]

Still later, in order to establish the absolute constancy of the ratio in question, Stas made yet another series of determinations,[3] in which he employed potassium chloride prepared from four different sources. One lot of silver was used throughout. The values obtained were as follows:

69.1227
69.1236
69.1234
69.1244
69.1235
69.1228
69.1222
69.1211
69.1219
69.1249
69.1238
69.1225
69.1211

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[1] Oeuvres Complètes, 1, 762-767, 775-777.

[2] Ann. Chim. Phys. (6), 7, 513. 1886.

[3] Oeuvres Complètes, 3, 516, 539.

A series was also begun in which one sample of potassium chloride was to be balanced against silver from various sources, but only one result is given, namely, 69.1240. This, with the previous series, gives a mean of 69.1230, ±.0002.

The difference between the highest and the lowest of Stas' series corresponds to a difference of 0.021 in the atomic weight of potassium. The rejection of the earlier work might be quite justifiable, but would exert a very slight influence upon our final result.

In 1903, incidentally to their work on cæsium, Richards and Archibald[1] published two analyses of potassium chloride, in which both ratios were determined. That is, the silver chloride was weighed, giving data for the second ratio, AgCl : KCl : : 100 : *x*. The results, with vacuum weights, follow:

KCl.	*AgCl.*	*Ag.*	*Ag ratio.*	*AgCl ratio.*
2.50019	4.80600	3.61747	69.114	52.022
2.50391	4.81325	3.62283	69.115	52.021
		Mean,	69.1145, ± .0003	52.0215, ± .0003

In 1904 Archibald[2] gave an additional series of determinations of these ratios, also with vacuum weights, as follows:

KCl.	*AgCl.*	*Ag.*	*Ag ratio.*	*AgCl ratio.*
2.21586	4.25916	3.20598	69.116	52.026
1.99379	3.83250	2.88479	69.114	52.023
2.89977	5.57396	4.19557	69.115	52.024
4.73606	9.10362	6.85280	69.111	52.024
		Mean,	69.114, ± .0007	52.024, ± .00055

The measurements of these ratios by Richards and Staehler[3] are probably the most conclusive, for every care was taken to detect and avoid constant errors, such as the authors believe were present, despite all precautions, in the work of Stas. The occlusion of silver nitrate by silver chloride is an error of this kind. The figures for the silver ratio are as follows, with vacuum weights:

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[1] Proc. Amer. Acad., 38, 456. 1903.

[2] Trans. Roy. Soc. Canada, 1904, Section III, p. 47.

[3] Publ. Carnegie Inst., Washington, No. 69, p. 7. 1907. An advance publication in Ber. Deutsch. chem. Ges., 39, 3611, contained also the figures of some preliminary experiments, which the authors discard in their final report.

KCl.	*Ag.*	*Ratio.*
3.88074	5.61536	69.109
7.44588	10.77156	69.107
5.00681	7.24514	69.106
5.04833	7.30515	69.107
8.19225	11.85412	69.109
4.99795	7.23230	69.106
5.16262	7.47042	69.107
		Mean, 69.1073, ± .00032

For the silver chloride ratio Richards and Staehler give the subjoined figures:

KCl.	*AgCl.*	*Ratio.*
4.36825	8.3986	52.012
5.56737	10.7038	52.013
6.41424	12.3323	52.012
3.27215	6.2913	52.011
4.83028	9.2870	52.011
		Mean, 52.0118, ± .00025

Several earlier measurements of the silver chloride ratio remain to be mentioned. First, Berzelius[1] found that 100 parts of KCl were equivalent to 192.4 of AgCl, a value which, corrected for weighing in air, becomes 192.32. Hence AgCl : KCl : : 100 : 51.997.

In 1842 Marignac[2] published two determinations, as follows:

KCl.	*AgCl.*	*Ratio.*
17.034	32.761	51.995
14.427	27.749	51.991
		Mean, 51.993,

Four years later, Marignac[3] published a second series of determinations. The new figures are:

KCl.	*AgCl.*	*Ratio.*
15.028	28.910	51.982
15.131	29.102	51.993
15.216	29.271	51.983
		Mean, 51.986,

---

[1] Poggend. Annal., 8, 1. 1826.

[2] Oeuvres Complètes, 1, 60.

[3] Oeuvres Complètes, 1, 78. The figures of the first set are republished in this series, but not repeated here. Marignac treats the five experiments as one series.

The mean of both series, taken as one, is 51.989; which, corrected to a vacuum standard, becomes 52.011, ± .0018.

In three determinations Maumené[1] obtained the following figures:

KCl.	*AgCl.*	*Ratio.*
10.700	20.627	51.874
10.5195	20.273	51.892
8.587	16.556	51.868
	Mean,	51.878, ± .0049

These figures seem to represent weights in air, but they are hardly worth correcting.

Two other analyses, with vacuum reductions, were made by Thiel[2] incidentally to his research upon indium:

KCl.	*AgCl.*	*Ratio.*
7.4314	14.2903	52.003
7.4321	14.2939	51.995
	Mean,	51.999, ± .0027

Assembling the data for both ratios, we now have the following combinations:

Ratio Ag : KCl :: 100 : x.

Marignac	69.098, ± .0017
Stas, first	69.1036, ± .0003
Stas, second	69.1033, ± .0003
Stas, third	69.1190, ± .0003
Stas, fourth	69.1230, ± .0002
Richards and Archibald	69.1145, ± .0003
Archibald	69.114, ± .0007
Richards and Staehler	69.1073, ± .00032
General mean	69.1138, ± .00011

Ratio AgCl : KCl :: 100 : x.

Berzelius	51.997, ± .0049
Marignac	52.011, ± .0018
Maumené	51.878, ± .0049
Richards and Archibald	52.0215, ± .0003
Thiel	51.999, ± .0027
Archibald	52.024, ± .00055
Richards and Staehler	52.0118, ± .00025
General mean	52.0163, ± .00018

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[1] Ann. Chim. Phys. (3), 18, 41. 1846.

[2] Zeitsch. anorg. Chem., 40, 313. 1904.

In the last combination the single experiment by Berzelius is given equal weight with Maumené's series. Both general means differ from Richards and Staehler's averages by less than one part in 10,500, or 0.01 per cent.

POTASSIUM BROMIDE AND IODIDE RATIOS.

The ratio between silver and potassium bromide was first accurately determined by Marignac.[1] I give, with his weighings, the quantity of KBr proportional to 100 parts of Ag:

Ag.	*KBr.*	*Ratio.*
2.131	2.351	110.324
2.559	2.823	110.316
2.447	2.700	110.339
3.025	3.336	110.283
3.946	4.353	110.314
11.569	12.763	110.321
20.120	22.191	110.293
		Mean, 110.313, ± .005

Corrected to a vacuum this becomes 110.343, ±.005.

Stas,[2] working in essentially the same manner as when he compared potassium chloride and silver, and with bromide from several distinct sources, found the following values for this ratio:

110.361
110.360
110.360
110.342
110.346
110.338
110.360
110.336
110.344
110.332
110.343
110.357
110.334
110.335

Mean, 110.3463, ± .0020

In his paper on the atomic weight of nitrogen, Dean [3] gives three measurements of the Ag : KBr ratio, but with a bromide which was sup-

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[1] Oeuvres Complètes, 1, 82. Four preliminary analyses are discarded.

[2] Oeuvres Complètes, 1, 747.

[3] Journ. Chem. Soc., 77, 177. 1900.

posed to be not quite pure. His results, however, are so close to later determinations that they are worth citing:

Ag.	*KBr.*	*Ratio.*
8.52439	9.40336	110.311
7.83113	8.63900	110.316
8.92432	9.84450	110.312

Mean, 110.313, ± .0010

The recent measurements of this ratio by Richards and Mueller[1] differ considerably from the concordant results of Stas and Marignac. The modern work was probably based upon purer materials, especially in the case of the silver employed. For details upon this side of the discussion the original memoirs must be consulted. The figures, with vacuum weights, given by Richards and Mueller, are as follows:

KBr.	*Ag.*	*Ratio.*
4.33730	3.93164	110.318
4.18763	3.79587	110.320
4.15849	3.76943	110.321
3.67867	3.33450	110.321
3.60484	3.26776	110.315
4.78120	4.33387	110.322
5.67997	5.14860	110.321
6.41587	5.81571	110.320
2.88134	2.61184	110.318
3.64383	2.30309	110.316
3.12757	2.83504	110.318

Mean, 110.319, ± .0004

This combines with the former determinations thus:

Marignac	110.343, ± .0050
Stas	110.3463, ± .0020
Dean	110.313, ± .0010
Richards and Mueller	110.3190, ± .0004
General mean	110.3193, ± .00033

Richards and Mueller also determined the second ratio, AgBr : KBr :: 100 : x. Their figures are—

KBr.	*AgBr.*	*Ratio.*
2.19027	3.45617	63.3728
4.19705	6.62285	63.3723
2.06723	3.26206	63.3719
2.58494	4.07889	63.3736

Mean, 63.3727, ± .0003

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[1] Publ. Carnegie Inst., Washington, No. 69, p. 27. 1907.

When applied to the determination of the atomic weight of potassium, the Richards and Mueller ratios yield almost absolutely identical results, which also coincide with the figures obtained by Richards and Staehler with the chloride. This agreement is strong evidence in favor of the new determinations.

The ratio between silver and potassium iodide seems to have been measured only by Marignac,[1] but without remarkable accuracy. The figures are as follows:

Ag.	*KI.*	*Ratio.*
1.616	2.483	153.651
2.503	3.846	153.665
3.427	5.268	153.720
2.141	3.290	153.667
10.821	16.642	153.794
		Mean, 153.6994, ± .0178

Corrected to a vacuum by Marignac, this becomes 153.800.

THE SODIUM HALIDE-SILVER RATIOS.

The ratio between silver and sodium chloride has been fixed by several investigators. Pelouze[2] dissolved a weighed quantity of silver in nitric acid, and then titrated with sodium chloride. Equivalent to 100 parts of silver he found of chloride:

54.158
54.125
54.139

Mean, 54.141, ± .0063

By Dumas[3] we have seven experiments, with results as follows:

NaCl.	*Ag.*	*Ratio.*
2.0535	3.788	54.211
2.169	4.0095	54.097
4.3554	8.0425	54.155
6.509	12.0140	54.178
6.413	11.8375	54.175
2.1746	4.012	54.202
5.113	9.434	54.187
		Mean, 54.172, ± .0096

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[1] Oeuvres Complètes, 1, 86.

[2] Compt. Rend., 20, 1047. 1845.

[3] Ann. Chem. Pharm., 113, 31. 1860.

Stas,[1] applying the method used in establishing the similar ratio for potassium chloride, and working with salt from six different sources, found of sodium chloride equivalent to 100 parts of silver:

54.2093
54.2088
54.2070
54.2070
54.2070
54.2060
54.2076
54.2081
54.2083
54.2089

Mean, 54.2078, ± .0002

As in the case of the corresponding ratio for potassium chloride, these data needed to be checked by others which took into account the solubility of silver chloride. Such data are given in Stas' paper of 1882,[2] and four results are as follows:

54.2065
54.20676
54.2091
54.2054

Mean, 54.20694, ± .00045

Corrected for a trace of silica in the sodium chloride, this mean becomes 54.2047, ± .00045.

The elaborate research of Richards and Wells[3] upon this ratio, gave a lower value than that found by Stas. According to Richards and Wells, the silver used by Stas probably contained occluded oxygen, and his silver chloride carried down occlusions of sodium salts. The new data, with vacuum weights as usual, are as follows, the last two experiments forming a small supplementary series:

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[1] Oeuvres Complètes, 1, 370.

[2] Oeuvres Complètes, 1, 768, 773.

[3] Publ. Carnegie Inst., Washington, No. 28, pp. 52, 56. 1905.

NaCl.	*Ag.*	*Ratio.*
3.96051	7.30896	54.187
2.32651	4.29355	54.186
5.36802	9.90699	54.184
4.00548	7.39210	54.186
4.69304	8.66101	54.186
3.27189	6.03842	54.185
5.08685	9.38795	54.185
3.66793	9.76952	54.183
5.48890	10.12993	54.185
3.55943	6.56909	54.185
3.38684	6.25046	54.185
4.68529	8.64634	54.188

Mean, 54.1854, ± .00025

The five series of determinations combine thus:

Pelouze	54.141, ± .0063
Dumas	54.172, ± .0096
Stas, earlier	54.2078, ± .0002
Stas, later	54.2047, ± .00045
Richards and Wells..........	54.1854, ± .00025
General mean	54.1995, ± .00015

In this combination the work of Pelouze and Dumas counts for almost nothing. Stas' determinations carry high weight, and it is not easy to understand how their supposed systematic errors could have been so uniform in magnitude. Such errors should vary from experiment to experiment, and so tend to increase the "probable error" of the mean.

In their research upon the atomic weight of boron, Ramsay and Aston[1] converted borax into sodium chloride. In the latter the chlorine was afterwards estimated gravimetrically by weighing as silver chloride on a Gooch filter. Hence the ratio, AgCl : NaCl : : 100 : *x*, as follows:

NaCl.	*AgCl.*	*Ratio.*
3.0761	7.5259	40.874
2.7700	6.7794	40.859
2.8930	7.0804	40.859
2.7360	6.6960	40.860
1.9187	4.6931	40.863

Mean, 40.867, ± .0033

The same ratio was also measured, much more exactly, by Richards and Wells. The occlusion of sodium salts by the silver chloride was especially considered and guarded against. The figures obtained are as follows:

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[1] Chem. News, 66, 92. 1892.

NaCl.	*AgCl.*	*Ratio.*
3.27527	8.03143	40.781
5.56875	13.65609	41.779
4.18052	10.25176	40.779
4.54319	11.14095	40.779
1.97447	4.84196	40.778
3.97442	9.74547	40.782
6.69495	16.41725	40.780
2.88692	7.07955	40.778
5.56991	13.65833	40.780
5.85900	14.36693	40.781
		Mean, 40.7797, ± .00028

This mean, combined with that of Ramsay and Aston, gives a general mean of 40.7803, ±.00028, which falls within the limits of variation of Richards and Wells' series.

For the ratio between silver and sodium bromide we have one set of measurements by Stas.[1] The bromide was prepared by saturating Na_2CO_3 with HBr. The NaBr proportional to 100 parts of silver was—

95.4420
95.4383
95.4426
95.4392

Mean, 95.4405, ± .0007

The second bromide ratio, AgBr : NaBr, is represented by two experiments, made by Richards and Wells[2] in a research upon the transition temperature of sodium bromide. With vacuum weights the figures are—

NaBr.	*AgBr.*	*Ratio.*
5.49797	10.03253	54.8014
3.64559	6.65248	54.8005
		Mean, 54.8010, ± .0005

THE AMMONIUM HALIDE-SILVER RATIOS.

Ratios connecting silver with the chloride and bromide of ammonium have been repeatedly determined, by methods essentially the same as those adopted in the similar analyses of potassium and sodium halides.

For the ammonium chloride equivalent to 100 parts of silver, Pelouze[3] found:

49.556
49.517

Mean, 49.5365, ± .013

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[1] Oeuvres Complètes, 1, 796.

[2] Proc. Amer. Acad., 41, 443.

[3] Compt. Rend., 20, 1047. 1845.

Marignac[1] obtained the following results. The usual ratio for 100 parts of silver is given also:

Ag.	*NH_4Cl.*	*Ratio.*
8.063	3.992	49.510
9.402	4.656	49.521
10.339	5.120	49.521
12.497	6.191	49.540
11.337	5.617	49.546
11.307	5.595	49.483
4.326	2.143	49.538
		Mean, 49.523, ± .0055

Corrected to a vacuum this becomes 49.556, ±.0055.

Stas[2] made three series of determinations of this important ratio, at different times and under varying conditions. All of his weights, as usual, were reduced to a vacuum standard. The third series, published in 1882, was undertaken in order to correct for the solubility of silver chloride, which was not sufficiently guarded against in the earlier work. The values found for the ratio $Ag : NH_4Cl :: 100 : x$ are as follows:

First series.	*Second series.*[3]	*Third series.*
49.568	49.598	49.599
49.581	49.597	49.600
49.572	49.593	49.597
49.577	49.597	49.5987
49.595	49.5974	49.597
49.588	49.602	
49.591	49.597	Mean, 49.598, ± .0005
49.593	49.598	
49.600	49.592	
49.599		
49.598	Mean, 49.597, ± .0006	
49.597		
49.591		
49.592		
Mean, 49.589, ± .0018		

The first four determinations in the first series are rejected by Stas as unsatisfactory.

By Scott[4] two determinations of this ratio are available, with vacuum weights, as follows:

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[1] Oeuvres Complètes, 1, 89.

[2] Oeuvres Complètes, 1, 378, 478, 781.

[3] Excluding three determinations repeated from the first series.

[4] Journ. Chem. Soc., 79, 147. 1901.

Ag.	*NH_4Cl.*	*Ratio.*
9.64484	4.78257	49.587
11.12810	5.51744	49.581
		Mean, 49.584, ± .0020

Scott also made one determination of the ratio $AgCl:NH_4Cl$. 4.7850 grammes NH_4Cl balance 12.82048 of Ag. The ratio, therefore, is 100 : 37.3234.

The several values for the ratios $Ag : NH_4Cl :: 100 : x$ now combine as follows:

Pelouze	49.5365,	± .0130
Marignac	49.556,	± .0055
Stas, first series	49.589,	± .0018
Stas, second series	49.597,	± .0006
Stas, third series	49.594,	± .0005
Scott	49.584,	± .0020
General mean	49.5965,	± .00038

For the ratio between ammonium chloride and silver chloride there is a series of nine determinations by Richards, Koethner and Tiede.[1] The values found are as follows:

NH_4Cl.	*AgCl.*	*Ratio.*
2.02087	5.41469	37.3220
2.23894	5.99903	37.3217
1.55284	4.16076	37.3211
1.36579	3.65959	37.3209
1.61939	4.33914	37.3205
1.93795	5.19219	37.3243
2.89057	7.74498	37.3219
1.31405	3.52082	37.3223
1.82091	4.87921	37.3198
		Mean, 37.3217

---

[1] Journ. Amer. Chem. Soc., 31, 6. 1909. The actual analyses were made by Tiede. Professor Richards has kindly furnished me with the three following determinations of this ratio, which were made by Tiede, but not used in the published memoir:

NH_4Cl.	*AgCl.*	*Ratio.*
1.10489	2.9607	37.3185
.95997	2.5723	37.3195
.75331	2.0186	37.3184
		Mean, 37.3188, ± .00025

These figures were received too late for use in the systematic discussion. If included in the main series they would tend to lower the atomic weight of nitrogen by 0.001.

Scott's single determination of this ratio, 37.3234, falls within the limits of variation of the foregoing series. Including it in the computation, the ratio becomes

$$\mathrm{AgCl : NH_4Cl :: 100 : 37.3218}, \pm .0003$$

All weights were reduced to a vacuum basis.

For the ratio $\mathrm{Ag : NH_4Br :: 100} : x$ there are determinations by Stas and Scott.

Stas[1] obtained the following values for x:

90.831
90.831
90.8297
90.823
90.8317
90.8311
90.8302

Mean, 90.8297, ± .0008

Scott's[2] data, rejecting three preliminary experiments in which the ammonium bromide was distinctly acid, are as follows, with vacuum weights:

Ag.	*NH_4Br.*	*Ratio.*
4.92273	4.46957	90.795
4.20661	4.63303	90.796
4.23664	4.66644	90.790
4.31464	4.75175	90.801
6.19233	6.82047	90.790
8.77664	9.66708	90.789
10.47233	11.53416	90.794
4.91997	5.41834	90.802
5.00442	5.51164	90.797
5.17914	5.70390	90.800
4.84099	5.33177	90.795
5.10677	5.62515	90.784
		Mean, 90.7944, ± .0011

Combining this with the series by Stas, the general mean is 90.8175, ±.00065.

THE SILVER NITRATE RATIOS.

The quantity of silver nitrate which can be formed from a known weight of metallic silver has been determined by several investigators.

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[1] Oeuvres Complètes, 1, 801.

[2] Journ. Chem. Soc., 79, 147. 1901. For a criticism by Richards, see Proc. Amer. Phil. Soc., 43, 116. 1904.

Penny[1] dissolved silver in nitric acid in a flask, evaporated to dryness without transfer, and weighed. One hundred parts of silver thus gave of nitrate:

157.430
157.437
157.458
157.440
157.430
157.455

Mean, 157.4417, ± .0033

Marignac's[2] results were as follows. In the third column they are reduced to the common standard of 100 parts of silver:

68.987 grm. Ag gave	108.608 grm. $AgNO_3$.	157.433
57.844 "	91.047 "	157.401
66.436 "	104.592 "	157.433
70.340 "	110.718 "	157.404
200.000 "	314.894 "	157.447
	Mean,	157.4236, ± .0061

Corrected for weighing in air this becomes 157.449.

Stas,[3] employing from 77 to 405 grammes of silver in each experiment, made two different series of determinations at two different times. The silver was dissolved with all the usual precautions against loss and against impurity, and the resulting nitrate was weighed, first after long drying without fusion, just below its melting point; and again, fused. Between the fused and the unfused salt there was in every case a slight difference in weight, the latter giving a maximum and the former a minimum value.

In Stas' first series there are eight experiments; but the seventh he himself rejects as inexact. The values obtained for the nitrate from 100 parts of silver are given below in two columns, representing the two conditions in which the salt was weighed. The general mean given at the end I have deduced from the means of the two columns considered separately:

---

[1] Phil. Trans., 1839.

[2] Oeuvres Complètes, 1, 88. From the sum of the weights, corrected to a vacuum, Marignac computes the ratio 1 : 157.455.

[3] Oeuvres Complètes, 1, 346, 724.

Unfused.	*Fused.*
157.492	157.474
157.510	157.481
157.485	157.477
157.476	157.471
157.478	157.470
157.471	157.463
157.488	157.469
Mean, 157.4857	Mean, 157.472

General mean, 157.474, ± .0014

In the later series there are but two experiments, as follows:

Unfused.	*Fused.*
157.4964	157.488
157.4940	157.480
Mean, 157.4952	Mean, 157.484

General mean, 157.486, ± .0003

The reverse ratio, namely, the amount of silver obtainable from a weighed quantity of nitrate, has been determined electrolytically by Hardin.[1] The data obtained, however, are reducible to the same form as in the preceding series, and all are properly combinable together. Pure silver was dissolved in pure aqueous nitric acid, and the crystalline salt thus formed was dried, fused and used for the determinations. The silver nitrate, mixed with an excess of pure potassium cyanide solution, was electrolyzed in a platinum dish. The results obtained, reduced to vacuum weights, were as follows:

$AgNO_3$.	*Ag.*	*Ratio.*
.31202	.19812	157.490
.47832	.30370	157.498
.56742	.36030	157.485
.57728	.36655	157.490
.69409	.44075	157.479
.86367	.54843	157.479
.86811	.55130	157.466
.93716	.59508	157.485
1.06170	.67412	157.494
1.19849	.76104	157.477
		Mean, 157.484, ± .0020

The most thorough and recent investigation of this ratio is that by Richards and Forbes.[2] They effected the synthesis of the nitrate from the purest silver, the nitrate having been fused and tested for such

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[1] Journ. Amer. Chem. Soc., 18, 995. 1896.

[2] Publ. Carnegie Inst., Washington, No. 69, p. 47. 1907.

impurities as dissolved air, retained water and ammonia, and nitric or nitrous acids. Only two of these were found, and in minute traces, between 0.001 and 0.002 per cent. in all. The final data, with vacuum weights, are as follows:

Ag.	*$AgNO_3$.*	*Ratio.*
6.14837	9.68249	157.481
4.60825	7.25706	157.480
4.97925	7.84131	157.480
9.07101	14.28503	157.480
9.13702	14.38903	157.481
9.01782	14.20123	157.480

Mean, 157.480, ± .0001

The impurities above mentioned may lower this value to 157.478, their maximum effect. The authors accept the intermediate figure, 157.479.

Combining the several determinations, we have—

Penny	157.4417,	± .0033
Marignac	157.449,	± .0061
Stas, earlier	157.474,	± .0014
Stas, later	157.486,	± .0003
Hardin	157.484,	± .0020
Richards and Forbes	157.479,	± .0001
General mean	157.479,	± .000095

For the direct ratio between silver nitrate and silver chloride there are two series of estimations. A weighed quantity of nitrate is easily converted into chloride, and the weight of the latter ascertained. In two experiments Turner[1] found of chloride from 100 parts of nitrate:

84.357
84.389

Mean, 84.373, ± .011

Penny,[2] in five determinations, found the following percentages:

84.370
84.388
84.377
84.367
84.370

Mean, 84.3744, ± .0025

The general mean from both series is 84.3743, ± .0025.

---

[1] Phil. Trans., 1833. 537.

[2] Phil. Trans., 1839.

The ratio directly connecting silver nitrate with ammonium chloride has been determined only by Stas.[1] The usual method of working was followed, namely, nearly equivalent quantities of the two salts were weighed out, the solutions mixed, and the slight excess of one estimated by titration. In four experiments 100 parts of silver nitrate were found equivalent to chloride of ammonium, as follows:

31.489
31.490
31.487
31.486

Mean, 31.488, ± .0006

The similar ratio between potassium chloride and silver nitrate has been determined by both Marignac and Stas.

Marignac[2] gives the following weights. I add the quantity of KCl proportional to 100 parts of $AgNO_3$:

KCl.	*$AgNO_3$.*	*Ratio.*
1.849	4.218	43.836
2.473	5.640	43.848
3.317	7.565	43.847
2.926	6.670	43.868
6.191	14.110	43.877
4.351	9.918	43.870
		Mean, 43.858, ± .0044

Corrected to a vacuum this becomes 43.874, ±.0044.

Stas'[3] results are given in three series, representing silver nitrate from three different sources. In the third series the nitrate was weighed in vacuo, while for the other series this correction was applied in the usual way. For the KCl equivalent to 100 parts of $AgNO_3$ Stas found:

First Series.

43.878
43.875
43.875
43.874

Mean, 43.8755, ± .0005

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[1] Oeuvres Complètes, 1, 382.
[2] Oeuvres Complètes, 1, 88.
[3] Oeuvres Complètes, 1, 381.

Second Series.

43.864
43.869
43.876

Mean, 43.8697, ± .0023

Third Series.

43.884
43.878
43.885

Mean, 43.8823, ± .0015

Combining all four series we have—

Marignac	43.874, ± .0044
Stas, first series	43.8755, ± .0005
Stas, second series	43.8697, ± .0023
Stas, third series	43.8823, ± .0015
General mean	43.8759, ± .00046

POTASSIUM AND SODIUM NITRATE RATIOS.

Ratios connecting the alkaline nitrates, chlorates and chlorides have been determined by Penny, Stas and Hibbs.

The general method of working upon these ratios is due to Penny.[1] Applied to the ratio between the chloride and nitrate of potassium, it is as follows: A weighed quantity of the chloride is introduced into a flask which is placed upon its side and connected with a receiver. An excess of pure nitric acid is added, and the transformation is gradually brought about by the aid of heat. Then, upon evaporating to dryness over a sand bath, the nitrate is brought into weighable form. The liquid in the receiver is also evaporated, and the trace of solid matter which had been mechanically carried over is recovered and also taken into account. In another series of experiments the nitrate was taken, and by pure hydrochloric acid converted into chloride, the process being the same. In the following columns of figures I have reduced both series to one standard, namely, so as to express the number of parts of nitrate corresponding to 100 of chloride:

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[1] Phil. Trans., 1839.

First Series.—KCl treated with HNO_3.

135.639
135.637
135.640
135.635
135.630
135.640
135.630

Mean, 135.636, ± .0011

Second Series.—KNO_3 treated with HCl.

135.628
135.635
135.630
135.641
135.630
135.635
135.630

Mean, 135.633, ± .0011

Stas,[1] who converted potassium chloride into nitrate, gives the following figures:

KCl.	*KNO_3.*	*Ratio.*
50.7165	68.7938	135.643
80.2610	108.8665	135.638
72.1022	99.8050	135.647
50.2175	68.1200	135.649
48.9274	63.3675	135.645
69.8836	94.7900	135.640
14.2578	19.3415	135.655
		Mean, 135.6453, ± .0014

These figures by Stas represent weighings in the air. Reduced to a vacuum standard, this mean becomes 135.6423.

The determinations made by Hibbs[2] differ slightly in method from those of Penny and Stas. He converted the nitrate into the chloride by heating in a stream of gaseous hydrochloric acid. His results were as follows, vacuum weights being given:

---

[1] Oeuvres Complètes, 1, 683.

[2] Doctoral dissertation, University of Pennsylvania, 1896. Work done under the direction of Professor Edgar F. Smith.

Weight KNO_3.	*Weight KCl.*	*Ratio.*
.11090	.08177	135.624
.14871	.10965	135.622
.21067	.15533	135.627
.23360	.17225	135.620
.24284	.17903	135.642
		Mean, 135.627, ± .0026

Now, combining, we have:

Penny, 1st series	135.636,	± .0011
Penny, 2d series	135.633,	± .0011
Stas	135.6423,	± .0014
Hibbs	135.627,	± .0026
General mean	135.636,	± .0007

By the same general process Penny[1] determined how much potassium nitrate could be formed from 100 parts of chlorate. He found as follows:

82.505
82.497
82.498
82.500

Mean, 82.500, ± .0012

For 100 parts of sodium chlorate he found of nitrate:

79.875
79.882
79.890

Mean, 79.8823, ± .0029

For the ratio between the chloride and nitrate of sodium Penny made two sets of estimations, as in the case of potassium salts. The subjoined figures give the amount of nitrate equivalent to 100 parts of chloride:

First Series.—NaCl treated with HNO_3.

145.415
145.408
145.420
145.424
145.410
145.418
145.420

Mean, 145.4164, ± .0015

---

[1] Phil. Trans., 1839.

Second Series.—$NaNO_3$ treated with HCl.

145.419
145.391
145.412
145.415
145.412
145.412

Mean, 145.410, ± .0026

The sodium chloride to nitrate series of Stas[1] is as follows:

NaCl.	*$NaNO_3$.*	*Ratio.*
120.0110	174.5590	145.453
32.4837	47.2550	145.468
68.1295	99.1045	145.465
47.9226	69.7075	145.459
14.5380	21.1465	145.443
		Mean, 145.4576, ± .0030

Reduced to a vacuum basis this becomes 145.4526.

Hibbs'[2] data, obtained by the method employed in the case of the potassium compounds, are as follows, vacuum weights being stated:

Weight $NaNO_3$.	*Weight NaCl.*	*Ratio.*
.01550	.01066	145.403
.20976	.14426	145.404
.26229	.18038	145.410
.66645	.45829	145.429
.93718	.64456	145.399
		Mean, 145.407, ± .0026

Combining, we have as follows:

Penny, 1st series	145.4164, ± .0015
Penny, 2d series	145.410, ± .0026
Stas	145.4526, ± .0030
Hibbs	145.407, ± .0026
General mean	145.418, ± .0012

One other potassium nitrate ratio has been measured by Richards and Archibald.[3] On heating the nitrate with silica, potassium silicate is formed, and the equivalent of N_2O_5 is volatilized. The vacuum weights are given in the following table, together with the ratio $N_2O_5 : K_2O :: 100 : x$:

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[1] Oeuvres Complètes, 1, 688.
[2] Dissertation, University of Pennsylvania, 1896.
[3] Proc. Amer. Acad., 38, 462. 1903.

KNO_3 taken.	N_2O_5 lost.	Ratio.
1.81034	0.96692	87.227
3.14564	1.68005	87.235
2.55598	1.36512	87.235
		Mean, 87.232, ± .0017

THE SILVER CARBON RATIOS.

The determination of atomic weights by the analysis of organic silver salts has been repeatedly attempted. The measurements of this class may, for present purposes, be conveniently grouped together.

In 1840 Redtenbacher and Liebig[1] sought to determine the atomic weight of carbon, that of silver being assumed as known, by analyses of the acetate, tartrate, racemate and malate of silver. There were five determinations with each compound, the salt being ignited, and the residual silver weighed. From one to nine grammes of material were used in each experiment.

In the acetate the following percentages of silver were found:

64.615
64.624
64.623
64.614
64.610

Mean, 64.6172, ± .0018

After applying corrections for weighing in air, this mean becomes 64.6006.

In the tartrate the silver was as follows:

59.297
59.299
59.287
59.293
59.293

Mean, 59.2938, ± .0014
Or, reduced to a vacuum, 59.2806

In the racemate we have:

59.290
59.292
59.287
59.283
59.284

Mean, 59.2872, ± .0012
Or, corrected, 59.2758

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[1] Ann. Chem. Pharm., 38, 113. 1841. Mem. Chem. Soc., 1, 9. Phil. Mag. (3), 19, 210.

And from the malate:

61.996
61.972
62.015
62.059
62.011

Mean, 62.0106, ± .0096
Or, corrected, 62.0016

These results are by no means unimpeachable. They involve two possible sources of constant error, namely, impurity of material and the volatility of the silver. These objections have both been raised by Stas, who found that the silver tartrate, prepared as Redtenbacher and Liebig prepared it, always carried traces of the nitrate, and that he, by the ignition of that salt, could not get results at all agreeing with theirs. In the case of the acetate a similar impurity would lower the percentage of silver, and thus both sources of error would reinforce each other and make the atomic weight of carbon apparently too high. With the three other salts the two sources of error act in opposite directions, although the volatility of the silver is probably far greater in its influence than the impurity. Even if we had no other data relating to the atomic weight of carbon, it would be clear from these facts that the results obtained by Redtenbacher and Liebig must be decidedly in excess of the true figure.

Strecker,[1] however, discussed the data given by Redtenbacher and Liebig by the method of least squares, using the Berzelian scale, and assuming H = 12.51. Thus treated, they gave C = 75.415, and Ag = 1348.79; or, with O = 16, C = 12.066 and Ag = 107.903. These values of course would change somewhat upon adoption of the modern ratio between O and H.

Observations upon silver acetate, like those of Redtenbacher and Liebig, were also made by Marignac.[2] The salt was prepared by dissolving silver carbonate in acetic acid, and repeatedly recrystallizing. Two experiments gave as follows:

3.3359 grm. acetate gave	2.1561 Ag.	64.633 per cent.
3.0527 "	1.9727 "	64.621 "
	Mean,	64.627, ± .0040

Reduced to a vacuum, this becomes 64.609.

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[1] Ann. Chem. Pharm., 59, 280. 1846.

[2] Ann. Chem. Pharm., 59, 287. 1846. Oeuvres Complètes, 1, 184.

In a second series, conducted with special precautions to avoid mechanical loss by spurting, Marignac found:

24.717 grm. acetate gave	15.983 Ag.	64.665 per cent.
21.202 “	13.709 “	64.661 “
31.734 “	20.521 “	64.666 “

Mean, 64.664, ± .0010

Other experiments, comparable with the preceding series, have more recently been published by Hardin,[1] who sought to redetermine the atomic weight of silver. Silver acetate and silver benzoate, carefully purified, were subjected to electrolysis in a platinum dish, and the percentage of silver so determined. For the acetate, using vacuum weights, he gives the following data, the percentage column being added by myself:

.32470 grm. acetate gave	.20987 Ag.	64.635 per cent.
.40566 “	.26223 “	64.643 “
.52736 “	.34086 “	64.635 “
.60300 “	.38976 “	64.637 “
.67235 “	.43455 “	64.631 “
.72452 “	.46830 “	64.636 “
.78232 “	.50563 “	64.632 “
.79804 “	.51590 “	64.646 “
.92101 “	.59532 “	64.638 “
1.02495 “	.66250 “	64.637 “

Mean, 64.637, ± .0011

Combining this series with those of the earlier investigators we have:

Redtenbacher and Liebig	64.6006, ± .0018
Marignac, 1st series	64.609, ± .0040
Marignac, 2d series	64.664, ± .0010
Hardin	64.637, ± .0011
General mean	64.6434, ± .0007

With silver benzoate, $C_7H_5AgO_2$, Hardin's results are as follows:

.40858 grm. benzoate gave	.19255 Ag.	47.127 per cent.
.46674 “	.21999 “	47.133 “
.48419 “	.22815 “	47.120 “
.62432 “	.29418 “	47.120 “
.66496 “	.31340 “	47.131 “
.75853 “	.35745 “	47.124 “
.76918 “	.36247 “	47.124 “
.81254 “	.38286 “	47.119 “
.95673 “	.45079 “	47.118 “
1.00840 “	.47526 “	47.130 “

Mean, 47.125, ± .0012

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[1] Journ. Amer. Chem. Soc., 18, 990. 1896.

A different method of dealing with organic silver salts was adopted by Maumené,[1] in 1846, for the purpose of establishing by reference to carbon the atomic weight of silver. He effected the combustion of the acetate and the oxalate of silver, and, by weighing both the residual metal and the carbon dioxide formed, he fixed the ratio between these two substances. In the case of the acetate his weighings show that for every gramme of metallic silver the weights of CO_2 were produced which are shown in the third column:

8.083 grm. Ag.	= 6.585 grm. CO_2.	.8147
11.215 "	9.135 "	.8136
14.351 "	11.6935 "	.8148
9.030 "	7.358 "	.8148
20.227 "	16.475 "	.8145
	Mean,	.81448

The oxalate of silver, ignited by itself, decomposes too violently to give good results; and for this reason it was not used by Redtenbacher and Liebig. Maumené, however, found that when the salt was mixed with sand the combustion could be tranquilly effected. The oxalate employed, however, with the exception of the sample represented in the last experiment of the series, contained traces of nitrate, so that these results involve slight errors. For each gramme of silver the appended weights of CO_2 were obtained:

14.299 grm. Ag.	= 5.835 grm. CO_2.	.4081
17.754 "	7.217 "	.4059
11.550 "	4.703 "	.4072
10.771 "	4.387 "	.4073
8.674 "	3.533 "	.4073
11.4355 "	4.658 "	.4073
	Mean,	.40718

Now, one of these salts being formed by a dibasic and the other by a monobasic acid, it is well to reduce both to a common standard. Doing this, we have for the ratio between carbon dioxide and 100 parts of silver the following combination:

From the acetate..............	40.724, ± .0076
From the oxalate..............	40.718, ± .0185
General mean	40.723, ± .0071

That is, $Ag : CO_2 :: 100 : 40.723, \pm .0071$.

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[1] Ann. Chim. Phys. (3), 18, 41. 1846.

The experiments of Dean[1] on silver cyanide, may be conveniently summarized here, although they involve nitrogen as well as carbon. Dean's object was to determine the atomic weight of nitrogen, the values for silver and carbon being supposedly known. The cyanide was dissolved in nitric acid, or, in the last experiment in sulphuric acid, and its content of silver was determined by titration with a standard solution of potassium bromide. The silver equivalent of the latter compound was previously fixed by titration against a definite solution of silver. The weights obtained, corrected to a vacuum, are subjoined, together with a column giving the percentages of silver:

Weight AgCN.	*Weight Ag.*	*Per cent. Ag.*
6.2671	5.0490	80.564
17.60585	14.18496	80.570
17.1049	13.7801	80.561
17.9210	14.43881	80.569
12.11215	9.75875	80.570
14.6672	11.81727	80.569
		Mean, 80.567, ± .0010

Still another pair of ratios, involving bromine, were measured by Scott.[2] Tetræthylammonium bromide, purified with great care, was titrated with silver solutions of known strength. The results obtained, with vacuum weights, were as follows:

$(C_2H_5)_4NBr$.	*Ag.*	*Ratio.*
5.07039	2.60146	194.906
5.26380	2.70142	194.853
7.10662	3.64683	194.876
6.79951	3.48976	194.842
2.72225	1.39695	194.871
6.24530	3.20481	194.873
5.74581	2.94853	194.870
5.21663	2.67699	194.869
		Mean, 194.870, ± .0045

A single experiment with the corresponding tetramethyl compound was also made. 8.64585 grammes of $(CH_3)_4NBr$ are equivalent to 6.05348 of silver. Ratio, 142.824, ± .0123, when the probable error is assumed equal to that of one experiment in the ethyl series. From these figures Scott deduces a value for the atomic weight of carbon much higher than that given by the direct O : C ratio.

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[1] Journ. Chem. Soc., 77, 117. 1900.

[2] Journ. Chem. Soc., 95, 1200. 1909.

In a criticism of Scott's work, Thorpe[1] has pointed out the possibility of errors due to the vacuum reductions; errors discussed long ago by Marignac, and recently, in more detail, by Guye and Zachariades.[2] The substances analyzed were weighed in powder, under which conditions they are liable to condense and occlude air. A probable correction, applied to Scott's weighings, reduced the atomic weight of carbon to 12.008, in harmony with other good determinations. To this criticism Scott[3] published a rejoinder, seeking to show, on the basis of experimental evidence, that the supposed errors do not, in fact, exist. According to Guye and Zachariades, the errors noted by them in the study of 26 compounds may amount to as much as, or even more than, 3 parts in 10,000.

Since silver tartrate and silver racemate are isomeric compounds, their figures may be consolidated into one series. We then have the following ratios in this group, to be discussed in connection with other ratios later:

$AgC_2H_3O_2 : Ag :: 100 : 64.6434, \pm .0007$
$Ag_2C_4H_4O_6 : 2Ag :: 100 : 59.2778, \pm .0009$
$Ag_2C_4H_4O_5 : 2Ag :: 100 : 62.0016, \pm .0096$
$AgC_7H_5O_2 : Ag :: 100 : 47.125, \pm .0012$
$Ag : CO_2 :: 100 : 40.723, \pm .0071$
$AgCN : Ag :: 100 : 80.567, \pm .0010$
$Ag : (C_2H_5)_4NBr :: 100 : 194.870, \pm .0045$
$Ag : (CH_3)_4NBr :: 100 : 142.824, \pm .0123$

THE SULPHUR RATIOS.

The atomic weight of sulphur has been determined by means of several ratios connecting it with silver, chlorine, oxygen, hydrogen, sodium and carbon. Other ratios have also been measured, but they are hardly available here. The earlier results of Berzelius were wholly inaccurate, and his later experiments upon the synthesis of lead sulphate will be used in discussing the atomic weight of lead. Erdmann and Marchand determined the amount of calcium sulphate which could be formed from a known weight of pure Iceland spar; and later they made analyses of cinnabar, in order to fix the value of sulphur by reference to calcium and to mercury. Their results will be applied in this discussion toward ascertaining the atomic weights of the metals just named.

First in order let us take up the composition of silver sulphide, as directly determined by Dumas, Stas and Cooke. Dumas'[4] experiments were made with sulphur which had been thrice distilled and twice crystallized from carbon disulphide. A known weight of silver was heated in a tube in the vapor of the sulphur, the excess of the latter was distilled

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[1] Proc. Chem. Soc., 25, 285.
[2] Compt. Rend., 149, pp. 593 and 1122.
[3] Proc. Chem. Soc., 25, 286.
[4] Ann. Chem. Pharm., 113, 24. 1860.

away in a current of carbon dioxide, and the resulting silver sulphide was weighed.

I subjoin Dumas' weighings, and also the quantity of Ag_2S proportional to 100 parts of Ag, as deduced from them:

Weight Ag.	*Weight S.*	*Ratio.*
9.9393	1.473	114.820
9.962	1.4755	114.811
30.637	4.546	114.838
30.936	4.586	114.824
30.720	4.554	114.824
		Mean, 114.8234, ± .0029

Dumas used from ten to thirty grammes of silver in each experiment. Stas,[1] however, in his work employed much larger quantities. Three of Stas' determinations were made by Dumas' method, while in the other two the sulphur was replaced by pure sulphuretted hydrogen. In all cases the excess of sulphur was expelled by carbon dioxide, purified with scrupulous care. Impurities in the dioxide may cause serious error. The data are as follows, with vacuum weights:

Weight Ag.	*Weight Ag_2S.*	*Ratio.*
59.4225	68.24823	114.854
104.139	119.6078	114.853
191.9094	220.4158	114.854
150.000	172.2765	114.851
249.676	286.061	114.849
		Mean, 114.8522, ± .0007

The experiments made by Professor Cooke[2] with reference to this ratio were only incidental to his elaborate researches upon the atomic weight of antimony. They are interesting, however, for two reasons: they serve to illustrate the volatility of silver, and they represent, not syntheses, but reductions of the sulphide by hydrogen. Cooke gives three series of results. In the first the silver sulphide was long heated to full redness in a current of hydrogen. Highly concordant and at the same time plainly erroneous figures were obtained, the error being eventually traced to the fact that some of the reduced silver, although not heated to its melting point, was actually volatilized and lost. The second series, from reductions at low redness, are decidedly better. In the third series the sulphide was fully reduced below a visible red heat. Rejecting the first series, we have from Cooke's figures in the other two the subjoined quantities of sulphide corresponding to 100 parts of silver:

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[1] Oeuvres Complètes, 1, 349.

[2] Proc. Amer. Acad., 13, 47-52. 1877.

7.5411 grm. Ag_2S.	lost	.9773 grm. S.		Ratio,	114.889
5.0364	“	.6524	“	“	114.882
2.5815	“	.3345	“	“	114.886
2.6130	“	.3387	“	“	114.892
2.5724	“	.3334	“	“	114.891
				Mean,	114.888, ± .0012

1.1357 grm. Ag_2S.	lost	.1465 S.		Ratio,	114.810
1.2936	“	.1670	“	“	114.823
				Mean,	114.8165, ± .0044

Now, combining all four series, we have—

Dumas	114.8234, ± .0029
Stas	114.8522, ± .0007
Cooke's 2d	114.888, ± .0012
Cooke's 3d	114.8165, ± .0044
General mean	114.8581, ± .0006

The percentage of silver in silver sulphate has been determined by Struve and by Stas. Struve[1] reduced the sulphate by heating in a current of hydrogen, and obtained these results:

Ag_2SO_4.	*Ag.*	*Per cent. Ag.*
5.1860	3.5910	69.244
6.0543	4.1922	69.243
8.6465	5.9858	69.228
11.6460	8.0608	69.215
9.1090	6.3045	69.212
9.0669	6.2778	69.239
		Mean, 69.230, ± .004

Stas,[2] working by essentially the same method obtained the following figures, which imply vacuum weights:

Ag_2SO_4.	*Ag.*	*Per cent. Ag.*
72.137	49.919	69.200
60.251	41.692	69.197
81.023	56.071	69.204
33.115	57.523	69.209
55.716	38.5595	69.207
63.922	44.2355	69.202
		Mean, 69.203, ± .0012

Combining this mean with that of Struve, the general mean becomes 69.205, ± .0011.

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[1] Ann. Chem. Pharm., 80, 203. 1851.

[2] Oeuvres Complètes, 1, 410.

The third sulphur ratio to be considered is one of minor importance. When silver chloride is heated in a current of sulphuretted hydrogen the sulphide is formed. This reaction was applied by Berzelius[1] to determining the atomic weight of sulphur. He gives the results of four experiments; but the fourth varies so widely from the others that I have rejected it. I have reason to believe that the variation is due, not to error in experiment, but to error in printing; nevertheless, as I am unable to discover the cause of the mistake, I must exclude the figures from our discussion.

The three available experiments, however, give the following results. The last column contains the ratio of silver sulphide to 100 parts of chloride:

6.6075	grm. AgCl. gave	5.715	grm. Ag_2S.	86.478
9.2523	“	7.98325	“	86.471
10.1775	“	8.80075	“	86.472
				Mean, 86.4737, ± .0015

We have also a single determination of this value by Svanberg and Struve.[2] After converting the chloride into sulphide they dissolved the latter in nitric acid. A trifling residue of chloride, which had been enclosed in sulphide, and so protected against change, was left undissolved. Hence a slight constant error probably affects this whole ratio. The experiment of Svanberg and Struve gave 86.472 per cent. of silver sulphide derived from 100 of chloride. If we assign this figure equal weight with the results of Berzelius, and combine, we get a general mean of 86.4733, ±.0011.

The work done by Richards[3] relative to the atomic weight of sulphur is of a different order from any of the preceding determinations. Sodium carbonate was converted into sodium sulphate, fixing the ratio $Na_2CO_3 : Na_2SO_4 :: 100 : x$. The data are as follows, with vacuum weights:

Na_2CO_3.	*Na_2SO_4.*	*Ratio.*
1.29930	1.74113	134.005
3.18620	4.26790	133.950
1.01750	1.36330	133.985
2.07680	2.78260	133.985
1.22427	1.63994	133.952
1.77953	2.38465	134.005
2.04412	2.73920	134.004
3.06140	4.10220	133.997
		Mean, 133.985, ± .0055

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[1] Berzelius' Lehrbuch, 5 Aufl., 3, 1187.

[2] Journ. prakt. Chem., 44, 320. 1848.

[3] Proc. Amer. Acad., 26, 268. 1891. Incidental to work on the atomic weight of copper.

Still another method for fixing the atomic weight of sulphur was adopted by Richards and Jones.[1] Silver sulphate was converted into chloride by heating in a current of pure, dry hydrochloric acid gas. The data obtained, with vacuum weights, were as follows:

Ag_2SO_4.	$AgCl$.	*Per cent.* $AgCl$.
5.21962	4.79859	91.934
5.27924	4.85330	91.932
5.08853	4.67810	91.934
5.36381	4.93118	91.934
5.16313	4.74668	91.934
5.08383	4.67374	91.933
5.13372	4.71946	91.931
5.16148	4.74490	91.929
5.19919	4.77992	91.936
5.37436	4.94088	91.934

Mean. 91.933, ± .0004

In recent years attempts have been made to deduce the atomic weight of sulphur from the density of sulphur dioxide, for which there are several modern determinations. Leduc,[2] in a series of measurements, found the density to range between 2.2638 and 2.2641; in mean, 2.2639. If we take these three values for the entire series the probable error of the mean becomes ±.000067. For oxygen Leduc's density figures give 1.10514, ±.0000321. Hence the crude density ratio $O_2 : SO_2 :: 32 : 65.553$, ±.0020. From these figures, with the aid of the compressibilities and critical constants of the gases, Leduc[3] determines $SO_2 = 64.056$. From the density of H_2S he finds a molecular weight of 34.071. Hence $S = 32.056$. By the method of limiting densities, D. Berthelot,[4] from Leduc's figures, finds $S = 32.050$.

Jaquerod and Pintza[5] give for the weight of a normal litre of SO_2, 2.92664 grammes. For the corresponding volume of oxygen their weight is 1.4292 grammes. Hence the crude molecular ratio 32 : 65.528. Since individual determinations are not given, the probable error of this ratio cannot be calculated, and I shall assign it equal weight with Leduc's determinations. Jaquerod and Pintza also measured the density of SO_2 at pressures lower than the normal, namely, at 570 and 380 mm. Then extrapolating to zero pressure they deduce $SO_2 = 64.01$, and $S = 32.01$.

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[1] Publ. Carnegie Inst., Washington, No. 69, p. 69. 1907. Richards and Jones give a thorough criticism of the previous work on sulphur.

[2] Compt. Rend., 117, 219. 1893.

[3] Ann. Chim. Phys. (7), 15, 94. 1898. Leduc here puts the density of O = 1.1052. See also *ante*, p. 33.

[4] Journ. Physique (3), 8, 263. 1899.

[5] Compt. Rend., 139, 129. 1904.

Jaquerod and Scheuer,[1] from the same density figures, but with measurements of compressibility, found S = 32.036.

The density determinations by Baume[2] are much more elaborate. Two series were made, in globes of different capacity, and at pressures varying slightly from the normal. His crude figures for the weight of a litre of sulphur dioxide are as follows:

Series I.	*Series II.*
2.92836	2.92662
2.92592	2.92718
2.92683	2.92632
2.92500	2.92711
	2.92623

Mean of both series as one, 2.92667, ± .00030. As corrected by Baume the normal litre of SO_2 weighs 2.92661 grammes. Morley's value for the normal litre of oxygen is 1.42896, ± .000028 grammes. Hence the ratio $O_2 : SO_2 :: 32 : 65.538$, ± .0067. This combines with the previous series thus:

Leduc	65.553, ± .0020
Jaquerod and Pintza	65.528, ± .0020
Baume	65.538, ± .0067
General mean	65.540, ± .0014

Guye,[3] in his recalculation of the density ratio for SO_2, assigns to the weight of the normal litre of oxygen the value 1.4290, and to SO_2 the value 2.9266. Hence the crude ratio is 65.536, which is close to Baume's figure and also near the general mean as given above. In reducing this by means of the critical constants he assumes $a_0 = 0.02644$, and $b_0 = 0.00255$. Baume, on the other hand, finds $a_0 = 0.02837$, and $b_0 = 0.00267$. The formula for reduction, as employed in relation to the carbon and nitrogen gases, is

$$\frac{22.412L}{(1 + a_0)(1 - b_0)}$$

Hence, using Guye's value for L, which is sensibly identical with that of Baume, we have—

By Guye's critical data..........$SO_2 = 64.065$

By Baume's critical data.........$SO_2 = 63.952$

The difference between these figures shows the uncertainty of the method as applied to sulphur dioxide. If we accept Guye's figures, as

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[1] Compt. Rend., 140, 1384. 1905.

[2] Journ. Chim. Phys., 6, 43. 1908.

[3] Journ. Chim. Phys., 3, 321. 1905.

yielding results more nearly in harmony with the chemical methods of determination, the general mean for sulphur dioxide gives $SO_2=64.069$, $\pm .0014$, and $S=32.069, \pm .0014$.

Another value for the atomic weight of sulphur is derivable from the density of hydrogen sulphide, as determined by Baume and Perrot.[1] Their crude values for the weight of a litre of the gas are as follows:

1.53934
1.54126
1.53843
1.53862
1.53789
1.53843
1.53798
1.53890
1.53929
1.53860
1.53943
1.53900
1.53917
1.53921
1.53960
1.53938
1.53964
1.54069

Mean, 1.53916, ± .00013

Corrected to the usual standards, the weight of the normal litre becomes 1.5392 grammes. With the critical constants determined by Olzewski, $a_0=.01438$, and $b_0=.00240$. Hence the molecular weight of H_2S is 34.0893, and $S=32.074, \pm .0030$. This, combined with the value deduced from the density of sulphur dioxide, gives a general mean of $S=32.070, \pm .0013$.

GENERAL DISCUSSION.

There are now before us, as developed in the preceding pages, 55 ratios, from which the atomic weights of ten elements are to be computed. These elements are hydrogen, silver, chlorine, bromine, iodine, nitrogen, carbon, sulphur, sodium and potassium. The first twelve "ratios" are really positive values, referred to $O=16$, which can be regarded as first approximations to the true quantities. These values are applicable to the reduction of the remaining ratios, by which they are themselves to be adjusted in turn.

The rigorous method of dealing with such a mass of data is well understood.[2] The several ratios should be transformed into linear equations, and each one weighted inversely as the square of its "probable error." The 55 equations should then be combined into 10 normal equations, which, when solved, would give the 10 atomic weights now under consideration. But that method of reduction is exceedingly laborious, and would possibly be premature. There is great activity at present in the measurement of fundamental ratios, and for that reason the rigorous dis-

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[1] Journ. Chim. Phys., 6, 610. 1908. Baume and Perrot reduce their data with the aid of the constant 22.410, instead of the 22.412 adopted here. Leduc's single determination of the density (Ann. Chim. Phys. (7), 15, 35) may be neglected. His gas was not certainly pure.

[2] See Clarke, Am. Chem. Journ., 27, 321. 1902.

cussion of them may well be deferred. There is, moreover, one practical disadvantage in it; namely, that the specific influence of each individual ratio is more or less obscured, except to the computer himself. The extent to which a given ratio affects the final results is not readily seen in a general combination of all the data, whereas for present purposes some such insight is likely to be helpful in guiding future work. An approximate method of reduction is therefore adopted here, which will give highly probable values for the several atomic weights, even if it does not yield the "most probable values" of the method of least squares. The uncertainties will not be large, and perhaps no larger in reality than if the rigid mathematical procedure were followed implicitly.

The 55 ratios may now be tabulated, and numbered for reference, as follows:

(1). H = 1.00779, ± .00001
(2). C = 12.0000, ± .00029
(3). N = 14.0074, ± .00018
(4). S = 32.070, ± .0013
(5). Cl = 35.4643, ± .00039
(6). NaCl = 58.500, ± .0048
(7). KCl = 74.593, ± .00086
(8). KBr = 119.249, ± .0596
(9). KI = 165.590, ± .0384
(10). AgCl = 143.390, ± .0060
(11). AgBr = 187.884, ± .0133
(12). AgI = 234.734, ± .0126
(13). I_2O_5:2Ag::100:64.6229, ± .0001
(14). Ag:Cl::100:32.8606, ± .00031
(15). Ag:Br::100:74.0802, ± .00029
(16). Ag:I::100:117.6351, ± .00034
(17). AgCl:AgBr::100:131.0172, ± .00012
(18). AgCl:AgI::100:163.8118, ± .00038
(19). AgI:AgBr::100:79.9799, ± .00028
(20). Ag:KCl::100:69.1138, ± .00011
(21). AgCl:KCl::100:52.0163, ± .00018
(22). Ag:KBr::100:110.3193, ± .00033
(23). AgBr:KBr::100:63.3727, ± .0003
(24). Ag:KI::100:153.800, ± .0178
(25). Ag:NaCl::100:54.1995, ± .00015
(26). AgCl:NaCl::100:40.7803, ± .00028
(27). Ag:NaBr::100:95.4405, ± .0007
(28). AgBr:NaBr::100:54.8010, ± .0005
(29). Ag:NO_3::100:57.479, ± .000095
(30). $AgNO_3$:AgCl::100:84.3743, ± .0025
(31). $AgNO_3$:KCl::100:43.8759, ± .00046
(32). $AgNO_3$:NH_4Cl::100:31.488, ± .0006
(33). Ag:NH_4Cl::100:49.5965, ± .00038
(34). AgCl:NH_4Cl::100:37.3218, ± .0003
(35). Ag:NH_4Br::100:90.8175, ± .00065
(36). NH_3:HCl::100:213.934, ± .0053
(37). Cl:N::100:39.489, ± .0033

(38). $N_2O_5:K_2O::100:87.232$, ± .0017
(39). $KCl:KNO_3::100:135.636$, ± .0007
(40). $KClO_3:KNO_3::100:82.500$, ± .0012
(41). $NaCl:NaNO_3::100:145.418$, ± .0012
(42). $NaClO_3:NaNO_3::100:79.8823$, ± .0029
(43). $AgC_2H_3O_2:Ag::100:64.6434$, ± .0007
(44). $Ag_2C_4H_4O_6:2Ag::100:59.2778$, ± .0009
(45). $Ag_2C_4H_4O_5:2Ag::100:62.0016$, ± .0096
(46). $AgC_7H_5O_2:Ag::100:47.125$, ± .0012
(47). $Ag:CO_2::100:40.723$, ± .0071
(48). $AgCN:Ag::100:80.567$, ± .0010
(49). $Ag:C_8H_{20}NBr::100:194.870$, ± .0045
(50). $Ag:C_4H_{12}NBr::100:142.824$, ± .0123
(51). $2Ag:S::100:14.8581$, ± .0006
(52). $Ag_2SO_4:2Ag::100:69.205$, ± .0011
(53). $2AgCl:Ag_2S::100:86.4733$, ± .0011
(54). $Ag_2SO_4:2AgCl::100:91.933$, ± .0004
(55). $Na_2CO_3:Na_2SO_4::100:133.985$, ± .0055

Now, using the formulæ for the calculation of probable error that were given at the beginning of this work, the foregoing ratios yield twenty-nine values for the atomic weight of silver, as follows:

From ratios	9 and 24	Ag = 107.666, ± .0279
“ “	1, 2, and 45	107.717, ± .0320
“ “	1, 2, and 44	107.742, ± .0030
“ “	13 and 16	107.791, ± .0011
“ “	2, 3, and 48	107.826, ± .0066
“ “	5, 12, and 18	107.831, ± .0077
“ “	12 and 16	107.857, ± .0058
“ “	1, 2, and 46	107.874, ± .0039
“ “	1, 3, 5, and 33	107.876, ± .0011
“ “	3 and 29	107.878, ± .00036
“ “	1, 3, 5, and 34	107.891, ± .0016
“ “	1, 3, 5, and 32	107.908, ± .0015
“ “	1, 2, and 43	107.914, ± .0027
“ “	4 and 51	107.921, ± .0061
“ “	5 and 14	107.923, ± .0016
“ “	5 and 10	107.926, ± .0060
“ “	7 and 20	107.927, ± .0013
“ “	11 and 15	107.930, ± .0077
“ “	6 and 25	107.934, ± .0089
“ “	10 and 54	107.937, ± .0066
“ “	3, 10, and 30	107.938, ± .0087
“ “	5, 7, and 21	107.939, ± .0014
“ “	5, 11, and 17	107.940, ± .0101
“ “	4 and 52	107.948, ± .0045
“ “	10 and 53	107.959, ± .0055
“ “	5, 6, and 26	107.988, ± .0118
“ “	3, 7, and 31	108.002, ± .0180
“ “	2 and 47	108.047, ± .0189
“ “	8 and 22	108.094, ± .0540
	General mean,	Ag = 107.880, ± .00029

This final mean is almost identical with the value derived from ratio 29, which gives the composition of silver nitrate. That ratio, moreover, is presumably the best of all, and has the smallest probable error. It dominates the entire combination; but its rejection would only raise the atomic weight of silver to 107.883. If we should reject all the values for silver dependent upon analyses of chlorates, bromates and iodates, which are generally high, the final mean becomes 107.877. It is clear, therefore, that the true value cannot be very far from the general mean of all, namely,

Ag = 108.880, ± .00029

As for the widely aberrant values, especially the first two and the last four, their probable errors are so large that it is a matter of no moment whether they are retained or rejected. Their influence is negligible.

With the aid of the value thus found for silver, we can now compute twenty values for the atomic weight of chlorine, as follows:

From ratios	12, 18, and Ag	Cl = 35.4150, ± .0080
“ “	4, 53, and Ag	35.4186, ± .0058
“ “	1, 3, and 36	35.4269, ± .0029
“ “	3, 31, 38, and Ag	35.4279, ± .0012
“ “	3, 38, and 39	35.4401, ± .0012
“ “	3, 20, 38, and Ag	35.4483, ± .00096
“ “	14 and Ag	35.4502, ± .00035
“ “	1, 3, 32, and Ag	35.4556, ± .0010
“ “	3, 38, and 40	35.4569, ± .0022
“ “	4, 54 and Ag	35.4575, ± .00093
“ “	3, 30, and Ag	35.4610, ± .0043
“ “	5	35.4643, ± .00039
“ “	1, 3, 33, and Ag	35.4661, ± .00051
“ “	3 and 37	35.4717, ± .0030
“ “	3, 10, 21, 38, and Ag	35.4745, ± .0032
“ “	1, 3, 10, and 34	35.4772, ± .0023
“ “	3, 7, and 38	35.4813, ± .0013
“ “	10 and Ag	35.5100, ± .0061
“ “	3, 7, 21, 38, and Ag	35.5235, ± .0018
“ “	11, 17, and Ag	35.5240, ± .0102
	General mean,	Cl = 35.4584, ± .0002

Here, again, the extreme values are evidently of no real significance, and have practically no effect upon the final result. The rounded-off figure, 35.458, is in good agreement with the determinations made by Noyes and Weber, and also with the ratio between silver and chlorine as measured by Richards and Wells.

For bromine, using the new value for chlorine in place of that given by ratio 5, eleven values are deducible:

From ratios 12, 19, and Ag......Br = 79.8600, ± .0101
" " 3, 22, 38, and Ag.........79.9008, ± .0011
" " 15 and Ag...............79.9177, ± .00038
" " 17, Ag, and Cl...........79.9189, ± .00063
" " 1, 3, 35, and Ag..........79.9353, ± .00079
" " 3, 11, 23, 38, and Ag......79.9555, ± .0085
" " 1, 2, 3, 50, and Ag....... 79.9775, ± .0133
" " 11 and Ag...............80.0040, ± .0134
" " 1, 2, 3, 49, and Ag........80.0624, ± .0054
" " 3, 8, and 38..............80.1320, ± .0597
" " 8, 23, and Ag............80.2910, ± .0940

General mean, Br = 79.9197, ± .0003

From Baxter's measurement of the silver bromine ratio, when Ag = 107.88, Br = 79.916. The difference is less than 1 part in 21,000.

For iodine seven values are computable, thus:

From ratios 3, 9, and 38.........I = 126.478, ± .0385
" " 3, 24, 38, and Ag........126.807, ± .0192
" " 12 and Ag..............126.854, ± .0127
" " 16 and Ag..............126.905, ± .0005
" " 18, Ag, and Cl..........126.925, ± .0008
" " 19, Ag, and Br..........126.928, ± .0011
" " 13 and Ag..............126.938, ± .0006

General mean, I = 126.9204, ± .00033

The first two of these values for iodine are meaningless. The third and fourth involve the determinations made by Stas and Marignac. The final mean, however, agrees with Baxter's determinations to within 1 part in 13,000.

For potassium there are twelve values, as follows:

From ratios 9 and I..............K = 38.6696, ± .0385
" " 24, Ag, and I............38.9989, ± .0192
" " 3, 39, and Cl.............39.0420, ± .0017
" " 1, 3, 31, Ag, and Cl.......39.0812, ± .00089
" " 22, Ag, and Br...........39.0927, ± .00056
" " 23, Ag, and Br...........39.0940, ± .00068
" " 21, Ag, and Cl...........39.1009, ± .00039
" " 20, Ag, and Cl...........39.1016, ± .00031
" " 3, 40, and Cl.............39.1097, ± .0085
" " 3 and 38.................39.1117, ± .00093
" " 7 and Cl.................39.1346, ± .00095
" " 8 and Br.................39.3293, ± .0598

General mean, K = 39.0999, ± .0002

This value is in good agreement with the determinations made by Richards and his collaborators in the Harvard laboratory.

The eight values for sodium, which come next, are less satisfactory than any of the preceding figures:

From ratios 26, Ag, and Cl.....Na = 22.9954, ± .00048
" " 28, Ag, and Br...........22.9963, ± .0011
" " 3, 41, and Cl.............22.9964, ± .0017
" " 25, Ag, and Cl............23.0120, ± .0003
" " 27, Ag, and Br...........23.0415, ± .0009
" " 6 and Cl.................23.0416, ± .0048
" " 2, 4, and 55..............23.0675, ± .0088
" " 3, 42, and Cl.............23.1692, ± .0050

General mean, Na = 23.0108, ± .00024

The first four values, taken by themselves, give a general mean of 23.0072, ± .00025. This harmonizes better with the determinations made by Richards and Wells than the general mean of all. The fifth and sixth values, however, cannot be safely rejected, for their discordance with the others is not explained. The last two values signify little or nothing.

For sulphur there are six values, as follows:

From ratios 52 and Ag...........S = 32.0094, ± .0038
" " 2, 55, and Na............32.0314, ± .0059
" " 51 and Ag...............32.0578, ± .0013
" " 4 ....32.070, ± .0013
" " 54, Ag, and Cl............32.0723, ± .0016
" " 53, Ag, and Cl............32.1392, ± .0032

General mean, S = 32.0667, ± .00075

For nitrogen, the fundamental ratios give eighteen values, as follows:

From ratios 38 and K............N = 13.9938, ± .0011
" " 37 and Cl................14.0022, ± .0012
" " 1, 32, Ag, and Cl.........14.0032, ± .0015
" " 30, Ag, and Cl............14.0040, ± .0051
" " 1, 34, Ag, and Cl.........14.0069, ± .0005
" " 3 ....14.0074, ± .00018
" " 29 and Ag...............14.0083, ± .00020
" " 40, K, and Cl............14.0107, ± .0015
" " 41, Na, and Cl............14.0140, ± .0009
" " 1, 33, Ag, and Cl.........14.0151, ± .0005
" " 2, 48, and Ag............14.0209, ± .0014
" " 1, 36, and Cl.............14.0221, ± .00043
" " 1, 35, Ag, and Br.........14.0230, ± .0009
" " 39, K, and Cl.............14.0279, ± .0007
" " 42, Na, and Cl............14.0392, ± .0030
" " 31, Ag, K, and Cl.........14.0500, ± .0020
" " 1, 2, 50, Ag, and Br......14.0652, ± .0133
" " 1, 2, 49, Ag, and Br......14.1501, ± .0054

General mean, N = 14.0101, ± .0001

The mean is distinctly higher than the atomic weight of nitrogen as determined directly, or as derived from the study of silver nitrate.

Finally, there are ten values for carbon:

From ratios 47 and Ag..........C = 11.9320, ± .0077
" " 1, 43, and Ag............11.9957, ± .0004
" " 2 ....12.0000, ± .00029
" " 1, 46, and Ag............12.0007, ± .0007
" " 1, 50, N, Ag, and Br......12.0138, ± .0044
" " 1, 49, N, Ag, and Br......12.0175, ± .00052
" " 48, N, and Ag...........12.0197, ± .0014
" " 55, Na, and S...........12.0270, ± .0044
" " 1, 44, and Ag...........12.0475, ± .0010
" " 1, 45, and Ag...........12.0499, ± .0098

General mean, C = 12.0038, ± .0002

That this mean is higher than the atomic weight given in ratio (2) does not prove it to be in error. Scott's recent determinations, the fifth and sixth given above, are even higher, and the cause of the discrepancy is undetermined. The general mean of all determinations agrees well with the results obtained by modern physical methods, and may, therefore, stand, until it is superseded by something of less uncertainty.

As for hydrogen, new values for its atomic weight can be deduced from eleven of the fundamental ratios. The computation has been roughly made, and found to be without significance. The combined values, so obtained, are of such small weight in comparison with ratio (1) that they only modify it in the sixth decimal place, a change which is not worth considering.

To sum up: The subjoined values, referred to O = 16 as the standard, have been computed from all the ratios, old and new, good, bad and indifferent:

H = 1.00779, ± .0001
C = 12.0038, ± .0002
N = 14.0101, ± .0001
Na = 23.0108, ± .00024
S = 32.0667, ± .00075
Cl = 35.4584, ± .0002
K = 39.0999, ± .0002
Br = 79.9197, ± .0003
Ag = 107.880, ± .00029
I = 126.9204, ± .00033

That these values are final, is not to be supposed. That they are, in the strict mathematical sense, the most probable values deducible from the experimental data, is also questionable. But that they are highly probable values, in harmony with the best modern evidence, can safely be asserted. The inferior determinations, low in weight, have practically vanished, one might almost say self-rejected, but not thrown out arbitrarily. The good measurements overwhelm the doubtful ones, whose influence upon the final computations is almost negligible. The nine values as given above, will be used in calculating the atomic weights of all the other elements.

LITHIUM.

The earlier determinations of the atomic weight of lithium by Arfvedson, Stromeyer, C. G. Gmelin and Kralovanzky were all erroneous, because of the presence of sodium compounds in the material employed. The results of Berzelius, Hagen and Hermann were also incorrect, and need no further notice here. The only investigations which we need to consider are those of Mallet, Diehl, Troost, Stas, Dittmar and Richards and Willard.

Mallet's experiments[1] were conducted upon lithium chloride, which had been purified as completely as possible. In two trials the chloride was precipitated by nitrate of silver, which was collected upon a filter and estimated in the ordinary way. The figures in the third column represent the LiCl proportional to 100 parts of AgCl:

7.1885 grm. LiCl gave	24.3086 grm. AgCl.	29.606
8.5947 "	29.0621 "	29.574

In a third experiment the LiCl was titrated with a standard solution of silver. 3.9942 grm. LiCl balanced 10.1702 grm. Ag, equivalent to 13.511 grm. AgCl. Hence 100 AgCl = 29.563 LiCl. Mean of all three experiments, 29.581, ± .0087. Hence Li = 6.943.

Diehl,[2] whose paper begins with a good résumé of all the earlier determinations, describes experiments made with lithium carbonate. This salt, which was spectroscopically pure, was dried at 130° before weighing. It was then placed in an apparatus from which the carbon dioxide generated by the action of pure sulphuric acid upon it could be expelled, and the loss of weight determined. From this loss the following percentages of CO_2 in Li_2CO_3 were determined:

59.422
59.404
59.440
59.401

Mean, 59.417, ± .006

Hence Li = 7.024.

Diehl's investigation was quickly followed by a confirmation from Troost.[3] This chemist, in an earlier paper,[4] had sought to fix the atomic

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[1] Amer. Journ. Sci., November, 1856. Chem. Gazette, 15, 7.

[2] Ann. Chem. Pharm., 121, 93.

[3] Zeit. Anal. Chem., 1, 402.

[4] Ann. Chim. Phys., 51, 108.

weight of lithium by an analysis of the sulphate, and had found a value not far from 6.5, thus confirming the results of Berzelius and of Hagen, who had employed the same method. But Diehl showed that the $BaSO_4$ precipitated from Li_2SO_4 always retained traces of Li, which were recognizable by spectral analysis, and which accounted for the error. In the later paper Troost made use of the chloride and the carbonate of lithium, both spectroscopically pure. The carbonate was strongly ignited with pure quartz powder, thus losing carbon dioxide, which loss was easily estimated. The subjoined results were obtained:

.970 grm. Li_2CO_3 lost	.577 grm. CO_2.	59.485 per cent.
1.782 "	1.059 "	59.427 "
		Mean, 59.456, ± .020

Hence Li = 7.003.

The lithium chloride employed by Troost was heated in a stream of dry hydrochloric acid gas, of which the excess, after cooling, was expelled by a current of dry air. The salt was weighed in the same tube in which the foregoing operations had been performed, and the chlorine was then estimated as silver chloride. The usual ratio between LiCl and 100 parts of AgCl is given in the third column:

1.309 grm. LiCl gave	4.420 grm. AgCl.	29.615
2.750 "	9.300 "	29.570
		Mean, 29.5925, ± .0145

Hence Li = 6.959.

Next in order is the work of Stas,[1] which was executed with his usual care. In three titrations, in which all the weights were reduced to a vacuum standard, the following quantities of LiCl balanced 100 parts of pure silver:

39.356
39.357
39.361
Mean, 39.358, ± .001

Hence Li = 7.0110.

In a second series of experiments, intended for determining the atomic weight of nitrogen, LiCl was converted into $LiNO_3$. The method was that employed for a similar purpose with the chlorides of sodium and of potassium. One hundred parts of LiCl gave of $LiNO_3$:

162.588
162.600
162.598
Mean, 162.5953, ± .0025

Hence Li = 6.956.

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[1] Oeuvres Complètes, 1, 710-716.

The determinations of Dittmar[1] resemble those of Diehl; but the lithium carbonate used was dehydrated by fusion in an atmosphere of carbon dioxide. The carbonate was treated with sulphuric acid, and the CO_2 was collected and weighed in an absorption apparatus, which was tared by a similar apparatus after the method of Regnault. The following percentages of CO_2 in Li_2CO_3 were found:

59.601
59.645
59.529—rejected
59.655
59.683
59.604
59.517
59.663
60.143—rejected
59.794
59.584

Mean of all, 59.674

Rejecting the two experiments which Dittmar regards as untrustworthy, the mean of the remaining nine becomes 59.638, ± .0173, and Li = 6.891. This combines with the work of Diehl and Troost, as follows:

Diehl	59.417, ± .0060
Troost	59.456, ± .0200
Dittmar	59.638, ± .0173
General mean	59.442, ± .0054

The unique merit of the determinations by Richards and Willard[2] is, not only that their work was done with scrupulous accuracy, but that their ratios give simultaneous values for the atomic weights of lithium, silver and chlorine, which are independent of all other data. Analyses of lithium perchlorate gave directly the molecular weight of lithium chloride, with reference to oxygen alone, and with that their other ratios are reducible. The data for the perchlorate are as follows, with vacuum weights:

Preliminary Series.

$LiClO_4$.	*LiCl.*	*Per cent. LiCl.*
10.64596	4.24171	39.8434
12.77683	5.09073	39.8435
10.12750	4.03587	39.8506
13.04021	5.19638	39.8489
		Mean, 39.8466, ± .00125

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[1] Trans. Roy. Soc. Edinburgh, 35, II, 429. 1889.

[2] Journ. Amer. Chem. Soc., 32, 4. 1910.

Final Series.

$LiClO_4$.	$LiCl$.	*Per cent. LiCl.*
12.79265	5.09744	39.8466
10.55416	4.20534	39.8453
11.39912	4.54205	39.8456
11.17008	4.45070	39.8448
17.84842	7.11167	39.8448
22.58273	8.99846	39.8466
		Mean, 39.8456, ± .00023

The two series combined give a general mean of 39.8457, ± .00023. Hence LiCl = 42.393, and Li = 6.9346.

The two other ratios determined by Richards and Willard are those between lithium chloride, silver chloride and silver. With vacuum weights, their data, first for the ratio Ag : LiCl are these:

Preliminary Series.[1]

LiCl.	*Ag.*	*Ratio.*
8.99620	22.89013	39.3017
5.25395	13.36777	39.3030
		Mean, 39.3023, ± .00048

Hence Li = 6.9409.

Final Series.

LiCl.	*Ag.*	*Ratio.*
5.82422	14.82035	39.2988
6.28662	15.99687	39.2991
5.82076	14.81122	39.2996
6.70863	17.07038	39.2998
6.24717	15.89620	39.2998
7.75349	19.72977	39.2984
7.99108	20.33415	39.2988
		Mean, 39.2992, ± .00014

Hence Li = 6.9300.

Combining the figures for this ratio we have—

Stas	39.358, ± .0011
Richards and Willard 1	39.3023, ± .00048
Richards and Willard 2	39.2992, ± .00014
General mean	39.3002, ± .00013

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[1] The material used in the preliminary series contained a trace of sodium.

For the silver chloride ratio Richards and Willard give the following data:

Preliminary Series.

LiCl.	*AgCl.*	*Ratio.*
4.01994	13.59125	29.5774
6.32840	21.39635	29.5770
8.99620	30.41341	29.5797
4.66824	15.78111	29.5812
5.43032	18.35734	29.5812
5.10725	17.26504	29.5815
5.74000	19.40375	29.5819
5.42038	18.32417	29.5805
5.21573	17.63280	29.5797
6.56925	22.20617	29.5817
4.84268	16.37121	29.5805

Mean, 29.5802, ± .00033

Hence Li = 6.9414.

Final Series.

LiCl.	*AgCl.*	*Ratio.*
6.28662	21.25442	29.5779
5.82076	19.67875	29.5790
6.70863	22.68030	29.5791
6.24717	21.12073	29.5784
5.50051	18.59600	29.5790
8.34521	28.21438	29.5779
6.65987	22.51564	29.5788

Mean, 29.5786, ± .00014

Hence Li = 6.9391.

Combining the several series for this ratio we have—

Mallet	29.581, ± .0087
Troost	29.5925, ± .0145
Richards and Willard 1	29.5802, ± .00033
Richards and Willard 2	29.5786, ± .00014
General mean	29.5789, ± .00013

The older work, with its high probable errors, vanishes.

Summing up, the following ratios are now available, from which to compute the atomic weight of lithium:

(1). $LiClO_4:LiCl::100:39.8457$, ± .00023
(2). $Ag:LiCl::100:39.3002$, ± .00013
(3). $AgCl:LiCl::100:29.5789$, ± .00013
(4). $LiCl:LiNO_3::100:162.5953$, ± .0025
(5). $Li_2CO_3:CO_2::100:59.442$, ± .0054

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
N = 14.0101, ± .0001
C = 12.0038, ± .0002

Hence—

From ratio 1 .... Li = 6.9346, ± .00036
" " 2 .... 6.9387, ± .00028
" " 3 .... 6.9395, ± .00095
" " 4 .... 6.9563, ± .0056
" " 5 .... 7.0122, ± .0024

General mean, Li = 6.9379, ± .00021

Richards and Willard, from their three *final* series of determinations, deduce

Ag = 107.871
Cl = 35.454
Li = 6.939

The slightly lower value for lithium given in the general combination above is due to the higher value here assigned to chlorine. From the final silver and silver chloride series of Richards and Willard, the ratio Ag : Cl :: 100 : 32.8637 is derivable. This is a little lower than the value determined by Richards and Wells directly.

RUBIDIUM.

The atomic weight of rubidium has been determined by analyses of the chloride and bromide.

Bunsen,[1] employing ordinary gravimetric methods, estimated the ratio between AgCl and RbCl. His rubidium chloride was purified by fractional crystallization of the chloroplatinate. He obtained the following results, to which, in a third column, I add the ratio between RbCl and 100 parts of AgCl:

One grm. RbCl gave	1.1873	grm. AgCl.	84.225
"	1.1873	"	84.225
"	1.1850	"	84.388
"	1.1880	"	84.175
		Mean,	84.253, ± .031

Hence Rb = 85.309.

The work of Piccard[2] was similar to that of Bunsen. In weighing, the crucible containing the silver chloride was balanced by a precisely similar crucible, in order to avoid the correction for displacement of air. The filter was burned separately from the AgCl, as usual; but the small amount of material adhering to the ash was reckoned as metallic silver. The rubidium chloride was purified by Bunsen's method. The results, expressed according to the foregoing standard, are as follows:

1.1587	grm. RbCl =	1.372	AgCl +	.0019	Ag.		84.300
1.4055	"	1.6632	"	.0030	"		84.303
1.001	"	1.1850	"	.0024	"		84.245
1.5141	"	1.7934	"	.0018	"		84.313
						Mean,	84.290, ± .0105

Hence Rb = 85.362.

Godeffroy,[3] starting with material containing both rubidium and cæsium, separated the two metals by fractional crystallization of their alums, and obtained salts of each spectroscopically pure. The nitric acid employed was tested for chlorine and found to be free from that impurity, and the weights used were especially verified. In two of his analyses of RbCl the AgCl was handled by the ordinary process of filtration. In the other two it was washed by decantation, dried and weighed in a glass dish. The usual ratio is appended in the third column:

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[1] Zeit. Anal. Chem., 1, 136. Poggend. Annal., 113, 339. 1861.

[2] Journ. prakt. Chem., 86, 454. 1862. Zeit. Anal. Chem., 1, 518.

[3] Ann. Chem. Pharm., 181, 185. 1876.

1.4055 grm. RbCl	gave	1.6665 grm. AgCl.		84.338
1.8096	"	2.1461	"	84.320
2.2473	"	2.665	"	84.326
2.273	"	2.6946	"	84.354
			Mean,	84.3345, ± .0051

Hence Rb = 85.426.

Heycock[1] worked by two methods, but unfortunately his results are given only in abstract, without details. First, silver solution was added in slight deficiency to a solution of rubidium chloride, and the excess of the latter was measured by titration. The mean of seven experiments gave—

Ag : RbCl : : 107.93 : 120.801

Hence Rb = 85.287.

Two similar experiments with the bromide gave—

Ag : RbBr : : 107.93 : 165.437
Ag : RbBr : : 107.93 : 165.342

Mean, 165.3895, ± .0320

Hence Rb = 85.393.

The determinations by Archibald[2] were made with scrupulously purified materials, and with all of the precautions observed in the best modern investigations. The chloride and bromide were precipitated with known weights of silver, and the silver halide produced was also weighed. Two ratios were thus measured for each salt, and checked by the cross ratios between silver and chlorine or bromine, respectively. The weights, corrected to a vacuum, are given below, and also the four principal ratios:

Weight RbCl.	*Weight AgCl.*	*Weight Ag.*	*AgCl ratio.*	*Ag ratio.*
1.99966	2.37070	1.78454	84.349	112.054
2.06480	2.44778	1.84241	84.354	112.070
2.29368	2.71960	2.04710	84.339	112.046
1.09495	1.29796	.97702	84.360	112.070
2.14381	2.54118	1.91316	84.364	112.056
2.89700	3.43475	2.58550	84.344	112.047
2.19692	2.60452	1.96076	84.350	112.044
2.14543	2.54386	1.91462	84.338	112.055
2.12164	2.51557	1.89346	84.341	112.052
2.25777	2.67685	2.01515	84.344	112.040
2.18057	2.58528	1.94594	84.346	112.057
2.32699	2.75878	2.07668	84.348	112.053
4.00035	2.74233	3.56998	84.354	112.055
2.43440	2.88613	2.17233	84.348	112.064
		Mean,	84.3485, ± .0014	112.0545, ± .0016

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[1] British Association Report, 1882, 449.

[2] Journ. Chem. Soc., 85, 776. 1904.

From the AgCl ratio, Rb=85.446.
From the Ag ratio, Rb=85.426.
And Ag: Cl:: 100: 32.847.

The values for the AgCl ratio combine as follows:

Bunsen	84.253, ± .031
Piccard	84.290, ± .0105
Godeffroy	84.3345, ± .0051
Archibald	84.3485, ± .0014
General mean	84.3433, ± .0013

Heycock's single value for the Ag ratio, reduced to the usual standard, becomes Ag: RbCl:: 100: 111.926. It is not worth while to combine it with Archibald's values, for its influence would be quite negligible. In the AgCl ratio the older determinations count for something, but the general mean falls within the range of variation of Archibald's series.

The bromide analyses by Archibald are as follows:

Weight RbBr.	*Weight AgBr.*	*Weight Ag.*	*AgBr ratio.*	*Ag ratio.*
2.68170	3.04578	1.74930	88.047	153.301
2.07280	2.35401	1.35230	88.054	153.280
2.10086	2.38589	1.37061	88.053	153.278
2.61044	2.96462	1.70300	88.053	153.285
3.84082	4.36215	2.50590	88.049	153.272
3.77852	4.29084	2.46502	88.061	153.287
4.34299	4.93210	2.83340	88.056	153.278
		Mean,	88.0533, ± .0012	153.283, ± .0024

From the Ag ratio, Rb=85.442.
From the AgBr ratio, Rb=85.444.
And Ag: Br:: 100: 74.080.

Heycock's mean for the Ag ratio, reduced, becomes Ag: RbBr:: 100: 153.238, ±.0300. Its probable error is so high that combination with Archibald's data would be useless.

There are now four ratios from which to compute the atomic weight of rubidium:

(1). Ag:RbCl::100:112.0545, ± .0016
(2). AgCl:RbCl::100:84.3433, ± .0013
(3). Ag:RbBr::100:153.283, ± .0024
(4). AgBr:RbBr::100:88.0533, ± .0012

Reducing these ratios with Ag=107.880, ±.00029, Cl=35.4584, ±.0002, and Br=79.9197, ±.0003, we have—

From ratio 1	Rb=85.426, ± .0018
" " 2	85.438, ± .0019
" " 4	85.442, ± .0026
" " 3	85.444, ± .0023
General mean,	Rb=85.436, ± .0010

CÆSIUM.

The atomic weight of cæsium, like that of rubidium, has been computed from analyses of the chloride and bromide, and also from experiments upon the nitrate. The earliest determination, by Bunsen,[1] was incorrect, because of impurity in the material studied. The first trustworthy determinations were published by Johnson and Allen[2] in 1863. Their material was extracted from the lepidolite of Hebron, Maine, and the cæsium was separated from the rubidium as bitartrate. From the pure cæsium bitartrate cæsium chloride was prepared, and in this the chlorine was estimated as silver chloride by the usual gravimetric method. Reducing their results to the convenient standard adopted in preceding chapters, we have, in a third column, the quantities of CsCl equivalent to 100 parts of AgCl:

1.8371 grm. CsCl	gave	1.5634 grm. AgCl.		117.507
2.1295	"	1.8111	"	117.580
2.7018	"	2.2992	"	117.511
1.56165	"	1.3302	"	117.399
			Mean,	117.499, ± .025

Hence Cs = 132.963.

Shortly after the results of Johnson and Allen appeared a new series of estimations was published by Bunsen.[3] His cæsium chloride was purified by repeated crystallizations of the chloroplatinate, and the ordinary gravimetric process was employed. The following results represent, respectively, material thrice, four times and five times purified:

1.3835 grm. CsCl	gave	1.1781 grm. AgCl.		Ratio,	117.435
1.3682	"	1.1644	"	"	117.503
1.2478	"	1.0623	"	"	117.462
				Mean,	117.467, ± .013

Hence Cs = 132.917.

Godeffroy's work[4] was, in its details of manipulation, sufficiently described under rubidium. In three of the experiments upon cæsium the silver chloride was washed by decantation, and in one it was collected upon a filter. The results are subjoined:

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[1] Zeitsch. Anal. Chem., 1, 137.

[2] Amer. Journ. Sci. (2), 35, 94.

[3] Poggend. Annalen, 119, 1. 1863.

[4] Ann. Chem. Pharm., 181, 185. 1876.

1.5825	grm. CsCl gave	1.351	grm. AgCl.	Ratio,	117.135
1.3487	"	1.1501	"	"	117.265
1.1880	"	1.0141	"	"	117.148
1.2309	"	1.051	"	"	117.107
				Mean,	117.164, ± .023

Hence Cs = 132.483.

The foregoing investigations may now be regarded as merely preliminary, in comparison with the more elaborate determinations made by Richards and Archibald.[1] Their material was purified by fractional crystallization as cæsium dichloriodide, from which the chloride, bromide and nitrate were afterwards prepared. The chloride and bromide were freed from possible traces of moisture by fusion in an atmosphere of nitrogen, and analyzed by the usual method. That is, they were precipitated by known weights of silver dissolved as nitrate, and the silver chloride or bromide produced was also weighed. All the weights given are reduced to a vacuum standard. The results obtained with cæsium chloride are given in the next table:

Weight CsCl.	*Weight AgCl.*	*Weight Ag.*	*AgCl ratio.*	*Ag ratio.*
3.83054	3.26240	2.45600	117.415	155.967
3.95120	3.36532	2.53351	117.409	155.958
2.27237	1.93555	1.45686	117.402	155.977
3.02935	2.58003	1.94244	117.415	155.956
3.19774	2.72382	2.05023	117.399	155.970
2.35068	2.00253	1.50270	117.386	155.963
2.06245	1.75678	1.32251	117.399	155.950
2.56372	2.18358	......	117.409	......
2.01881	1.71972	1.29434	117.392	155.972
1.77391	1.51093	1.13743	117.405	155.958
3.08160	2.62484	1.97590	117.401	155.959
3.13117	2.66720	2.00760	117.395	155.966
5.06656	4.31570	3.24850	117.398	155.966
		Mean,	117.402, ± .0016	155.9635, ± .0016

From Ag ratio, Cs = 132.795.
From AgCl ratio, Cs = 132.824.
And Ag : Cl : : 100 : 32.846.

The silver chloride ratio combines with previous determinations thus:

Johnson and Allen............	117.499, ± .025
Bunsen	117.467, ± .013
Godeffroy	117.164, ± .023
Richards and Archibald......	117.402, ± .0016
General mean	117.405, ± .0016

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[1] Proc. Amer. Acad., 38, 443. 1903. Zeitsch. anorg. Chem., 34, 353.

The older determinations practically vanish, leaving the Richards and Archibald mean almost unchanged.

The figures for the bromide series are as follows:

Weight CsBr.	*Weight AgBr.*	*Weight Ag.*	*AgBr ratio.*	*Ag ratio.*
3.49820	3.08815	1.77402	113.278	197.192
6.20409	5.47673	3.14606	113.281	197.202
2.17300	6.33213	2.63740	113.279	197.201
		Mean,	113.279, ± .0007	197.198, ± .0045

From Ag ratio, Cs = 132.817.
From AgBr ratio, Cs = 132.818.
And Ag : Br : : 100 : 74.078.

When cæsium nitrate is fused with finely powdered silica, cæsium silicate is formed, and the elements of the nitric radicle are expelled. With weighed quantities of the nitrate, the loss of weight is equivalent to N_2O_5. The ratio $N_2O_5 : Cs_2O :: 100 : x$ is thus easily determined. In four experiments Richards and Archibald obtained the following results:

Weight $CsNO_3$.	*Weight N_2O_5.*	*Ratio.*
3.76112	1.04273	260.699
3.33334	.92416	260.689
4.81867	1.33590	260.706
5.04807	1.39960	260.679
	Mean,	260.693, ± .0039

Hence Cs = 132.801.

The five ratios for the atomic weight of cæsium are now as follows:

(1). Ag:CsCl::100:155.9635, ± .0016
(2). AgCl:CsCl::100:117.405, ± .0016
(3). Ag:CsBr::100:197.198, ± .0045
(4). AgBr:CsBr::100:113.279, ± .0007
(5). $N_2O_5:Cs_2O::100:260.693$, ± .0039

Reducing these ratios with

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
N = 14.0101, ± .0039

we have—

From ratio 1 .... Cs = 132.795, ± .0018
" " 5 .... 132.801, ± .0022
" " 3 .... 132.818, ± .0049
" " 4 .... 132.818, ± .0015
" " 2 .... 132.828, ± .0024

General mean, Cs = 132.811, ± .0010

COPPER.

The atomic weight of copper has been chiefly determined by means of the oxide, the sulphate and the bromide, and by direct comparison of the metal with silver.

In dealing with the first-named compound nearly all experimenters have agreed in reducing it with a current of hydrogen, and weighing the metal thus set free.

The earliest experiments of any value were those of Berzelius,[1] whose results were as follows:

7.68075 grm. CuO lost	1.55 grm. O.	79.820 per cent. Cu in CuO.
9.6115 "	1.939 "	79.826 " "
		Mean, 79.823, ± .002

Hence Cu = 63.298.

Erdmann and Marchand,[2] who come next in chronological order, corrected their results for weighing in air. Their weighings, thus corrected, give us the subjoined percentages of metal in CuO:

63.8962 grm. CuO gave	51.0391 grm. Cu.	79.878 per cent.
65.1590 "	52.0363 "	79.860 "
60.2878 "	48.1540 "	79.874 "
46.2700 "	36.9449 "	79.846 "
		Mean, 79.8645, ± .0038

Hence Cu = 63.462.

Still later we find a few analyses by Millon and Commaille.[3] These chemists not only reduced the oxide by hydrogen, but they also weighed, in addition to the metallic copper, the water formed in the experiments. In three determinations the results were as follows:

6.7145 grm. CuO gave	5.3565 grm. Cu and	1.5325 grm. H_2O.	79.775 per cent.
3.3945 "	2.7085 "	.7680 "	79.791 "
2.7880 "	2.2240 "	.... "	79.770 "
			Mean, 79.7787, ± .0043

Hence Cu = 63.125.

For the third of these analyses the water estimation was not made, but for the other two it yielded results which, in sum, would make the atomic weight of copper 63.165. This figure has so high a probable error that we need not consider it further.

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[1] Poggend. Annal., 8, 177. 1826.

[2] Journ. prakt. Chem., 31, 380. 1844.

[3] Fresenius' Zeitschrift, 2, 475. 1863.

The results obtained by Dumas[1] are wholly unavailable. Indeed, he does not even publish them in detail. He merely says that he reduced copper oxide, and also effected the synthesis of the subsulphide, but without getting figures which were wholly concordant. He puts Cu = 63.5.

In 1873 Hampe[2] published his careful determinations, which were for many years almost unqualifiedly accepted. First, he attempted to estimate the atomic weight of copper by the quantity of silver which the pure metal could precipitate from its solutions. This attempt failed to give satisfactory results, and he fell back upon the old method of reducing the oxide. From ten to twenty grammes of material were taken in each experiment, and the weights were reduced to a vacuum standard:

20.3260	grm. CuO gave	16.2279	grm. Cu.	79.838	per cent.
20.68851	"	16.51669	"	79.835	"
10.10793	"	8.06926	"	79.831	"
			Mean,	79.8347, ± .0013	

Hence Cu = 63.344.

Hampe also determined the quantity of copper in the anhydrous sulphate, $CuSO_4$. From 40 to 45 grammes of the salt were taken at a time, the metal was thrown down by electrolysis, and the weights were all corrected. I subjoin the results:

40.40300	grm. $CuSO_4$ gave	16.04958	grm. Cu.	39.724	per cent.
44.64280	"	17.73466	"	39.726	"
			Mean,	39.725, ± .0007	

The last series of data gives Cu = 63.314, and is interesting for comparison with results obtained by Richards later.

In all of the foregoing experiments with copper oxide, that compound was obtained by ignition of the basic nitrate. But, as was shown in the chapter upon oxygen, copper oxide so prepared always carries occluded gases, which are not wholly expelled by heat. This point was thoroughly worked up by Richards[3] in his fourth memoir upon the atomic weight of copper, and it vitiates all the determinations previously made by this method.

By a series of experiments with copper oxide ignited at varying temperatures, and with different degrees of heat during the process of reduction, Richards obtained values for Cu ranging from 63.20 to 63.62. In two cases selected from this series he measured the amount of gaseous

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[1] Ann. Chim. Phys. (3), 55, 129. 1859.

[2] Fresenius' Zeitschrift, 13, 352.

[3] Proc. Amer. Acad., 26, 276. 1891.

impurity, and corrected the results previously obtained. The results were as follows, with a vacuum reduction:

1.06253 grm. CuO gave	.84831 grm. Cu.	79.802 per cent.
1.91656 "	1.5298 "	79.820 "
		Mean, 79.811, ± .0061

Correcting for the occluded gases in the oxide, the sum of the two experiments gives 79.901 per cent. of copper, whence Cu = 63.605. Three other indirect results, similarly corrected, gave 79.900 per cent. Cu in CuO, or Cu = 63.603. If we assign all five experiments equal weight, and judge their value by the two detailed above, the mean percentage becomes 79.900, ± .0038.

The recent experiments on copper oxide, by Murmann,[1] are of very doubtful utility. Copper was oxidized by heating in oxygen, and the oxide was also reduced in hydrogen, giving values for Cu varying from 63.513 to 64.397. The five experiments, with all corrections, including reduction to a vacuum, and eliminating the excessively high figure given above, may be stated in the following form:

Weight Cu.	*Weight CuO.*	*Per cent. Cu.*
1.13625	1.41856	80.099
2.64333	3.30923	79.878
1.07874	1.35045	79.880
5.12489	6.41350	79.908
3.33515	4.17315	79.919
		Mean, 79.937, ± .0278

Hence Cu = 63.749. Murmann himself selected values from his series varying between 63.512 and 63.560.

These figures, by Richards and Murmann, need not be combined with the data given by previous observers, so far as practical purposes are concerned; but as this work is, in part, at least, a study of the compensation of errors, it may not be wasted time to effect the combination, as follows:

Berzelius	79.823, ± .0020
Erdmann and Marchand	79.8645, ± .0038
Millon and Commaille	79.7787, ± .0043
Hampe	79.8347, ± .0013
Richards	79.900, ± .0038
Murmann	79.937, ± .0278
General mean	79.836, ± .0010

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[1] Monatsh. Chem., 27, 351. 1906.

This result is practically identical with that of Hampe, whose work receives excessive weight, as does also that of Berzelius. The oxide of copper is evidently of doubtful value in the measurement of this atomic weight.

The composition of copper sulphate has been studied, not only by Hampe, but also by Baubigny[1] and by Richards.[2] Baubigny merely ignited the anhydrous salt, weighing both it and the residual oxide, as follows:

4.022 grm. $CuSO_4$ gave	2.0035	CuO.	49.813	per cent.
2.596 “	1.293	“	49.807	“
		Mean,	49.810, ± .002	

Hence Cu = 63.460.

The same ratio, in reverse—that is, the synthesis of the sulphate from the oxide—was investigated by Richards, who shows that the results obtained are vitiated by the same errors which affect the copper oxide experiments previously cited. The weights given are reduced to vacuum standards. The percentage of oxide in the sulphate is stated in the third column of figures:

1.0084 grm. CuO gave	2.0235	grm. $CuSO_4$.	49.835	per cent.
2.7292 “	5.4770	“	49.830	“
1.0144 “	2.0350	“	49.848	“
		Mean,	49.838, ± .0036	

Hence Cu = 63.550.

The two series combine thus:

Baubigny	49.810, ± .0020
Richards	49.838, ± .0036
General mean	49.816, ± .0017

Here, plainly, the rigorous discussion gives Baubigny's work weight in excess of its merits.

In the memoir by Richards now under consideration, his fourth upon copper, the greater part of his attention is devoted to the sulphate, Hampe being followed closely in order to ascertain what sources of error affected the work of the latter. Crystallized sulphate, $CuSO_4.5H_2O$ was purified with every precaution and made the basis of operations. Three series of experiments were carried out, the water being determined by loss of weight upon heating, and the copper being estimated electrolytically. In the first series the following data were found, the weights being reduced to a vacuum, as in all of Richards' determinations:

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[1] Compt. Rend., 97, 906. 1883.

[2] Proc. Amer. Acad., 26, 240. 1891.

	$CuSO_4$. 5 aq.	*$CuSO_4$ at 250°.*	*Cu.*
1	2.8815	.....	.7337
2	2.7152	.....	.6911
3	3.4639	2.2184	.8817

Hence the subjoined percentages:

	Water at 250°.	*Cu in Cryst. Salt.*	*Cu in $CuSO_4$.*
1	.....	25.462	.....
2	.....	25.452	.....
3	35.958	25.454	39.745
		Mean, 25.456	

In the second series of analyses, which are stated with much detail, several refinements were introduced, in order to estimate also the sulphuric acid. These will be considered later. The results, given below, are numbered consecutively with the former series:

	$CuSO_4$. 5 aq.	*$CuSO_4$ at 260°.*	*$CuSO_4$ at 360°.*	*Cu.*
4	3.06006	1.9597	1.95637	.77886
5	2.81840	1.8048	......	.71740
6	7.50490	4.8064	4.79826	1.90973

Hence percentages as follows:

	Water, 260°.	*Water, 360°.*	*Cu in Cryst. Salt.*	*Cu in $CuSO_4$, 260°.*	*Ditto, 360°.*
4	35.959	36.068	25.452	39.744	39.811
5	35.964	.....	25.454	39.750	.....
6	35.957	36.065	25.446	39.733	39.799
Mean,	35.960	36.067	25.450	39.742	39.805

Hampe worked with a sulphate dried at 250°, but these data show that a little water is retained at that temperature, and consequently that his results must have been too low. The third of Richards' series resembles the second, but extra precautions were taken to avoid conceivable errors.

	$CuSO_4$. 5 aq.	*$CuSO_4$ at 260°.*	*$CuSO_4$ at 370°.*	*Cu.*
7	2.88307	......	......	.73380
8	3.62913	2.32373	......	.92344
9	5.81352	......	3.71680	1.47926

And the percentages are:

	Water at 260°.	*At 370°.*	*Cu in Cryst. Salt.*	*Cu in $CuSO_4$.*
7	.....	.....	25.452	.....
8	35.970	.....	25.446	39.740 (260°)
9	.....	36.067	25.445	39.799 (370°)
			25.448	

In this series the determinations of sulphuric acid gave essentially the same results for all three samples of sulphate, although one was not dehydrated, and the others were heated to 260° and 370°, respectively. Hence the loss of weight in dehydration at either temperature represents water only, and does not involve partial decomposition of the sulphate. Between 360° and 400° copper sulphate is at essentially constant weight, but further experiments indicated that even at 400° it retained traces of water, and possibly as much as .042 per cent. The last trace is not expelled until the salt itself begins to decompose.

Richards also effected two syntheses of the sulphate directly from the metal by dissolving the latter in nitric acid, then evaporating to dryness with sulphuric acid, and heating to constant weight at 400°.

.67720 grm. Cu gave	1.7021 grm. $CuSO_4$.	39.786 per cent. Cu.	
1.00613 "	2.5292 "	39.781 "	

If we include these percentages in a series with the data from analyses 4, 6 and 9, which gave percentages of 39.811, 39.799 and 39.799, respectively, of copper in sulphate dried at 360° and upwards, the mean becomes

$$CuSO_4 : Cu :: 100 : 39.795, \pm .0036$$

Hence Cu = 63.499.

Since even this result is presumably too low, the other figures from sulphate dried at 250° must be rejected. Since Hampe's work on the sulphate is affected by the same sources of error, and apparently to a still greater extent, it need not be considered farther. As for Richards' nine determinations of Cu in $CuSO_4.5H_2O$, we may take them as one series giving a mean percentage of 25.451, ± .0011, and Cu = 63.55. This salt seems to retain occluded water, for the percentage of copper in it leads to a value for the atomic weight which is inconsistent with the best evidence, as will be seen later.

In the second and third series of Richards' experiments upon copper sulphate, the sulphuric acid was estimated by a method which gave valuable results. After the copper had been electrolytically precipitated, the acid which was set free was nearly neutralized by a weighed amount of pure sodium carbonate, and the slight excess remaining was determined by titration. Thus the weight of sodium carbonate equivalent to the copper was ascertained. The resulting solution of sodium sulphate was then evaporated to dryness, and a new ratio, connecting that salt with copper, was also determined. The cross ratio $Na_2CO_3 : Na_2SO_4$ has already been utilized in a previous chapter. The results, ignoring the weights of hydrated copper sulphate, are as follows, with the experiments numbered as before:

	Cu.	Na_2CO_3.	Na_2SO_4.
4	.77886	1.2993	1.7411
6	1.90973	3.1862	4.2679
7	.73380	1.22427	1.63994
8	.92344	1.54075	......
9	1.47926	......	3.30658

Hence,

$Cu : Na_2CO_3 :: 100 : x$.	$Cu : Na_2SO_4 :: 100 : x$
166.824	223.549
166.840	223.482
166.840	223.538
166.849	223.529
Mean, 166.838, ± .0035	Mean, 223.525, ± .0098
Hence Cu = 63.55.	Hence Cu = 63.571.

In one more experiment the sulphuric acid was weighed as barium sulphate, the latter being corrected for occluded salts. 3.1902 grm. $CuSO_4.5H_2O$ gave 2.9761 $BaSO_4$; hence $CuSO_4.5H_2O : BaSO_4 :: 100 : 93.289$. The sulphate contained 25.448 per cent. of Cu; hence $BaSO_4 : Cu :: 93.289 : 25.448$, and Cu = 63.676. Still other ratios can be deduced from Richards' work on the sulphate, but in view of the uncertainties relative to the water in the salt they are hardly worth computing.

In his third paper upon the atomic weight of copper,[1] Richards studied the dibromide, $CuBr_2$. In preparing this salt he used hydrobromic acid made from pure materials, and further purified by ten distillations. This was saturated with copper oxide prepared from pure electrolytic copper, and the solution obtained was proved to be free from basic salts. As the crystallized compound was not easily obtained in a satisfactory condition, weighed quantities of the solution were taken for analysis, in which, after expulsion of bromine by nitric and sulphuric acids, the copper was determined by electrolysis. In other portions of solution the bromine was precipitated by silver nitrate, and weighed as silver bromide. The first preliminary series of experiments gave the subjoined results, with vacuum weights as usual:

In 25 Grammes of Solution.

Cu.	$AgBr$.
.4164	2.4599
.4164	2.4605
.4164	2.4605
.4165	2.4599

Hence $2AgBr : Cu :: 100 : 16.927, \pm .0013$.

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[1] Proc. Amer. Acad., 25, 195. 1890.

The second, also preliminary series, was made with more dilute solutions, and came out as follows:

In 25 Grammes of Solution.

Cu.	*AgBr.*
.26190	1.5478
.26185	1.5477
	1.5479

Hence 2AgBr : Cu : : 100 : 16.919, ±.0012.

In the third series, two distinct lots of crystallized bromide were dissolved, and the solutions examined in the same way:

Cu.	*AgBr.*	*Ratio.*
.2500	1.4771	16.925
.5473	3.2348	16.919

Mean, 16.922, ± .0020

In the final set of analyses, the materials used were purified even more scrupulously than before, and the process was distinctly modified, as regards the determination of the bromine. The solution of the bromide was added to a solution of pure silver in nitric acid, not quite sufficient for complete precipitation. The slight excess of bromine was then determined by titration with a solution containing one gramme of silver to the litre. Thus silver proportional to the copper in the bromide was determined, and the silver bromide was weighed in a Gooch crucible as before. The results are subjoined:

In 50 Grammes of Solution.

Cu.	*Ag.*	*AgBr.*
.54755	1.8586	3.2350
.54750	1.8579	3.2340
	1.8583	3.2348

Hence Cu : 2Ag : : 100 : 339.392, ±.0108, and 2AgBr : Cu : : 100 : 16.927, ±.0012.

The latter ratio, combined with the results of the three preceding series, gives a general mean of:

2AgBr : Cu : : 100 : 16.924, ± .0007

Hence Cu = 63.566.

In his two earlier papers[1] Richards determined the copper-silver ratio directly—that is, without the weighing of any compound of either metal. By placing pure copper in an *ice-cold* solution of silver nitrate, metallic

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[1] Proc. Amer. Acad., 22, 346, and 23, 177. 1886 and 1887.

silver is thrown down, and the weights of the two metals were in equivalent proportions. In the first paper the following results were obtained. The third column gives the value of x in the ratio Cu : 2Ag : : 100 : x.

Cu taken.	*Ag found.*	*Ratio.*
.53875	1.8292	339.527
.56190	1.9076	339.491
1.00220	3.4016	339.414
1.30135	4.4173	339.440
.99870	3.39035	339.477
1.02050	3.4646	339.500
		Mean, 339.475, ± .0114

In the second paper Richards states that the silver of the fifth experiment, which had been dried at 150°, as were also the others, still retained water, to the extent of four-tenths milligramme in two grammes. If we assume this correction to be fairly uniform, as the concordance of the series indicates, and apply it throughout, the mean value for the ratio then becomes 339.408, ± .0114. This procedure, however, leaves the ratio in some uncertainty, and accordingly some new determinations were made, in which the silver, collected in a Gooch crucible, was heated to incipient redness before final weighing. Copper from two distinct sources was taken, and three experiments were carried out upon one sample to two with the other. Treating both sets as one series, the results were as follows:

Cu taken.	*Ag found.*	*Ratio.*
.75760	2.5713	339.40
.95040	3.2256	339.39
.75993	2.5794	339.42
1.02060	3.4640	339.42
.90460	3.0701	339.39
		Mean, 339.404, ± .0046

a value practically identical with the corrected mean of the previous determinations, and with that found in the later experiments upon copper bromide. Hence Cu = 63.570.

In various electrical investigations the same ratio, the electrochemical equivalent of copper, has been repeatedly measured, and the later results of Lord Rayleigh and Mrs. Sidgewick,[1] Gray,[2] Shaw,[3] and Vanni[4] may properly be included in this discussion. As the data are somewhat differently stated, I have reduced them all to the common standard adopted

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[1] Phil. Trans., 175, 458.

[2] Phil. Mag. (5), 22, 389.

[3] British Assoc. Report, 1886. Abstract in Phil. Mag. (5), 23, 138.

[4] Ann. der Phys. (Wiedemann's) (2), 44. 214.

above. Gray gives two sets of measurements, one made with large and the other with small metallic plates:

Rayleigh and S.	*Gray 1.*	*Gray 2.*	*Shaw.*	*Vanni.*
340.483	341.297	340.252	339.68	340.483
340.832	341.413	339.674	340.05	340.600
340.367	340.815	340.020	339.84	340.367
———	340.252	339.905	339.71	340.252
340.561,	339.905	339.674	340.04	340.600
± .0935	341.064	339.328	339.94	340.136
	340.832	340.136	340.35	———
	341.297	340.136	339.82	340.406,
	341.064	340.136	340.09	± .0520
	341.413	340.020	339.84	
	———	340.020	339.90	
	340.935,	340.136	339.98	
	± .1072	———	340.14	
		339.953,	340.56	
		± .0521	339.82	
			339.983,	
			± .0411	

The lack of sharp concordance in these data and the consequently high probable errors seem to indicate a distinct superiority of the purely chemical method of determination over that adopted by the physicist. This supposition is strengthened by the electrochemical experiments of Richards, Collins and Heimrod,[1] who precipitated copper and silver simultaneously in the same current of electricity. Their first series, with vacuum weights, is as follows:

Weight Cu.	*Weight Ag.*	*Ratio.*
.44478	1.51064	339.64
.31645	1.07473	339.63
.24968	.84792	339.60
1.02186	3.47056	339.63
.66166	2.24538	339.36
.63027	2.14050	339.63
.45919	1.55905	339.72
.39177	1.33071	339.67
1.11030	3.76990	339.54
.67564	2.29655	339.91
.48232	1.63768	339.54
.83092	2.82203	339.63
.63491	2.15735	339.79
.70102	2.37868	339.32
.84469	2.86608	339.55
.87462	2.97114	339.71
.69405	2.35683	339.58
		Mean, 339.615, ± .0230

Hence Cu = 63.532.

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[1] Proc. Amer. Acad., 35, 123. 1899.

In a second series of experiments the copper was deposited from solutions saturated with cuprous sulphate:

Weight Cu.	*Weight Ag.*	*Ratio.*
.71847	2.43935	339.52
.71861	2.43940	339.46
.72019	2.44603	339.64
.97193	3.30100	339.63
.50916	1.72859	339.50
.76188	2.58664	339.51

Mean, 339.543, ± .0200

Hence Cu=63.544.

In the foregoing series the temperature of the solution was 0°. Two experiments at higher temperatures, 56°-61°, gave lower values for the ratio, and consequently a higher atomic weight for copper:

Weight Cu.	*Weight Ag.*	*Ratio.*
.97295	3.30100	339.28
.76214	2.58664	339.39

Mean, 339.335, ± .0370

Hence Cu=63.569.

There is also an electrochemical series of determinations by Gallo,[1] of slight importance. The figures with vacuum weights are—

Weight Cu.	*Weight Ag.*	*Ratio.*
.21805	.73937	339.083
.27153	.92062	339.049
.19001	.64571	339.829
.39585	1.34578	339.972

Mean, 339.483, ± .164

Hence Cu=63.555.

The general combination of all the data relative to the copper-silver ratio is as follows:

Richards, first series, corrected	339.408, ± .0114
Richards, second series	339.404, ± .0046
Richards, $CuBr_2$ series	339.392, ± .0108
Richards, Collins, and Heimrod, first	339.615, ± .0230
Richards, Collins, and Heimrod, second	339.543, ± .0200
Richards, Collins and Heimrod, third	339.335, ± .0370
Rayleigh and Sidgewick	340.561, ± .0935
Gray, large plates	340.935, ± .1072
Gray, small plates	339.953, ± .0521
Shaw	339.983, ± .0411
Vanni	340.406, ± .0520
Gallo	339.483, ± .1640
General mean	339.423, ± .0038

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[1] Atti Acad. Lincei (5), 14, 23. 1905.

If we combine Richards' three series into a general mean separately, the value found for the ratio becomes 339.402, ±.0040. The other determinations, having high probable errors, affect this mean but slightly, and it makes little difference whether they are retained or rejected.

We now have the following ratios from which to compute the atomic weight of copper:

(1). Percentage of Cu in CuO, 79.836, ± .0010
(2). Percentage of Cu in $CuSO_4$, 39.795, ± .0036
(3). Percentage of Cu in $CuSO_4$, $5H_2O$, 25.451, ± .0011
(4). Percentage of CuO in $CuSO_4$, 49.816, ± .0017
(5). $Cu:Na_2CO_3::100:166.838$, ± .0035
(6). $Cu:Na_2SO_4::100:223.525$, ± .0098
(7). $BaSO_4:Cu::93.289:25.448$
(8). $2AgBr:Cu::100:16.924$, ± .0007
(9). $Cu:2Ag::100:339.423$, ± .0038

Ratio 7 rests upon a single experiment, and must be arbitrarily weighted. For this purpose, the value for copper derived from it may be given double the probable error of the highest among the other determinations. To reduce the ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
C = 12.0038, ± .0002
Na = 23.0108, ± .00024
Ba = 137.363, ± .0025
H = 1.0079, ± .00001

Hence,

From ratio 1	Cu = 63.3493, ± .0032
“ “ 4	63.4796, ± .0039
“ “ 2	63.4993, ± .0069
“ “ 3	63.5497, ± .0033
“ “ 5	63.5499, ± .0024
“ “ 8	63.5664, ± .0027
“ “ 9	63.5667, ± .00075
“ “ 6	63.5714, ± .0029
“ “ 7	63.6765, ± .0138
General mean,	Cu = 63.5550, ± .00063

This value is possibly, but not certainly, a little too low. The rejection of the first value, derived from copper oxide, raises the general mean to 63.561, which may be nearer the truth.

GOLD.

Among the early estimates of the atomic weight of gold the only ones worthy of consideration are those of Berzelius and Levol.

The earliest method adopted by Berzelius[1] was that of precipitating a solution of gold chloride by means of a weighed quantity of metallic mercury. The weight of gold thus thrown down gave the ratio between the atomic weights of the two metals. In the single experiment which Berzelius publishes, 142.9 parts of Hg precipitated 93.55 of Au. Hence if Hg=200, Au=196.397.

In a later investigation[2] Berzelius resorted to the analysis of potassio-auric chloride, $KCl.AuCl_3$. Weighed quantities of this salt were ignited in hydrogen; the resulting gold and potassium chloride were separated by means of water, and both were collected and estimated. The loss of weight upon ignition was, of course, chlorine. As the salt could not be perfectly dried without loss of chlorine, the atomic weight under investigation must be determined by the ratio between the KCl and the Au. If we reduce to a common standard, and compare with 100 parts of KCl, the equivalent amounts of gold will be those which I give in the last of the subjoined columns:

4.1445	grm. $KAuCl_4$ gave	.8185	grm. KCl and	2.159	grm. Au.	263.775
2.2495	"	.44425	"	1.172	"	263.815
5.1300	"	1.01375	"	2.67225	"	263.600
3.4130	"	.674	"	1.77725	"	263.687
4.19975	"	.8295	"	2.188	"	263.773

Mean, 263.730, ± .026

Hence Au=196.69.

Still a third series of experiments by Berzelius[3] may be included here. In order to establish the atomic weight of phosphorus he employed that substance to precipitate gold from a solution of gold chloride in excess. Between the weight of phosphorus taken and the weight of gold obtained it was easy to fix a ratio. Since the atomic weight of phosphorus has been better established by other methods, we may properly reverse this ratio and apply it to our discussion of gold. One hundred parts of P precipitate the quantities of Au given in the third column:

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[1] Poggend. Annalen, 8, 177.

[2] Lehrbuch, 5 Aufl., 3, 1212.

[3] Lehrbuch, 5 Aufl., 3, 1188.

.829 grm. P precipitated	8.714 grm. Au.	1051.15
.754 "	7.930 "	1051.73
	Mean,	1051.44, ± .196

Hence, if P = 31, Au = 195.568.

Levol's[1] estimation of the atomic weight under consideration can hardly have much value. A weighed quantity of gold was converted in a flask into $AuCl_3$. This was reduced by a stream of sulphur dioxide, and the resulting sulphuric acid was determined as $BaSO_4$. One gramme of gold gave 1.782 grm. $BaSO_4$. Hence Au = 196.49.

All these values may be neglected as worthless, except that derived from Berzelius' K_2AuCl_5 series.

In 1886 Krüss[2] published the first of the recent determinations of the atomic weight under consideration, several distinct methods being recorded. First, in a solution of pure auric chloride the gold was precipitated by means of aqueous sulphurous acid. In the filtrate from the gold the chlorine was thrown down as silver chloride, and thus the ratio Au : 3AgCl was measured. I subjoin Krüss' weights, together with a third column giving the gold equivalent to 100 parts of silver chloride:

Au.	*AgCl.*	*Ratio.*
7.72076	16.84737	45.828
5.68290	12.40425	45.814
3.24773	7.08667	45.828
4.49167	9.80475	45.811
3.47949	7.59300	45.825
3.26836	7.13132	45.832
5.16181	11.26524	45.821
4.86044	10.60431	45.834
	Mean,	45.824, ± .0020

Hence Au = 197.05.

The remainder of Krüss' determinations were made with potassium auribromide, $KAuBr_4$, and with this salt several ratios were measured. The salt was prepared from pure materials, repeatedly recrystallized under precautions to exclude access of atmospheric dust, and dried over phosphorus pentoxide. First, its percentage of gold was determined, sometimes by reduction with sulphurous acid, sometimes by heating in a stream of hydrogen. For this ratio, the weights and percentages are as follows, the experiments being numbered for further reference, and the reducing agent being indicated:

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[1] Untersuchungen über das Atomgewicht des Goldes. München, 1886. 112 pp., 8vo.

[2] Ann. Chim. Phys. (3), 30, 355. 1850.

	$KAuBr_4$.	Au.	Per cent.
1. SO_2	10.64821	3.77753	35.476
2. SO_2	4.71974	1.67330	35.453
3. H	7.05762	2.50122	35.440
4. H	4.49558	1.59434	35.465
5. SO_2	8.72302	3.09448	35.475
6. SO_2	7.66932	2.71860	35.448
7. SO_2	7.15498	2.53695	35.457
8. H	12.26334	4.34997	35.471
9. H	7.10342	2.51919	35.465
			Mean, 35.461, ± .0028

Hence Au = 197.13.

In five of the foregoing experiments the reductions were effected with sulphurous acid; and in these, after filtering off the gold, the bromine was thrown down and weighed as silver bromide. This, in comparison with the gold, gives the ratio Au : 4AgBr : : 100 : x:

	Au.	4AgBr.	Ratio.
1..............	3.77753	14.39542	381.080
2..............	1.67330	6.37952	381.254
5..............	3.09448	11.78993	380.999
6..............	2.71860	10.35902	381.042
7..............	2.53695	9.66117	380.731
			Mean, 381.021, ± .057

Hence Au : AgBr : : 100 : 95.255, ± .0142, and Au = 197.16.

In the remaining experiments, Nos. 3, 4, 8 and 9, the $KAuBr_4$ was reduced in a stream of hydrogen, the loss of weight, Br_3, being noted. In the residue the gold was determined, as noted above, and the KBr was also collected and weighed. The weights were as follows:

	Au.	Loss, Br_3.	KBr.
3...............	2.50122	3.04422	1.51090
4...............	1.59434	1.93937	.96243
8...............	4.34997	5.29316	2.62700
9...............	2.51919	3.06534	1.52153

From these data we obtain two more ratios, viz., Au : Br_3 : : 100 : x, and Au : KBr : : 100 : x, thus:

	Au : Br_3.	Au : KBr.
3....................	121.710	60.405
4....................	121.641	60.365
8....................	121.683	60.391
9....................	121.680	60.398
	Mean, 121.678, ± .0100	Mean, 60.390, ± .0059

Hence Au = 197.04, and 197.08.

From all the ratios, taken together, Krüss deduces a final value of Au=197.13, if O=16. It is obviously possible to derive still other ratios from the results given, but to do so would be to depart unnecessarily from the author's methods as stated by himself.

Thorpe and Laurie,[1] whose work appeared shortly after that of Krüss, also made use of the salt $KAuBr_4$, but, on account of difficulty in drying it without change, they did not weigh it directly. After proving the constancy in it of the ratio Au : KBr, even after repeated crystallizations, they adopted the following method: The unweighed salt was heated with gradual increase of temperature, up to about 160°, for several hours, and afterwards more strongly over a small Bunsen flame. This was done in a porcelain crucible, tared by another in weighing, which latter was treated in precisely the same way. The residue, KBr+Au, was weighed, the KBr dissolved out, and the gold then weighed separately. The weight of KBr was taken by difference. The ratio Au : KBr : : 100 : x appears in a third column:

Au.	*KBr.*	*Ratio.*
6.19001	3.73440	60.329
4.76957	2.87715	60.323
4.14050	2.49822	60.336
3.60344	2.17440	60.342
3.67963	2.21978	60.326
4.57757	2.76195	60.337
5.36659	3.23821	60.326
5.16406	3.11533	60.327
		Mean, 60.331, ± .0016

Hence Au=197.28.

This mean combines with Krüss' thus:

Krüss	60.390, ± .0059
Thorpe and Laurie	60.331, ± .0016
General mean	60.338, ± .0015

The potassium bromide of the previous experiments was next titrated with a solution of pure silver by Stas' method, the operation being performed in red light. Thus we get the following data for the ratio Ag : Au : : 100 : x, using the weights of gold already obtained:

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[1] Journ. Chem. Soc., 51, 565. 1887.

Ag.	*Au.*	*Ratio.*
3.38451	6.19001	182.893
2.60896	4.76957	182.813
2.28830	4.18266	182.786
2.26415	4.14050	182.868
1.97147	3.60344	182.775
2.01292	3.67963	182.801
2.50334	4.57757	182.863
2.93608	5.36659	182.780
2.82401	5.16406	182.865

Mean, 182.827, ± .0101

Hence Au = 197.24.

Finally, in eight of these experiments, the silver bromide formed during titration was collected and weighed, giving values for the ratio Au : AgBr :: 100 : x, as follows:

Au.	*AgBr.*	*Ratio.*
6.19001	5.89199	95.186
4.76957	4.54261	95.242
4.18266	3.98288	95.224
4.14050	3.94309	95.232
3.60344	3.43015	95.191
3.67963	3.50207	95.175
4.57757	4.35736	95.189
5.36659	5.11045	95.227

Mean, 95.208, ± .0061
Krüss found, 95.255, ± .0142

General mean, 95.222, ± .0056

From Thorpe and Laurie's mean, Au = 197.25.

From the second and third of the ratios measured by Thorpe and Laurie an independent value for the ratio Ag : Br may be computed. It becomes 100 : 74.072, which agrees fairly with the direct determinations made by other chemists. Similarly, the ratios Ag : KBr and AgBr : KBr may be calculated, giving additional checks upon the accuracy of the manipulation, though not upon the purity of the original material studied.

Thorpe and Laurie suggest objections to the work done by Krüss, on the ground that the salt $KAuBr_4$ cannot be completely dried without loss of bromine. This suggestion led to a controversy between them and Krüss, which in effect was briefly as follows:

First, Krüss[1] urges that the potassium auribromide ordinarily contains traces of free gold, not belonging to the salt, produced by the reducing action of dust particles taken up from the air. He applies a correction

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[1] Ber. Deutsch. chem. Gesell., 20, 2365. 1887.

for this supposed free gold to the determinations made by Thorpe and Laurie, and thus brings their results into harmony with his own. To this argument Thorpe and Laurie[1] reply, somewhat in detail, stating that the error indicated was guarded against by them, and that they had dissolved quantities of from eight to nineteen grammes of the auribromide without a trace of free gold becoming visible. A final note in defense of his work was published by Krüss a little later.[2]

In 1889 an elaborate set of determinations of this constant was published by Mallet,[3] whose experiments are classified into seven distinct series. First, a neutral solution of auric chloride was prepared, which was weighed off in two approximately equal portions. In one of these the gold was precipitated by pure sulphurous acid, collected, washed, dried, ignited in a Sprengel vacuum, and weighed. To the second portion a solution containing a known weight of pure silver was added. After filtering, with all due precautions, the silver remaining in the filtrate was determined by titration with a weighed solution of pure hydrobromic acid. We have thus a weight of gold, and the weight of silver needed to precipitate the three atoms of chlorine combined with it: in other words, the ratio 3Ag : Au :: 100 : x. All weights in this and the subsequent series are reduced to a vacuum standard, and all weighings were made against corresponding tares:

Au.	*Ag.*	*Ratio.*
7.6075	12.4875	60.921
8.4212	13.8280	60.900
6.9407	11.3973	60.898
3.3682	5.5286	60.923
2.8244	4.6371	60.909
		Mean, 60.910, ± .0034

Hence Ag : Au :: 100 : 182.730, ± .0102, and Au = 197.13.

The second series of determinations was essentially like the first, except that auric bromide was taken instead of the chloride. The ratio measured, 3Ag : Au, is precisely the same as before. Results as follows:

Au.	*Ag.*	*Ratio.*
8.2345	13.5149	60.929
7.6901	12.6251	60.911
10.5233	17.2666	60.945
2.7498	4.5141	60.916
3.5620	5.8471	60.919
3.9081	6.4129	60.941
		Mean, 60.927, ± .0038

Hence Ag : Au :: 100 : 182.781, ± .0114, and Au = 197.18.

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[1] Berichte, 20, 3036, and Journ. Chem. Soc., 51, 866. 1887.

[2] Berichte, 21, 126. 1888.

[3] Philosophical Transactions, 180, 395. 1889.

In the third series of experiments the salt $KAuBr_4$ was taken, purified by five recrystallizations. The solution of this was weighed out into nearly equal parts, the gold being measured as in the two preceding series in one portion, and the bromine thrown down by a standard silver solution as before. This gives the ratio 4Ag : Au :: 100 : x.

Au.	*Ag.*	*Ratio.*
5.7048	12.4851	45.693
7.9612	17.4193	45.693
2.4455	5.3513	45.699
4.1632	9.1153	45.673

Mean, 45.689, ± .0040

Hence Ag : Au :: 100 : 182.756, ± .0160, and Au = 197.16.

The fifth series of determinations, which for present purposes naturally precedes the fourth, was electrolytic in character, gold and silver being simultaneously precipitated by the same current. The gold was in solution as potassium aurocyanide, and the silver in the form of potassium silver cyanide. The equivalent weights of the two metals, thrown down in the same time, were as follows, giving directly the ratio Ag : Au :: 100 : x.

Au.	*Ag.*	*Ratio.*
5.2721	2.8849	182.748
6.3088	3.4487	182.933
4.2770	2.3393	182.832
3.5123	1.9223	182.713
3.6804	2.0132	182.814

Mean, 182.808, ± .0256

Hence Au = 197.22.

This mean may be combined with the preceding means, and also with the determination of the same ratio by Thorpe and Laurie, thus:

Thorpe and Laurie...........	182.827, ± .0101
Mallet, chloride series........	182.730, ± .0102
Mallet, bromide series........	182.781, ± .0114
Mallet, $KAuBr_4$ series........	182.756, ± .0160
Mallet, electrolytic	182.808, ± .0256
General mean	182.778, ± .0055

In Mallet's fourth series a radically new method was employed. Trimethyl-ammonium aurichloride, $N(CH_3)_3HAuCl_4$, was decomposed by heat, and the residual gold was determined. In order to avoid loss by spattering, the salt was heated in a crucible under a layer of fine siliceous sand of known weight. Several crops of crystals of the salt were studied, as a check against impurities, but all gave concordant values.

Salt.	*Residual Au.*	*Per cent. Au.*
14.9072	7.3754	49.475
15.5263	7.6831	49.484
10.4523	5.1712	49.474
6.5912	3.2603	49.464
5.5744	2.7579	49.474
		Mean, 49.474, ± .0021

Hence Au=197.73.

In his sixth and seventh series Mallet seeks to establish, by direct measurement, the ratio between hydrogen and gold. In their experimental details his methods are somewhat elaborate, and only the processes, in the most general way, can be indicated here. First, gold was precipitated electrolytically from a solution of potassium aurocyanide, and its weight was compared with that of the amount of hydrogen simultaneously liberated in a voltameter by the same current in the same time. The hydrogen was measured, and its weight was then computed from its density. The volumes are given, of course, at 0° and 760 mm.

Wt. Au.	*Vol. H, cc.*	*Wt. H.*
4.0472	228.64	.0205483
4.0226	227.03	.0204046
4.0955	231.55	.0208103

These data, with the weight of one litre of hydrogen taken as 0.89872 gramme, give the subjoined values in the ratio H : Au :: 1 : x.

196.960
197.151
196.805

Mean, 196.972, ± .0675

In the last series of experiments a known quantity of metallic zinc was dissolved in dilute sulphuric acid, and the amount of hydrogen evolved was measured. Then a solution of pure auric chloride or bromide was treated with a definite weight of the same zinc, and the quantity of gold thrown down was determined. The zinc itself was purified by fractional distillation in a Sprengel vacuum. From these data the ratio 3H : Au was computed by direct comparison of the weight of gold and that of the liberated hydrogen. The results were as follows:

Wt. Au.	*Vol. H. cc.*	*Wt. H.*
10.3512	1756.10	.157824
8.2525	1400.38	.125857
8.1004	1374.87	.123565
3.2913	558.64	.050206
3.4835	590.93	.053109
3.6421	618.11	.055551

Hence for the ratio 3H : Au : : 1 : x we have:

65.587
65.571
65.557
65.556
65.593
65.563

Mean, 65.571, ± .00436

And H : Au : : 1 : 196.713, ± .0131. This, combined with the value found in the preceding series, gives a general mean of 196.722, ± .0129.

The ratios available for gold are now as follows:

(1). KCl : Au : : 100 : 263.730, ± .026
(2). 3AgCl : Au : : 100 : 45.824, ± .0020
(3). $KAuBr_4$: Au : : 100 : 35.461, ± .0028
(4). Au : AgBr : : 100 : 95.222, ± .0056
(5). Au : 3Br : : 100 : 121.678, ± .0100
(6). Au : KBr : : 100 : 60.338, ± .0015
(7). Ag : Au : : 100 : 182.778, ± .0055
(8). $NC_3H_{10}AuCl_4$: Au : : 100 : 49.474, ± .0021
(9). H : Au : : 1 : 196.722, ± .0129

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
N = 14.0101, ± .0001
K = 39.0999, ± .0002
C = 12.0038, ± .0002
H = 1.00779, ± .00001

Hence,

From ratio 1 ....Au = 196.687, ± .0195
" " 5 ....197.044, ± .0163
" " 2 ....197.050, ± .0086
" " 3 ....197.131, ± .0178
" " 7 ....197.181, ± .0060
" " 4 ....197.223, ± .0116
" " 6 ....197.255, ± .0049
" " 8 ....197.728, ± .0118
" " 9 ....198.254, ± .0130

General mean, Au = 197.269, ± .0030

Rejection of the very doubtful values from ratios 1, 8 and 9 lowers the mean to 197.19. The atomic weight of gold is probably not far from 197.2.

The ninth or last value in the foregoing series represents Mallet's ratio between gold and hydrogen, and is peculiarly instructive. In Mallet's paper the several ratios determined were discussed upon the basis of O=15.96, referred to hydrogen as unity. This, on the oxygen scale, is equivalent to H=1.0025. On that basis the determination in question agreed well with the others; but with H=1.00779, the present value, it is enormously raised. The former agreement between the several series of gold values was therefore only apparent, and shows that concordance among determinations may be only coincidence, and no real proof of accuracy. It is probable, furthermore, that direct comparisons of metals with hydrogen cannot give good measurements of atomic weights, for several reasons. First, it is not possible to be certain that every trace of hydrogen has been collected and measured, and any loss tends to raise the apparent atomic weight of the metal studied; secondly, the weight of the hydrogen is computed from its volume, and a slight change in the factors used in reduction of the observations may make a considerable difference in the final result. These uncertainties exist in all determinations of atomic weights hitherto made by the hydrogen method.

CALCIUM.

Much of the older work on the atomic weight of calcium, including the earliest determinations by Berzelius, may be disregarded as having no present value. Baup's[1] analyses of organic salts of calcium are interesting, but carry no weight now. They led to the value Ca=39.98. As for Salvétat's[2] determination, that was merely given as a statement of results, without such details as would make his work available for discussion.

The largest factor in measuring the atomic weight of calcium, is the composition of calcium carbonate, as determined by several investigators. This will be considered first, and the determinations based upon calcium sulphate and calcium chloride follow later.

In 1842 Dumas[3] made three ignitions of Iceland spar, and determined the percentages of carbon dioxide driven off and of lime remaining. The impurities of the material were also determined, the correction for them applied, and the weighings reduced to a vacuum standard. His figures are as follows:

49.916 grm.	$CaCO_3$	gave 28.016 grm.	CaO.	56.12	per cent.
50.497	"	28.305	"	56.04	"
64.508	"	36.167	"	56.06	"
				Mean, 56.073, ± .016	

Hence Ca=40.171.

About this same time Erdmann and Marchand[4] began their researches upon the same subject. Two ignitions of spar, containing .04 per cent. of impurity, gave resepectively 56.09 and 56.18 per cent. of residue; but these results are not exact enough for us to consider further. Four other results obtained with artificial calcium carbonate are more noteworthy. The carbonate was precipitated from a solution of pure calcium chloride by ammonium carbonate, was washed thoroughly with hot water, and dried at a temperature of 180°. With this preparation the following residues of lime were obtained:

56.03
55.98
56.00
55.99

Mean, 56.00, ± .007

Hence Ca=40.005.

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[1] Bull. Universelle des Sciences de Genève, 39, 347. 1842.

[2] Compt. Rend., 17, 318. 1843.

[3] Compt. Rend., 14, 547. 1842.

[4] Journ. prakt. Chem., 26, 472. 1842.

It was subsequently shown by Berzelius that calcium carbonate prepared by this method retains traces of water even at 200°, and that minute quantities of chloride are also held by it. These sources of error are, however, in opposite directions, since one would tend to diminish and the other to increase the weight of residue.

In the same paper there are also two direct estimations of carbonic acid in pure Iceland spar, which correspond to the following percentages of lime:

56.00
56.02

Mean, 56.01, ± .007

In a still later paper[1] the same investigators give another series of results based upon the ignition of Iceland spar. The impurities were carefully estimated, and the percentages of lime are suitably corrected:

4.2134	grm. $CaCO_3$ gave	2.3594	grm. CaO.	55.997 per cent.
15.1385	"	8.4810	"	56.022 "
23.5503	"	13.1958	"	56.031 "
23.6390	"	13.2456	"	56.032 "
42.0295	"	23.5533	"	56.044 "
49.7007	"	27.8536	"	56.042 "
				Mean, 56.028, ± .0047

Hence Ca = 40.068.

Six years later Erdmann and Marchand[2] published one more result upon the ignition of calcium carbonate. They found that the compound began giving off carbon dioxide below the temperature at which their previous samples had been dried, or about 200°, and that, on the other hand, traces of the dioxide were retained by the lime after ignition. These two errors do not compensate each other, since both tend to raise the percentage of lime. In the one experiment now under consideration these errors were accurately estimated, and the needful corrections were applied to the final result. The percentage of residual lime in this case was 55.998. This agrees tolerably well with the figures found in the direct estimation of carbonic acid, and, if combined with those two, gives a mean for all three of 56.006, ± .0043.

Hence Ca = 40.018.

Herzfeld,[3] in his determinations of atomic weight, made use of artificial calcium carbonate. The lime was prepared by ignition of the oxalate, and then converted into bicarbonate by treatment with solid

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[1] Journ. prakt. Chem., 31, 269. 1844.

[2] Journ. prakt. Chem., 50, 237. 1850.

[3] Zeitschr. Ver. Rübenzucker Industrie, 47, 497. 1897. Partly reproduced in Ber., 34, 559. 1904.

carbon dioxide and water under pressure. By heating in a silver dish the bicarbonate was converted into the normal salt, which was weighed, and then ignited at a temperature of 1300°-1400°. The data are as follows:

3.9772	grm. CaCO$_3$ gave	2.2268	grm. CaO.	55.989	per cent.
2.3614	“	1.3218	“	55.975	“
3.2966	“	1.8456	“	55.985	“

Mean, 55.983, ± .0028

Hence Ca=39.966.

Hinrichsen,[1] in his two separate communications, gives analyses of spar from two distinct localities, namely, Iceland and the Crimea. In each case very small quantities of impurity were present, which were carefully determined and corrected for. The spar, previously freed from all traces of moisture, was ignited in an electric furnace, at a temperature between 1200° and 1400°. The results obtained, with all corrections applied, and vacuum weights, are subjoined:

First Series.

30.72157	grm. CaCO$_3$ gave	17.22354	grm. CaO.	56.0633	per cent.
32.77791	“	18.375587	“	56.0617	“
34.45625	“	19.31698	“	56.0623	“
33.36885	“	18.70723	“	56.0620	“

Second Series.

31.20762	grm. CaCO$_3$ gave	17.49526	grm. CaO.	59.0608	per cent.
22.00588	“	12.33642	“	56.0602	“

Mean of both series as one, 56.0617, ± .0003

Hence Ca=40.145.

Combining all these determinations, we have for the percentage of CaO from CaCO$_3$:

Dumas	56.073,	± .016
Erdmann and Marchand, 1	56.000,	± .007
Erdmann and Marchand, 2	56.028,	± .0047
Erdmann and Marchand, 3	56.006,	± .0043
Herzfeld	55.983,	± .0028
Hinrichsen	56.0617,	± .0003
General mean	56.0603,	± .0003

The effect of this combination is practically to discard all of the determinations except that of Hinrichsen. Herzfeld's figures are certainly too low, and probably because of undetermined impurity in his

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[1] Zeitschr. physikal. Chem., 39, 311, 1901; and 40, 747, 1902.

artificial carbonate. The extreme difficulty of preparing absolutely pure compounds of calcium is well known.[1]

In the earliest of the three papers by Erdmann and Marchand there is also given a series of determinations of the ratio between calcium carbonate and sulphate. Pure Iceland spar was carefully converted into calcium sulphate, and the gain in weight noted. One hundred parts of spar gave of sulphate:

136.07
136.06
136.02
136.06

Mean, 136.0525, ± .0071

Hence Ca = 40.025.

In 1843 the atomic weight of calcium was redetermined by Berzelius,[2] who investigated the ratio between lime and calcium sulphate. The calcium was first precipitated from a pure solution of nitrate by means of ammonium carbonate, and the thoroughly washed precipitate was dried and strongly ignited in order to obtain lime wholly free from extraneous matter. This lime was then, with suitable precautions, treated with sulphuric acid, and the resulting sulphate was weighed. Correction was applied for the trace of solid impurity contained in the acid, but not for the weighing in air. The figures in the last column represent the percentage of weight gained by the lime upon conversion into sulphate:

1.80425 grm. CaO	gained	2.56735	grm.	142.295
2.50400	"	3.57050	"	142.592
3.90000	"	5.55140	"	142.343
3.04250	"	4.32650	"	142.202
3.45900	"	4.93140	"	142.567

Mean, 142.3998, ± .0518

Hence Ca = 40.227.

The atomic weight of calcium has been several times computed from analyses of the chloride. The earliest determination by Berzelius[3] was based upon this compound, and Marignac[4] also used it in some provisional experiments, to which, however, he assigns little importance. They gave values for Ca far in excess of the truth. Dumas[5] also published a series of determinations of more than questionable value. Supposedly pure

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[1] See Stas, Oeuvres Complètes, 3, 337.

[2] Journ. prakt. Chem., 31, 263. Ann. Chem. Pharm., 46, 241.

[3] Poggend. Annalen, 8, 189.

[4] Oeuvres Complètes, 1, 90.

[5] Ann. Chim. Phys. (3), 55, 129. 1859. Ann. Chem. Pharm., 113, 34.

calcium chloride was first ignited in a stream of dry hydrochloric acid, and the solution of this salt was afterwards titrated with a silver solution in the usual way. The $CaCl_2$ proportional to 100 parts of Ag is given in a third column:

2.738 grm. $CaCl_2$	= 5.309 grm. Ag.	51.573
2.436 "	4.731 "	51.490
1.859 "	3.617 "	51.396
2.771 "	5.3885 "	51.424
2.240 "	4.3585 "	51.394
		Mean, 51.4554, ± .0230

Hence Ca = 40.103.

Better results were obtained by Richards.[1] Calcium chloride, purified by repeated crystallizations, and fused in a stream of nitrogen, was precipitated by a solution of silver, and the silver chloride so produced was weighed. The data, with vacuum weights, are subjoined:

1.56454 grm. $CaCl_2$ gave	4.0409 AgCl.	Ratio, 38.7177
3.57630 "	9.2361 "	" 38.7209
3.59281 "	9.2788 "	" 38.7206
5.00880 "	12.9364 "	" 38.7187
9.00246 "	23.2506 "	" 38.7197
		Mean, 38.7195, ± .0004

Hence Ca = 40.083.

There are now five independent ratios for calcium, as follows:

(1). $CaCO_3:CaO::100:56.0603$, ± .0003
(2). $CaO:SO_3::100:142.3998$, ± .0518
(3). $CaCO_3:CaSO_4::100:136.0525$, ± .0071
(4). $2Ag:CaCl_2::100:51.4554$, ± .0230
(5). $2AgCl:CaCl_2::100:38.7195$, ± .0004

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
S = 32.0667, ± .00075
C = 12.0038, ± .0002

Hence,

From ratio 3	Ca = 40.0250, ± .0200
" " 5	40.0829, ± .0013
" " 4	40.1034, ± .0497
" " 1	40.1421, ± .00055
" " 2	40.2267, ± .0207
General mean,	Ca = 40.1323, ± .0005

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[1] Journ. Amer. Chem. Soc., 24, 374. 1902.

STRONTIUM.

The ratios which fix the atomic weight of strontium resemble in general terms those relating to barium, only they are fewer in number and represent a smaller amount of work. The early experiments of Stromeyer,[1] who measured the volume of CO_2 evolved from a known weight of strontium carbonate, are hardly available for the present discussion. So also we may exclude the determination by Salvétat,[2] who neglected to publish sufficient details.

Taking the ratio between strontium chloride and silver first in order, we have series of figures by Pelouze, Dumas, Marignac and Richards. Pelouze[3] employed the volumetric method to be described under barium, and in two experiments obtained the subjoined results. In another column I append the ratio between $SrCl_2$ and 100 parts of silver:

1.480 grm.	$SrCl_2$ =	2.014 grm.	Ag.	73.486
2.210	"	3.008	"	73.471
				Mean, 73.4781 ± .0050

Hence Sr = 87.614.

Dumas,[4] by the same general method, made sets of experiments with three samples of chloride which had previously been fused in a current of dry hydrochloric acid. His results, expressed in the usual way, are as follows:

Series A.

3.137 grm.	$SrCl_2$ =	4.280 grm.	Ag.	Ratio,	73.2944
1.982	"	2.705	"	"	73.2717
3.041	"	4.142	"	"	73.4186
3.099	"	4.219	"	"	73.4534
				Mean,	73.3595

Series B.

3.356 grm.	$SrCl_2$ =	4.574 grm.	Ag.	Ratio,	73.3713
6.3645	"	8.667	"	"	73.4327
7.131	"	9.712	"	"	73.4246
				Mean,	73.4095

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[1] Schweigg. Journ., 19, 228. 1816.

[2] Compt. Rend., 17, 318. 1843.

[3] Compt. Rend., 20, 1047. 1845.

[4] Ann. Chim. Phys. (3), 55, 29. 1859. Ann. Chem. Pharm., 113, 34.

Series C.

7.213 grm. $SrCl_2$	= 9.811 grm. Ag.	Ratio.	73.5195
2.206 "	3.006 "	"	73.3866
4.268 "	5.816 "	"	73.5529
4.018 "	5.477 "	"	73.3613
		Mean,	73.4551
		Mean of all as one series,	73.4079, ± .0170

Hence Sr = 87.468.

The foregoing determinations are now supplanted by the much more recent work of Richards,[1] who fused his strontium chloride in a stream of gaseous hydrochloric acid and nitrogen, and adopted all of the precautions relative to the solubility of silver chloride which modern experience has shown to be necessary. The results, with vacuum weights, follow:

4.2516 grm. $SrCl_2$	= 5.7864 Ag.	Ratio,	73.476
2.4019 "	3.2688 "	"	73.480
3.5184 "	4.7886 "	"	73.475
3.0264 "	4.1189 "	"	73.476
		Mean,	73.4767, ± .0008

Hence Sr = 87.616.

Combining this series with the others we have:

Pelouze	73.4781, ± .0050
Dumas	73.4079, ± .0170
Richards	73.4767, ± .0008
General mean	73.4766, ± .0008

Dumas' determinations practically vanish, but those of Pelouze are confirmed.

The foregoing figures apply to anhydrous strontium chloride. The ratio between silver and the crystallized salt, $SrCl_2.6H_2O$, was determined in two series of experiments by Marignac.[2] Five grammes of the salt were used in each estimation, and, in the second series, the water was also determined. The quantities of the chloride corresponding to 100 parts of silver are given in the last column:

Series A.

5 grm. $SrCl_2.6H_2O$	= 4.0515 grm. Ag.	Ratio,	123.411
" "	4.0495 "	"	123.472
" "	4.0505 "	"	123.442
		Mean,	123.442

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[1] Proc. Amer. Acad., 40, 603. 1905. Three of the determinations were made by H. G. Parker.

[2] Arch. Sci. Phys. Nat., 1, 220. 1858. Journ. prakt. Chem., 74, 216. Oeuvres Complètes, 1, 568.

Series B.

5 grm. $SrCl_2.6H_2O$	= 4.0490	grm. Ag.	Ratio,	123.487
" "	4.0500	"	"	123.457
" "	4.0490	"	"	123.487
			Mean,	123.477

Mean of all as one series, 123.460, ± .0082

Hence Sr = 87.37.

From series B, by deducting Marignac's water determinations, 40.563 per cent. in mean, the ratio between silver and the anhydrous chloride can be determined. The value found is $2Ag : SrCl_2 :: 100 : 73.3907$, ± .0065. This is so much lower than the measurements previously cited that it needs no further consideration.

Marignac also determined the ratio between strontium chloride and strontium sulphate. By direct conversion, one hundred parts of the former salt gave the quantities of sulphate shown in the third column of the next table:

5.942 grm. $SrCl_2$	gave	6.887 $SrSO_4$.	Ratio,	115.932
5.941	"	6.8855 "	"	115.949
5.942	"	6.884 "	"	115.927
			Mean,	115.936, ± .004

Hence Sr = 86.90.

Richards,[1] in his study of strontium bromide, followed pretty much the lines laid down in his work on barium. The properties of the bromide itself were carefully investigated, and its purity established beyond reasonable doubt, and then the two usual ratios were determined. First, the ratio $2Ag : SrBr_2 :: 100 : x$, by titration with standard solutions of silver. For this ratio there are three series of measurements, by varied processes, concerning which full details are given. The data obtained, with weights reduced to a vacuum, are as follows:

First Series.

Wt. Ag.	*Wt. $SrBr_2$.*	*Ratio.*
1.30755	1.49962	114.689
2.10351	2.41225	114.677
2.23357	2.56153	114.683
5.3684	6.15663	114.683
		Mean, 114.683

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[1] Proc. Amer. Acad., 1894, 369.

Second Series.

Wt. Ag.	*Wt. $SrBr_2$.*	*Ratio.*
1.30762	1.49962	114.683
2.10322	2.41225	114.693
4.57502	5.24727	114.694
5.3680	6.15663	114.691
		Mean, 114.690

Third Series.

Wt. Ag.	*Wt. $SrBr_2$.*	*Ratio.*
2.5434	2.9172	114.697
3.3957	3.8946	114.692
3.9607	4.5426	114.692
4.5750	5.2473	114.695
		Mean, 114.694

Mean of all as one series, 114.689, ± .0012

Hence Sr = 87.614.

For the ratio, measured gravimetrically, $2AgBr : SrBr_2 :: 100 : x$, two series of determinations are given:

First Series.

Wt. AgBr.	*Wt. $SrBr_2$.*	*Ratio.*
2.4415	1.6086	65.886
2.8561	1.8817	65.884
6.9337	4.5681	65.883
		Mean, 65.884

Second Series.

Wt. AgBr.	*Wt. $SrBr_2$.*	*Ratio.*
2.27625	1.49962	65.881
3.66140	2.41225	65.883
3.88776	2.56153	65.887
9.34497	6.15663	65.882
		Mean, 65.883

Mean of all as one series, 65.884. ± .0006

Hence Sr = 87.621.

From the two bromide ratios the silver bromine ratio can be calculated, with the following result: Ag : Br :: 100 : 74.077.

There are now five ratios for strontium, as follows:

(1). $2Ag : SrCl_2 :: 100 : 73.4766$, ± .0008
(2). $2Ag : SrCl_2.6H_2O :: 100 : 123.460$, ± .0082
(3). $SrCl_2 : SrSO_4 :: 100 : 115.936$, ± .0040
(4). $2Ag : SrBr_2 :: 100 : 114.689$, ± .0012
(5). $2AgBr : SrBr_2 :: 100 : 65.884$, ± .0006

The atomic weights used in reducing these ratios are:

Ag = 107.880, ± .00029
Br = 79.9197, ± .0003
Cl = 35.4584, ± .0002
S = 32.0667, ± .00075
H = 1.00779, ± .00001

Hence,

From ratio 3 ....Sr = 86.899, ± .0811
" " 2 ....87.366, ± .0178
" " 4 ....87.614, ± .0026
" " 1 ....87.616, ± .0018
" " 5 ....87.621, ± .0024

General mean, Sr = 87.616, ± .0013

Ratios 2 and 3 evidently count for nothing in this combination. The final value for strontium is practically that of Richards alone.

Addendum. Since the manuscript of this volume went to the printer, Sir Edward Thorpe has kindly sent me, in advance of publication, the work of Thorpe and Francis[1] on the atomic weight of strontium. Six ratios were measured, involving the chloride, bromide, and sulphate of strontium, all with vacuum weights, and with every known precaution to ensure accuracy. For details the published memoir must be consulted.

First. The ratio $2Ag : SrBr_2$:

$SrBr_2$.	*Ag.*	*Ratio.*
1.77884	1.55073	114.710
1.86109	1.62216	114.729
1.85254	1.61511	114.701
1.73801	1.51534	114.694
1.85787	1.61994	114.688
1.70563	1.48707	114.697
		Mean, 114.703, ± .0040

Hence Sr = 87.644.

Second. The ratio $2AgBr : SrBr_2$:

$SrBr_2$.	*AgBr.*	*Ratio.*
1.86112	2.82438	65.895
1.85261	2.81155	65.893
1.73807	2.63762	65.895
1.85798	2.81999	65.886
1.70571	2.58866	65.892
		Mean, 65.892, ± .0011

Hence Sr = 87.651.

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[1] Proc. Roy. Soc., 83A, 277. 1910.

The two bromide ratios combined give Ag : Br : : 100 : 74.077, ±.0067.

Third. The ratio 2Ag : $SrCl_2$:

$SrCl_2$.	*Ag.*	*Ratio.*
1.64759	2.24203	73.486
1.66352	2.26356	73.491
1.53462	2.08817	73.491
1.64619	2.24011	73.487
1.76006	2.39486	73.493
1.56224	2.12572	73.492

Mean, 73.490, ± .0008

Hence Sr = 87.645.

Fourth. The ratio 2AgCl : $SrCl_2$:

$SrCl_2$.	*AgCl.*	*Ratio.*
1.64764	2.97899	55.309
1.66357	3.00762	55.312
1.53467	2.77446	55.314
1.64624	2.97653	55.307
1.76010	2.18202	55.314

Mean, 55.311, ± .0009

Hence Sr = 87.610.

The two chloride ratios combined give Ag : Cl : : 100 : 32.867, ±.0026.

Fifth. Ratio $SrBr_2$: $SrSO_4$:

$SrBr_2$.	*$SrSO_4$.*	*Ratio.*
7.14570	5.30466	74.236
7.64281	5.67326	74.230
9.86072	7.32047	74.239

Mean, 74.235, ± .0018

Hence Sr = 87.677.

Sixth. Ratio $SrCl_2$: $SrSO_4$:

$SrCl_2$.	*$SrSO_4$.*	*Ratio.*
7.30246	8.46071	115.861
8.71628	10.09868	115.861
8.46493	9.80743	115.859
8.79502	10.18957	115.855

Mean, 115.859, ± .0010

Hence Sr = 87.668.

The arithmetic mean of the six values obtained by Thorpe and Francis is 87.649, a little higher than the value found by Richards. A general combination of all the figures for strontium, however, would fall very near Richards' determinations.

BARIUM.

For the atomic weight of barium we have a series of seven ratios, established by the labors of Berzelius, Turner, Struve, Marignac, Dumas, Richards and Thorpe. Andrews[1] and Salvétat,[2] in their papers upon this subject, gave no details nor weighings, and therefore their work may be properly disregarded. First in order, we may consider the ratio between silver and barium chloride, as determined by Pelouze, Marignac, Dumas and Richards.

Pelouze,[3] in 1845, made the three subjoined estimations of this ratio, using his well known volumetric method. A quantity of pure silver was dissolved in nitric acid, and the amount of barium chloride needed to precipitate it was carefully ascertained. In the last column I give the quantity of barium chloride proportional to 100 parts of silver:

3.860	grm. $BaCl_2$ ppt.	4.002	grm. Ag.	96.452
5.790	"	6.003	"	96.452
2.895	"	3.001	"	96.468
				Mean, 96.4573, ± .0036

Hence Ba = 137.199.

Essentially the same method was adopted by Marignac[4] in 1848. His experiments were made upon four samples of barium chloride, as follows: A, commercial barium chloride, purified by recrystallization from water. B, the same salt, calcined, redissolved in water, the solution saturated with carbonic acid, filtered and allowed to crystallize. C, the preceding salt, washed with alcohol and again recrystallized. D, the same, again washed with alcohol. For 100 parts of silver the following quantities of chloride were required, as given in the third column:

	Ag.	*$BaCl_2$.*	*Ratio.*	
A	3.4445	3.3190	96.356	Mean, 96.354
	3.7480	3.6110	96.345	
	6.3446	6.1140	96.362	
B	4.3660	4.1780	96.356	Mean, 96.354
	4.8390	4.6625	96.352	
C	6.9200	6.6680	96.358	Mean, 96.360
	5.6230	5.4185	96.363	
D	5.8435	5.6300	96.346	Mean, 96.367
	8.5750	8.2650	96.384	
	4.8225	4.6470	96.361	
	6.8460	6.5980	96.377	
			Mean, 96.360, ± .0024	

Hence Ba = 136.989.

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[1] Chemical Gazette, October, 1852.

[2] Compt. Rend., 17, 318.

[3] Compt. Rend., 20, 1047. Journ. prakt. Chem., 35, 73.

[4] Arch. Sci. Phys. Nat., 8, 271. Oeuvres Complètes, 1, 219.

Dumas[1] employed barium chloride prepared from pure barium nitrate, and took the extra precaution of fusing the salt at a red heat in a current of dry hydrochloric acid gas. Three series of experiments upon three samples of chloride gave the following results:

	Ag.	*$BaCl_2$.*	*Ratio.*	
A	1.8260	1.7585	96.303	Mean, 96.333
	3.9980	3.8420	96.339	
	2.2405	2.1585	96.340	
	4.1680	4.0162	96.358	
B	1.7270	1.6625	96.265	Mean, 96.290
	2.5946	2.4987	96.304	
	3.5790	3.4468	96.306	
	4.2395	4.0822	96.290	
	4.3683	4.2062	96.289	
	4.6290	4.4564	96.271	
	9.0310	8.6975	96.307	
C	2.3835	2.2957	96.316	Mean, 96.338
	4.2930	4.1372	96.371	
	4.4300	4.2662	96.303	
	4.6470	4.4764	96.329	
	5.8520	5.6397	96.372	
			Mean, 96.316, ± .0055	

Hence Ba = 136.894.

The work done by Richards[2] was of a much more elaborate kind, for it involved some collateral investigations as to the effect of heat upon barium chloride, etc. Every precaution was taken to secure the spectroscopic purity of the material, which was prepared from several sources, and similar care was taken with regard to the silver. For details upon these points the original paper must be consulted. As for the titrations, three methods were adopted, and a special study was made with reference to the accurate determination of the end point; in which particular the investigations of Pelouze, Marignac and Dumas were at fault. In the first series of determinations, silver was added in excess, and the latter was measured with a standard solution of hydrochloric acid. The end point was ascertained by titrating backward and forward with silver solution and acid, and was taken as the mean between the two apparent end points thus observed. The results of this series, with weights reduced to a vacuum standard, were as follows:

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[1] Ann. Chem. Pharm., 113, 22. 1860. Ann. Chim. Phys. (3), 55, 129.

[2] Proc. Amer. Acad., 29, 55. 1893.

Ag.	*$BaCl_2$.*	*Ratio.*
6.1872	5.9717	96.517
5.6580	5.4597	96.495
3.5988	3.4728	96.499
9.4010	9.0726	96.507
.7199	.6950	96.541
		Mean, 96.512, ± .0055

In the second series of experiments a small excess of silver was added as before, and the precipitate of silver chloride was removed by filtration. The filtrate and wash waters were concentrated to small bulk, whereupon a trace of silver chloride was obtained and taken into account. The excess of silver remaining was then thrown down as silver bromide, and from the weight of the latter the silver was calculated, and subtracted from the original amount.

Ag.	*$BaCl_2$.*	*Ratio.*
6.59993	6.36974	96.512
5.55229	5.36010	96.539
4.06380	3.92244	96.522
		Mean, 96.524, ± .0054

The third series involved mixing solutions of barium chloride and silver in as nearly as possible equivalent amounts, and then determining the actual quantities of silver and chlorine left unprecipitated. The filtrate and wash waters were divided into two portions, one-half being evaporated with hydrobromic acid and the other with silver nitrate. The small amounts of silver bromide and chloride thus obtained were determined by reduction and the use of Volhard's method:

Ag.	*$BaCl_2$.*	*Ratio.*
4.4355	4.2815	96.528
2.7440	2.6488	96.531
6.1865	5.9712	96.520
3.4023	3.2841	96.526
		Mean, 96.526, ± .0016

Two final experiments were carried out by Stas' method, somewhat as in the first series, with variations and greater refinement in the observation of the end point. The results were as follows:

Ag.	*$BaCl_2$.*	*Ratio.*
6.7342	6.50022	96.525
10.6023	10.23365	96.523
		Mean, 96.524, ± .0007

A careful study of Richards' paper will show that, although the last two experiments are probably the best, they are not entitled to such preponderance of weight as the "probable error" here computed would give them. If all of the determinations are assigned equal weight, and treated as one series, the mean becomes 96.520, ±.0025, but this figure is not satisfactory. The four series are unequal in merit, and that fact may be fairly recognized by combining the first and second series into one, and the third and fourth series similarly. On this basis the combination of all the data assumes the following form:

Pelouze	96.457, ± .0036
Marignac	96.360, ± .0024
Dumas	96.316, ± .0055
Richards, Series 1, 2	96.5165, ± .0040
Richards, Series 3, 4	96.5255, ± .0010
General mean	96.4947, ± .00086

Richards' determinations alone give Ba=137.345.

The ratio between silver and crystallized barium chloride has been fixed by Marignac.[1] The usual method was employed, and two series of experiments were made, in the second of which the water of crystallization was also determined. Five grammes of chloride were taken in each determination, to which the subjoined weights of silver correspond. The ratio to 100 parts of silver is given in the second column:

	Weight Ag.	*Ratio.*
B	4.4205	113.109
	4.4195	113.135
	4.4210	113.097
A	4.4195	113.135
	4.4200	113.122
	4.4215	113.060
		Mean, 113.110, ± .0079

Hence Ba=137.098.

The direct ratio between the chlorides of silver and barium has been measured by Berzelius, Turner, Richards and Thorpe. Berzelius[2] found of barium chloride proportional to 100 parts of silver chloride—

72.432
72.422

Mean, 72.427

Hence Ba=136.714.

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[1] Arch. Sci. Phys. Nat., 1, 209. 1858. Journ. prakt. Chem., 74, 212. Oeuvres Complètes, 1, 559.

[2] Poggend. Annalen, 8, 177.

Turner[1] made five experiments, with the following results:

72.754
72.406
72.622
72.664
72.653

Mean, 72.680, ± .0154

Hence Ba = 137.439.

Of these, Turner regards the fourth and fifth as the best; but for present purposes it is not desirable to so discriminate.

Richards' determinations[2] fall into three series, and all are characterized by their taking into account chloride of silver recovered from the wash waters. In the first series the barium chloride was ignited at low redness in air or nitrogen; in the second series it was fused in a stream of pure hydrochloric acid; and in the third series it was not ignited at all. In the last series it was weighed in the crystallized state, and the amount of anhydrous chloride was computed from the data so obtained. The data, corrected to vacuum standards, are as follows:

	AgCl.	*$BaCl_2$.*	*Ratio.*	
A	8.7673	6.3697	72.653	Mean, 72.649
	5.1979	3.7765	72.654	
	4.9342	3.5846	72.648	
	2.0765	1.5085	72.646	
	4.4271	3.2163	72.650	
B	2.09750	1.52384	72.650	Mean, 72.6563
	7.37610	5.36010	72.669	
	5.39906	3.92244	72.650	
C	8.2189	5.97123	72.6524	Mean, 72.6555
	4.5199	3.28410	72.6587	

Mean, 72.653, ± .0014

Hence Ba = 137.362.

If we combine this with Richards' silver series, which, in mean, may be written $2Ag : BaCl_2 :: 100 : 96.525$, the cross ratio between silver and chlorine becomes $Ag : Cl :: 100 : 32.858$.

Thorpe's[3] measurements of this ratio are not important, for they were merely intended as a check upon the method he used in determining the atomic weight of radium, which involved the manipulation of very small quantities of material. His data are given here for the sake of completeness:

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[1] Phil. Trans., 1829, 291.

[2] Proc. Amer. Acad., 29, 55, 1893.

[3] Proc. Roy. Soc., 80A, 298. 1908.

AgCl.	*$BaCl_2$.*	*Ratio.*
.1578	.1147	72.687
.2368	.1721	72.677
.0788	.0571	72.462
.0861	.0626	72.706
.0937	.0681	72.679
.1914	.1395	72.884
.1083	.0788	72.761
		Mean, 72.694, ± .0320

Hence Ba=137.48.

Assigning to Berzelius' work equal weight with that of Turner, the four series representing the ratio 2AgCl : $BaCl_2$ combine as follows:

Berzelius	72.427, ± .0154
Turner	72.680, ± .0154
Richards	72.653, ± .0014
Thorpe	72.694, ± .0320
General mean	72.650, ± .0014

The ratio between barium nitrate and barium sulphate has been determined only by Turner.[1] According to his experiments 100 parts of sulphate correspond to the following quantities of nitrate:

112.060
111.990
112.035

Mean, 112.028, ± .014

Hence Ba=136.338, ±.2706.

For the similar ratio between barium chloride and barium sulphate, there are determinations by Turner, Berzelius, Struve, Marignac and Richards.

Turner[2] found that 100 parts of chloride ignited with sulphuric acid gave 112.19 parts of sulphate. By the common method of precipitation and filtration a lower figure was obtained, because of the slight solubility of the sulphate. This observation bears directly upon many other atomic weight determinations.

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[1] Phil. Trans., 1833, 538.
[2] Phil. Trans., 1829, 291.

Berzelius,[1] treating barium chloride with sulphuric acid, obtained the following results in $BaSO_4$ for 100 parts of $BaCl_2$:

112.17
112.18

Mean, 112.175

Hence Ba = 135.653.

Struve,[2] in two experiments, found:

112.0912
112.0964

Mean, 112.0938

Hence Ba = 137.037.

Marignac's[3] three results are as follows:

8.520 grm. $BaCl_2$	gave	9.543 $BaSO_4$.	Ratio,	112.007
8.519	"	9.544 "	"	112.032
8.520	"	9.542 "	"	111.995

Mean, 112.011, ± .0071

Hence Ba = 138.473.

Richards, in his work on this ratio, regards the results as of slight value, because of the occlusion of the chloride by the sulphate. This source of error he was never able to avoid entirely. Another error in the opposite direction is found in the retention of sulphuric acid by the precipitated sulphate. Eight experiments were made in two series, one set by adding sulphuric acid to a strong solution of barium chloride in a platinum crucible, the other by precipitation in the usual way. Richards gives in his published paper only the end results and the mean of his determinations: the details cited below I owe to his personal kindness. The weights are reduced to a vacuum standard:

	$BaCl_2$.	*$BaSO_4$.*	*Ratio.*
First	1.78934	2.0056	112.086
	2.07670	2.3274	112.072
	1.58311	1.7741	112.064
	3.27563	3.6712	112.076
	3.02489	3.3903	112.080
	3.87091	4.3385	112.080
Second	3.02489	3.9726	112.076
	3.87091	3.4880	112.085

Mean, 112.077, ± .0017

Hence Ba = 137.398.

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[1] Poggend. Annalen, 8, 177.

[2] Ann. Chem. Pharm., 80, 204. 1851.

[3] Arch. Sci. Phys. Nat., 1, 209. 1858. Journ. prakt. Chem., 74, 212. Oeuvres Complètes, 1, 559.

This mean is subject to a small correction due to loss of chlorine on drying the chloride, which reduces it to 112.073. Omitting Turner's single determination as unimportant, and assigning to the work of Berzelius and of Struve equal weight with that of Marignac, the measurements of this ratio combine thus:

Berzelius	112.175, ± .0071
Struve	112.094, ± .0071
Marignac	112.011, ± .0071
Richards	112.073, ± .0017
General mean	112.075, ± .0016

In an earlier paper than the one previously cited, Richards[1] studied with great care the ratios connecting barium bromide with silver and silver bromide. The barium bromide was prepared by several distinct processes, its behavior upon dehydration and even upon fusion was studied, and its specific gravity was determined. The ratio with silver was measured by titration, a solution of hydrobromic acid being used for titrating back. The data are subjoined, with the $BaBr_2$ equivalent to 100 parts of silver stated:

$BaBr_2$.	*Ag.*	*Ratio.*
2.28760	1.66074	137.746
3.47120	2.52019	137.736
2.19940	1.59687	137.732
2.35971	1.71323	137.735
2.94207	2.13584	137.748
1.61191	1.17020	137.747
2.10633	1.52921	137.740
2.19682	2.11740	137.755
2.37290	1.72276	137.738
1.84822	1.34175	137.747
5.66647	4.11360	137.750
3.52670	2.56010	137.756
4.31690	3.13430	137.731
3.36635	2.44385	137.748
3.46347	2.51415	137.759
		Mean, 137.745, ± .0015

Hence Ba = 137.360.

The silver bromide in most of these determinations, and in some others, was collected and weighed in a Gooch crucible with all necessary pre-

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[1] Proc. Amer. Acad., 28, 1. 1893.

cautions. Vacuum standards were used throughout for both ratios. I give in a third column the $BaBr_2$ equivalent to 100 parts of AgBr:

$BaBr_2$.	*AgBr.*	*Ratio.*
2.28760	2.89026	79.149
3.47120	4.38635	79.136
3.81086	4.81688	79.133
2.35971	2.98230	79.124
2.94207	3.71809	79.129
2.10633	2.66191	79.128
2.91682	3.68615	79.129
2.37290	2.99868	79.131
1.84822	2.33530	79.143
1.90460	2.40733	79.116
5.66647	7.16120	79.127
3.52670	4.45670	79.133
2.87743	3.63644	79.127
3.46347	4.37669	79.135
		Mean, 79.132, ± .0015

Hence Ba = 137.380. From the two bromide ratios combined, Ag: Br : : 100 : 74.070.

The last ratio was also determined by Thorpe,[1] incidentally to his work on the atomic weight of radium:

$BaBr_2$.	*AgBr.*	*Ratio.*
.0899	.1136	79.137
.0960	.1214	79.077
.1110	.1403	79.116
.0910	.1149	79.199
.0808	.1021	79.138
		Mean, 79.133, ± .0134

Hence Ba = 137.384, in confirmation of Richards' series. On combination with the latter no noteworthy change is produced.

The ratios for barium are now as follows:

(1). $2Ag:BaCl_2::100:96.4947$, ± .00086
(2). $2AgCl:BaCl_2::100:72.650$, ± .0014
(3). $2Ag:BaCl_2.2H_2O::100:113.110$, ± .0079
(4). $BaSO_4:BaN_2O_6::100:112.028$, ± .014
(5). $BaCl_2:BaSO_4::100:112.075$, ± .0016
(6). $2Ag:BaBr_2::100:137.745$, ± .0015
(7). $2AgBr:BaBr_2::100:79.132$, ± .0015

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[1] Proc. Roy. Soc., 80A, 298. 1908.

Reducing these ratios with

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003

N = 14.0101, ± .0001
S = 32.0667, ± 00075
H = 1.0078

we have—

From ratio 4 ....Ba = 136.338, ± .2706
" " 3 ....137.098, ± .0171
" " 1 ....137.280, ± .0015
" " 2 ....137.354, ± .0046
" " 6 ....137.360, ± .0034
" " 5 ....137.364, ± .0285
" " 7 ....137.380, ± .0057

General mean, Ba = 137.302, ± .0013

This mean is probably too low, for the value from ratio 1 is affected by the doubtful determinations of several early investigators. That ratio, however, has the highest weight in the combination. Rejecting the first three values, the last four give a general mean of

Ba = 137.363, ± .0025

which will be adopted in subsequent computations.

A few experiments are on record with reference to determining the atomic weight of barium from the percentage of water in the hydrated chloride. This method has been carefully investigated by Guye and Tsakalotos,[1] who conclude that the chloride in question is not suited to the purpose. Their data give Ba = 139.5 approximately; while similar data by Marignac give 136.5. The subject needs no further consideration here.

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[1] Journ. Chim. Phys., 7, 215. 1909. Marignac's figures are discussed in the second edition of this work.

RADIUM.

The early, preliminary attempts to determine the atomic weight of radium may be ignored, for they were made with confessedly impure material. In 1902 Madame Curie[1] published the first determinations of any value, basing them upon the following analyses of radium chloride. The ratio $2AgCl : RaCl_2$ is given in the third column:

$RaCl_2$.	$AgCl$.	*Ratio.*
.09192	.08890	103.397
.02936	.08627	103.582
.08839	.08589	102.911
		Mean, 103.297, ± .1349

Hence Ra = 225.21.

In the foregoing determinations the radium chloride still contained appreciable amounts of barium chloride. In a later series of determinations Madame Curie[2] used purer material, and in much larger quantities. The results obtained were as follows:

$RaCl_2$.	$AgCl$.[3]	*Ratio.*
.4052	.39054	103.768
.4020	.38784	103.651
.39335	.37944	103.666
		Mean, 103.695, ± .0236

Hence Ra = 226.35.

Still more recent are the determinations by Thorpe,[4] on small quantities of material:

$RaCl_2$.	$AgCl$.	*Ratio.*
.0627	.0604	103.808
.0639	.0618	103.398
.0784	.0753	104.117
		Mean, 103.774, ± .1399

Hence Ra = 226.58.

Thorpe regards his figures, however, as merely corroborative of Madame Curie's. A combination of the two series gives 103.700 for the general mean, and Ra = 226.37.

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[1] Ann. Chim. Phys. (7), 30, 140. 1903. Preliminary data in Compt. Rend., 135, 80. 1902.

[2] Compt. Rend., 145, 422. 1907.

[3] Corrected for the weight of filter ash.

[4] Proc. Roy. Soc., 80, A, 298. 1908.

LEAD.

For the atomic weight of lead we have to consider experiments made upon the oxide, chloride, nitrate and sulphate. The researches of Berzelius upon the carbonate and various organic salts need not now be considered, nor is it worth while to take into account any work of his done before the year 1818. The results obtained by Döbereiner[1] and by Longchamp[2] are also without special present value.

For the exact composition of lead oxide we have to depend upon the researches of Berzelius. His experiments were made at different times through quite a number of years; but were finally summed up in the last edition of his famous "Lehrbuch."[3] In general terms his method of experiment was very simple. Perfectly pure lead oxide was heated in a current of hydrogen, and the reduced metal weighed. From his weighings I have calculated the percentages of lead thus found and given them in a third column:

Earlier Results.

8.045 grm.	PbO gave	7.4675	grm. Pb.	92.8217	per cent.
14.183	"	13.165	"	92.8224	"
10.8645	"	10.084	"	92.8160	"
13.1465	"	12.2045	"	92.8346	"
21.9425	"	20.3695	"	92.8313	"
11.159	"	10.359	"	92.8309	"

Latest.

6.6155	"	6.141	"	92.8275	"
14.487	"	13.448	"	92.8280	"
14.626	"	13.5775	"	92.8313	"

Mean, 92.8271, ± .0013

Hence Pb = 207.062.

For the synthesis of lead sulphate we have data by Berzelius, Turner and Stas. Berzelius,[4] whose experiments were intended rather to fix the atomic weight of sulphur, dissolved in each estimation ten grammes of pure lead in nitric acid, then treated the resulting nitrate with sulphuric acid, brought the sulphate thus formed to dryness, and weighed. One hundred parts of metal yield of $PbSO_4$:

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[1] Schweig. Journ., 17, 241. 1816.

[2] Ann. Chim. Phys., 34, 105. 1827.

[3] Bd. 3, s. 1218.

[4] Lehrbuch, 5th ed., 3, 1187.

146.380
146.400
146.410
146.458

Mean, 146.419, ± .012

Hence Pb = 206.96.

Turner,[1] in three similar experiments, found as follows:

146.430
146.398
146.375

Mean, 146.401, ± .011

Hence Pb = 207.04.

In these results of Turner's, *absolute* weights are implied.

The results of Stas' syntheses,[2] effected after the same general method, but with variations in details, are as follows. Corrections for weighing in air were applied:

Weight Pb.	*Weight $PbSO_4$.*	*Ratio.*
141.9925	207.9388	146.443
148.016	217.6141	146.427
100.000	146.419	146.419
200.000	292.864	146.432
250.000	366.0525	146.421
250.000	366.0575	146.423
		Mean, 146.4275, ± .0024

Hence Pb = 206.92.

Combining, we get the subjoined result:

Berzelius	146.419, ± .012
Turner	146.401, ± .011
Stas	146.4275, ± .0024
General mean	146.4262, ± .0023

Turner, in the same paper, also gives a series of syntheses of lead sulphate, in which he starts from the oxide instead of from the metal. One hundred parts of PbO, upon conversion into $PbSO_4$, gained weight as follows:

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[1] Phil. Trans., 1833, 527-538.

[2] Oeuvres Complètes, 1, 390.

35.84
35.71
35.84
35.75
35.79
35.78
35.92

Mean, 35.804, ± .018

Hence Pb=207.625.

These figures are not wholly reliable. Numbers one, two and three represent lead oxide contaminated with traces of nitrate. The oxide of four, five and six contained traces of minium. Number seven was free from these sources of error, and, therefore, deserves more consideration. The series as a whole undoubtedly gives too low a figure, and this error would tend to slightly raise the atomic weight of lead.

Still a third series by Turner establishes the ratio between the nitrate and the sulphate, a known weight of the former being in each experiment converted into the latter. One hundred parts of sulphate represent of nitrate:

109.312
109.310
109.300

Mean, 109.307, ± .002

Hence Pb=204.75.

In all these experiments by Turner the necessary corrections were made for weighing in air.

In 1846 Marignac[1] published two sets of determinations of only moderate value. First, chlorine was conducted over weighed lead, and the amount of chloride so formed was determined. The lead chloride was fused before weighing. The ratio to 100 Pb is given in the last column:

20.506 grm. Pb	gave	27.517	$PbCl_2$.	134.190
16.281	"	21.858	"	134.225
25.454	"	34.149	"	134.159
				Mean, 134.191, ± .013

Hence Pb=207.41.

Secondly, lead chloride was precipitated by silver nitrate and the ratio between $PbCl_2$ and 2AgCl determined. The third column gives the $PbCl_2$ equivalent to 100 parts of AgCl:

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[1] Ann. Chem. Pharm., 59, 289 and 290. 1846. Oeuvres Complètes, 1, 186.

12.534 grm. $PbCl_2$	gave 12.911	AgCl.	97.080
14.052	" 14.506	"	96.870
25.533	" 26.399	"	96.720
		Mean,	96.890, ± .0704

Hence Pb=206.84.

For the ratio between lead chloride and silver we have a series of results by Marignac and one experiment by Dumas. There are also unavailable data by Turner and by Berzelius.

Marignac,[1] applying the method used in his researches upon barium and strontium, and working with lead chloride which had been dried at 200°, obtained these results. The third column gives the ratio between $PbCl_2$ and 100 parts of Ag:

4.9975 grm. $PbCl_2$	= 3.8810 grm.	Ag.	128.768
4.9980 "	3.8835	"	128.698
5.0000 "	3.8835	"	128.750
5.0000 "	3.8860	"	128.667
		Mean,	128.721, ± .016

Hence Pb=206.79.

Dumas,[2] in his investigations, found that lead chloride retains traces of water even at 250°, and is sometimes also contaminated with oxychloride. In one estimation 8.700 grammes $PbCl_2$ saturated 6.750 of Ag. The chloride contained .009 of impurity; hence, correcting, Ag : $PbCl_2$ :: 100 : 128.750. If we assign this figure equal weight with those of Marignac, we get as the mean of all 128.7266, ± .013. The sources of error indicated by Dumas, if they are really involved in this mean, would tend slightly to raise the atomic weight of lead.

The synthesis of lead nitrate, as carried out by Stas,[3] gives better results. Two series of experiments were made, with from 103 to 250 grammes of lead in each determination. The metal was dissolved in nitric acid, the solution evaporated to dryness with extreme care, and the nitrate weighed. All weighings were reduced to the vacuum standard. In series A the lead nitrate was dried in an air current at a temperature of about 155°. In series B the drying was effected in vacuo. The data are as follows, together with the ratio of nitrate to 100 parts of lead:

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[1] Journ. prakt. Chem., 74, 218. 1858. Oeuvres Complètes, 1, 574.

[2] Ann. Chem. Pharm., 113, 35. 1860.

[3] Oeuvres Complètes, 1, 386.

Series A.

Weight Pb.	*Weight PbN_2O_6.*	*Ratio.*
103.000	164.773	159.973
140.6887	225.0674	159.975
110.2672	176.408	159.982
141.9927	227.1527	159.975
148.616	237.702	159.968
123.348	198.924	159.973

Mean, 159.9743, ± .0012

Series B.

Weight Pb.	*Weight PbN_2O_6.*	*Ratio.*
100.000	159.970	159.970
200.000	319.928	159.964
250.000	399.8975	159.959
250.000	399.914	159.965

Mean, 159.9645, ± .0015

Mean from both series, 159.9704, ± .0010

Hence Pb = 206.80.

There is still another set of experiments upon lead nitrate, originally intended to fix the atomic weight of nitrogen, which may properly be included here. It was carried out by Anderson[1] in Svanberg's laboratory, and has also appeared under Svanberg's name. Lead nitrate was carefully ignited, and the residual oxide weighed, with the following results:

5.19485	grm. PbN_2O_3	gave	3.5017	grm. PbO.	67.4071	per cent.
9.7244	"		6.5546	"	67.4037	"
9.2181	"		6.2134	"	67.4044	"
9.6530	"		6.5057	"	67.3957	"

Mean, 67.4027, ± .0016

Hence Pb = 207.34.

The direct ratio between lead and silver has been roughly measured by the electrochemical experiments of Betts and Kern.[2] Lead silicofluoride was dissolved in hydrofluosilicic acid, and from the solution the lead was thrown down electrolytically, silver being simultaneously precipitated by the same current. Two series of experiments gave the following data. The ratio 2Ag : Pb :: 100 : x is stated in the third column:

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[1] Ann. Chim. Phys. (3), 9, 254. 1843.

[2] Trans. Amer. Electrochem. Soc., 6, 67.

First Series.

Weight Ag.	*Weight Pb.*	*Ratio.*
5.8958	5.6221	95.476
“	5.6396	95.654
5.7863	5.5246	95.477
“	5.5450	95.830
7.8408	7.5108	95.791
“	7.5168	95.868
7.6253	7.3191	95.984
“	7.3221	96.025
6.2287	5.9600	95.676
“	5.9605	95.694
16.6804	15.9996	95.919
“	16.0014	95.923
6.8652	6.5815	95.868
“	6.5812	95.863
9.3253	8.9390	95.858
“	8.9419	95.889
6.8566	6.5695	95.813
6.8754	6.5877	95.816

Mean, 95.801, ± .0243

Second Series.

Weight Ag.	*Weight Pb.*	*Ratio.*
9.0470	8.6678	95.809
“	8.6663	95.792
13.4113	12.8607	95.895
“	12.8558	95.858
7.2780	6.9716	95.790
“	6.9755	95.844
7.2738	6.9605	95.693
“	6.9698	95.821
6.5278	6.2550	95.821
6.4864	6.2168	95.844

Mean, 95.817, ± .0109

General mean of both series, 95.814, ± .0097

Hence Pb = 206.73.

Baxter and Wilson[1] determined the atomic weight of lead by analyses of lead chloride, which had been previously fused in an atmosphere of gaseous hydrochloric acid. The ratio to Ag and to AgCl were both determined, and the figures obtained, with vacuum weights, are as follows:

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[1] Proc. Amer. Acad., 43, 365. 1907. Journ. Amer. Chem. Soc., 30, 187.

$PbCl_2$.	Ag.	AgCl.	Ag ratio.	AgCl ratio.
4.67691	3.62987	4.82273	128.845	96.976
3.67705	2.85375	......	128.850	.....
4.14110	3.21408	4.27016	128.842	96.978
4.56988	3.54672	......	128.848	.....
5.12287	3.97568	5.28272	128.855	96.974
3.85844	2.99456	3.97949	128.848	96.958
4.67244	3.62628	......	128.849	.....
3.10317	2.40837	3.19909	128.849	97.002
4.29613	3.33407	4.42982	128.856	96.982
		Mean,	128.849,	96.978,
			± .0010	± .0039

From Ag ratio, Pb = 207.088.
From AgCl ratio, Pb = 207.096.
And Ag : Cl : : 100 : 32.864.

These ratios combine with others thus:

Ratio $2Ag:PbCl_2$.

Marignac with Dumas........	128.727, ± .0130
Baxter and Wilson...........	128.849, ± .0010
General mean	128.848, ± .0010

Ratio $2AgCl:PbCl_2$.

Marignac	96.890, ± .0704
Baxter and Wilson............	96.978, ± .0039
General mean	96.977, ± .0039

The older determinations practically reject themselves, leaving Baxter and Wilson's figures alone.

The work done upon the atomic weight of lead by Meaglia[1] is confessedly an approximation, and nothing more. Metallic lead was used to precipitate silver from a nitrate solution, and in that way the ratio Ag : Pb was determined. Two series of observations were made, with different preparations of lead. Calculated with Ag = 107.93, the following values for Pb were obtained:

1.	2.
206.872	206.866
206.907	206.897
206.903	206.927
206.909	206.933
206.930	206.935
206.903	
206.929	

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[1] Thesis, University of Grenoble, 1907.

Treating both series as one, and reducing the figures to the form of ratio adopted in this work, the mean becomes

2Ag:Pb::100:95.853, ± .0020

Combined with the series by Betts and Kern, 95.814, ± .0097, the general mean becomes 95.850, ± .0019.

The following ratios are now available from which to compute the atomic weight of lead:

(1). PbO:Pb::100:92.8271, ± .0013
(2). PbN_2O_6:PbO::100:67.4027, ± .0016
(3). Pb:$PbSO_4$::100:146.4262, ± .0023
(4). PbO:$PbSO_4$::100:135.804, ± .0180
(5). $PbSO_4$:PbN_2O_6::100:109.307, ± .0020
(6). Pb:PbN_2O_6::100:159.9704, ± .0010
(7). Pb:$PbCl_2$::100:134.191, ± .0130
(8). 2Ag:$PbCl_2$::100:128.843, ± .0010
(9). 2AgCl:$PbCl_2$::100:96.977, ± .0039
(10). 2Ag:Pb::100:95.850, ± .0019

Computing with

Ag = 107.880, ± .00029 N = 14.0101, ± .0001
Cl = 35.4584, ± .0002 S = 32.0667, ± .00075

we have—

From ratio 5	Pb = 204.749, ± .0475
" " 6	206.802, ± .0035
" " 10	206.806, ± .0041
" " 3	206.923, ± .0104
" " 1	207.062, ± .0376
" " 8	207.075, ± .0023
" " 9	207.094, ± .0112
" " 2	207.337, ± .0118
" " 7	207.414, ± .0789
" " 4	207.625, ± .1125
General mean,	Pb = 206.970, ± .0017

The rejection of the first and last two values in this series only raises the general mean to 207.972, and it is therefore immaterial whether they are retained or cast aside. On chemical grounds the values from ratios 8 and 9 are probably the best, but they need additional confirmation. The final result is presumably, but not certainly, too low.

GLUCINUM.

Our knowledge of the atomic weight of glucinum is derived from experiments made upon the sulphate and three organic salts. Leaving out of account the single determination by Berzelius,[1] we have to consider the data furnished by Awdejew, Weeren, Klatzo, Debray, Nilson and Pettersson, Krüss and Moraht, and Parsons.

Awdejew,[2] whose determination was the earliest of any value, analyzed the sulphate. The sulphuric acid was thrown down as barium sulphate; and in the filtrate, from which the excess of barium had been first removed, the glucina was precipitated by ammonia. The figures which Awdejew publishes represent the ratio between SO_3 and GlO, but not absolute weights. As, however, his calculations were made with $SO_3 = 501.165$, and Ba probably$=855.29$, we may add a third column showing how much $BaSO_4$ is proportional to 100 parts of GlO:

SO_3.	*GlO.*	*Ratio.*
4457	1406	921.242
4531	1420	927.304
7816	2480	915.903
12880	4065	920.814
		Mean, 921.316, ± 1.577

Hence Gl$=9.337$.

The same method was followed by Weeren and by Klatzo, except that Weeren used ammonium sulphide instead of ammonia for the precipitation of the glucina. Weeren[3] gives the following weights of GlO and $BaSO_4$. The ratio is given in a third column, just as with the figures by Awdejew:

GlO.	$BaSO_4$.	*Ratio*
.3163	2.9332	927.348
.2872	2.6377	918.419
.2954	2.7342	925.592
.5284	4.8823	923.978
		Mean, 923.834, ± 1.303

Hence Gl$=9.267$.

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[1] Poggend. Annal., 8, 1.

[2] Poggend. Annal., 56, 106. 1842.

[3] Poggend. Annal., 92, 124. 1854.

Klatzo's[1] figures are as follows, with the third column added by the writer:

GlO.	*$BaSO_4$.*	*Ratio.*
.2339	2.1520	920.052
.1910	1.7556	919.162
.2673	2.4872	930.490
.3585	3.3115	923.710
.2800	2.5842	922.929
		Mean, 923.268, ± 1.346

Hence Gl=9.283.

Combining these series into a general mean, we have—

Awdejew	921.316, ± 1.577
Weeren	923.834, ± 1.303
Klatzo	923.268, ± 1.346
General mean	922.977, ± 0.805

Debray[2] analyzed a double oxalate of glucinum and ammonium, $Gl(NH_4)_2C_4O_8$. In this the glucina was estimated by calcination, after first converting the salt into nitrate. The following percentages were found:

11.5
11.2
11.6

Mean, 11.433, ± .081

The carbon was estimated by an organic combustion. I give the weights, and put in a third column the percentages of CO_2 thus obtained:

Salt.	*CO_2.*	*Per Cent. CO_2.*
.600	.477	79.500
.603	.478	79.270
.600	.477	79.500
		Mean, 79.423, ± .052

Hence, from the ratio between $4CO_2$ and GlO, Gl=9.3375.

In 1880 the careful determinations of Nilson and Pettersson appeared.[3] These chemists first attempted to work with the sublimed chloride of glucinum, but abandoned the method upon finding the compound to

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[1] Zeitsch. anal. Chem., 8, 523. 1869.

[2] Ann. Chim. Phys. (3), 44, 37. 1855.

[3] Ber. Deutsch. chem. Ges., 13, 1451. 1880.

be contaminated with traces of lime derived from a glass tube. They finally resorted to the crystallized sulphate as the most available salt for their purposes. This compound, upon strong ignition, yields pure glucina. The data are as follows:

$GlSO_4.4H_2O$.	*GlO.*	*Per cent. GlO.*
3.8014	.5387	14.171
2.6092	.3697	14.169
4.3072	.6099	14.160
3.0091	.4266	14.176
		Mean, 14.169, ± .0023

Hence Gl = 9.113.

Krüss and Moraht[1] in their work followed the general method adopted by Nilson and Pettersson, but with various added precautions and greater elaboration of detail. Their glucina was derived from three sources, namely, leucophane, beryl and gadolinite, and the sulphate was repeatedly recrystallized. The results are subjoined:

$GlSO_4.4H_2O$.	*GlO.*	*Per cent. GlO.*
21.1928	3.0008	14.160
16.2038	2.29455	14.161
15.49345	2.1902	14.136
20.1036	2.8433	14.143
22.0465	3.1167	14.137
4.9619	.7019	14.146
18.3249	2.5921	14.145
24.3907	3.0253	14.143
20.18045	2.85255	14.135
20.0253	2.8328	14.146
18.9840	2.6832	14.134
17.0072	2.4073	14.155
22.5044	3.1805	14.133
20.88675	2.95645	14.154
19.0591	2.69305	14.130
17.8227	2.5226	14.153
		Mean, 14.144, ± .0017

Hence Gl = 9.062.

The first two determinations, which give the highest percentage, were made upon sulphate thrice crystallized. The others were made upon a salt four times crystallized, except in one instance, when there were five crystallizations. To the data derived from the four times crystallized compound Krüss and Moraht give preference, and so find a slightly lower

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[1] Liebig's Annalen, 262, 38. 1891.

value for the atomic weight of glucinum. Combining, we have for the mean percentage:

By Nilson and Pettersson......	14.169, ± .0023
By Krüss and Moraht.........	14.144, ± .0017
General mean	14.153, ± .0014

The determinations, by Parsons,[1] of this atomic weight, were based upon analyses of two organic salts, namely, the acetylacetonate, $Gl(C_5H_7O_2)_2$, and the basic acetate, $Gl_4O(C_2H_3O_2)_6$. These compounds are volatile at moderately high temperatures, and can therefore be purified by sublimation; an advantage which the sulphate does not possess. Parsons attempted to make determinations with the sulphate, also but obtained unsatisfactory results.

Weighed quantities of the two organic compounds were first decomposed, in platinum crucibles, with nitric acid. The nitrate solutions so formed were then evaporated to dryness, and the residual salt was converted into oxide by prolonged ignition. The oxide was examined for occluded gases, and its weight was given the necessary correction for them. The data obtained, with vacuum weights, were as follows:

Acetylacetonate.	*Oxide.*	*Per cent. GlO.*
2.62245	.31798	12.125
3.28037	.39757	12.119
2.08993	.25286	12.099
2.41401	.29233	12.109
1.61353	.19554	12.118
1.39714	.16905	12.100
1.85023	.22419	12.117
		Mean, 12.1124, ± .0025

Hence Gl=9.103.

Acetate.	*Oxide.*	*Per cent. GlO.*
2.61484	.64630	24.716
2.67721	.66109	24.693
3.11534	.76930	24.693
1.89291	.46788	24.717
1.47931	.36534	24.703
1.09012	.26911	24.686
1.35642	.33493	24.692
1.56787	.38715	24.693
1.34465	.33204	24.693
		Mean, 24.698, ± .0025

Hence Gl=9.106.

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[1] Journ. Amer. Chem. Soc., 26, 721. 1904.

In a later note,[1] Parsons combined the two series of determinations, and deduced simultaneous values for Gl and C. If O = 16, and H = 1.008, then Gl = 9.112, and C = 12.007. These figures furnish a good check upon the general accuracy of the manipulations.

The atomic weight of glucinum is now fixed by the following ratios:

(1). $GlO : BaSO_4 :: 100 : 922.977, \pm .805$
(2). $4CO_2 : GlO :: 79.423, \pm .0052 : 11.433, \pm .081$
(3). $GlSO_4.4H_2O : GlO :: 100 : 14.153, \pm .0014$
(4). $Gl(C_5H_7O_2)_2 : GlO :: 100 : 12.1124, \pm .0025$
(5). $Gl_4O(C_2H_3O_2)_6 : 4GlO :: 100 : 24.698, \pm .0025$

Reducing these ratios with

C = 12.0038, ± .0002 S = 32.0667, ± .00075
H = 1.00779, ± .00001 Ba = 137.363, ± .0025

we have—

From ratio 3 ....Gl = 9.0805, ± .0025
" " 4 ....9.1032, ± .0052
" " 5 ....9.1061, ± .0027
" " 1 ....9.2912, ± .0221
" " 2 ....9.3375, ± .1775

General mean, Gl = 9.0945, ± .0017

The last two values are evidently worthless, but they carry practically no weight in the combination. For all practical purposes the atomic weight of glucinum may be taken as 9.1, which must be very near the true value.

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[1] Journ. Amer. Chem. Soc., 27, 1204. 1905.

MAGNESIUM.

There is perhaps no common metal of which the atomic weight has been subjected to closer scrutiny than that of magnesium. The value is low, and its determination should, therefore, be relatively free from many of the ordinary sources of error: it is extensively applied in chemical analysis, and ought consequently to be accurately ascertained.

The early determinations made by Berzelius, Longchamp and Gay-Lussac need not be considered here, as they have only antiquarian value. The investigations which demand attention are those of Scheerer, Svanberg and Nordenfeldt, Jacquelain, Macdonnell, Bahr, Marchand and Scheerer, Dumas, Marignac, Burton and Vorce, and Richards and Parker.

Scheerer's method of investigation was exceedingly simple.[1] He merely estimated the sulphuric acid in anhydrous magnesium sulphate, employing the usual process of precipitation as barium sulphate. He gives no weighings, but reports the percentages of SO_3 thus found. In his calculations, $O=100$, $SO_3=500.75$, and $BaO=955.29$. It is easy, therefore, to recalculate the figures which he gives, so as to establish what his method really represents, viz., the ratio between the sulphates of barium and magnesium.

Thus revised, his four analyses show that 100 parts of $MgSO_4$ yield the following quantities of $BaSO_4$:

	Per cent. SO_3.
193.575	66.573
193.677	66.608
193.767	66.639
193.631	66.592

Mean, 193.6625, ± .0274

Hence $Mg=24.467$.

In a later note[2] Scheerer shows that the barium sulphate of these experiments carries down with it magnesium salts in such quantity as to make the atomic weight of magnesium 0.039 too low.

The work of Bahr, Jacquelain, Macdonnell, and Marignac, and in part that of Svanberg and Nordenfeldt, also relates to the composition of magnesium sulphate.

Jacquelain's experiments were as follows:[3] Dry magnesium sulphate was prepared by mixing the ordinary hydrous salt to a paste with sul-

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[1] Poggend. Annal., 69, 535. 1846.

[2] Poggend. Annal., 70, 407.

[3] Ann. Chim. Phys. (3), 32, 202.

phuric acid, and calcining the mass in a platinum crucible over a spirit lamp to constant weight and complete neutrality of reaction. This dry sulphate was weighed and intensely ignited three successive times. The weight of the residual MgO having been determined, it was moistened with sulphuric acid and recalcined over a spirit lamp, thus reproducing the original weight of $MgSO_4$. Jacquelain's weighings for these two experiments show that 100 parts of MgO correspond to the quantities of $MgSO_4$ given in the last column:

1.466 grm.	$MgSO_4$	gave	.492 grm.	MgO.		297.968
.492 "	MgO	"	1.466 "	$MgSO_4$.		297.968

Hence Mg = 24.444.

Jacquelain also made one estimation of sulphuric acid in the foregoing sulphate as $BaSO_4$. His result (1.464 grm. $MgSO_4$ = 2.838 grm. $BaSO_4$), reduced to the standard adopted in dealing with Scheerer's experiments, gives for 100 parts of $MgSO_4$, 193.852 $BaSO_4$. If this figure be given equal weight with a single experiment in Scheerer's series, and combined with the latter, the mean will be 193.700, ± .0331. This again is subject to the correction pointed out by Scheerer for magnesium salts retained by the barium sulphate, but such a correction determined by Scheerer for a single experiment is only a rough approximation, and hardly worth applying.

The determinations published by Macdonnell[1] are of slight importance, and all depend upon magnesium sulphate. First, the crystallized salt, $MgSO_4.7H_2O$, was dried in vacuo over sulphuric acid and then dehydrated at a low red heat. The following percentages of water were found:

51.17
51.13
51.14
51.26
51.28
51.29

Mean, 51.21, ± .020

Secondly, anhydrous magnesium sulphate was precipitated with barium chloride. From the weight of the barium sulphate, with SO_3 = 80 and Ba = 137, Macdonnell computes the percentages of SO_3 given below. I calculate them back to the observed ratio in uniformity with Scheerer's work:

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[1] Proc. Royal Irish Acad., 5, 303. British Assoc. Report, 1852, part 2, p. 36.

Per cent. SO_3.	*Ratio, $MgSO_4$:$BaSO_4$.*
66.67	194.177
66.73	194.351
66.64	194.089
66.65	194.118
66.69	194.239

In another experiment 60.05 grains $MgSO_4$ gave 116.65 grains $BaSO_4$, a ratio of 100 : 194.254. Including this with the preceding figures, they give a mean of 194.205, ± .027. This, combined with the work of Scheerer and Jacquelain, 193.700, ± .033, gives a general mean of—

$$MgSO_4 : BaSO_4 : : 100 : 194.003, \pm .021$$

In one final experiment Macdonnell found that 41.44 grains of pure magnesia gave 124.40 grains of $MgSO_4$, or 300.193 per cent.

From Macdonnell's data the atomic weight of magnesium ranges between 24.00 and 24.43.

Bahr's[1] work resembles in part that of Jacquelain. This chemist converted pure magnesium oxide into sulphate, and from the increase in weight determined the composition of the latter salt. From his weighings 100 parts of MgO equal the amounts of $MgSO_4$ given in the third column:

1.6938 grm. MgO gave	5.0157 grm. $MgSO_4$.		296.122
2.0459	"	6.0648	"	296.437
1.0784	"	3.1925	"	296.040
			Mean,	296.200, ± .0815

Hence Mg = 24.812.

About four years previous to the investigations of Bahr the paper of Svanberg and Nordenfeldt[2] appeared. These chemists started with the oxalate of magnesium, which was dried at a temperature of from 100° to 105° until it no longer lost weight. The salt then contained two molecules of water, and upon strong ignition it left a residue of MgO. The percentage of MgO in the oxalate was as follows:

7.2634 grm. oxalate gave	1.9872 grm. oxide.		27.359 per cent.	
6.3795	"	1.7464	"	27.375 "
6.3653	"	1.7418	"	27.364 "
6.2216	"	1.7027	"	27.368 "
			Mean,	27.3665, ± .0023

Hence Mg = 24.706.

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[1] Journ. prakt. Chem., 56, 310. 1852.
[2] Journ. prakt. Chem., 45, 473. 1848.

In three of these experiments the MgO was treated with H_2SO_4, and converted, as by Jacquelain and by Bahr in their later researches, into $MgSO_4$. One hundred parts of MgO gave of $MgSO_4$ as follows:

1.9872 grm. MgO	gave	5.8995 grm. $MgSO_4$.		296.875
1.7464	"	5.1783	"	296.513
1.7418	"	5.1666	"	296.624
				Mean, 296.671, ± .072

Hence Mg=24.711.

In 1850 the elaborate investigations of Marchand and Scheerer[1] appeared. These chemists undertook to determine the composition of some natural magnesites, and, by applying corrections for impurities, to deduce from their results the sought-for atomic weight. The magnesite chosen for the investigation was, first, a yellow, transparent variety from Snarum; second, a white opaque mineral from the same locality; and, third, a very pure quality from Frankenstein. In each case the impurities were carefully determined; but only a part of the details need be cited here. Silica was, of course, easily corrected for by simple subtraction from the sum of all of the constituents; but iron and calcium, when found, having been present in the mineral as carbonates, required the assignment to them of a portion of the carbonic acid. In the atomic weight determinations the mineral was first dried at 300°. The loss in weight upon ignition was then carbon dioxide. It was found, however, that even here a correction was necessary. Magnesite, upon drying at 300°, loses a trace of CO_2, and still retains a little water; on the other hand, a minute quantity of CO_2 remains even after ignition. The CO_2 expelled at 300° amounted in one experiment to .054 per cent.; that retained after calcination to .055 per cent. Both errors tend in the same direction, and increase the apparent percentage of MgO in the magnesite. On the yellow mineral from Snarum the crude results are as follows, giving percentages of MgO, FeO and CO_2 after eliminating silica:

CO_2.	*MgO.*	*FeO.*
51.8958	47.3278	.7764
51.8798	47.3393	.7809
51.8734	47.3154	.8112
51.8875	47.3372	.7753
	Mean, 47.3299, ± .0037	

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[1] Journ. prakt. Chem., 50, 385.

After applying corrections for loss and retention of CO_2, as previously indicated, the mean results of the foregoing series become—

CO_2.	*MgO.*	*FeO.*
51.9931	47.2743	.7860

The ratio between the MgO and the CO_2, after correcting for the iron, will be considered further on.

Of the white magnesite from Snarum but a single analysis was made, which for present purposes may be ignored. As for the Frankenstein mineral three series of analyses were executed. In the first series the following results were obtained:

8.996	grm. CO_2 =	8.2245	grm. MgO.	47.760	per cent. MgO.
7.960	"	7.2775	"	47.761	"
9.3265	"	8.529	"	47.767	"
7.553	"	6.9095	"	47.775	"

Mean, 47.766, ± .0022

This mean, corrected for loss of CO_2 in drying, becomes 47.681. I give series second with corrections applied:

6.8195	grm. $MgCO_3$ gave	3.2500	grm. MgO.	47.658	per cent.
11.3061	"	5.3849	"	47.628	"
9.7375	"	4.635	"	47.599	"
12.3887	"	5.9033	"	47.650	"
32.4148	"	15.453	"	47.674	"
38.8912	"	18.5366	"	47.663	"
26.5223	"	12.6445	"	47.675	"

Mean, 47.650, ± .0069

The third series was made upon very pure material, so that the corrections, although applied, were less influential. The results were as follows:

4.2913	grm. $MgCO_3$ gave	2.0436	grm. MgO.	47.622	per cent.
27.8286	"	13.2539	"	47.627	"
14.6192	"	6.9692	"	47.672	"
18.3085	"	8.7237	"	47.648	"

Mean, 47.642, ± .0077

In a supplementary paper[1] by Scheerer, it was shown that an important correction to the foregoing data had been overlooked. Scheerer, re-

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[1] Ann. Chem. Pharm., 110, 240.

examining the magnesites in question, discovered in them traces of lime, which had escaped notice in the original analyses. With this correction the two magnesites in question exhibit the following mean composition:

	Snarum.	*Frankenstein.*
CO_2	52.131	52.338
MgO	46.663	47.437
CaO	.430	.225
FeO	.776	.....
	100.000	100.000

Correcting for lime and iron, by assigning each its share of CO_2, the Snarum magnesite gives as the true percentage of magnesia in pure magnesium carbonate, the figure 47.624. To this, without serious mistake, we may assign the weight indicated by the probable error, ± .0037, the quantity previously deduced from the percentages of MgO given in the uncorrected analyses.

From the Frankenstein mineral, similarly corrected, the final mean percentage of MgO in $MgCO_3$ becomes 47.628. This, however, represents three series of analyses, whose combined probable errors may be properly assigned to it. The combination is as follows:

± .0022
± .0069
± .0077

Result, ± .0020, probable error of the general mean.

We may now combine the results obtained from both magnesites:

Snarum mineral	Per cent. MgO,	47.624, ± .0037
Frankenstein mineral	"	47.628, ± .0020
General mean	Per cent. MgO,	47.627, ± .0018

Hence Mg = 24.016.

The next investigation upon the atomic weight of magnesium which we have to consider is that of Dumas.[1] Pure magnesium chloride was placed in a boat of platinum, and ignited in a stream of dry hydrochloric acid gas. The excess of the latter having been expelled by a current of dry carbon dioxide, the platinum boat, still warm, was placed in a closed vessel and weighed therein. After weighing, the chloride was dissolved and titrated in the usual manner with a solution containing a known quantity of pure silver. The weighings which Dumas reports give, as poportional to 100 parts of silver, the quantities of $MgCl_2$ stated in the third column:

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[1] Ann. Chem. Pharm., 113, 33. 1860.

2.203	grm. $MgCl_2$ =	4.964	grm. Ag.	44.380
2.5215	"	5.678	"	44.408
2.363	"	5.325	"	44.376
3.994	"	9.012	"	44.319
2.578	"	5.834	"	44.189
2.872	"	6.502	"	44.171
2.080	"	4.710	"	44.161
2.214	"	5.002	"	44.262
2.086	"	4.722	"	44.176
1.688	"	3.823	"	44.154
1.342	"	3.031	"	44.276

Mean, 44.261, ± .020

Hence Mg = 24.581.

This determination gives a very high value to the atomic weight of magnesium, which is unquestionably wrong. The error, probably, is due to the presence of oxychloride in the magnesium chloride taken, an impurity tending to raise the apparent atomic weight of the metal. Richards' and Parker's revision of this ratio is more satisfactory.

Marignac,[1] in 1883, resorted to the old method of determination, depending upon the direct ratio between MgO and SO_3. This ratio he measured both synthetically and analytically. First, magnesia from various sources was converted into sulphate. The $MgSO_4$ from 100 parts of MgO is given in the third column:

	MgO.	*$MgSO_4$.*	*Ratio.*
1.	1.5635	4.6620	298.17
2.	1.4087	4.2025	298.32
3.	1.5917	4.7480	298.30
4.	1.4705	4.3855	298.23
5.	1.4778	4.4060	298.15
6.	1.6267	4.8530	298.33
7.	1.3657	4.0740	298.37
8.	1.9575	5.8390	298.29
9.	1.6965	5.0600	298.26
10.	1.8680	5.5715	298.26

Mean, 298.27, ± .0149

Hence Mg = 40.383.

The magnesia for experiments 1 to 5 was prepared by calcination of the nitrate, that of 6 to 8 from the sulphate, and the remaining two from the carbonate. But Richards and Rogers[2] have shown that magnesia derived from the nitrate always contains occluded gaseous impurity, so that the experiments depending upon its use are somewhat questionable. The results tend to give an atomic weight for magnesium which is possibly too high. Whether the other samples of magnesia are subject to similar objections I cannot say.

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[1] Arch. Sci. Phys. Nat. (3), 10, 206. Oeuvres Complètes, 2, 742.

[2] Amer. Chem. Journ., 15, 567. 1893.

Marignac's second series was obtained by the calcination of the sulphate, with results as follows:

$MgSO_4$.	MgO.	*Ratio.*
3.7705	1.2642	298.25
4.7396	1.5884	298.39
3.3830	1.1345	298.19
4.7154	1.5806	298.33
4.5662	1.5302	298.43
4.5640	1.5300	298.30
3.2733	1.0979	298.14
4.8856	1.6378	298.30
5.0092	1.6792	298.31
5.3396	1.7898	298.33
5.1775	1.7352	298.38
5.0126	1.6807	298.24
5.0398	1.6894	298.32
		Mean, 298.30, ± .0150

Hence Mg=40.377.

These data may now be combined with the work of previous investigators, giving Macdonnell's one result and Jacquelain's two, each equal weight with a single experiment in Bahr's series:

Macdonnell	300.193, ± .1413
Jacquelain	297.968, ± .0999
Bahr	296.200, ± .0815
Svanberg and Nordenfeldt	296.671, ± .0720
Marignac, synthetic	298.27, ± .0149
Marignac, calcination	298.30, ± .0150
General mean	298.210, ± .0103

Burton and Vorce,[1] who published their work on magnesium in 1890, began with the metal itself, which had been purified by distillation in a Sprengel vacuum. This metal was dissolved in pure nitric acid, and the resulting nitrate was converted into oxide by calcination at a white heat. The oxide was carefully tested for oxides of nitrogen, which were proved to be absent, but occluded gases, the impurity pointed out by Richards and Rogers, were not suspected. This impurity must have been present, and it would tend to lower the apparent atomic weight of magnesium as calculated from the data obtained. The results were as follows, together with the percentage of Mg in MgO:

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[1] Amer. Chem. Journ., 12, 219. 1890.

Mg taken.	*MgO formed.*	*Per cent. Mg.*
.33009	.54766	60.273
.34512	.57252	60.281
.26058	.43221	60.290
.28600	.47432	60.297
.30917	.51273	60.299
.27636	.45833	60.271
.36457	.60475	60.284
.32411	.53746	60.304
.32108	.53263	60.282
.28323	.46988	60.262
		Mean, 60.2845, ± .0027

Hence Mg = 24.287.

The best determinations of all are those of Richards and Parker,[1] who studied magnesium chloride with all the precautions suggested by the most recent researches. The salt itself was not only free from oxychloride, but also spectroscopically pure as regards alkaline contaminations, and all weighings were reduced to a vacuum standard. The first series of experiments gives the ratio between silver chloride and magnesium chloride, and I have reduced the data to the form $2AgCl : MgCl_2 :: 100 : x$. The weighings and values for x are subjoined:

$MgCl_2$.	*AgCl.*	*Ratio.*
1.33550	4.01952	33.225
1.51601	4.56369	33.219
1.32413	3.98528	33.226
1.40664	4.23297	33.231
1.25487	3.77670	33.227
		Mean, 33.226, ± .0013

Hence Mg = 24.335.

The remaining series of experiments, three in number, relate to the ratio $2Ag : MgCl_2$, which was earlier investigated by Dumas. For the elaborate details of manipulation the original memoir must be consulted. I can give little more than the weights found, and their reduction to the usual form of ratio, $2Ag : MgCl_2 :: 100 : x$:

Second Series.

$MgCl_2$.	*Ag.*	*Ratio.*
2.78284	6.30284	44.152
2.29360	5.19560	44.145
2.36579	5.35989	44.130
		Mean, 44.142, ± .0043

Hence Mg = 24.324.

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[1] Zeitsch. anorg. Chem., 13, 81. 1896.

This series gives slightly higher results than the others, and the authors, for reasons which they assign, discard it:

Third Series.

$MgCl_2$.	*Ag.*	*Ratio.*
1.99276	4.51554	44.131
1.78870	4.05256	44.138
2.12832	4.82174	44.140
2.51483	5.69714	44.141
2.40672	5.45294	44.136
1.95005	4.41747	44.144

Mean, 44.138, ± .0013

Hence Mg=24.315.

The fourth series, because of the experience gained in the conduct of the preceding determinations, is best of all, and the authors adopt its results in preference to the others:

Fourth Series.

$MgCl_2$.	*Ag.*	*Ratio.*
2.03402	4.60855	44.136
1.91048	4.32841	44.138
2.09932	4.75635	44.137
1.82041	4.12447	44.137
1.92065	4.35151	44.138
1.11172	2.51876	44.138

Mean, 44.137, ± .0003

Hence Mg=24.313.

These series combine with that of Dumas as follows:

Dumas	44.261, ± .0200
Richards and Parker, second series	44.142, ± .0043
Richards and Parker, third series	44.138, ± .0013
Richards and Parker, fourth series	44.137, ± .0003
General mean	44.138, ± .0003

Here the first two values practically vanish, and the third and fourth series of Richards and Parker appear alone. Combining this figure with their value for the AgCl ratio, the subjoined cross ratio appears: Ag : Cl :: 100 : 32.842.

To sum up, we now have the following ratios, bearing upon the atomic weight of magnesium:

(1). $MgSO_4$:$BaSO_4$::100:194.003, ± .021
(2). MgO:$MgSO_4$::100:298.210, ± .0103
(3). Per cent. of water in $MgSO_4$, $7H_2O$, 51.21, ± .020
(4). Per cent. of MgO in oxalate, 27.3665, ± .0023
(5). Per cent. of MgO in carbonate, 47.627, ± .0018
(6). Per cent. of Mg in MgO, 60.2845, ± .0027
(7). 2Ag:$MgCl_2$::100:44.138, ± .0003
(8). 2AgCl:$MgCl_2$::100:33.226, ± .0013

The antecedent values for reducing these ratios are:

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Ba = 137.363, ± .0025
S = 32.0667, ± .00075
C = 12.0038, ± .0002
H = 1.00779, ± .00001

Hence, for magnesium, we have

From ratio 5 .... Mg = 24.0162, ± .0020
" " 3 .... 24.0803, ± .0680
" " 1 .... 24.2561, ± .0333
" " 6 .... 24.2865, ± .0020
" " 7 .... 24.3154, ± .0007
" " 8 .... 24.3344, ± .0033
" " 2 .... 24.394, ± .0021
" " 4 .... 24.7063, ± .0037

General mean, Mg = 24.3039, ± .0006

This final value is possibly a little too low, as compared with the individual values which are presumably the best. The figures are, however, peculiarly instructive. Ratios 2, 7 and 8, representing essentially the work of Marignac and Richards and Parker, were originally reduced with the Stas values for sulphur, silver and chlorine. These values are Ag = 107.93, Cl = 35.457 and S = 32.074. With these figures, and using only Marignac's data for ratio 2, the following values for magnesium are obtained:

From $MgSO_4:MgO$, Mg = 24.383
" $2Ag:MgCl_2$, Mg = 24.382
" $2AgCl:MgCl_2$, Mg = 24.371

From the general mean represented by ratio 2, Mg = 24.398, a slightly higher value.

The concordance here is much greater than in the reduction with modern values, and may be interpreted in either of two ways. Either the Stas values are more exact than the new values for Ag, Cl and S, or the earlier concordance is deceptive. In short, an agreement between determinations of atomic weight made by diverse methods, is dependent in great part upon the antecedent values used in the computations. Concordance and discordance may be equally deceptive. Illustrations of this statement are not uncommon.

ZINC.

The several determinations of the atomic weight of zinc are by no means closely concordant. The results obtained by Gay-Lussac[1] and Berzelius[2] were undoubtedly too low, and may be disregarded here. We need consider only the work done by later investigators.

In 1842 Jacquelain published the results of his investigations upon this important constant.[3] In two experiments a weighed quantity of zinc was converted into nitrate, and that by ignition in a *platinum* crucible was reduced to oxide. In two other experiments sulphuric acid took the place of nitric. As the zinc contained small quantities of lead and iron, these were estimated, and the necessary corrections applied. From the weights of metal and oxide given by Jacquelain the percentages have been calculated:

Nitric Series.

9.917 grm. Zn gave	12.3138 grm. ZnO.	80.536 per cent. Zn.
9.809 "	12.1800 "	80.534 "

Sulphuric Series.

2.398 grm. Zn gave	2.978 grm. ZnO.	80.524 "
3.197 "	3.968 "	80.570 "

Mean of all four, 80.541, ± .007

Hence Zn = 66.224.

The method adopted by Axel Erdmann[4] is essentially the same as that of Jacquelain, but varies from the latter in certain important details. First, pure zinc oxide was prepared, ignited in a covered crucible with sugar, and then, to complete the reduction, ignited in a porcelain tube in a current of hydrogen. The pure zinc thus obtained was converted into oxide by means of treatment with nitric acid and subsequent ignition in a *porcelain* crucible. Erdmann's figures give us the following percentages of metal in the oxide:

80.247
80.257
80.263
80.274

Mean, 80.260, ± .0037

Hence Zn = 65.054.

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[1] Mémoire d'Arceuil, 2, 174.

[2] Gilb. Annal., 37, 460.

[3] Compt. Rend., 14, 636.

[4] Poggend. Annal., 62, 611. Berz. Lehrb., 3, 1219.

Upon comparing Erdmann's results with those of Jacquelain two points are worth noticing: First, Erdmann worked with purer material than Jacquelain, although the latter applied corrections for the impurities which he knew were present; secondly, Erdmann calcined his zinc nitrate in a porcelain crucible, while Jacquelain used platinum. In the latter case it has been shown that portions of zinc may become reduced and alloy themselves with the platinum of the crucible; hence a lower weight of oxide from a given quantity of zinc, a higher percentage of metal, and an increased atomic weight. This source of constant error has undoubtedly affected Jacquelain's experiments, and vitiated his results. In Erdmann's work no such errors seem to be present.

Favre[1] employed two methods of investigation. First, zinc was dissolved in sulphuric acid, the hydrogen evolved was burned, and the weight of water thus formed was determined. To his weighings I append the ratio between metallic zinc and 100 parts of water:

25.389 grm. Zn	gave 6.928 grm. H_2O.		366.469
30.369 "	8.297 "		366.024
31.776 "	8.671 "		366.463
		Mean,	366.319, ± .088

Hence Zn = 65.995.

The second method adopted by Favre was to burn pure zinc oxalate, and to weigh the oxide and carbonic acid thus produced. From the ratio between these two sets of weights the atomic weight of zinc is easily deducible. From Favre's weighings, if $CO_2 = 100$, ZnO will be as given in the third column below:

7.796 grm. ZnO	= 8.365 grm. CO_2.		93.198
7.342 "	7.883 "		93.137
5.2065 "	5.588 "		93.173
		Mean,	93.169, ± .012

Hence Zn = 65.996.

Both of these determinations are open to objections. In the water series it was essential that the hydrogen should first be thoroughly dried before combustion, and then that every trace of water formed should be collected. A trivial loss of hydrogen or of water would tend to increase the apparent atomic weight of zinc.

In the combustion of the zinc oxalate equally great difficulties are encountered. Here a variety of errors are possible, such as are due, for example, to impurity of material, to imperfect drying of the carbon dioxide, and to incomplete collection of the latter. Indeed, a fourth

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[1] Ann. Chim. Phys. (3), 10, 163. 1844.

combustion is omitted from the series as given, having been rejected by Favre himself. In this case the oxide formed was contaminated by traces of sulphide.

Baubigny,[1] in 1883, resorted to the well-known sulphate method. Zinc sulphate, elaborately purified, was dried at 440° to constant weight, and then calcined at a temperature equal to the fusing point of gold. These data were obtained:

$ZnSO_4$.	ZnO.	Per cent. ZnO.
6.699	3.377	50.410
8.776	4.4245	50.416
		Mean, 50.413, ± .0020

Hence Zn = 65.400.

In Marignac's determinations of the atomic weight of zinc, published also in 1883,[2] there is a peculiar complication. After testing and criticising some other methods, he finally decided to study the double salt K_2ZnCl_4, which, however, is difficult to obtain in absolutely definite condition. Although the compound was purified by repeated crystallizations, it was found to deliquesce readily, and thereby to undergo partial dissociation, losing chloride of zinc, and leaving the porous layer on the crystalline surfaces richer in potassium. In order to evade this difficulty, Marignac placed a large quantity of the salt in a funnel, and collected the liquid product of deliquescence as it ran down. In this product he determined chlorine by volumetric titration with a standard solution of silver, and also estimated zinc by precipitation with sodium carbonate, and weighing as oxide. From the data thus obtained equations were formed, giving for each analysis an atomic weight of zinc which is independent of the proportion between $ZnCl_2$ and KCl in the substance analyzed. The data unfortunately are too bulky for reproduction here and the calculations are complex; but the results found for zinc, when Ag = 107.93, Cl = 35.457, and K = 39.137, are as follows:

1. One titration	Zn = 65.22
2. Two titrations	65.37
3. Two titrations	65.31
4. Two titrations	65.28
5. One titration	65.26

Each of these values represents a distinct sample of the deliquesced material, and the number of chlorine determinations is indicated.

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[1] Compt. Rend., 97, 906. 1883.

[2] Arch. Sci. Phys. Nat. (3), 10, 194. Oeuvres Complètes, 2, 731.

A second set of determinations was made by the same analytical method directly upon the recrystallized and carefully dried K_2ZnCl_4. The values for Zn are as follows:

6. Two titrations	Zn = 65.28
7. Two titrations	65.39
8. One titration	65.32

In order to adapt these data to the uniform scheme of calculation employed in this work, taking into account their probable error and the probable errors of the antecedent values for K, Cl and Ag, it seems to be best to calculate them back with the atomic weights used by Marignac into the form of the ratio $4Ag : K_2ZnCl_4 :: 100 : x$. Doing this, and taking each value as many times as there are titrations represented in it—that is, giving the results of a double determination twice the weight of a single one—we have the following series of data for the ratio in question:

From 1..............................	66.090
From 2..............................	66.124 66.124
From 3..............................	66.110 66.110
From 4..............................	66.104 66.104
From 5..............................	66.099
From 6..............................	66.104 66.104
From 7..............................	66.129 66.129
From 8..............................	66.113

Mean, 66.111, ± .0023

Hence, from Marignac's work, $4Ag : K_2ZnCl_4 :: 100 : 66.111, \pm .0023$, a ratio which can be discussed along with others at the close of this chapter. It corresponds to Zn = 65.249.

During the years between 1883 and 1889, a number of determinations were made of the direct ratio between zinc and hydrogen—that is, weighed quantities of zinc were dissolved in acid, the hydrogen evolved was measured, and from its volume, with Regnault's data, the weight of H was computed. First in order are Van der Plaats' determinations,[1] whose results, as given by himself, are subjoined. The weights are reduced to a vacuum. Sulphuric acid was the solvent:

Zn, grm.	*H, litres.*	*Zn =*
6.6725	1.1424	65.21
9.1271	1.5643	65.14
13.8758	2.3767	65.18

Mean, 65.177, ± .0137

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[1] Compt. Rend., 100, 52. 1885.

With the new value for the weight of hydrogen, 0.89872 gramme per litre, this becomes Zn = 64.980, ± .0137, when H = 1.

Reynolds and Ramsay made 29 determinations of this ratio,[1] rejecting, however, all but 5. The weighings were reduced to vacuum, and in each experiment the volume of hydrogen was fixed by the mean of seven or eight readings. The values for Zn are as follows:

65.5060
65.4766
65.4450
65.5522
65.4141

Mean, 65.4787, ± .0161

These values were computed with Regnault's data for the weight of H. Corrected by the new value the mean becomes Zn = 65.280, ± .0161.

A few determinations by Mallet were made incidentally to his work on the atomic weight of gold, and appear in the same paper.[2] According to these experiments, one gramme of zinc gives—

341.85 cc. H., and Zn = 65.158
341.91 " " 65.146
341.93 " " 65.143
342.04 " " 65.122

Mean, 65.142, ± .0039

In this case the Crafts-Regnault weight of H was taken, one litre = .08979 gramme. Corrected, the mean gives Zn = 65.082, ± .0039.

Two other series of determinations of questionable value remain to be noticed before leaving the consideration of the direct H : Zn ratio. They represent really the practice work of students, and are interesting as an illustration of the closeness with which such work can be done. The first series was made in the laboratory of the Johns Hopkins University, under the direction of Morse and Keiser,[3] and contains 51 determinations, as follows:

Zn =

64.68
65.26
65.32
65.20
65.60
64.60
65.00
65.74
64.72
65.26
64.74
64.72
65.10
64.76
65.40
64.80
65.20
64.40
65.00
64.40
65.24

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[1] Journ. Chem. Soc., 51, 854. 1887.
[2] Amer. Chem. Journ., 12, 205. 1890.
[3] Amer. Chem. Journ., 6, 347. 1884.

65.68	64.90	64.60
65.38	64.92	64.80
65.06	64.64	65.14
64.84	65.24	64.84
64.88	64.72	64.82
65.00	65.20	64.80
65.08	65.12	64.40
65.06	66.40	64.60
64.74	64.60	64.80
65.12	65.60	64.74

Mean of all, Zn = 64.997, ± .0328

Corrected for the difference between Regnault's value for H and the new value, this becomes Zn = 64.800, ± .0328.

The second student series was published by Torrey,[1] who gives 15 determinations, as follows:

Zn =

65.36	64.96
65.30	64.70
64.92	65.00
64.72	64.78
65.04	64.44
64.80	65.24
65.20	64.92
64.90	

Mean, 64.952, ± .0436

Corrected as in the other series, this gives Zn = 64.755, ± .0436.

The five corrected means for the ratio H : Zn may now be combined, thus:

	H = 1.	*O = 16.*
Van der Plaats	64.980, ± .0137	65.487
Reynolds and Ramsay...	65.280, ± .0161	65.789
Mallet	65.082, ± .0039	65.590
Morse and Keiser........	64.800, ± .0328	65.305
Torrey	64.755, ± .0036	65.260
General mean	65.079, ± .0036	65.587, ± .0036

Morse and Burton,[2] in their determinations of the atomic weight of zinc, returned essentially to the old method adopted by Erdmann and by Jacquelain. Their zinc was obtained spectroscopically pure by distillation in a vacuum, and was oxidized by nitric acid which left absolutely no residue upon evaporation. The conversion to oxide was effected in a porcelain crucible, which was enclosed in a larger one, and the ignition of the nitrate was carried out in a muffle. In weighing, the crucible was tared by one of nearly equal weight. Results as follows:

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[1] Amer. Chem. Journ., 10, 74. 1888.

[2] Amer. Chem. Journ., 10, 311. 1888.

Wt. Zn.	*Wt ZnO.*	*Per cent. Zn in ZnO.*
1.11616	1.38972	80.320
1.03423	1.28782	80.308
1.11628	1.38987	80.315
1.05760	1.31681	80.316
1.04801	1.30492	80.313
1.02957	1.28193	80.318
1.09181	1.35944	80.315
1.16413	1.44955	80.305
1.07814	1.34248	80.305
1.12754	1.40400	80.306
.91112	1.13446	80.310
1.10011	1.36981	80.311
1.17038	1.45726	80.313
1.03148	1.28436	80.310
1.05505	1.31365	80.308
		Mean, 80.3115, ± .00084

Hence Zn = 65.266.

Morse and Burton verified by experiment the stability of oxide of zinc at the temperatures of ignition, and found that it did not dissociate. They also proved the absence of oxides of nitrogen from the zinc oxide. The investigations of Richards and Rogers,[1] however, have shown that zinc oxide prepared by ignition of the nitrate always carries gaseous occlusions, so that the atomic weight of zinc computed from the data of Morse and Burton is certainly too low. This consideration led Morse and Arbuckle[2] to reinvestigate zinc oxide, with the purpose of avoiding the indicated error. The zinc used was a portion of the sample employed by Morse and Burton, and the process was essentially the same, except that the oxide, after weighing, was dissolved in sulphuric acid, and the gases which were evolved were collected, measured and analyzed. All weights were corrected for displacement of air. The crude data are as follows:

Wt. Zn.	*Wt. ZnO.*	*Gases, cc.*	*Per cent. Zn, uncorrected.*
1.19573	1.48860	.468	80.326
1.03381	1.28707	.402	80.323
1.06519	1.32599	.342	80.332
1.05802	1.31711	.312	80.329
1.26618	1.57619	.521	80.332
1.03783	1.29198	.408	80.329
1.08655	1.35276	.412	80.321
1.11364	1.38647	.456	80.322
			Mean, 80.327, ± .0011

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[1] Proc. Amer. Acad., 1893, 200.

[2] Am. Chem. Journ., 20, 195. 1898. Also published as a doctoral dissertation, Johns Hopkins University, by Arbuckle.

The gases evolved contained only nitrogen and oxygen, in varying proportions, which were determined in each case. Uncorrected, Zn = 65.328; corrected, the value ranged between 65.437 and 65.489, in mean, 65.456. The last figure corresponds to 80.358 per cent. of zinc in the oxide, an increase of 0.031. If we assume that the same proportional error existed in all the other experiments upon zinc oxide, the several series may be corrected and combined as follows:

Jacquelain	80.572, ± .0070
Erdmann	80.291, ± .0037
Morse and Burton	80.343, ± .00084
Morse and Arbuckle	80.358, ± .0011
General mean	80.349, ± .00065

Here the two earlier series practically disappear, and the modern determinations alone are retained.

The determinations made by Gladstone and Hibbard[1] represent still another process for measuring the atomic weight of zinc. Zinc was dissolved in a voltameter, and the same current was used to precipitate metallic silver or copper in equivalent amount. The weight of zinc dissolved, compared with the weight of the other metal thrown down, gives the atomic weight sought for. Two voltameters were used in the experiments, giving duplicate estimates for zinc with reference to each weighing of silver or copper. The silver series is as follows, with the ratio 2Ag : Zn :: 100 : *x* in the third column:

Zn.	*Ag.*	*Ratio.*
.7767	2.5589	30.353
.7758	2.5589	30.318
.5927	1.9551	30.316
.5924	1.9551	30.300
.2277	.7517	30.291
.2281	.7517	30.345
.7452	2.4588	30.307
.7475	2.4588	30.401
.8770	2.9000	30.241
.8784	2.9000	30.290
.9341	3.0809	30.319
.9347	3.0809	30.339
		Mean, 30.318, ± .0077

Hence Zn = 65.414.

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[1] Journ. Chem. Soc., 55, 443. 1889.

To the copper series I add the ratio Cu : Zn : : 100 : *x*:

Zn.	*Cu.*	*Ratio.*
.7767	.7526	103.13
.7758	.7526	103.08
.5927	.5737	103.31
.5924	.5737	103.26
.2277	.2209	103.08
.2281	.2209	103.26
.8770	.8510	103.05
.8784	.8510	103.22
.9341	.9038	103.36
.9347	.9038	103.42
		Mean, 103.22, ± .0261

Hence Zn = 65.601.

Richards and Rogers,[1] in their investigation of the atomic weight of zinc, studied the anhydrous bromide. This was prepared by solution of zinc oxide in hydrobromic acid, evaporation to dryness, and subsequent distillation in an atmosphere of carbon dioxide. In some experiments, however, the bromide was heated in an atmosphere of nitrogen, mingled with gaseous hydrobromic acid. All water can thus be removed, without formation of oxybromides.

The zinc bromide so obtained was dissolved in water and precipitated with a solution containing a known amount of silver in the form of nitrate. The silver bromide was weighed on a Gooch crucible, and the ratio $2AgBr : ZnBr_2$ thus found. An excess of silver was always used, and in one series of experiments it was estimated by precipitation with hydrobromic acid. Deducting the excess thus found from the original quantity of silver, the amount of the latter proportional to the zinc bromide was found; hence the ratio $Ag_2 : ZnBr_2$. The results, with vacuum weights, are as follows:

Series A.

$ZnBr_2$.	*AgBr.*	*Ratio.*
1.69616	2.82805	59.976
1.98198	3.30450	59.978
1.70920	2.84949	59.984
2.35079	3.91941	59.978
2.66078	4.43751	59.961
		Mean, 59.975, ± .0034

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[1] Zeitsch. anorg. Chem., 10, 1. 1895.

Series B.

$ZnBr_2$.	*Ag.*	*AgBr.*	*Ag Ratio.*	*AgBr Ratio.*
2.33882	2.24063	3.90067	104.382	59.959
1.97142	1.88837	3.28742	104.398	59.969
2.14985	2.05971	3.58539	104.376	59.961
2.00966	1.92476	3.35074	104.411	59.977
			Mean, 104.392, ± .0054	Mean, 59.967, ± .0027

At the end of the same paper, Richards alone gives two more series of determinations made upon zinc bromide prepared by the action of pure bromine upon pure electrolytic zinc. The bromide so obtained was further refined by sublimation or distillation, and dried by heating in a stream of carbon dioxide and gaseous hydrobromic acid. Thus was ensured the absence of basic salts and water. The weights and results found in the two series were as follows:

Series C.

$ZnBr_2$.	*Ag.*	*Ratio.*
6.23833	5.09766	104.379
5.26449	5.0436	104.380
9.36283	8.9702	104.377

Mean, 104.379, ± .0007

Series D.

$ZnBr_2$.	*AgBr.*	*Ratio.*
2.65847	4.43358	59.962
2.30939	3.85149	59.961
5.26449	8.77992	59.961

Mean, 59.961, ± .0004

In some details of manipulation these series differ from those given by Richards and Rogers jointly, but their minutiæ are not essential to the present discussion.

Combining these several series, we have—

For $2Ag:ZnBr_2::100:x$.

Series B	104.392, ± .0054
Series C	104.379, ± .0007
General mean	104.380, ± .0007

For $2AgBr:ZnBr_2::100:x$.

Series A	59.975, ± .0034
Series B	59.967, ± .0027
Series D	59.961, ± .0004
General mean	59.962, ± .0004

From the Ag ratio, Zn = 65.371.

From the AgBr ratio, Zn = 65.378.

And Ag : Br : : 100 : 74.077.

In order to determine the atomic weight of zinc, Meaglia[1] measured the direct ratios between that metal and silver or gold. The silver was precipitated from a sulphate solution by zinc, and the gold from a solution of sodium chloraurate. From the weights obtained the following values for zinc were computed, when Ag = 107.93 and Au = 197.2.

Silver Series.	*Gold Series.*
65.58	65.509
65.45	65.424
65.50	65.440
65.41	65.470
Mean, 65.485, ± .0247	Mean, 65.436, ± .0087

From the silver ratio, with Ag = 107.88, Zn = 65.455.

From the gold ratio, with Au = 197.269, Zn = 65.459.

For the ratio 2Ag : Zn, Gladstone and Hibbert's data give the value 30.318, ± .0077. Meaglia's figures, reduced to the same basis, give 30.337, ± .0115. The two series combined give

2Ag : Zn : : 100 : 30.324, ± .0064

For computing the atomic weight of zinc we now have the subjoined ratios:

(1). $ZnO:Zn::100:80.349$, ± .00065
(2). $ZnSO_4:ZnO::100:50.413$, ± .0020
(3). $H_2O:Zn::100:366.319$, ± .088
(4). $2CO_2:ZnO::100:93.169$, ± .012
(5). $H:Zn::1:65.079$, ± .0036
(6). $4Ag:K_2ZnCl_4::100:66.111$, ± .0023
(7). $2Ag:Zn::100:30.324$, ± .0060
(8). $Cu:Zn::100:103.22$, ± .0261
(9). $2Ag:ZnBr_2::100:104.38$, ± .0007
(10). $2AgBr:ZnBr_2::100:59.962$, ± .0004
(11). $Au:Zn::197.2:65.436$, ± .0087

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[1] Thesis, University of Grenoble, 1907.

The values used in reducing these ratios are:

Ag = 107.880, ± .00029 C = 12.0038, ± .0002
Cl = 35.4584, ± .0002 K = 39.0999, ± .0002
Br = 79.9197, ± .0003 Cu = 63.5550, ± .00063
S = 32.0667, ± .00075 Au = 197.269, ± .0030
H = 1.00779, ± .00001

Hence,

From ratio 6 ....Zn = 65.2488, ± .0100
" " 9 ....65.3709, ± .0018
" " 10 ....65.3775, ± .0017
" " 2 ....65.4004, ± .0047
" " 1 ....65.4208, ± .00053
" " 7 ....65.4271, ± .0129
" " 11 ....65.4589, ± .0088
" " 5 ....65.5870, ± .0036
" " 8 ....65.6015, ± .0166
" " 3 ....65.9946, ± .0159
" " 4 ....65.9958, ± .0106

General mean, Zn = 65.4182, ± .00048

This mean is almost identical with one of the values determined by Gladstone and Hibbert, Zn = 65.414. It is distinctly higher than the figure derived from the work of Richards and Rogers. The work of Morse and his colleagues upon zinc oxide evidently dominates the entire combination and, mathematically, at least, outweighs all else. The five highest values in the mean count for very little, in fact their rejection only lowers the atomic weight found for zinc to 65.4137.

CADMIUM.

The earliest determination of the atomic weight of this metal was by Stromeyer, who found that 100 parts of cadmium united with 14.352 of oxygen.[1] Hence Cd = 111.483. This result has now only a historical interest.

The more modern estimates of the atomic weight of cadmium begin with the work of v. Hauer.[2] He heated pure anhydrous cadmium sulphate in a stream of dry hydrogen sulphide, and weighed the cadmium sulphide thus obtained. His results were as follows, with the percentage of CdS in $CdSO_4$ therefrom deduced:

7.7650	grm. $CdSO_4$ gave	5.3741	grm. CdS.	69.209	per cent.
6.6086	"	4.5746	"	69.222	"
7.3821	"	5.1117	"	69.245	"
6.8377	"	4.7336	"	69.228	"
8.1956	"	5.6736	"	69.227	"
7.6039	"	5.2634	"	69.220	"
7.1415	"	4.9431	"	69.217	"
5.8245	"	4.0335	"	69.251	"
6.8462	"	4.7415	"	69.257	"

Mean, 69.231, ± .0042

Hence Cd = 111.935.

Lenssen[3] worked upon pure cadmium oxalate, handling, however, only small quantities of material. This salt, upon ignition, leaves the following percentages of oxide:

.5128	grm. oxalate gave	.3281	grm. CdO.	63.982	per cent.
.6552	"	.4193	"	63.996	"
.4017	"	.2573	"	64.053	"

Mean, 64.010, ± .014

Hence Cd = 112.07.

Dumas[4] dissolved pure cadmium in hydrochloric acid, evaporated the solution to dryness, and fused the residue in hydrochloric acid gas. The cadmium chloride thus obtained was dissolved in water and titrated with a solution of silver after the usual manner. From Dumas' weighings I calculate the ratio between $CdCl_2$ and 100 parts of silver:

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[1] See Berz. Lehrbuch, 5th Aufl., 3, 1219.

[2] Journ. prakt. Chem., 72, 350. 1857.

[3] Journ. prakt. Chem., 79, 281. 1860.

[4] Ann. Chem. Pharm., 113, 27. 1860.

2.369	grm. $CdCl_2$ =	2.791	grm. Ag.	84.880
1.540	"	5.348	"	84.892
6.177	"	7.260	"	85.083
2.404	"	2.841	"	84.618
3.5325	"	4.166	"	84.794
4.042	"	4.767	"	84.791

Mean, 126.076, ± .0052

Hence Cd = 112.14.

Next in order comes Huntington's[1] work, carried out in the laboratory of J. P. Cooke. Bromide of cadmium was prepared by dissolving the carbonate in hydrobromic acid, and the product, dried at 200°, was purified by sublimation in a porcelain tube. Upon the compound thus obtained two series of experiments were made.

In one series the bromide was dissolved in water, and a quantity of silver not quite sufficient for complete precipitation of the bromine was then added in nitric acid solution. After the precipitate had settled, the supernatant liquid was titrated with a standard solution of silver containing one gramme to the litre. The precipitate was washed by decantation, collected by reverse filtration and weighed. To the weighings I append the ratio between $CdBr_2$ and 100 parts of silver bromide:

1.5592	grm. $CdBr_2$ gave	2.1529	grm. AgBr.	Ratio,	72.423
*3.7456	"	5.1724	"	"	72.415
2.4267	"	3.3511	"	"	72.415
*3.6645	"	5.0590	"	"	72.435
*3.7679	"	5.2016	"	"	72.437
2.7938	"	3.8583	"	"	72.410
*1.9225	"	2.6552	"	"	72.405
3.4773	"	4.7593	"	"	72.433

Mean, 72.4216, ± .0028

Hence Cd = 112.18.

The second series was like the first, except that the weight of silver needed to effect precipitation was noted, instead of the weight of silver bromide formed. In the experiments marked with an asterisk, both the amount of silver required and the amount of silver bromide thrown down were determined in one set of weighings. The third column gives the $CdBr_2$ proportional to 100 parts of silver:

*3.7456	grm. $CdBr_2$ =	2.9715	grm. Ag.	126.051
5.0270	"	3.9874	"	126.072
*3.6645	"	2.9073	"	126.045
*3.7679	"	2.9888	"	126.067
*1.9225	"	1.5248	"	126.082
2.9101	"	2.3079	"	126.093
3.6510	"	2.8951	"	126.110
3.9782	"	3.1551	"	126.088

Mean, 84.843, ± .026

Hence Cd = 112.19.

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[1] Proc. Amer. Acad., 17, 28. 1881.

According to Huntington's own calculations, these experiments fix the ratio between silver, bromine and cadmium as Ag : Br : Cd :: 108 : 80 : 112.31.

In 1890, Partridge[1] published determinations of the atomic weight of cadmium, made by three methods, the weighings being reduced to a vacuum standard throughout. First, Lenssen's method was followed, viz., the ignition of the oxalate, with the subjoined results:

CdC_2O_4.	CdO.	*Per cent.* CdO.
1.09898	.70299	63.966
1.21548	.77746	63.962
1.10711	.70807	63.957
1.17948	.75440	63.959
1.16066	.74327	63.959
1.17995	.75471	63.964
1.34227	.85864	63.968
1.43154	.91573	63.970
1.53510	.98197	63.968
1.41311	.90397	63.971

Mean, 63.964, ± .0010

Hence Cd = 111.80.

Secondly, v. Hauer's experiments were repeated, cadmium sulphate being reduced to sulphide by heating in a stream of H_2S. The following data were obtained:

$CdSO_4$.	CdS.	*Per cent.* CdS.
1.60514	1.11076	69.204
1.55831	1.07834	69.197
1.67190	1.15669	69.185
1.66976	1.15554	69.200
1.40821	.97450	69.202
1.56290	1.08156	69.205
1.63278	1.12985	69.194
1.58270	1.09524	69.198
1.53873	1.06481	69.201
1.70462	1.17962	69.201

Mean, 69.199, ± .0012
v. Hauer found, 69.231, ± .0042

General mean, 69.202, ± .0012

The Partridge series alone gives Cd = 111.718.

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[1] Amer. Journ. Sci. (3), 40, 377. 1890.

In the third set of determinations cadmium oxalate was transformed to sulphide by heating in H_2S, giving the ratio $CdC_2O_4 : CdS :: 100 : x$:

CdC_2O_4.	*CdS.*	*Per cent. CdS.*
1.57092	1.13065	71.972
1.73654	1.24979	71 973
2.19276	1.57825	71.974
1.24337	.89492	71.974
1.18743	.85463	71.975
1.54038	1.10858	71.968
1.38905	.99974	71.976
2.03562	1.46517	71.979
2.03781	1.46658	71.970
1.91840	1.38075	71.971

Mean, 71.973, ± .0007

Hence Cd = 111.61.

This work of Partridge was presently discussed by Clarke,[1] with reference to the concordance of the data, and it was shown that the three ratios determined could be discussed algebraically, giving values for the atomic weights of Cd, S and C when O = 16. These values are—

Cd = 111.7850
C = 11.9958
S = 32.0002

and are independent of all antecedent values except that assumed for the standard, oxygen.

Morse and Jones,[2] starting with cadmium purified by fractional distillation in vacuo, adopted two methods for their determinations. First, they effected the synthesis of the oxide from known weights of metal by dissolving the latter in nitric acid, evaporating to dryness, and subsequent ignition of the product. The oxide thus obtained was thought to be completely free from oxides of nitrogen. The weighings, which are given below, were made in tared crucibles. The third column gives the percentage of Cd in CdO:

Cd taken.	*CdO found.*	*Per cent. Cd.*
1.77891	2.03288	87.507
1.82492	2.08544	87.508
1.74688	1.99626	87.507
1.57000	1.79418	87.505
1.98481	2.26820	87.506
2.27297	2.59751	87.504
1.75695	2.00775	87.508
1.70028	1.94305	87.505
1.92237	2.19679	87.508
1.92081	2.19502	87.508

Mean, 87.5066, ± .00032

Hence Cd = 112.068.

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[1] Amer. Chem. Journ., 13, 34. 1891.

[2] Amer. Chem. Journ., 14, 261. 1892.

The second method employed by Morse and Jones was that of Lenssen with cadmium oxalate. This salt they found to be somewhat hygroscopic, a property against which the operator must be on his guard. The data found are as follows:

CdC_2O_4.	CdO.	Per cent. CdO.
1.53937	.98526	64.004
1.77483	1.13582	63.996
1.70211	1.08949	64.008
1.70238	1.08967	64.004
1.74447	1.11651	64.003
		Mean, 64.003, ± .0042

Hence Cd = 112.03.

Lorimer and Smith,[1] like Morse and Jones, determined the atomic weight of cadmium by means of the oxide, but by analysis instead of synthesis. Weighed quantities of oxide were dissolved in potassium cyanide solution, from which metallic cadmium was thrown down electrolytically. The weights are reduced to a vacuum standard:

CdO taken.	Cd found.	Per cent. Cd.
.34767	.30418	87.491
.41538	.36352	87.515
1.04698	.91618	87.507
1.04066	.91500	87.493
1.26447	1.10649	87.506
.78493	.68675	87.492
.86707	.75884	87.518
.67175	.58785	87.510
1.44362	1.26329	87.508
		Mean, 87.5044, ± .0023

Hence Cd = 112.042.

Mr. Bucher's dissertation[2] upon the atomic weight of cadmium does not claim to give any final measurements, but rather to discuss the various methods by which that constant has been determined. Nevertheless, it gives many data which seem to have positive value, and which are certainly fit for discussion along with those which have preceded this paragraph. Bucher began with cadmium purified by distillation nine times in vacuo, and from this his various compounds were prepared. His first series of determinations was made by reducing cadmium oxalate to oxide, the oxalate having been dried fifty hours at 150°. The reduction was effected by heating in jacketed porcelain crucibles, with various precautions, and the results obtained, reduced to a vacuum standard, are as follows:

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[1] Zeitsch. anorg. Chem., 1, 364. 1892.

[2] "An examination of some methods employed in determining the atomic weight of cadmium." Johns Hopkins University doctoral dissertation. By John E. Bucher. Baltimore, 1895.

Oxalate.	*Oxide.*	*Per cent. oxide.*
1.97674	1.26414	63.951
1.94912	1.24682	63.968
1.96786	1.25886	63.971
1.87099	1.19675	63.958
1.37550	.87994	63.972
1.33313	.85308	63.991
1.94450	1.24452	64.002
2.01846	1.29210	64.014

Mean, 63.978, ± .0052

Hence Cd = 111.89.

Combining this with the means found by previous experimenters, we have for the percentage of oxide in oxalate—

Lenssen	64.010, ± .0140
Partridge	63.964, ± .0010
Morse and Jones	64.003, ± .0042
Bucher	63.978, ± .0052
General mean	63.966, ± .0010

Bucher's next series of determinations was by Partridge's method—the conversion of cadmium oxalate into cadmium sulphide by heating in a stream of sulphuretted hydrogen. The sulphide was finally cooled in a current of dry nitrogen. The vacuum weights and ratios are subjoined:

Oxalate.	*Sulphide.*	*Percentage.*
2.56319	1.84716	72.065
2.18364	1.57341	72.055
2.11643	1.52462	72.037
3.13105	2.25582	72.047

Mean, 72.051, ± .0127
Partridge found, 71.973, ± .0007

General mean, 71.974, ± .0007

Here Bucher's mean practically vanishes. Taken alone, it gives Cd = 112.15.

The third method employed by Bucher was that of weighing cadmium chloride, dissolving in water, precipitating with silver nitrate, and weighing the silver chloride found. The cadmium chloride was prepared, partly by solution of cadmium in hydrochloric acid, evaporation to dryness, and sublimation in vacuo; and partly by the direct union of the metal with chlorine. The silver chloride was weighed in a Gooch crucible, with platinum sponge in place of the asbestos. To the vacuum weights I append the ratio $2AgCl : CdCl_2 :: 100 : x$.

$CdCl_2$.	*AgCl.*	*Ratio.*
3.09183	4.83856	63.900
2.26100	3.53854	63.896
1.35729	2.12431	63.893
2.05582	3.21727	63.899
1.89774	2.97041	63.886
3.50367	5.48473	63.880
2.70292	4.23087	63.886
4.24276	6.63598	63.936
3.40200	5.32314	63 910
4.60659	7.20386	63.946
2.40832	3.76715	63.930
2.19144	3.42724	63 942
2.84628	4.45477	63.893
2.56748	4.01651	63.923
2.31003	3.61370	63.924
1.25008	1.95652	63.893
1.96015	3.06541	63.944
2.29787	3.59391	63.938
1.94227	3.03811	63.915
1.10976	1.73547	63.946
1.63080	2.55016	63.949

Mean, 63.916, ± .0032

Hence Cd = 112.315.

Bucher gives a rather full discussion of the presumable errors in this method, which, however, he regards as somewhat compensatory. The series is followed by a similar one with cadmium bromide, the latter having been sublimed in vacuo. Results as follows:

$CdBr_2$.	*AgBr.*	*Ratio.*
4.39941	6.07204	72.454
3.18030	4.38831	72.472
3.60336	4.97150	72.480
4.04240	5.58062	72.453
3.60505	4.97519	72.461

Mean, 72.464, ± .0035

Hence Cd = 112.34.

In order to fix a minimum value for the atomic weight of cadmium, Bucher effected the synthesis of the sulphate from the metal. 1.15781 grammes of cadmium gave 2.14776 of sulphate.

Hence Cd = 112.36.

The sulphate produced was dried at 400°, and afterwards examined for free sulphuric acid, giving a correction which was applied to the weighings. The corrected weight is given above. Any impurity in the sulphate would tend to lower the apparent atomic weight of cadmium, and therefore the result is believed by the author to be a minimum.

Finally, Bucher examined the oxide method followed by Morse and Jones. The syntheses of oxide were effected in double crucibles, first with both crucibles porcelain, and afterwards with the small inner crucible of platinum. Two experiments were made by the first method, three by the last. Weights and percentages (Cd in CdO) as follows:

Cd.	*CdO.*	*Percentage.*
1.26142	1.44144	87.511
.99785	1.14035	87.504
		Mean, 87.5075, ± .0024

Cd.	*CdO.*	*Percentage.*
1.11321	1.27247	87.484
1.02412	1.17054	87.491
2.80966	3.21152	87.487
		Mean, 87.4873, ± .0016

The two means given above, representing work done with porcelain and with platinum crucibles, correspond to a difference of about 0.2 in the atomic weight of cadmium. Experiments were made with pure oxide of cadmium by converting it into nitrate and then back to oxide, exactly as in the foregoing syntheses. In each case the oxide obtained at the end of the operation represented an increase in weight, but the increase was greater in platinum than in porcelain. Hence the weighings of cadmium oxide in the foregoing determinations are subject to constant errors, and cannot be trusted to fix the atomic weight of cadmium.

A different class of determinations relative to the atomic weight of cadmium are those of Hardin,[1] who effected the electrolysis of the chloride and bromide, and also made a direct comparison between cadmium and silver. The aqueous solutions of the salts, mixed with potassium cyanide, were electrolyzed in platinum dishes. The cadmium which served as the starting point for the investigation was purified by distillation in hydrogen. All weights are reduced to a vacuum. The data for the chloride series are as follows, with a column added for the percentage of Cd in $CdCl_2$:

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[1] Journ. Amer. Chem. Soc., 18, 1016. 1896.

Weight $CdCl_2$.	*Weight Cd.*	*Percentage Cd.*
.43140	.26422	61.247
.49165	.30112	61.247
.71752	.43942	61.241
.72188	.44208	61.241
.77264	.47319	61.245
.81224	.49742	61.240
.90022	.55135	61.246
1.02072	.62505	61.236
1.26322	.77365	61.244
1.52344	.93314	61.252

Mean, 61.244, ± .0010

Hence Cd = 112.07.

The results for the bromide, similarly stated, are these:

Weight $CdBr_2$.	*Weight Cd.*	*Percentage Cd.*
.57745	.23790	41.198
.76412	.31484	41.203
.91835	.37842	41.207
1.01460	.41808	41.206
1.15074	.47414	41.203
1.24751	.51392	41.196
1.25951	.51905	41.210
1.51805	.62556	41.208
1.63543	.67378	41.199
2.15342	.88722	41.200

Mean, 41.203, ± .0010

Hence Cd = 112.01.

The direct comparison of cadmium and silver was effected by the simultaneous electrolysis, in the same current, of double cyanide solutions. Silver was thrown down in one platinum dish and cadmium in another. The process was not altogether satisfactory, and gave divergent results, those which are cited below having been selected by Hardin from the mass of data obtained. I have added in a third column the cadmium proportional to 100 parts of silver:

Weight Cd.	*Weight Ag.*	*Ratio.*
.12624	.24335	51.876
.11032	.21262	51.886
.12720	.24515	51.887
.12616	.24331	51.852
.22058	.42520	51.877

Mean, 51.876, ± .0041

Hence Cd = 111.93.

The work of Morse and Arbuckle[1] upon the atomic weight of cadmium was similar in character and purpose to their work upon zinc. The presence of occluded gases in the oxide was recognized, and in the new determinations they were extracted, measured and analyzed. Cadmium was converted into oxide, and corrections for the gaseous impurities were applied. The vacuum weights of metal and oxide are given below, together with the volume of extracted gas, and the crude, *uncorrected* percentage of Cd in CdO:

Weight Cd.	*Weight CdO.*	*Gas cc.*	*Per cent.*
1.931882	2.207639	.574	87.509
1.679348	1.919096	.480	87.507
1.484296	1.696195	.441	87.507
1.364861	1.559717	.402	87.507
1.502948	1.717441	.419	87.511
1.438035	1.643297	.431	87.509
1.440410	1.646037	.406	87.508
1.459384	1.667714	.421	87.508
1.403791	1.604196	.390	87.507

Mean, 87.508, ± .0003

This gives Cd = 112.082. Corrected for occluded gases, Cd = 112.377 in mean, ranging from 112.359 to 112.395. The correction adds 0.029 to the percentage of metal; and if we assume the same correction to the older determinations of this ratio, the several series combine as follows:

Morse and Jones	87.5356, ± .0003
Lorimer and Smith	87.5334, ± .0023
Bucher, 1	87.5365, ± .0024
Bucher, 2	87.5163, ± .0016
Morse and Arbuckle	87.5370, ± .0003
General mean	87.5360, ± .0002

This combination is equivalent to a rejection of all the data except those of Morse and his colleagues.

Baxter and Hines,[2] in order to determine the atomic weight of cadmium, resorted to the analysis of the chloride, with all the precautions characteristic of the Harvard laboratory. First, the gravimetric ratio $2AgCl : CdCl_2$ was determined, with the subjoined results. Vacuum weights are given throughout:

Weight $CdCl_2$.	*Weight AgCl.*	*Ratio.*
5.53421	8.65356	63.953
7.77758	12.16166	63.952
8.87917	13.88344	63.955

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[1] Amer. Chem. Journ., 20, 536. 1898. See also Arbuckle, Thesis, Johns Hopkins University, 1898.

[2] Journ. Amer. Chem. Soc., 27, 222. 1905.

Secondly, the ratio $2Ag : CdCl_2$ was measured by adding to the solution of the cadmium salt as nearly as possible its exact equivalent of a standard silver solution, and then determining the slight excess of silver or chlorine by titration. The results are as follows:

Weight $CdCl_2$.	*Weight Ag.*	*Ratio.*
4.92861	5.80063	84.967
3.86487	4.54891	84.963
5.08551	5.98569	84.961
5.84335	6.87704	84.969
5.99952	7.06084	84.969
3.73092	4.39095	84.968

A year later, the same ratios were remeasured by Baxter, Hines and Frevert.[1] I subjoin their data:

Weight $CdCl_2$.	*Weight Ag.*	*Weight AgCl.*	*Ag ratio.*	*AgCl ratio.*
5.62500	6.61993	....	84.972	......
6.81031	8.01496	10.64918	84.970	63.9515
5.50089	6.47393	8.60174	84.970	63.9509
6.11750	......	9.56590	.....	63.9511

These series are so nearly identical and so short that it seems well to treat both investigations as one. On this basis, $2Ag : CdCl_2 :: 100 : 84.9677, \pm .0008$, and $2AgCl : CdCl_2 :: 100 : 63.9523, \pm .0004$.

Hence, from the Ag ratio, Cd = 112.41.

From the AgCl ratio, Cd = 112.42.

And Ag : Cl :: 100 : 32.861.

Combined with the values found by former investigators, the ratios assume the following form:

Silver Ratio.

Dumas	84.843, ± .0260
Baxter, etc.	84.9677, ± .0008
General mean	84.9676, ± .0008

Silver Chloride Ratio.

Bucher	63.916, ± .0032
Baxter, etc.	63.9523, ± .0004
General mean	63.9518, ± .0004

Baxter, Hines and Frevert also made analyses of cadmium bromide by the usual Harvard methods. Their data follow:

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[1] Journ. Amer. Chem. Soc., 28, 770. 1906.

Weight $CdBr_2$.	*Weight Ag.*	*Weight AgBr.*	*Ag ratio.*	*AgBr ratio.*
11.46216	9.08379	15.81319	126.182	72.485
6.82282	5.40724	9.41267	126.182	72.486
6.75420	5.35277	9.31830	126.181	72.483
[1] 7.08588	[1] 5.61597	[1] 9.77649	126.174	72.479
5.13859	4.07226	7.08933	126.183	72.483
5.84324	4.63072	8.06130	126.183	72 485
5.99704	4.75259	8.27360	126.183	72.484
5.90796	4.68200	8.15070	126.183	72.484
		Mean,	126.181.	72.4836,
			± .0009	± .0005

From the Ag ratio, Cd = 112.42.
From the AgBr ratio, Cd = 112.41.
And Ag : Br : : 100 : 74.082.

These ratios combine with former series as follows:

Silver Ratio.

Huntington	126.076, ± .0052
Baxter, etc.	126.181, ± .0009
General mean	126.178, ± .0009

Silver Bromide Ratio.

Huntington	72.4216, ± .0028
Bucher	72.464, ± .0035
Baxter, etc.	72.4836, ± .0005
General mean	72.4813, ± .0005

The determinations of the atomic weight of cadmium by Meaglia[2] were based upon the quantitative precipitation by that metal of silver from a sulphate solution, and gold from a solution of sodium chloraurate. With Ag = 107.93 and Au = 197.2 the following values for cadmium were obtained:

Silver series.	*Gold series.*
112.37	112.41
112.56	112.45
112.45	112.65
112.38	112.47
	112.48
Mean, 112.44, ± .0295	112.40
	112.42
	112.41
	Mean, 112.461, ± .0196

From the silver series, with Ag = 107.88, Cd = 112.39.
From the gold series, with Au = 197.269, Cd = 112.50.

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[1] This analysis is rejected by the authors.

[2] Thesis, University of Grenoble, 1907.

For the ratio 2Ag: Cd Hardin found the value 51.876, ± .0041. Meaglia's series gives 52.090, ± .0136. The general mean of both series combined is

2Ag:Cd::100:51.893, ± .0039

The determinations made by Blum[1] depended upon the conversion of CdO into CdS by heating in a stream of hydrogen sulphide. His figures, with vacuum weights, are given below, together with the ratio CdO: CdS :: 100 : *x*:

CdO.	*CdS.*	*Ratio.*
1.80552	2.03108	112.493
.66349	.74617	112.461
1.82460	2.05256	112.494
1.88424	2.11974	112.498
3.59206	4.04081	112.493
4.38093	4.92695	112.464
	Mean,	112.484, ± .0046

Hence Cd = 112.69. This ratio is not of much value.

For cadmium the subjoined ratios are now available.

Bucher's single experiment upon the synthesis of the sulphate, although important and interesting, cannot carry weight enough to warrant its consideration in connection with the other ratios, and is therefore not included.

(1). CdO:Cd::100:87.536, ± .0002
(2). CdC_2O_4:CdO::100:63.966, ± .0010
(3). CdC_2O_4:CdS::100:71.974, ± .0007
(4). $CdSO_4$:CdS::100:69.202, ± .0012
(5). 2Ag:$CdCl_2$::100:84.9676, ± .0008
(6). 2AgCl:$CdCl_2$::100:63.9518, ± .0004
(7). 2Ag:$CdBr_2$::100:126.178, ± .0009
(8). 2AgBr:$CdBr_2$::100:72.4813, ± .0005
(9). $CdCl_2$:Cd::100:61.244, ± .0010
(10). $CdBr_2$:Cd::100:41.203, ± .0010
(11). 2Ag:Cd::100:51.893, ± .0039
(12). Au:Cd::197.2:112.461, ± .0196
(13). CdO:CdS::100:112.484, ± .0046

Reducing these ratios with

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
C = 12.0038, ± .0002
Au = 197.269, ± .0030

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[1] Thesis, University of Pennsylvania, 1908.

we have—

From ratio 3	Cd = 111.607, ± .0050
“ “ 4	111.739, ± .0062
“ “ 2	111.822, ± .0041
“ “ 11	111.964, ± .0071
“ “ 10	112.010, ± .0033
“ “ 9	112.066, ± .0035
“ “ 1	112.370, ± .0018
“ “ 8	112.400, ± .0021
“ “ 7	112.403, ± .0022
“ “ 5	112.410, ± .0018
“ “ 6	112.416, ± .0013
“ “ 12	112.500, ± .0197
“ “ 13	112.689, ± .0478
General mean,	Cd = 112.323, ± .0007

This mean value is almost certainly too low. If the six lowest values in the foregoing series are omitted, the general mean of the seven higher values is

$$Cd = 112.402, \pm .0008$$

which agrees well with the determinations by Baxter and his colleagues, and yet takes into account the work of Morse and Arbuckle. In short, Cd = 112.4, within the limits of experimental uncertainty.

MERCURY.

In dealing with the atomic weight of mercury we may reject the early determinations by Sefström[1] and a large part of the work done by Turner.[2] The latter chemist, in addition to the data which will be cited below, gives figures to represent the percentage composition of both the chlorides of mercury; but these results are neither trustworthy nor in proper shape to be used.

First in order we may consider the percentage composition of mercuric oxide, as established by Turner and by Erdmann and Marchand. In both investigations the oxide was decomposed by heat, and the mercury was accurately weighed. Gold leaf served to collect the last traces of mercurial vapor.

Turner gives four estimations. Two represent oxide obtained by the ignition of the nitrate, and two are from commercial oxide. In the first two the oxide still contained traces of nitrate, but hardly in weighable proportions. A comparison of the figures from this source with the others is sufficiently conclusive on this point. The third column represents the percentage of mercury in HgO:

144.805	grains Hg =	11.54	grains O.	92.619	per cent.
125.980	"	10.08	"	92.592	"
173.561	"	13.82	"	92.625	"
114.294	"	9.101	"	92.620	"

Mean, 92.614, ± .0050

Hence Hg = 200.626.

In the experiments of Erdmann and Marchand[3] every precaution was taken to ensure accuracy. Their weighings, reduced to a vacuum standard, give the subjoined percentages:

82.0079	grm. HgO gave	75.9347	grm. Hg.	92.594	per cent.
51.0320	"	47.2538	"	92.597	"
84.4996	"	78.2501	"	92.604	"
44.6283	"	41.3285	"	92.606	"
118.4066	"	109.6408	"	92.597	"

Mean, 92.5996, ± .0015

Hence Hg = 200.205.

Hardin's determination of the same ratio, being different in character, will be considered later.

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[1] Sefström. Berz. Lehrb., 5th ed., 3, 1215. Work done in 1812.

[2] Phil. Trans., 1833, 531-535.

[3] Journ. prakt. Chem., 31, 395. 1844.

With a view to establishing the atomic weight of sulphur, Erdmann and Marchand also made a series of analyses of mercuric sulphide. These data are now best available for discussion under mercury. The sulphide was mixed with pure copper and ignited, mercury distilling over and copper sulphide remaining behind. Gold leaf was used to retain traces of mercurial vapor, and the weighings were reduced to vacuum:

34.3568 grm. HgS gave	29.6207	grm. Hg.	86.215	per cent. Hg.
24.8278 "	21.40295	"	86.206	"
37.2177 "	32.08416	"	86.207	"
80.7641 "	69.6372	"	86.223	"
		Mean,	86.2127, ± .0027	

Hence Hg = 200.520.

For the percentage of mercury in mercuric chloride we have data by Turner, Millon, Svanberg and Hardin. Turner,[1] in addition to some precipitations of mercuric chloride by silver nitrate, gives two experiments in which the compound was decomposed by pure stannous chloride, and the mercury thus set free was collected and weighed. The results were as follows:

44.782 grains Hg =	15.90 grains Cl.	73.798	per cent.
73.09 "	25.97 "	73.784	"
	Mean,	73.791, ± .005	

Hence Hg = 199.665.

Millon[2] purified mercuric chloride by solution in ether and sublimation, and then subjected it to distillation with lime. The mercury was collected as in Erdmann and Marchand's experiments. Percentages of metal as follows:

73.87
73.81
73.83
73.87

Mean, 73.845, ± .010

Hence Hg = 200.224.

Svanberg,[3] following the general method of Erdmann and Marchand, made three distillations of mercuric chloride with lime, and got the following results:

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[1] Phil. Trans., 1833, 531-535.

[2] Ann. Chim. Phys. (3), 18, 345. 1846.

[3] Journ. prakt. Chem., 45, 472. 1848.

12.048 grm. $HgCl_2$ gave 8.889 grm. Hg.		73.780 per cent.
12.529 "	9.2456 "	73.794 "
12.6491 "	9.3363 "	73.810 "
		Mean, 73.795, ± .006

Hence Hg=199.706.

Much more recent determinations of the atomic weight of mercury are due to Hardin,[1] whose methods were entirely electrolytic. First, pure mercuric oxide was dissolved in dilute, aqueous potassium cyanide, and electrolyzed in a platinum dish. Six determinations are published, out of a larger number, but without reduction of the weights to a vacuum. The data, with a percentage column added, are as follows:

Weight HgO.	*Weight Hg.*	*Per cent. Hg.*
.26223	.24281	92.594
.23830	.22065	92.593
.23200	.21482	92.595
.14148	.13100	92.593
.29799	.27592	92.594
.19631	.18177	92.593
		Mean, 92.594, ± .0003

Hence Hg=200.041.

Various sources of error were detected in these experiments, and the series is therefore rejected by Hardin. It combines with previous series as follows:

Turner	92.614, ± .0050
Erdmann and Marchand	92.5996, ± .0015
Hardin	92.594, ± .0003
General mean	92.595, ± .0003

Hardin also studied mercuric chloride, bromide and cyanide, and the direct ratio between mercury and silver, with reduction of weights to a vacuum. Electrolysis was conducted in a platinum dish, as usual. With the chloride and bromide, the solutions were mixed with dilute potassium cyanide. The data for the chloride are as follows, the percentage column being added by myself:

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[1] Journ. Amer. Chem. Soc., 18, 1003. 1896.

Weight $HgCl_2$.	*Weight Hg.*	*Per cent. Hg.*
.45932	.33912	73.831
.54735	.40415	73.838
.56002	.41348	73.833
.63586	.46941	73.823
.64365	.47521	73.831
.73281	.54101	73.827
.86467	.63840	73.832
1.06776	.78825	73 823
1.07945	.79685	73.820
1.51402	1.11780	73.830

Mean, 73.829, ± .0012

Hence Hg = 200.058.

For the bromide Hardin's data are—

Weight $HgBr_2$.	*Weight Hg.*	*Per cent Hg.*
.70002	.38892	55.558
.56430	.31350	55.555
.57142	.31750	55.563
.77285	.42932	55.550
.80930	.44955	55.548
.85342	.47416	55.560
1.11076	.61708	55.555
1.17270	.65145	55.551
1.26186	.70107	55.559
1.40142	.77870	55.565

Mean, 55.556, ± .0012

Hence Hg = 199.803.

And for the cyanide—

Weight HgC_2N_2.	*Weight Hg.*	*Per cent. Hg.*
.55776	.44252	79.337
.63290	.50215	79.341
.70652	.56053	79.337
.80241	.63663	79.340
.65706	.52130	79.338
.81678	.64805	79.342
1.07628	.85392	79.340
1.22615	.97282	79.339
1.66225	1.31880	79.338
2.11170	1.67541	79.339

Mean, 79.339, ± .0004

Hence Hg = 199.835.

In the last series cited no potassium cyanide was used, but the solution of mercuric cyanide, with the addition of one drop of sulphuric acid, was electrolyzed directly.

The direct ratio between silver and mercury was determined by throwing down the two metals, simultaneously, in the same electric current. Both metals were taken in double cyanide solution. With Hardin's equivalent weights I give a third column, showing the quantity of mercury corresponding to 100 parts of silver. Many experiments were rejected, and only the following seven are published by the author:

Weight Hg.	*Weight Ag.*	*Ratio.*
.06126	.06610	92.678
.06190	.06680	92.665
.07814	.08432	92.671
.10361	.11181	92.666
.15201	.16402	92.678
.26806	.28940	92.626
.82808	.89388	92.639
		Mean, 92.660, ± .0051

Hence Hg=199.923.

The determinations by Easley[1] are quite unlike those made by his predecessors. First, mercuric chloride in solution was reduced to metal by means of hydrogen dioxide, and was precipitated partly as a globule and partly in finely divided form. The globule was washed with water and acetone and weighed. The finely divided mercury was again dissolved, and with a little mercury remaining in solution, was deposited electrolytically upon a gold cathode. Its weight was then added to that of the globule. The following results, with vacuum weights, were obtained:

$HgCl_2$.	*Hg.*	*Per cent. Hg.*
23.43239	17.30826	73.865
12.59751	9.30608	73.873
10.94042	8.08154	73.869
11.73734	8.67044	73.871
		Mean, 73.8695, ± .0012

Hence Hg=200.478.

Combining this with the earlier determinations we have—

Turner	73.791,	± .0050
Millon	73.845,	± .0100
Svanberg	73.795,	± .0060
Hardin	73.829,	± .0012
Easley	73.8695,	± .0012
General mean	73.8459,	± .0008

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[1] Journ. Amer. Chem. Soc., 31, 1207. 1909.

In the filtrate from the mercury the chlorine was precipitated as silver chloride and so weighed. The results were as follows, with vacuum weights:

$HgCl_2$.	*AgCl.*	*Ratio.*
10.50276	11.08744	94.7257
9.03634	9.54027	94.7179
23.43239	24.73606	94.7297
10.94042	11.55158	94.7093
11.11409	11.73470	94.7113
16.63910	17.56808	94.7121
		Mean, 94.7177, ± .0023

Hence Hg = 200.617.

We now have seven ratios involving the atomic weight of mercury, as follows:

(1). Per cent. of Hg in HgO, 92.595, ± .0003
(2). Per cent. of Hg in HgS, 86.2127, ± .0027
(3). Per cent. of Hg in $HgCl_2$, 73.8459, ± .0008
(4). Per cent. of Hg in $HgBr_2$, 55.556, ± .0012
(5). Per cent. of Hg in HgC_2N_2, 79.339, ± .0004
(6). 2Ag:Hg::100:92.660, ± .0051
(7). 2AgCl:$HgCl_2$::100:94.7177, ± .0023

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
N = 14.0101, ± .0001
C = 12.0038, ± .0002

Hence,

From ratio 4 ....Hg = 199.803, ± .0069
" " 5 ....199.835, ± .0045
" " 6 ....199.923, ± .0110
" " 1 ....200.070, ± .0081
" " 3 ....200.233, ± .0066
" " 2 ....200.520, ± .0394
" " 7 ....200.617, ± .0067

General mean, Hg = 200.054, ± .0027

Mathematically, Hardin's determinations seem to outweigh the others. They are, moreover, comparatively concordant and by four methods. But it is quite possible that Easley's much higher figures may prove to be more correct. His work is to be continued; but, until it is finished, it would be unwise to adopt his results exclusively. The atomic weight of mercury is still much in doubt.

BORON.

In the first edition of this book the data relative to boron were few and unimportant. There was a little work on record by Berzelius and by Laurent, and this was eked out by a discussion of Deville's analyses of boron chloride and bromide. As the latter were not intended for atomic weight determinations they will be omitted from the present recalculation, which includes a number of later researches.

Berzelius[1] based his determination upon three concordant estimations of the percentage of water in borax. Laurent[2] made use of two similar estimations, and all five may be properly put in one series, thus:

47.10	Berzelius
47.10	
47.10	
47.15	Laurent
47.20	

Mean. 47.13, ± .013

Hence B = 11.019.

In 1869 Dobrovolsky[3] published a dissertation, in Russian, on the atomic weight of boron. The original I have not seen, and I am therefore compelled to use the data as cited by Brauner.[4] According to Dobrovolsky, borax is completely dehydrated by ignition when small quantities of it are taken. With large quantities, some water is retained. Two series of experiments are given to illustrate this assertion:

First Series.

Borax.	*Water.*	*Per cent. water.*
.138	.0651	47.174
.283	.1338	47.279
.312	.1472	47.179
		Mean, 47.211, ± .023

Hence B = 10.855.

Second Series.

Borax.	*Water.*	*Per cent. water.*
2.701	1.268	46.946
1.793	.843	47.016
3.004	1.402	46.671
		Mean. 46.878, ± .072

Hence B = 11.532.

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[1] Poggend. Annal., 8, 1. 1826.

[2] Journ. prakt. Chem., 47, 415. 1849.

[3] Doctoral Dissertation, Kiev, 1869.

[4] In Abegg's Handbuch der anorganischen Chemie, Bd. 3, Abth. 1, p. 6.

These figures are of no present importance, for the supposed difficulty of dehydration, in the light of more recent investigations, seems to be imaginary.

In 1892 the posthumous notes of the late Hoskyns-Abrahall were edited and published by Ewan and Hartog.[1] This chemist especially studied the ratio between boron bromide and silver, and also redetermined the percentage of water in crystallized borax. The latter work, which was purely preliminary, although carried out with great care, gave the following results, reduced to a vacuum standard:

$Na_2B_4O_7.10H_2O$.	$Na_2B_4O_7$.	Per cent. H_2O.
7.00667	3.69587	47.2069
12.95936	6.82560	47.3308
4.65812	2.45248	47.3504
4.47208	3.93956	47.2763
4.94504	2.60759	47.2686
	Mean,	47.2866, ± .0171

Hence B = 10.702.

Two sets of determinations were made with the bromide, which was prepared from boron and bromine directly, freed from excess of the latter by standing over mercury, and finally collected, after distillation, in small, weighed, glass bulbs. It was titrated with a solution of silver after all the usual precautions. The first series of experiments was as follows, with BBr_3 proportional to 100 parts of silver stated as the ratio:

BBr_3.	Ag.	Ratio.
1.31203	1.69406	77.449
4.39944	5.67829	77.478
5.04022	6.50820	77.444
6.51597	8.38919	77.433
7.75343	10.01235	77.439
	Mean,	77.449, ± .0053

This series of data is regarded by the editors as preliminary, and not entitled to much consideration. The second series, which follows, was the final one; both represent vacuum standards:

BBr_3.	Ag.	Ratio.
4.467835	5.771268	77.415
8.423151	10.880648	77.414
1.655111	2.137593	77.429
8.032352	10.374201	77.426
4.092743	5.285949	77.427
2.389993	3.086842	77.425
7.721944	9.974054	77.420
	Mean,	77.422, ± .0018
	First series,	77.449, ± .0053
	General mean,	77.425, ± .0017

Hence B = 10.819.

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[1] Journ. Chem. Soc., 61, 650. 1892.

Ramsay and Aston,[1] in their paper upon the atomic weight of boron, suggest that Abrahall's bromide may have contained hydrobromic acid, which would fully account for the low result obtained. They themselves adopt two distinct methods, the first one being the time-honored determination of water in crystallized borax. The latter was prepared from pure boric acid and pure sodium hydroxide. Results as follows, reduced to a vacuum:

$Na_2B_4O_7.10H_2O$.	$Na_2B_4O_7$.	Per cent. H_2O.
10.3581602	5.4784357	47.1099
5.3440080	2.8246677	47.1433
4.9962580	2.6378934	47.2026
5.7000256	3.0101127	47.1912
5.3142725	2.8065646	47.1882
4.9971924	2.6392016	47.1865
5.2366921	2.7674672	47.1524
		Mean, 47.1677, ± .0086

Hence B = 10.942.

The second method adopted by Ramsay and Aston was to distill anhydrous borax with hydrochloric acid and methyl alcohol, both scrupulously pure, thereby converting it into sodium chloride. The operation was conducted in a glass flask, and in the first series of determinations ordinary soft glass was used. This, however, was somewhat attacked, so that the sodium chloride contained silica; hence oxygen in the material of the flask had been replaced by chlorine, thereby increasing its weight and lowering the apparent atomic weight of boron. In a second series flasks of hard combustion tubing were taken, and the error, though not absolutely avoided, was reduced to a very small amount. Both series are subjoined, together with the percentage of chloride formed; but the weights, given by the authors to seven decimal places, are only quoted to the nearest tenth milligramme. They are reduced to a vacuum standard:

First Series.

$Na_2B_4O_7$.	$NaCl$.	Per cent. $NaCl$.
4.7684	2.7598	57.877
5.2740	3.0578	57.978
3.2344	1.8727	57.899
4.0862	2.3713	58.032
3.4970	2.0266	57.953
		Mean, 57.948, ± .0187

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[1] Journ. Chem. Soc., 63, 211. 1893.

Second Series.

$Na_2B_4O_7$.	*NaCl.*	*Per cent. NaCl.*
5.3118	3.0761	57.911
4.7806	2.7700	57.943
4.9907	2.8930	57.968
4.7231	2.7360	57.928
3.3138	1.9187	57.900

Mean, 57.930, ± .0081
First series, 57.948, ± .0187

General mean of both, 57.933, ± .0074

Hence B = 10.957.

As a check upon the last series of results, the sodium chloride was dissolved in water, and precipitated with silver nitrate. The silver chloride was collected and weighed in a Gooch crucible, and its weight gives a new ratio with anhydrous borax. The cross ratio between the two chlorides, silver and sodium, has already been used in the discussion upon sodium. The new ratio I give in terms of $Na_2B_4O_7$ equivalent to 100 parts of AgCl.

$Na_2B_4O_7$.	*AgCl.*	*Ratio.*
5.3118	7.5259	70.580
4.7806	6.7794	70.517
4.9907	7.0801	70.489
4.7231	6.6960	70.536
3.3138	4.6931	70.610

Mean, 70.546, ± .0146

Hence B = 11.054.

Rimbach[1] based his determination of the atomic weight of boron upon the fact that boric acid is neutral to methyl orange, and that therefore it is possible to titrate a solution of borax directly with hydrochloric acid. His borax was prepared from carefully purified boric acid and sodium carbonate, and his hydrochloric acid was standardized by a series of precipitations and weighings as silver chloride. It contained 1.84983 per cent. of actual HCl. The borax, dissolved in water, was titrated by means of a weight-burette. I give the weights found in the first and second columns of the following table, and in the third column, calculated by myself, the HCl proportional to 100 parts of crystallized borax. Rimbach himself computes the percentage of Na_2O and thence the atomic weight of boron, but the ratio $Na_2B_4O_7.10H_2O : 2HCl$ is the ratio actually determined.

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[1] Ber. Deutsch. chem. Ges., 26, 164. 1893.

$Na_2B_4O_7.10H_2O$.	HCl *Solution*.	*Ratio*.
10.00214	103.1951	19.0853
15.32772	158.1503	19.0864
15.08870	155.7271	19.0917
10.12930	104.5448	19.0922
5.25732	54.2571	19.0908
15.04324	155.2307	19.0883
15.04761	155.2959	19.0908
10.43409	107.6602	19.0868
5.04713	52.0897	19.0915
	Mean,	19.0893, ± .0006

Hence B = 10.970.

Obviously, this error should be increased by the probable errors involved in standardizing the acid, but they are too small to be worth considering.

The work of Armitage on the atomic weight of boron was published only in abstract.[1] The data, however, were fortunately given to Brauner,[2] who has stated them in partially available form. First, six determinations were made of the proportion of water in borax giving in mean, 47.1475 per cent., with a minimum of 47.1224 and a maximum of 47.1637. Hence B = 10.983. If these figures alone are considered, the probable error of the mean is ±.0091. Secondly, anhydrous borax was titrated with standard sulphuric acid, with the subjoined results:

$Na_2B_4O_7$.	SO_4.	*Ratio*.
1.94033	.924615	209.053
1.56303	.743413	210.251
	Mean,	210.052, ± .133

Hence B = 10.943. The determination is evidently of small significance.

Combining the data relative to the percentage of water in borax, we have—

Berzelius with Laurent.......	47.130,	± .0130
Dobrovolsky, 1	47.211,	± .0230
Dobrovolsky, 2	46.878,	± .0720
Hoskyns-Abrahall	47.2866,	± .0171
Ramsay and Aston............	47.1677,	± .0086
Armitage	47.1475,	± .0091
General mean	47.1654,	± .0052

This mean is very close to that of Ramsay and Aston. Dobrovolsky's figures count for practically nothing.

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[1] Proc. Chem. Soc., 14, 22. 1898. The communication was followed by several adverse criticisms. See also Leonard, Chem. News, 77, 104.

[2] Op. cit.

Gautier's determinations[1] were based upon analyses of four boron compounds. First, boron sulphide was decomposed by caustic soda; the solution was then oxidized with bromine water, and the sulphur was precipitated and weighed as barium sulphate. I give the ratio $3BaSO_4$: B_2S_3 :: 100 : x in the third column below. The weights are all reduced to a vacuum standard:

B_2S_3.	$BaSO_4$.	*Ratio.*
.2754	1.6312	16.883
.3380	2.0004	16.897
.3088	1.8300	16.874
.2637	1.5614	16.888
		Mean, 16.8855, ± .0033

Hence B = 11.024.

Secondly, boron carbide was heated in chlorine to expel the boron as BCl_3. The residual carbon was then burned in oxygen, and the dioxide so produced was weighed. I subjoin the weights, and also the ratio CO_2 : B_6C :: 100 : x:

B_6C.	CO_2.	*Ratio.*
.2686	.1515	177.293
.3268	.1844	177.224
		Mean, 177.258, ± .024

Hence B = 10.999.

Third, boron tribromide was decomposed by water, and its bromine content was then determined as silver bromide. The following data relate to two samples of the boron compound, with five analyses of the first lot and four of the second:

BBr_3.	$AgBr$.	*Ratio.*
3.1130	6.994	44.510
3.3334	7.490	44.505
3.7456	8.414	44.516
3.2780	7.364	44.514
4.2074	9.452	44.513
3.3956	7.628	44.515
4.0295	9.052	44.514
3.7886	8.512	44.509
3.1711	7.124	44.513
		Mean, 44.512, ± .0009

Hence B = 11.021.

Finally, the analysis of boron chloride was effected in the same way with the following results:

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[1] Ann. Chim. Phys. (7), 18, 352. 1899.

BCl_3.	*AgCl.*	*Ratio.*
2.6412	9.682	27.279
2.7920	10.234	27.282
2.4634	9.026	27.292
2.4489	12.640	27.285
2.2015	8.070	27.280
2.6957	9.878	27.289
		Mean, 27.2845, ± .0014

Hence B = 10.952.

The ratios from which to compute the atomic weight of boron are now as follows:

(1). $Na_2B_4O_7.10H_2O : 10H_2O :: 100 : 47.1654$, ± .0052
(2). $Na_2B_4O_7.10H_2O : 2HCl :: 100 : 19.0893$, ± .0006
(3). $Na_2B_4O_7 : 2NaCl :: 100 : 57.933$, ± .0074
(4). $2AgCl : Na_2B_4O_7 :: 100 : 70.546$, ± .0146
(5). $SO_4 : Na_2B_4O_7 :: 100 : 210.052$, ± .133
(6). $3AgCl : BCl_3 :: 100 : 27.2845$, ± .0014
(7). $3Ag : BBr_3 :: 100 : 77.425$, ± .0017
(8). $3AgBr : BBr_3 :: 100 : 44.512$, ± .0009
(9). $3BaSO_4 : B_2S_3 :: 100 : 16.8855$, ± .0033
(10). $CO_2 : B_6C :: 100 : 177.258$, ± .024

The values used in reducing these ratios are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
Na = 23.0108, ± .00024
S = 32.0667, ± .00075
C = 12.0038, ± .0002
Ba = 137.363, ± .0025
H = 1.00779, ± .00001

Hence,

From ratio 7 ....B = 10.8191, ± .0056
" " 5 ....10.9431, ± .0319
" " 1 ....10.9472, ± .0068
" " 6 ....10.9523, ± .0061
" " 3 ....10.9572, ± .0065
" " 2 ....10.9700, ± .0031
" " 10 ....10.9994, ± .0018
" " 8 ....11.0211, ± .0051
" " 9 ....11.0236, ± .0122
" " 4 ....11.0544, ± .0105

General mean, B = 10.9805, ± .0013

In this combination, ratio 10 is enormously overvalued. It receives weight out of all proportion to its merits. The uncertainties, however, are so great that the final mean may be allowed to stand until better evidence as to the true atomic weight of boron is obtained. The round number 11.0 is enough for common use.

ALUMINUM.

The atomic weight of aluminum has been determined by Berzelius, Mather, Tissier, Dumas, Isnard, Terreil, Mallet, Baubigny, Thomsen and Kohn-Abrest. The early calculations of Davy and of Thomson we may properly disregard.

Berzelius'[1] determination rests upon a single experiment. He ignited 10 grammes of dry aluminum sulphate, $Al_2(SO_4)_3$, and obtained 2.9934 grammes of Al_2O_3 as residue.

Hence $Al = 27.31$.

In 1835 Mather[2] published a single analysis of aluminum chloride, from which he sought to fix the atomic weight of the metal. 0.646 grm. of $AlCl_3$ gave him 2.056 of AgCl and 0.2975 of Al_2O_3. These figures give worthless values for Al, and are included here only for the sake of completeness. From the ratio between AgCl and $AlCl_3$, $Al = 28.737$.

Tissier's[3] determination, also resting on a single experiment, appeared in 1858. Metallic aluminum, containing .135 per cent. of sodium, was dissolved in hydrochloric acid. The solution was evaporated with nitric acid to expel all chlorine, and the residue was strongly ignited until only alumina remained. 1.935 grm. of Al gave 3.645 grm. of Al_2O_3. If we correct for the trace of sodium in the aluminum, we have $Al = 27.185$.

Essentially the same method of determination was adopted by Isnard,[4] who, although not next in chronological order, may fittingly be mentioned here. He found that 9 grm. of aluminum gave 17 grm. of Al_2O_3. Hence $Al = 27$.

In 1858 Dumas,[5] in his celebrated revision of the atomic weights, made seven experiments with aluminum chloride. The material was prepared in quantity, sublimed over iron filings, and finally resublimed from metallic aluminum. Each sample used was collected in a small glass tube, after sublimation from aluminum in a stream of dry hydrogen, and hermetically enclosed. Having been weighed in the tube, it was dissolved in water, and the quantity of silver necessary for precipitating the chlorine was determined. Reducing to a common standard, his weighings give the quantities of $AlCl_3$ stated in the third column, as proportional to 100 parts of silver:

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[1] Poggend. Annal., 8, 177.

[2] Amer. Journ. Sci., 27, 241.

[3] Compt. Rend., 46, 1105.

[4] Compt. Rend., 66, 508. 1868.

[5] Ann. Chim. Phys. (3), 55, 151. Ann. Chem. Pharm., 113, 26.

$AlCl_3$.	*Ag.*	*Ratio.*
1.8786	4.543	41.352
3.021	7.292	41.459—Bad
2.399	5.802	41.348
1.922	4.6525	41.311
1.697	4.1015	41.375
4.3165	10.448	41.314
6.728	16.265	41.365

In the second experiment the $AlCl_3$ contained traces of iron. Rejecting this experiment, the remaining six give a mean of 41.344, ±.007. These data give a value for Al approximating to 27.4, and were for many years regarded as satisfactory. It now seems probable that the chloride contained traces of an oxy-compound, which would tend to raise the atomic weight.

In 1879 Terreil[1] published a new determination of the atomic weight under consideration, based upon a direct comparison of the metal with hydrogen. Metallic aluminum, contained in a tube of hard glass, was heated strongly in a current of dry hydrochloric acid. Hydrogen was set free, and was collected over a strong solution of caustic potash. 0.410 grm. of aluminum thus was found equivalent to 508.2 cc., or .045671 grm. of hydrogen. Hence Al=27.142.

About a year after Terreil's determination appeared, the lower value for aluminum was thoroughly confirmed by J. W. Mallet.[2] After giving a full résumé of the work done by others, exclusive of Isnard, the author describes his own experiments, which may be summarized as follows:

Four methods of determination were employed, each one simple and direct, and at the same time independent of the others. First, pure ammonia alum was calcined, and the residue of aluminum oxide was estimated. Second, aluminum bromide was titrated with a standard solution of silver. Third, metallic aluminum was attacked by caustic soda, and the hydrogen evolved was measured. Fourth, hydrogen was set free by aluminum, and weighed as water. Every weight was carefully verified, the verification being based upon the direct comparison, by J. E. Hilgard, of a kilogramme weight with the standard kilogramme at Washington. The specific gravity of each piece was determined, and also of all materials and vessels used in the weighings. During each weighing both barometer and thermometer were observed, so that every result represents a real weight in vacuo.

The ammonium alum used in the first series of experiments was specially prepared, and was absolutely free from ascertainable impurities. The salt was found, however, to lose traces of water at ordinary

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[1] Bull. Soc. Chim., 31, 153.

[2] Phil. Trans., 1880, p. 1003.

temperatures—a circumstance which tended towards a slight elevation of the apparent atomic weight of aluminum as calculated from the weighings. Two sets of experiments were made with the alum: one upon a sample air-dried for two hours at 21°-25°, the other upon material dried for twenty-four hours at 19°-26°. These sets, marked A and B, respectively, differ slightly, B being the less trustworthy of the two, judged from a chemical standpoint. Mathematically, it is the better of the two. Calcination was effected with a great variety of precautions, concerning which the original memoir must be consulted. To Mallet's weighings I append the percentages of Al_2O_3 deduced from them:

Series A.

8.2144	grm. of the alum gave	.9258	grm. Al_2O_3.	11.270 per cent.
14.0378	"	1.5825	"	11.273 "
5.6201	"	.6337	"	11.275 "
11.2227	"	1.2657	"	11.278 "
10.8435	"	1.2216	"	11.266 "

Mean, 11.2724, ± .0014

Series B.

12.1023	grm. of the alum gave	1.3660	grm. Al_2O_3.	11.287 per cent.
10.4544	"	1.1796	"	11.283 "
6.7962	"	.7670	"	11.286 "
8.5601	"	.9654	"	11.278 "
4.8992	"	.5528	"	11.283 "

Mean, 11.2834, ± .0011

Combined, these series give a general mean of 11.2793, ± .0008. Hence Al = 27.153.

The aluminum bromide used in the second series of experiments was prepared by the direct action of bromine upon the metal. The product was repeatedly distilled, the earlier portions of each distillate being rejected, until a constant boiling point of 263.3° at 747 mm. pressure was noted. The last distillation was effected in an atmosphere of pure nitrogen, in order to avoid the possible formation of oxide or oxy-bromide of aluminum; and the distillate was collected in three portions, which proved to be sensibly identical. The individual samples of bromide were collected in thin glass tubes, which were hermetically sealed after nearly filling. For the titration pure silver was prepared, and after fusion upon charcoal it was heated in a Sprengel vacuum in order to eliminate occluded gases. This silver was dissolved in specially purified nitric acid, the latter but very slightly in excess. The aluminum bromide, weighed in the sealed tube, was dissolved in water, precautions being taken to avoid any loss by splashing or fuming which might result from the violence of the action. To the solution thus obtained the silver solution was added, the silver being something less than a decigramme in deficiency. The remaining amount of silver needed to complete the precipitation of the bromine was added from a burette, in the form of a standard solution containing one milligramme of metal to each cubic centimetre. The final results were as follows, the figures in the third column representing the quantities of bromide proportional to 100 parts of silver. Series A is from the first portion of the last distillate of $AlBr_3$; series B from the second portion, and series C from the third portion:

Series A.

$AlBr_3$.	*Ag.*	*Ratio.*
6.0024	7.2793	82.458
8.6492	10.4897	82.454
3.1808	3.8573	82.462

Series B.

$AlBr_3$.	*Ag.*	*Ratio.*
6.9617	8.4429	82.456
11.2041	13.5897	82.445
3.7621	4.5624	82.459
5.2842	6.4085	82.456
9.7338	11.8047	82.457

Series C.

$AlBr_3$.	*Ag.*	*Ratio.*
9.3515	11.3424	82.447
4.4426	5.3877	82.458
5.2750	6.3975	82.454
		Mean, 82.455, ± .001

Hence Al = 27.098.

The experiments to determine the amount of hydrogen evolved by the action of caustic soda upon metallic aluminum were conducted with pure metal, specially prepared, and with caustic soda made from sodium. The soda solution was so strong as to scarcely lose a perceptible amount of water by the passage through it of a dry gas at ordinary temperature. As the details of the experiments are somewhat complex, the original memoir must be consulted for them. The following results were obtained, the weight of the hydrogen being calculated from the volume, reckoned at .089872 gramme per litre.

Wt. Al.	*Vol. H.*	*Wt. H.*	*At. Wt. (H = 1).*
.3697	458.8	.041234	26.898
.3769	467.9	.042051	26.889
.3620	449.1	.040362	26.907
.7579	941.5	.084614	26.872
.7314	907.9	.081595	26.891
.7541	936.4	.084156	26.882
			Mean, 26.890, ± .0034

Hence Al = 27.099, when O = 16.

The closing series of experiments was made with larger quantities of aluminum than were used in the foregoing set. The hydrogen, evolved by the action of the caustic alkali, was dried by passing it through two drying tubes containing pumice stone and sulphuric acid, and two others containing asbestos and phosphorus pentoxide. Thence it passed through a combustion tube containing copper oxide heated to redness. A stream of dry nitrogen was employed to sweep the last traces of hydrogen into the combustion tube, and dry air was afterwards passed through the entire apparatus to reoxidize the surface of reduced copper, and to prevent the retention of occluded hydrogen. The water formed by the oxidation of the hydrogen was collected in three drying tubes. The results obtained were as follows. The third column gives the amount of water formed from 10 grammes of aluminum.

2.1704 grm. Al	gave	2.1661	grm. H_2O.	9.9802
2.9355	"	2.9292	"	9.9785
5.2632	"	5.2562	"	9.9867
				Mean, 9.9818, ± .0017

Hence Al = 27.073.

From the last two series of experiments an independent value for the atomic weight of oxygen may be calculated. They give O = 15.895, when H = 1. The closeness of this figure to some of the best determinations affords a good indication of the accuracy of Mallet's work.

In connection with Mallet's work it is worth noting that Torrey[1] published a series of measurements of the H : Al ratio, representing determinations made under his direction by elementary students. These measurements are thirteen in number, and calculated with Regnault's old value for the weight of hydrogen, range from 26.661 to 27.360, or in mean, 27.049, ± .323. Corrected by the latest value for the weight of H, this mean becomes 26.967, when H = 1. This figure, of course, has only confirmatory significance.

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[1] Amer. Chem. Journ., 10, 74. 1888.

By Baubigny[1] we have only two determinations, based upon the calcination of anhydrous aluminum sulphate, $Al_2(SO_4)_3$.

3.6745 grm. salt gave	1.0965 Al_2O_3.	29.841 per cent.	
2.539 "	.7572 "	29.823 "	
		Mean, 29.832, ± .0061	

Hence Al=27.061.

Thomsen's[2] value for the atomic weight of aluminum was derived from his earlier work on the hydrogen-oxygen ratio. In that investigation one part of aluminum was found equivalent to 0.11190, ±.000015 of hydrogen, and 0.88787, ±.000018 of oxygen. The aluminum, however, was impure, and the first step in the new research was to determine its impurities. These were, in one gramme of metal, 0.00819 gramme of silicon and .00322 of iron. Correcting for these, and also for the change of volume in the soda solution following the solution of the metal, the equivalent values become 0.99897 grm. Al, 0.11195 grm. H. and 0.88824 grm. O. From the oxygen ratio Al=26.992, ±.0011. From the hydrogen, the ratio H : Al : : 1 : 26.765, ±.0036 is derived. For the same ratio Mallet found 26.890, ±.0034. The two series, combined, give a general mean of 26.860, ±.0025.

The determinations by Kohn-Abrest[3] are of very slender value. Impure aluminum was dissolved in hydrochloric acid, the hydrogen evolved was burned over hot copper oxide, and the water formed was weighed. The weights of metal taken and the percentages of water produced are given below:

Weight Al.	*Per cent.* H_2O.
.7909	98.08
.7428	98.20
.5477	97.86
.5132	98.10
.6571	98.44
.4993	98.03
.5384	97.98
	Mean, 98.10, ± .0473

Corrected for the known impurities of the aluminum, this mean becomes 99.151. Hence Al=27.255.

Mallet's value for this ratio, reduced to the same standard, is 99.818, ±.0170. Combining, the general mean is 99.742, ±.0160.

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[1] Compt. Rend., 97, 1369. 1883.

[2] Zeitsch. anorg. Chem., 15, 447. 1897. See also *ante*, p. 25.

[3] Bull. Soc. Chim. (3), 33, 121. 1905. Preliminary in Compt. Rend., 139, 669.

Kohn-Abrest also made two determinations of atomic weight by converting metallic aluminum into oxide, as follows:

.3429 grm. Al	gave .6444 Al_2O_3.	53.212	per cent.
.4168	" .7850 "	53.095	"
		Mean, 53.153, ± .0387	

Hence Al = 27.230. This can be combined with Thomsen's figure for the Al : O ratio, but its probable error is so high that it exerts no appreciable influence.

It is clear that the single determinations of Berzelius, Mather, Tissier, Isnard and Terreil may now be safely left out of account, for the reason that none of them could affect appreciably the final value for Al. The ratios to consider are as follows:

(1). $3Ag:AlCl_3::100:41.344$, ± .0070
(2). Percentage Al_2O_3 in ammonium alum, 11.2793, ± .0008
(3). $3Ag:AlBr_3::100:82.455$, ± .0010
(4). $H:Al::1:26.860$, ± .0025
(5). $2Al:3H_2O::100:99.742$, ± .0160
(6). $Al_2(SO_4)_3:Al_2O_3::100:29.832$, ± .0061
(7). $O:Al::16:26.992$, ± .0011

The antecedent atomic weights are

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 7 .... Al = 26.9920, ± .0011
" " 6 .... 27.0607, ± .0115
" " 4 .... 27.0695, ± .0025
" " 5 .... 27.0933, ± .0087
" " 3 .... 27.0983, ± .0015
" " 2 .... 27.1533, ± .0041
" " 1 .... 27.4305, ± .0227

General mean, Al = 27.0400, ± .0008

The last value, from ratio 1, is worthless, but is of no influence in the general combination. No one of the other values is entitled to exclusive confidence. The atomic weight of aluminum needs reinvestigation.

GALLIUM.

Gallium has been so recently discovered, and obtained in such small quantities, that its atomic weight has not as yet been determined with much precision. The following data were fixed by the discoverer, Lecoq de Boisbaudran:[1]

3.1044 grammes gallium ammonium alum, upon ignition, left .5885 grm. Ga_2O_3.

Hence Ga = 70.12.

.4481 gramme gallium, converted into nitrate and ignited, gave .6024 grm. Ga_2O_3.

Hence Ga = 69.70.

These values, assigned equal weight, give in mean Ga = 69.91, with an uncertainty of perhaps half a unit.

INDIUM.

Reich and Richter, the discoverers of indium, were also the first to determine its atomic weight.[2] They dissolved weighed quantities of the metal in nitric acid, precipitated the solution with ammonia, ignited the precipitate, and ascertained its weight. Two experiments were made, as follows:

.5135	grm. indium gave	.6243	grm. In_2O_3.
.699	"	.8515	"

Hence, in mean, In = 110.61; a value known now to be too low.

An unweighed quantity of fresh, moist indium sulphide was also dissolved in nitric acid, yielding, on precipitation,

.2105 grm. In_2O_3 and .542 grm. $BaSO_4$

Hence, with $BaSO_4$ = 233.43, In = 111.99; also too low.

Soon after the publication of Reich and Richter's paper the subject was taken up by Winkler.[3] He dissolved indium in nitric acid, evaporated to dryness, ignited the residue, and weighed the oxide thus obtained.

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[1] Journ. Chem. Soc., 1878, p. 646.

[2] Journ. prakt. Chem., 92, 484.

[3] Journ. prakt. Chem., 94, 8.

.5574 grm. In gave .6817 grm. In_2O_3.
.6661 " .8144 "
.5011 " .6126 "

Hence, in mean, In = 107.76; a result even lower than the values already cited.

In a later paper by Winkler[1] better results were obtained. Two methods were employed. First, metallic indium was placed in a solution of pure, neutral, sodio-auric chloride, and the amount of gold precipitated was weighed. I give the weighings and, in a third column, the amount of indium proportional to 100 parts of gold:

In.	*Au.*	*Ratio.*
.4471 grm.	.8205 grm.	57.782
.8445 "	1.4596 "	57.858
	Mean,	57.820

Hence, if Au = 197.269, In = 114.06.

Winkler also repeated his earlier process, converting indium into oxide by solution in nitric acid and ignition of the residue. An additional experiment, the third as given below, was made after the method of Reich and Richter. The third column gives the percentage of In in In_2O_3:

1.124 grm. In gave	1.3616 grm. In_2O_3.	82.550 per cent.
1.015 "	1.2291 "	82.581 "
.6376 "	.7725 "	82.537 "

These figures were confirmed by a single experiment of Bunsen's,[2] published simultaneously with the specific heat determinations which showed that the oxide of indium was In_2O_3, and not InO, as had been previously supposed:

1.0592 grm. In gave 1.2825 grm. In_2O_3. 82.589 per cent.

For convenience we may add this figure in with Winkler's series, which gives a mean percentage of In in In_2O_3 of 82.564. Hence In = 113.646.

Recent investigations have shown that all of the foregoing determinations are untrustworthy, and that they give values for the atomic weight of indium which are too low. Thiel[3] carefully investigated the properties of indium oxide, and found it to be quite unsuited to atomic

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[1] Journ. prakt. Chem., 102, 282.

[2] Poggend. Annal., 141, 28.

[3] Zeitsch. anorg. Chem., 40, 280. 1904. Preliminary in Vol. 39, 119, and Ber., 37, 175.

weight determinations. Calcined at low temperatures it tends to retain gaseous occlusions; at high temperatures it is distinctly volatile. Syntheses of the indium halides also gave unsatisfactory results. Thiel finally made analyses of indium trichloride and tribromide, purified by sublimation, and obtained the following ratios with the corresponding silver salts. First, the ratio $3AgCl : InCl_3 : 100 : x$, with weights corrected to a vacuum:

Weight $InCl_3$.	*Weight AgCl.*	*Ratio.*
5.0194	9.7526	51.467
4.7049	9.1401	51.475
5.7067	11.0862	51.476
5.4075	10.5055	51.473
		Mean, 51.473, ± .0015

Hence In = 114.98.

In the bromide series the weights were not reduced to a vacuum standard.

Weight $InBr_3$.	*Weight AgBr.*	*Ratio.*
8.9040	14.1531	62.912
8.2140	13.0512	62.937
9.4016	14.9422	62.920
		Mean, 62.923. ± .0070

Hence In = 114.75.

Thiel regards the chloride series as the better of the two, and attaches little importance to the bromide determinations.

Mathers,[1] like Thiel, derived the atomic of indium from analyses of the two trihalides. His weights and ratios are as follows:

Weight $InCl_3$.	*Weight AgCl.*	*Ratio.*
2.1156	4.11421	51.422
4.95920	9.64176	51.435
1.98175	3.85125	51.457
5.54540	10.77904	51.446
1.46361	2.84557	51.435
4.08602	7.94054	51.458
		Mean, 51.442, ± .0038

Hence In = 114.83.

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[1] Journ. Amer. Chem. Soc., 29, 485. 1907.

Combined with Thiel's series, the general mean for the ratio is 51.469, ±.0014.

Weight $InBr_3$.	*Weight AgBr.*	*Ratio.*
2.73494	4.34550	62.937
7.69880	12.23341	62.933
6.27450	9.96917	62.939
5.36642	8.52741	62.931
5.16112	8.20128	62.931
4.98336	7.92009	62.921
		Mean, 62.932, ± .0011

Hence In = 114.80.

On combination of this series with Thiel's the general mean becomes 62.931, ±.0011.

Neglecting the older work there are now two ratios from which to deduce the atomic weight of indium:

(1). $3AgCl:InCl_3::100:51.469$, ± .0014
(2). $3AgBr:InBr_3::100:62.932$, ± .0011

Computing with Ag = 107.880, ±.00029; Cl = 35.4584, ±.0002, and Br = 79.9197, ±.0003 we have—

From ratio 2 .... In = 114.799, ± .0053
" " 1 .... 114.949, ± .0061

General mean, In = 114.864, ± .0040

This mean is, of course, not conclusive. Other indium ratios need to be determined before the atomic weight can be more than approximately known. The true value probably lies between 114.8 and 115.0. For the present the mean value 114.9 may be accepted.

THALLIUM.

The atomic weight of this interesting metal has been fixed by the researches of Lamy, Werther, Hebberling, Crookes and Lepierre.

Lamy and Hebberling investigated the chloride and sulphate; Werther studied the iodide; Crookes' experiments involved the synthesis of the nitrate. Lepierre's work is still more recent, and is based upon several compounds.

Lamy[1] gives the results of one analysis of thallium sulphate and three of thallium chloride. 3.423 grammes of Tl_2SO_4 gave 1.578 grm. $BaSO_4$; whence 100 parts of the latter are equivalent to 216.920 of the former. Hence Tl = 205.14.

In the thallium chloride the chlorine was estimated as silver chloride. The following results were obtained. In the third column I give the amount of TlCl proportional to 100 parts of AgCl:

3.912 grm. TlCl	gave	2.346 grm. AgCl.		166.752
3.000	"	1.8015	"	166.528
3.912	"	2.336	"	167.466
			Mean,	166.915, ± .1905

Hence Tl = 203.79.

Hebberling's[2] work resembles that of Lamy. Reducing his weighings to the standards adopted above, we have from his sulphate series, as equivalent to 100 parts of $BaSO_4$, the amounts of Tl_2SO_4 given in the third column:

1.4195 grm. Tl_2SO_4	gave	.6534 grm. $BaSO_4$.		217.248
1.1924	"	.5507	"	216.524
.8560	"	.3957	"	216.325
			Mean,	216.699

Hence Tl = 204.89.

Including Lamy's single result as of equal weight, we get a mean of 216.754, ± .1387.

From the chloride series we have these results, with the ratio stated as usual:

.2984 grm. TlCl	gave	.1791 grm. AgCl.		166.611
.5452	"	.3278	"	166.321
			Mean,	166.465, ± .097

Hence Tl = 203.15.

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[1] Zeit. Anal. Chem., 2, 211. 1863.

[2] Ann. Chem. Pharm., 134, 11. 1865.

Lamy's mean was 166.915, ±.1905. Both means combined give a general mean of 166.555, ±.0865.

Werther's[1] determinations of iodine in thallium iodide were made by two methods. In the first series TlI was decomposed by zinc and potassium hydroxide, and in the filtrate the iodine was estimated as AgI. One hundred parts of AgI correspond to the amounts of TlI given in the last column:

.720 grm. TlI gave	.51 grm. AgI.	141.176
2.072 "	1.472 "	140.761
.960 "	.679 "	141.384
.385 "	.273 "	141.026
1.068 "	.759 "	140.711
	Mean,	141.012, ± .085

In the second series the thallium iodide was decomposed by ammonia in presence of silver nitrate, and the resulting AgI was weighed. Expressed according to the foregoing standard, the results are as follows:

1.375 grm. TlI gave	.978 grm. AgI.	Ratio, 140.593
1.540 "	1.095 "	" 140.639
1.380 "	.981 "	" 140.673
		Mean, 140.635, ± .016

General mean of both series, 140.648, ±.016. Hence Tl = 203.32.

In 1873 Crookes,[2] the discoverer of thallium, published his final determination of its atomic weight. His method was to effect the synthesis of thallium nitrate from weighed quantities of absolutely pure thallium. No precaution necessary to ensure purity of materials was neglected; the balances were constructed especially for the research; the weights were accurately tested and all their errors ascertained; weighings were made partly in air and partly in vacuo, but all were reduced to *absolute* standards; and unusually large quantities of thallium were employed in each experiment. In short, no effort was spared to attain as nearly as possible absolute precision of results. The details of the investigation are too voluminous, however, to be cited here; the reader who wishes to become familiar with them must consult the original memoir.

The results of ten experiments by Professor Crookes may be stated as follows. In a final column I give the quantity of nitrate producible from 100 parts of thallium. The weights given are in grains:

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[1] Journ. prakt. Chem., 92, 128. 1864.
[2] Phil. Trans., 1873, p. 277.

Thallium.	*$TlNO_3$ + Glass.*	*Glass Vessel.*	*Ratio.*
497.972995	1121.851852	472.557319	130.3875
293.193507	1111.387014	729.082713	130.3930
288.562777	971.214142	594.949719	130.3926
324.963740	1142.569408	718.849078	130.3900
183.790232	1005.779897	766.133831	130.3912
190.842532	997.334615	748.491271	130.3920
195.544324	1022.176679	767.203451	130.3915
201.816345	1013.480135	750.332401	130.3897
295.683523	1153.947672	768.403621	130.3908
299.203036	1159.870052	769.734201	130.3917
			Mean, 130.3910, ±.00034

Hence Tl = 204.041.

Lepierre's[1] determinations were published in 1893, and represented several distinct methods. First, thallous sulphate was subjected to electrolysis in presence of an excess of ammonium oxalate, the reduced metal being dried and weighed in an atmosphere of hydrogen. The corrected weights, etc., are as follows:

1.8935 grm. Tl_2SO_4 gave	1.5327 Tl.	80.945 per cent.
2.7243 "	2.2055 "	80.957 "
2.8112 "	2.2759 "	80.958 "
		Mean, 80.953, ± .0030

Hence Tl = 204.150.

Secondly, weighed quantities of crystallized thallic oxide were converted into thallous sulphate by means of sulphurous acid, and the solution was then subjected to electrolysis, as in the preceding series.

3.2216 grm. Tl_2O_3 gave	2.8829 Tl.	89.487 per cent.
2.5417 "	2.2742 "	89.475 "
		Mean, 89.481, ± .0040

Hence Tl = 204.158.

In the third set of experiments a definite amount of thallous sulphate or nitrate was fused in a polished silver crucible with ten times its weight of absolutely pure caustic potash. Thallic oxide was thus formed, which, with various precautions, was washed with water and alcohol, and finally weighed in the original crucible. One experiment with the nitrate gave—

2.7591 grm. $TlNO_3$ yields 2.3649 Tl_2O_3. 85.713 per cent.

Hence Tl = 204.037.

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[1] Bull. Soc. Chim. (3), 9, 166.

Two experiments were made with the sulphate, as follows:

3.1012 grm. Tl_2SO_4 gave	2.8056 Tl_2O_3.	90.468 per cent.	
2.3478 "	2.1239 "	90.463 "	
		Mean, 90.465, ± .0020	

Hence Tl = 204.021.

Finally, crystallized thallic oxide was reduced by heat in a stream of hydrogen, and the water so formed was collected and weighed.

2.7873 grm. Tl_2O_3 gave	.3301 H_2O.	11.843 per cent.
3.9871 "	.4716 "	11.828 "
4.0213 "	.4761 "	11.839 "
		Mean, 11.837, ± .0029

Hence Tl = 204.300.

In a supplementary note[1] Lepierre states that his weights were all reduced to a vacuum standard.

Some work by Wells and Penfield,[2] incidentally involving a determination of atomic weight, but primarily intended for another purpose, may also be taken into account. Their question was as to the constancy of thallium itself. The nitrate was repeatedly crystallized, and the last crystallization, with the mother liquor representing the opposite end of the series, were both converted into chloride. In the latter the chlorine was estimated as silver chloride, which was weighed on a Gooch filter, with the results given below, which are sensibly identical. The TlCl equivalent to 100 parts of AgCl is stated in the last column.

	TlCl.	*AgCl.*	*Ratio.*
Crystals	3.9146	2.3393	167.341
Mother liquor	3.3415	1.9968	167.343
			Mean, 167.342

Hence Tl = 204.11.

The general mean of Lamy's and Hebberling's determinations of this ratio gave 166.555, ± .0865. If we arbitrarily assign Wells and Penfield's mean equal weight with that, we get a new general mean of 166.948, ± .0610.

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[1] Bull. Soc. Chim. (3), 11, 423. 1894.

[2] Amer. Journ. Sci. (3), 47, 466. 1894.

The ratios to be considered are now as follows:

(1). $BaSO_4 : Tl_2SO_4 :: 100 : 216.754$, ± .1387
(2). $AgCl : TlCl :: 100 : 166.948$, ± .0610
(3). $AgI : TlI :: 100 : 140.648$, ± .016
(4). $Tl : TlNO_3 :: 100 : 130.391$, ± .00034
(5). $Tl_2SO_4 : 2Tl :: 100 : 80.953$, ± .0030
(6). $Tl_2O_3 : 2Tl :: 100 : 89.481$, ± .0040
(7). $2TlNO_3 : Tl_2O_3 :: 100 : 85.713$
(8). $Tl_2SO_4 : Tl_2O_3 :: 100 : 90.465$, ± .0020
(9). $Tl_2O_3 : 3H_2O :: 100 : 11.837$, ± .0029

The antecedent atomic weights are as follows:

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
I = 126.9204, ± .00033
N = 14.0101, ± .0001
S = 32.0667, ± .00075
H = 1.00779, ± .00001

Ratio 7 rests upon a single experiment, and the atomic weight derived from it must therefore be arbitrarily weighted. To do this its probable error is assumed to be the same as that given by ratio 8. Taking so much for granted, the nine values for thallium are

From ratio 3 ....Tl = 203.322, ± .0376
" " 2 ....203.842, ± .0875
" " 8 ....204.021, ± .0695
" " 7 ....204.037, ± .0695
" " 4 ....204.041, ± .0023
" " 5 ....204.150, ± .0330
" " 6 ....204.158, ± .0780
" " 9 ....204.300, ± .0593
" " 1 ....204.950, ± .1619

A glance at the "probable errors" in this series of values will show that Crookes' ratio, No. 4, carries overwhelming weight. It is therefore unnecessary to compute the general mean, for it could not vary much from that. The value Tl = 204.04 is to be accepted as the best.

SILICON.

Although Berzelius[1] attempted to ascertain the atomic weight of silicon, first by converting pure Si into SiO_2, and later from the analysis of $BaSiF_6$, his results were not satisfactory. We need consider only the work of Pelouze, Schiel, Dumas, Thorpe and Young, and Becker and Meyer.

Pelouze,[2] experimenting upon silicon tetrachloride, employed his usual method of titration with a solution containing a known weight of silver. One hundred parts of Ag gave the following equivalencies of $SiCl_4$:

39.4325
39.4570

Mean, 39.4447, ± .0083

Hence Si = 28.373.

Essentially the same method was adopted by Dumas.[3] Pure $SiCl_4$ was weighed in a sealed glass bulb, then decomposed by water, and titrated. The results for 100 Ag are given in the third column:

2.899 grm. $SiCl_4$	= 7.3558 grm. Ag.	39.411
1.242 "	3.154 "	39.379
3.221 "	8.1875 "	39.340
		Mean, 39.377, ± .014

Hence Si = 28.080.

Dumas' and Pelouze's series combine as follows:

Pelouze	39.4447, ± .0083
Dumas	39.377, ± .014
General mean	39.4265, ± .0071

Schiel,[4] also studying the chloride of silicon, decomposed it by ammonia. After warming and long standing it was filtered, and in the filtrate the chlorine was estimated as AgCl. One hundred parts of AgCl correspond to the quantities of $SiCl_4$ given in the last column:

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[1] Lehrbuch, 5 Aufl., 3, 1200.

[2] Compt. Rend., 20, 1047. 1845.

[3] Ann. Chem. Pharm., 113, 31. 1860.

[4] Ann. Chem. Pharm., 120, 94.

.6738 grm. $SiCl_4$ gave	2.277 grm. AgCl.	29.592
1.3092 "	4.418 "	29.633

Mean, 29.6125 ± .0138

Hence Si=27.952.

Thorpe and Young,[1] working with silicon bromide, obtained better results. The bromide was perfectly clear and colorless, and boiled constantly at 153°. It was weighed, decomposed with water and evaporated to dryness, the crucible containing it being finally ignited. The crucible was tared by one precisely similar, in which an equal volume of water was also evaporated. Results as follows, with vacuum weights:

9.63007 grm. $SiBr_4$ gave	1.67070 SiO_2.	17.349 per cent.
12.36099 "	2.14318 "	17.338 "
12.98336 "	2.25244 "	17.349 "
9.02269 "	1.56542 "	17.350 "
15.38426 "	2.66518 "	17.324 "
9.74550 "	1.69020 "	17.343 "
6.19159 "	1.07536 "	17.368 "
9.51204 "	1.65065 "	17.353 "
10.69317 "	1.85555 "	17.353 "

Mean, 17.347, ± 0027

Hence Si=28.379.

The determinations by Becker and Meyer[2] resemble the foregoing series, except that silicon tetrachloride was used instead of the bromide. The carefully purified substance was decomposed by water, the solution was evaporated to dryness, and the silica produced was weighed. In a second communication Meyer[3] discusses the possible retention of chlorine by the silica, and shows that that error was avoided. The data obtained by Becker and Meyer follow, with vacuum weights, and a percentage column computed by myself:

4.16733 grm. $SiCl_4$ gave	1.47597 SiO_2.	35.417 per cent.
4.69585 "	1.66304 "	35.415 "
4.91918 "	1.74204 "	35.413 "
5.37434 "	1.90349 "	35.418 "
5.93985 "	2.10364 "	35.416 "
6.73605 "	2.38570 "	35.417 "
7.16361 "	2.53606 "	35.402 "
7.82779 "	2.77242 "	35.418 "

Mean, 35.4145, ± .0017

Hence Si=28.226.

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[1] Journ. Chem. Soc., 51, 576. 1887.

[2] Zeitsch. anorg. Chem., 43, 251. 1905.

[3] Zeitsch. anorg. Chem., 46, 45. 1905. In Vol. 43, p. 242, Meyer discusses the problem of the calculation of atomic weights.

The ratios for silicon are now—

(1). $4Ag:SiCl_4::100:39.4265$, ± .0071
(2). $4AgCl:SiCl_4::100:29.6125$, ± .0138
(3). $SiCl_4:SiO_2::100:35.4145$, ± .0017
(4). $SiBr_4:SiO_2::100:17.347$, ± .0027

Reducing these ratios with Ag = 107.880, ± .00029, Cl = 35.4584, ± .0002, and Br = 79.9197, ± .0003, we have—

From ratio 2 .... Si = 27.9516, ± .0792
" " 3 .... 28.2257, ± .0045
" " 1 .... 28.2996, ± .0307
" " 4 .... 28.3785, ± .0115

General mean, Si = 28.2462, ± .0041

The rounded-off mean, Si = 28.25, is probably as near the truth as any of the individual values.

TITANIUM.

The earliest determinations of the atomic weight of titanium are due to Heinrich Rose.[1] In his first investigation he studied the conversion of titanium sulphide into titanic acid, and obtained erroneous results; later, in 1829, he published his analyses of the chloride.[2] This compound was purified by repeated rectifications over mercury and over potassium, and was weighed in bulbs of thin glass. These were broken under water in tightly stoppered flasks; the titanic acid was precipitated by ammonia, and the chlorine was estimated as silver chloride. The following results were obtained. In a fourth column I give the TiO_2 in percentages referred to $TiCl_4$ as 100, and in a fifth column the quantity of $TiCl_4$ proportional to 100 parts of AgCl:

$TiCl_4$.	*TiO_2.*	*AgCl.*	*Per cent. TiO_2.*	*AgCl Ratio.*
.885 grm.	.379 grm.	2.661 grm.	42.825	33.258
2.6365 "	1.120 "	7.954 "	42.481	33.147
1.7157 "	.732 "	5.172 "	42.665	33.173
3.0455 "	1.322 "	9.198 "	43.423	33.100
2.4403 "	1.056 "	7.372 "	43.273	33.102
			Mean, 42.933, ± .121	33.156, ± .019

Hence Ti = 48.262, from column 5.

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[1] Gilbert's Annalen, 1823, 67 and 129.

[2] Poggend. Annalen, 15, 145. Berz. Lehrbuch, 3, 1210.

If we directly compare the AgCl with the TiO_2 we shall find 100 parts of the former proportional to the following quantities of the latter:

14.243
14.081
14.153
14.373
14.324

Mean, 14.235, ± .036

Hence Ti = 49.617.

Shortly after the appearance of Rose's paper, Mosander[1] published some figures giving the percentage of oxygen in titanium dioxide, from which a value for the atomic weight of titanium was deduced. Although no details are furnished as to experimental methods, and no actual weighings are given, I cite his percentages for whatever they may be worth:

40.814
40.825
40.610
40.180
40.107
40.050
40.780
40.660
39.830

Mean, 40.428

These figures give values for Ti ranging from 46.38 to 48.34; or, in mean, Ti = 47.15. They are not, however, sufficiently explicit to deserve any farther consideration.

In 1847 Isidor Pierre made public a series of important determinations.[2] Titanium chloride, free from silicon and from iron, was prepared by the action of chlorine upon a mixture of carbon with pure, artificial titanic acid. This chloride was weighed in sealed tubes, these were broken under water, and the resulting hydrochloric acid was titrated with a standard solution of silver after the method of Pelouze. I subjoin Pierre's weighings, and add, in a third column, the ratio of $TiCl_4$ to 100 parts of silver:

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[1] Berz. Jahresbericht, 10, 108. 1831.

[2] Ann. Chim. Phys. (3), 20, 257.

$TiCl_4$.	*Ag.*	*Ratio.*
.8215	1.84523	44.520
.7740	1.73909	44.506
.7775	1.74613	44.527
.7160	1.61219	44.412
.8085	1.82344	44.339
.6325	1.42230	44.470
.8155	1.83705	44.392
.8165	1.83899	44.399
.8065	1.81965	44.322
		Mean, 44.432, ± .0173

Hence Ti = 49.894.

It will be seen that the first three of these results agree well with each other and are much higher than the remaining six. The last four experiments were made purposely with tubes which had been previously opened, in order to determine the cause of the discrepancy. According to Pierre, the opening of a tube of titanium chloride admits a trace of atmospheric moisture. This causes a deposit of titanic acid near the mouth of the tube, and liberates hydrochloric acid. The latter gas being heavy, a part of it falls back into the tube, so that the remaining chloride is richer in chlorine and poorer in titanium than it should be. Hence, upon titration, too low figures for the atomic weight of titanium are obtained. Pierre accordingly rejects all but the first three of the above estimations. These give Ti = 50.265.

The memoir of Pierre upon the atomic weight of titanium was soon followed by a paper from Demoly,[1] who obtained much higher results. He also began with titanic chloride, which was prepared from rutile. The latter substance was found to contain 1.8 per cent. of silica; whence Demoly inferred that the $TiCl_4$ investigated by Rose and by Pierre might have been contaminated with $SiCl_4$, an impurity which would lower the value deduced for the atomic weight under consideration. Accordingly, in order to eliminate all such possible impurities, this process was resorted to: the chloride, after rectification over mercury and potassium, was acted upon by dry ammonia, whereupon the compound $TiCl_4.4NH_3$ was deposited as a white powder. This was ignited in dry ammonia gas, and the residue, by means of chlorine, was reconverted into titanic chloride, which was again repeatedly rectified over mercury, potassium and potassium amalgam. The product boiled steadily at 135°. This chloride, after weighing in a glass bulb, was decomposed by water, the titanic acid was precipitated by ammonia, and the chlorine was estimated in the

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[1] Ann. Chem. Pharm., 72, 214. 1849.

filtrate as silver chloride. Three analyses were performed, yielding the following results. I give the actual weighings:

1.470 grm. $TiCl_4$	gave 4.241 grm.	AgCl	and .565 grm.	TiO_2
2.330	"	6.752	"	.801	"
2.880	"	8.330	"	1.088	"

The ".801" in the last column is certainly a misprint for .901. Assuming this correction, the results may be given in three ratios, thus:

	Per cent. TiO_2 from $TiCl_4$.	*$TiCl_4$:100AgCl.*	*TiO_2:100AgCl.*
	38.435	34.662	13.322
	38.669	34.508	13.344
	37.778	34.574	13.061
Mean.	38.294, ± .180	34.581, ± .030	13.242, ± .061

These three ratios give three widely divergent values for the atomic weight of titanium, ranging from about 36 to more than 56, the latter figure being derived from the ratio between AgCl and $TiCl_4$. This value, 56, is assumed by Demoly to be the best, the others being practically ignored.

Upon comparing Demoly's figures with those obtained by Rose, certain points of similarity are plainly to be noted. Both sets of results were reached by essentially the same method, and in both the discordance between the percentages of titanic acid and of silver chloride is glaring. This discordance can rationally be accounted for by assuming that the titanic chloride was in neither case absolutely what it purported to be; that, in brief, it must have contained impurities, such for example as hydrochloric acid, as shown in the experiments of Pierre, or possibly traces of oxychlorides. Considerations of this kind also throw doubt upon the results attained by Pierre, for he neglected the direct estimation of the titanic acid altogether, thus leaving us without means for correctly judging as to the character of his material.

In 1883[1] Thorpe published a series of experiments upon titanium tetrachloride, determining three distinct ratios and getting sharply concordant results. The first ratio, which was essentially like Pierre's, by decomposition with water and titration with silver, was in detail as follows:

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[1] Ber. Deutsch. chem. Gesell., 16, 3014. 1883.

$TiCl_4$.	*Ag.*	*$TiCl_4$: 100 Ag.*
2.43275	5.52797	44.008
5.42332	12.32260	44.015
3.59601	8.17461	44.000
3.31222	7.52721	44.003
4.20093	9.54679	44.004
5.68888	12.92686	44.008
5.65346	12.85490	43.979
4.08247	9.28305	43.978

Mean, 43.999, ± .0032
Pierre found, 44.432, ± .0073

General mean, 44.017, ± .0031

Thorpe's figures alone give Ti = 48.025.

The second ratio, which involved the weights of $TiCl_4$ taken in the last five determinations of the preceding series, included the weighing of the silver chloride formed. The $TiCl_4$ proportional to 100 parts of AgCl is given in a third column:

$TiCl_4$.	*AgCl.*	*Ratio.*
3.31222	10.00235	33.114
4.20093	12.68762	33.111
5.68888	17.17842	33.117
5.65346	17.06703	33.125
4.08247	12.32442	33.125

Mean, 33.118, ± .0019
Rose found, 33.156, ± .019
Demoly found, 34.581, ± .030

General mean, 33.123, ± .0019

Hence Ti = 48.044 (Thorpe).

In the third series the chloride was decomposed by water, and after evaporation to dryness the resulting TiO_2 was strongly ignited:

$TiCl_4$.	*TiO_2.*	*Per cent. TiO_2.*
6.23398	2.62825	42.160
8.96938	3.78335	42.181
10.19853	4.30128	42.176
6.56894	2.77011	42.170
8.99981	3.79575	42.176
8.32885	3.51158	42.162

Mean, 42.171, ± .0022
Rose found, 42.933, ± .121
Demoly found, 38.294, ± .180

General mean, 42.171, ± .0022

Hence Ti = 48.095.

In short, the work of Rose, Pierre and Demoly practically vanishes. Furthermore, as will be seen later, the three ratios now give closely agreeing values for the atomic weight of titanium. The cross ratio, $4AgCl : TiO_2$ is not directly given by either of Thorpe's series; but the data furnished by Rose and Demoly combine into a general mean of $4AgCl : TiO_2 :: 100 : 13.980, \pm .0303$.

Some two years later Thorpe published his work more in detail,[1] and added a set of determinations, like those made upon the chloride, in which titanium tetrabromide was studied. Three ratios were measured, as was the case with the chloride. In the first, the bromide was decomposed by water and titrated with a silver solution.

$TiBr_4$.	*Ag.*	*$TiBr_4 : 100 Ag$.*
2.854735	3.34927	85.235
3.120848	3.66122	85.241
4.731118	5.55097	85.230
6.969075	8.17645	85.234
6.678099	7.83493	85.234
		Mean, 85.235, ± .0027

Hence Ti = 48.127.

In the four last experiments of the preceding series, the silver bromide formed was weighed. The third column gives the $TiBr_4$ proportional to 100 parts of AgBr.

$TiBr_4$.	*AgBr.*	*Ratio.*
3.120848	6.375391	48.951
4.731118	9.663901	48.957
6.969075	14.227716	48.982
6.678099	13.639956	48.959
		Mean, 48.962, ± .0049

Hence Ti = 48.123.

For the third ratio the bromide was decomposed by water; and after evaporation with ammonia the residual titanic oxide was ignited and weighed:

$TiBr_4$.	*TiO_2.*	*Per cent. TiO_2.*
6.969730	1.518722	21.790
8.836783	1.923609	21.768
9.096309	1.979513	21.762
		Mean, 21.773, ± .0062

Hence Ti = 48.070.

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[1] Journ. Chem. Soc., 47, 108 and 129. 1885.

Ignoring Mosander's work as unavailable, we have the following ratios to consider:

(1). $4Ag:TiCl_4::100:44.017$, ± .0031
(2). $4AgCl:TiCl_4::100:33.123$, ± .0019
(3). $4AgCl:TiO_2::100:13.980$, ± .0303
(4). $TiCl_4:TiO_2::100:42.171$, ± .0022
(5). $4Ag:TiBr_4::100:85.235$, ± .0027
(6). $4AgBr:TiBr_4::100:48.962$, ± .0049
(7). $TiBr_4:TiO_2::100:21.773$, ± .0062

Computing with Ag = 107.880, ± .00029, Cl = 35.4584, ± .0002, and Br = 79.9197, ± .0003, we have—

From ratio	7	Ti = 48.0699, ± .0292
" "	2	48.0794, ± .0135
" "	4	48.0947, ± .0072
" "	1	48.1085, ± .0135
" "	6	48.1232, ± .0369
" "	5	48.1273, ± .0117
" "	3	48.1553, ± .1742
	General mean,	Ti = 48.0991, ± .0049

This may be rounded off to 48.1.

GERMANIUM.

The data relative to the atomic weight of germanium are imperfect, and due entirely to the discoverer of the element, Winkler.[1] The pure tetrachloride was decomposed by sodium carbonate, mixed with a known excess of standard silver solution, and then titrated back with ammonium sulphocyanate. The data given are as follows:

$GeCl_4$.	Cl found.	Per cent. Cl.
.1067	.076112	66.177
.1258	.083212	66.146
.2223	.147136	66.188
.2904	.192190	66.182
		Mean, 66.173

Hence Ge = 72.504.

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[1] Journ. prakt. Chem. (2), 34, 177. 1886.

ZIRCONIUM.

The atomic weight of zirconium has been determined by Berzelius, Hermann, Marignac, Weibull, Bailey and Venable. Berzelius[1] ignited the neutral sulphate, and thus ascertained the ratio in it between the ZrO_2 and the SO_3. Putting SO_3 at 100, he gives the following proportional quantities of ZrO_2:

75.84
75.92
75.80
75.74
75.97
75.85

Mean, 75.853, ± .023

This gives 43.134, ±.0142 as the percentage of zirconia in the sulphate. Hence Zr = 89.46.

Hermann's[2] estimate of the atomic weight of zirconium was based upon analyses of the chloride, concerning which he gives no details nor weighings. From sublimed zirconium chloride he finds Zr = 831.8, when O = 100; and from two lots of the basic chloride $2ZrOCl_2.9H_2O$, Zr = 835.65 and 851.40, respectively. The mean of all three is 839.62; whence, with modern formulæ, Zr = 89.56.

Marignac's results[3] were obtained by analyzing the double fluoride of zirconium and potassium. His weights are as follows:

1.000	grm. gave	.431	grm. ZrO_2 and	.613 grm. K_2SO_4.
2.000	"	.864	"	1.232 "
.654	"	.282	"	.399 "
5.000	"	2.169	"	3.078 "

These figures give us three ratios. A, the ZrO_2 from 100 parts of salt; B, the K_2SO_4 from 100 parts of salt; and C, the ZrO_2 proportional to 100 parts of K_2SO_4:

	A.	*B.*	*C.*
	43.100	61.300	70.310
	43.200	61.600	70.130
	43.119	61.000	70.677
	43.380	61.560	70.468
Mean,	43.200, ± .043	Mean, 61.365, ± .094	Mean, 70.396, ± .079
Hence Zr =	90.03	91.54	90.68

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[1] Poggend. Annal., 4, 126. 1825.
[2] Journ. prakt. Chem., 31, 77. Berz. Jahresb., 25, 147.
[3] Ann. Chim. Phys. (3), 60, 270. 1860.

Weibull,[1] following Berzelius, ignited the sulphate, and also made a similar set of experiments with the selenate of zirconium, obtaining results as follows:

Sulphate. $Zr(SO_4)_2$.

1.5499	grm. salt gave	.6684	ZrO_2.	43.126	per cent.
1.5445	"	.6665	"	43.153	"
2.1683	"	.9360	"	43.168	"
1.0840	"	.4670	"	43.081	"
.7913	"	.3422	"	43.321	"
.6251	"	.2695	"	43.113	"
.4704	"	.2027	"	43.091	"

Mean, 43.150, ± .0207

Hence Zr = 89.54.

Selenate. $Zr(SeO_4)_2$.

1.0212	grm. salt gave	.3323	ZrO_2.	32.540	per cent.
.8418	"	.2744	"	32.597	"
.6035	"	.1964	"	32.544	"
.8793	"	.2870	"	32.640	"
.3089	"	.1003	"	32.470	"

Mean, 32.558, ± .0192

Hence Zr = 90.79.

Bailey[2] also ignited the sulphate, after careful investigation of his material, and of the conditions needful to ensure success. He found that the salt was perfectly stable at 400°, while every trace of free sulphuric acid was expelled at 350°. The chief difficulty in the process arises from the fact that the zirconia produced by the ignition is very light, and easily carried off mechanically, so that the percentage found is likely to be too low. This difficulty was avoided by the use of a double crucible, the outer one retaining particles of zirconia which otherwise might be lost. The results, corrected for buoyancy of the air, are as follows:

2.02357	salt gave	.87785	ZrO_2.	43.381	per cent.
2.6185	"	1.1354	"	43.360	"
2.27709	"	.98713	"	43.350	"
2.21645	"	.96152	"	43.385	"
1.75358	"	.76107	"	43.402	"
1.64065	"	.7120	"	43.397	"
2.33255	"	1.01143	"	43.361	"
1.81105	"	.78485	"	43.337	"

Mean, 43.372, ± .0056

Hence Zr = 90.65.

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[1] Lund. Arsskrift, Vol. 18. 1881-'82.

[2] Proc. Roy. Soc., 46, 74. Chem. News, 60, 32.

This, combined with previous determinations, gives—

Berzelius	43.134, ± .0142
Weibull	43.150, ± .0207
Bailey	43.372, ± .0056
General mean	43.317, ± .0051

Venable[1] determined the atomic weight of zirconium by analysis of the oxychloride, $ZrOCl_2.3H_2O$. This compound was purified by crystallization from hot hydrochloric acid and dried in a stream of hydrochloric acid gas. It was then dissolved in water, and after evaporating the solution to dryness in a platinum crucible the residue was converted into zirconia by prolonged ignition. The data are subjoined:

Weight $ZrOCl_2.3H_2O$.	*Weight* ZrO_2.	*Per cent.* ZrO_2.
5.25762	2.78450	52.961
3.53994	1.87550	52.981
3.25036	1.72435	53.051
1.52245	.80708	53.012
2.98802	1.58274	52.969
2.11371	1.11920	52.949
2.38139	1.26161	52.978
1.90285	1.00958	53.055
2.61847	1.38658	52.954
1.07347	.56840	52.951
		Mean, 52.986, ± .0085

Hence Zr = 90.805.

For computing the atomic weight of zirconium we now have the subjoined ratios:

(1). Percentage ZrO_2 in $Zr(SO_4)_2$, 43.317, ± .0051
(2). Percentage ZrO_2 in $Zr(SeO_4)_2$, 32.558, ± .0192
(3). Percentage ZrO_2 from K_2ZrF_6, 43.200, ± .043
(4). Percentage K_2SO_4 from K_2ZrF_6, 61.365, ± .094
(5). Percentage ZrO_2 in $ZrOCl_2.3H_2O$, 52.986, ± .0085
(6). $K_2SO_4:ZrO_2::100:70.396$, ± .0079

The antecedent values for reduction are—

Cl = 35.4584, ± .0002
S = 32.0667, ± .00075
Se = 79.176, ± .0029
K = 39.0999, ± .0002
F = 19.041, ± .00135
H = 1.00779, ± .00001

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[1] Journ. Amer. Chem. Soc., 20, 119. 1898.

Hence,

From ratio 3 .... Zr = 90.030, ± .2390
" " 1 .... 90.374, ± .0201
" " 6 .... 90.677, ± .0138
" " 2 .... 90.790, ± .0804
" " 5 .... 90.805, ± .0297
" " 4 .... 91.538, ± .4350

General mean, Zr = 90.621, ± .0105

The final combination, in this case, is unsatisfactory because of the wide divergence among the individual values. On chemical grounds, ratios 1 and 5 seem to be the only ones worth considering. Their weighted combination gives Zr = 90.183. The value adopted in the latest International table is 90.6. The atomic weight of zirconium evidently needs careful revision.

TIN.

The atomic weight of tin has been determined by means of the oxide, the chloride, the bromide, the sulphide and the stannichlorides of potassium and ammonium.

The composition of stannic oxide has been fixed in two ways: by synthesis from the metal and by reduction in hydrogen. For the first method we may consider the work of Berzelius, Mulder and Vlaanderen, Dumas, Van der Plaats and Bongartz and Classen.

Berzelius[1] oxidized 100 parts of tin by nitric acid, and found that 127.2 parts of SnO_2 were formed. Hence Sn = 117.65.

The work done by Mulder and Vlaanderen[2] was done in connection with a long investigation into the composition of Banca tin, which was found to be almost absolutely pure. For the atomic weight determinations, however, really pure tin was taken prepared from pure tin oxide. This metal was oxidized by nitric acid, with the following results. One hundred parts of tin gave of SnO_2:

127.56—Mulder
127.56—Vlaanderen
127.43—Vlaanderen

Mean, 127.517, ± .029

Hence Sn = 116.3.

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[1] Poggend. Annal., 8, 177.
[2] Journ. prakt. Chem., 49, 35. 1849.

Dumas[1] oxidized pure tin by nitric acid in a flask of glass. The resulting SnO_2 was strongly ignited, first in the flask and afterwards in platinum. His weighings, reduced to the foregoing standard, give for dioxide from 100 parts of tin the amounts stated in the third column:

12.443 grm. Sn gave	15.820 grm. SnO_2.	127.14
15.976 "	20.301 "	127.07

Mean, 127.105, ± .024

Hence Sn = 118.06.

In an investigation later than that previously cited, Vlaanderen[2] found that when tin was oxidized in glass or porcelain vessels, and the resulting oxide ignited in them, traces of nitric acid were retained. When, on the other hand, the oxide was strongly heated in platinum, the latter was perceptibly attacked, so much so as to render the results uncertain. He therefore, in order to fix the atomic weight of tin, reduced the oxide by heating it in a porcelain boat in a stream of hydrogen. Two experiments gave Sn = 118.08, and Sn = 118.21. These become, if reduced to the above common standard,

127.100
127.064

Mean, 127.082, ± .012

Hence Sn = 118.16.

Van der Plaats[3] prepared pure stannic oxide from Banca tin, and upon the material obtained made two series of experiments; one by reduction and one by oxidation. The results, with vacuum weights, are as follows, the ratio between Sn and SnO_2 appearing in the third column:

Oxidation Series.

9.6756 grm. tin gave	12.2967 SnO_2.	127.091
12.7356 "	16.1885 "	127.114
23.4211 "	29.7667 "	127.093

Reduction Series.

5.5015 grm. SnO_2 gave	4.3280 tin.	127.114
4.9760 "	3.9145 "	127.117
3.8225 "	3.0078 "	127.086
2.9935 "	2.3553 "	127.096

Mean of both series as one, 127.102, ± .0033

Hence Sn = 118.07.

The reductions were effected in a porcelain crucible.

Bongartz and Classen[4] purified tin by electrolysis, and oxidized the electrolytic metal by means of nitric acid. The oxide found was dried

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[1] Ann. Chem. Pharm., 113, 26.

[2] Jahresbericht, 1858, 183.

[3] Compt. Rend., 100, 52. 1885.

[4] Berichte Deutsch. chem. Gesell., 21, 2900. 1888.

over a water-bath, then heated over a weak flame, and finally ignited for several hours in a gas-muffle. Some reduction experiments gave values which were too low. The oxidation series was as follows, with the usual ratio stated in a third column:

Sn.	*SnO_2.*	*Ratio.*
2.5673	3.2570	126.865
3.8414	4.8729	126.852
7.3321	9.2994	126.831
5.4367	6.8962	126.845
7.3321	9.2994	126.831
9.8306	12.4785	126.935
11.2424	14.2665	126.896
5.5719	7.0685	126.860
9.8252	12.4713	126.932
4.3959	5.5795	126.925
6.3400	8.0440	126.877

Mean, 126.877, ± .0080

Hence Sn = 119.06.

We now have six series of experiments showing the amount of SnO_2 formed from 100 parts of tin. To Berzelius' single determination may be assigned the weight of one experiment in Mulder and Vlaanderen's series:

Berzelius	127.200, ± .041
Mulder and Vlaanderen........	127.517, ± .029
Dumas	127.105, ± .024
Vlaanderen	127.082, ± .012
Van der Plaats................	127.102, ± .0033
Bongartz and Classen..........	126.877, ± .0080
General mean	127.076, ± .0026

Dumas, in the paper previously quoted, also gives the results of some experiments with stannic chloride, $SnCl_4$. This was titrated with a solution containing a known weight of silver. From the weighings given, 100 parts of silver correspond to the quantities of $SnCl_4$ named in the third column:

1.839 grm. $SnCl_4$ =	3.054 grm. Ag.	60.216
2.665 “	4.427 “	60.199

Mean, 60.207, ± .006

Hence Sn = 117.97.

Tin tetrabromide and the stannichlorides of potassium and ammonium were all studied by Bongartz and Classen; who, in each compound, carefully purified, determined the tin electrolytically. The data given are as follows, the percentage columns being added by myself:

Tin Tetrabromide.

$SnBr_4$ taken.	*Sn found.*	*Per cent. Sn.*
8.5781	2.3270	27.127
9.5850	2.6000	27.126
9.9889	2.7115	27.145
10.4914	2.8445	27.113
16.8620	4.5735	27.123
16.6752	4.5236	27.119
11.1086	3.0125	27.116
10.6356	2.8840	27.113
11.0871	3.0060	27.123
19.5167	5.2935	27.128

Mean, 27.123, ± .0020

Hence Sn = 118.98.

Potassium Stannichloride.

K_2SnCl_6.	*Sn found.*	*Per cent. Sn.*
2.5718	.7472	29.054
2.2464	.6524	29.042
9.3353	2.7100	29.030
12.1525	3.5285	29.035
12.4223	3.6070	29.036
15.0870	4.3812	29.040
10.4465	3.0330	29.034
18.9377	5.5029	29.058
18.4743	5.3630	29.029
17.6432	5.1244	29.045

Mean, 29.040, ± .0021

Hence Sn = 119.07.

Ammonium Stannichloride.

Am_2SnCl_6.	*Sn found.*	*Per cent. Sn.*
1.6448	.5328	32.393
1.8984	.6141	32.347
2.0445	.6620	32.381
2.0654	.6690	32.391
2.0058	.6496	32.386
2.4389	.7895	32.371
4.0970	1.3254	32.351
3.4202	1.1078	32.390
3.6588	1.1836	32.349
1.5784	.5108	32.362
7.3248	2.3710	32.370
13.1460	4.2528	32.351
11.9483	3.8650	32.348
18.4747	5.9788	32.362
18.6635	6.0415	32.371
17.8894	5.7923	32.378

Mean, 32.369, ± .0088

Hence Sn = 119.1.

One other method of determination for the atomic weight of tin was employed by Bongartz and Classen. Electrolytic tin was converted into sulphide, and the sulphur so taken up was oxidized by means of hydrogen peroxide, by Classen's method, and weighed as barium sulphate. The results, as given by the authors, are subjoined:

Sn taken.	*Per cent. of S gained.*
2.6285	53.91
.7495	53.87
1.4785	53.94
2.5690	53.94
2.1765	53.85
1.3245	53.88
.9897	53.83
2.7160	53.86
	Mean, 53.885, ± .0098

This percentage of sulphur, however, was computed from weighings of barium sulphate. What values were assigned to the atomic weights of barium and sulphur is not stated, but as Meyer and Seubert's figures are used for other elements throughout this paper, we may assume that they apply here also. Putting O = 15.96, S = 31.98, and Ba = 136.86, the 53.885 per cent. of sulphur becomes 392.056, ± .0713 of $BaSO_4$, the compound actually weighed. This gives us the ratio—

$$Sn:2BaSO_4::100:392.056, \pm .0713$$

as the real result of the experiments, from which, with the later values for Ba, S and O, the atomic weight of tin may be calculated.

A single determination of the atomic weight of tin, made by Schmidt,[1] ought not to be overlooked, although it was only incidental to his research upon tin sulphide. In one experiment, 0.5243 grm. Sn gave 0.6659 SnO_2. Hence Sn = 118.49. This lies about midway between the two sets of values already computed.

We now have, for tin, the following available ratios:

(1). $Sn:SnO_2::100:127.076, \pm .0026$
(2). $4Ag:SnCl_4::100:60.207, \pm .0060$
(3). Percentage of tin in $SnBr_4$, 27.123, ± .0020
(4). Percentage of tin in K_2SnCl_6, 29.040, ± .0021
(5). Percentage of tin in Am_2SnCl_6, 32.369, ± .0088
(6). $Sn:2BaSO_4::100:392.056, \pm .0713$

The values to use in reduction of these ratios are—

Ag = 107.880, ± .00029
Cl = 35.4584 ± .0002
Br = 79.9197, ± .0003
N = 14.0101, ± .0001
S = 32.0667, ± .00075
Ba = 137.363, ± .0025
H = 1.00779, ± .00001

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[1] Berichte, 27, 2743. 1894.

Hence the following values for tin:

From ratio 2 .... $Sn = 117.971$, ± .0258
" " 1 .... 118.186, ± .0113
" " 3 .... 118.976, ± .0094
" " 4 .... 119.070, ± .0082
" " 6 .... 119.080, ± .0876
" " 5 .... 119.099, ± .0359

General mean, $Sn = 118.648$, ± .0052

The discordance between the first two and the last four of these values is glaring, and there seems to be no true compensation of errors. On chemical grounds, the five fairly concordant series of determinations by Bongartz and Classen seem to be better than the earlier measurements. Their arithmetical mean gives $Sn = 119.057$, which, until further evidence is obtained, should be accepted. New determinations of the atomic weight of tin are much to be desired.

THORIUM.

The atomic weight of thorium has been determined from analyses of the sulphate, oxalate, formate and acetate, with widely varying results. The earliest figures are due to Berzelius,[1] who worked with the sulphate, and with the double sulphate of potassium and thorium. The thoria was precipitated by ammonia, and the sulphuric acid was estimated as $BaSO_4$. The sulphate gave the following ratios in two experiments. The third column represents the weight of ThO_2 proportional to 100 parts of $BaSO_4$:

.6754 grm. $ThO_2 = 1.159$ grm. $BaSO_4$. Ratio, 58.274
1.0515 " 1.832 " " 57.396

The double potassium sulphate gave .265 grm. ThO_2, .156 grm. SO_3, and .3435 K_2SO_4. The SO_3, with the Berzelian atomic weights, represents .4537 grm. $BaSO_4$. Hence 100 $BaSO_4$ is equivalent to 58.408 ThO_2. This figure, combined with the two previous values for the same ratio, gives a mean of 58.026, ± .214, and $Th = 238.9$.

From the ratio between the K_2SO_4 and the ThO_2 in the double sulphate, $Th = 236.88$.

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[1] Poggend. Annal., 16, 398. 1829. Lehrbuch, 3, 1224.

In 1861 new determinations were published by Chydenius,[1] whose memoir is accessible to me only in an abstract[2] which gives results without details. Thoria is regarded as a monoxide, ThO, and the old equivalents (O=8) are used. The following values are assigned for the molecular weight of ThO, as found from analyses of several salts:

From Sulphate.	*From K. Th. Sulphate.*
66.33	67.02
67.13	
67.75	
68.03	

Mean, 67.252, ± .201

From Acetate.	*From Formate.*	*From Oxalate.*	
67.31	68.06	65.87	Two results by Berlin
66.59	67.89	65.95	
67.27	68.94	65.75	
67.06		65.13	
68.40	Mean, 68.297, ± .219	66.54	
		65.85	
Mean, 67.326, ± .201			
		Mean, 65.85, ± .123	

We may fairly assume that these figures were calculated with O=8, C=6, and S=16. Correcting by the values for these elements which have been found in previous chapters, ThO_2 becomes as follows:

From sulphate	$ThO_2 = 269.18$
From acetate	" $= 269.46$
From formate	" $= 273.25$
From oxalate	" $= 263.42$
Average	$ThO_2 = 268.83$

And Th=236.83.

The single result from the double potassium sulphate is included with the column from the ordinary sulphate, and the influence of the atomic weight of potassium is ignored.

Chydenius was soon followed by Marc Delafontaine, whose researches appeared in 1863.[3] This chemist especially studied thorium sulphate; partly in its most hydrous form, partly as thrown down by boiling. In $Th(SO_4)_2.9H_2O$, the following percentages of ThO_2 were found:

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[1] Kemisk undersökning af Thorjord och Thorsalter. Helsingfors, 1861. An academic dissertation.

[2] Poggend. Annal., 119, 55. 1863.

[3] Arch. Sci. Phys. Nat. (2), 18, 343.

45.08
44.90
45.06
45.21
45.06

Mean, 45.062, ± .0332

Hence Th=232.34.

The lower hydrate, $2Th(SO_4)_2.9H_2O$, was more thoroughly investigated. The thoria was estimated in two ways: First (A), by precipitation as oxalate and subsequent ignition; second (B), by direct calcination. These percentages of ThO_2 were found:

$$\left.\begin{array}{r}52.83\\52.52\\52.72\\52.13\end{array}\right\}A$$

$$\left.\begin{array}{r}52.47\\52.49\\52.53\\52.13\\52.13\\52.43\\52.60\\52.40\\52.96\\52.82\end{array}\right\}B$$

Mean, 52.511, ± .047

In three experiments with this lower hydrate the sulphuric acid was also estimated, being thrown down as barium sulphate after removal of the thoria:

1.2425 grm. gave	.400 SO_3.	(1.1656 grm. $BaSO_4$.)	
1.138 "	.366 "	(1.0665 "	)
.734 "	.2306 "	(.6720 "	)

The figures in parentheses are reproduced by myself from Delafontaine's results, he having calculated his analyses with O=100, S=200, and Ba=857. These data may be reduced to a common standard, so as to represent the quantity of $2Th(SO_4)_2.9H_2O$, equivalent to 100 parts of $BaSO_4$. We then have the following figures:

106.597
106.704
109.226

Mean, 107.509, ± .585

Delafontaine was soon followed by Hermann,[1] who published a single analysis of the lower hydrated sulphate, as follows:

ThO_2	52.87
SO_3	32.11
H_2O	15.02
	100.00

Hence, from the ratio between SO_3 and ThO_2, Th=231.67. Probably the SO_3 percentage was loss upon calcination.

Both Hermann's results and those of Delafontaine are affected by one serious doubt, namely, as to the true composition of the lower hydrated sulphate. The latest and best evidence seems to establish the fact that it contains four molecules of water instead of four and a half,[2] a fact which tends to change the resulting atomic weight of thorium considerably. In the final discussion of these data, therefore, the formula $Th(SO_4)_2.4H_2O$ will be adopted. As for Hermann's single analysis, his percentage of ThO_2, 52.87, may be included in one series with Delafontaine's, giving a mean of 52.535, ±.0473. Hence Th=229.

The next determinations to consider are those of Cleve,[3] whose results, obtained from both the sulphate and the oxalate of thorium, agree admirably. The anhydrous sulphate, calcined, gave the subjoined percentages of thoria:

62.442
62.477
62.430
62.470
62.357
62.366

Mean, 62.423, ± .014

Hence Th=234.01.

The oxalate was subjected to a combustion analysis, whereby both thoria and carbonic acid could be estimated. From the direct percentages of these constituents no accurate value can be deduced, there having undoubtedly been moisture in the material studied. From the ratio between CO_2 and ThO_2, however, good results are attainable. This ratio I put in a fourth column, making the thoria proportional to 100 parts of carbon dioxide:

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[1] Journ. prakt. Chem., 93, 114.

[2] See Hillebrand, Bull. 90, U. S. Geol. Survey, p. 29.

[3] K. Svenska Vet. Akad. Handling., Bd. 2, No. 6, 1874.

Oxalate.	ThO_2.	CO_2.	*Ratio.*
1.7135	1.0189	.6736	151.262
1.3800	.8210	.5433	151.114
1.1850	.7030	.4650	151.183
1.0755	.6398	.4240	150.896

Mean, 151.114, ± .053

Hence Th=233.98.

In 1882, Nilson's determinations appeared.[1] This chemist studied both the anhydrous sulphate, and the salt with nine molecules of water, using the usual calcination method, but guarding especially against the hygroscopic character of the dry $Th(SO_4)_2$ and the calcined ThO_2. The hydrated sulphate gave results as follows:

$Th(SO_4)_2.9H_2O$.	ThO_2.	*Per cent.* ThO_2.
2.0549	.9267	45.097
2.1323	.9615	45.092
3.0017	1.3532	45.081
2.7137	1.2235	45.086
2.6280	1.1849	45.088
1.9479	.8785	45.099

Mean, 45.091, ± .0019

Delafontaine found, 45.062, ± .0332

General mean, 45.090, ± .0019

Hence Th=232.64.

The anhydrous sulphate gave data as follows:

$Th(SO_4)_2$.	ThO_2.	*Per cent.* ThO_2.
1.4467	.9013	62.300
1.6970	1.0572	62.298
2.0896	1.3017	62.294
1.5710	.9787	62.298

Mean, 62.297, ± .0009

Hence Th=232.59.

The last four determinations appear again in a paper published five years later by Krüss and Nilson,[2] who, however, give four more made upon material obtained from a different source. The new data are subjoined:

$Th(SO_4)_2$.	ThO_2.	*Per cent.* ThO_2.
1.1630	.7245	62.296
.8607	.5362	62.298
1.5417	.9605	62.301
1.5217	.9479	62.292

Mean, 62.297, ± .0013

Hence Th=232.59.

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[1] Ber. Deutsch. chem. Gesell., 15, 2519. 1882.

[2] Ber. Deutsch. chem. Gesell., 20, 1665. 1887.

Urbain,[1] who purified his material by crystallizing thorium acetylacetonate from solution in chloroform, gives the following analyses of the anhydrous sulphate, effected by calcination:

$Th(SO_4)_2$.	ThO_2.	Per cent. ThO_2.
1.0925	.6815	62.374
.5926	.3699	62.420
1.0230	.6384	62.405
		Mean, 62.400, ± .0096

Hence Th = 233.75.

Meyer and Gumperz,[2] in order to determine whether thorium is complex or not, prepared the octohydrated sulphate from material of diverse origin, and analyzed it by dehydration and calcination. Their data, which I give as one series, represent, first, six experiments upon preparations obtained by fractional precipitation as chromate; and, secondly, six analyses of the sulphate prepared from three samples of thorium chloride. I give here only the weights of the anhydrous sulphate and the oxide, for the reason that the hydration of the compound was too irregular to yield good values for the atomic weight of thorium.

$Th(SO_4)_2$.	ThO_2.	Per cent. ThO_2.
.9301	.5793	62.284
.9927	.6184	62.295
1.0344	.6442	62.278
.9349	.5821	62.263
.6680	.4160	62.276
.4296	.2676	62.291
.9199	.5730	62.289
.7647	.4764	62.299
1.0650	.6633	62.300
.7758	.4834	62.310
.8824	.5496	62.285
.5545	.3454	62.290
		Mean, 62.288, ± .0024

Hence Th = 232.45. From the uniformity of their results, Meyer and Gumperz conclude that there is no evidence of a separation of thorium into substances of different atomic weights. Their figures combine with those of previous investigations thus:

Cleve	62.423, ± .0140
Nilson	62.297, ± .0009
Krüss and Nilson	62.297, ± .0013
Urbain	62.400, ± .0096
Meyer and Gumperz	62.288, ± .0024
General mean	62.296, ± .0007

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[1] Ann. Chim. Phys. (7), 19, 223. 1900.

[2] Ber. Deutsch. chem. Ges., 38, 817. 1905.

Neglecting the work of Chydenius, which has no present value, we have six ratios from which to deduce the atomic weight of thorium, as follows:

(1). $2BaSO_4 : ThO_2 :: 100 : 58.026$, ± .214
(2). $2BaSO_4 : Th(SO_4)_2.4H_2O :: 100 : 107.509$, ± .585
(3). $4CO_2 : ThO_2 :: 100 : 151.114$, ± .053
(4). $Th(SO_4)_2.9H_2O : ThO_2 :: 100 : 45.090$, ± .0019
(5). $Th(SO_4)_2.4H_2O : ThO_2 :: 100 : 52.535$, ± .0473
(6). $Th(SO_4)_2 : ThO_2 :: 100 : 62.296$, ± .0007

To reduce these ratios we have—

S = 32.0667, ± .00075 Ba = 137.363, ± .0025
C = 12.0038, ± .0002 H = 1.00779, ± .00001

Hence,

From ratio 2 .... Th = 226.295, ± 2.7311
" " 5 .... 228.998, ± .3451
" " 6 .... 232.579, ± .0063
" " 4 .... 232.639, ± .0145
" " 3 .... 233.983, ± .0933
" " 1 .... 238.900, ± .9998

Three of these values, the first two and the last, are absolutely worthless, and can be rejected at once. To include them would not appreciably affect the final combination. The values from ratios 3, 4 and 6, combined, give a general mean Th = 232.598, ± .0058, or 232.6 rounded off.

In this discussion the question of the definite individuality of thorium has not been touched. Recent investigations upon radioactivity have shown that the supposed element may be really complex, or at least that it contains traces of other substances. Baskerville[1] and Brauner[2] have both claimed to have fractionated thoria into different component earths, which differed widely in atomic weight and physical properties. These claims, however, are not as yet fully substantiated. Meyer and Gumperz believe that their atomic weight determinations establish the integrity of thorium; but the question is still open. Much work remains to be done before the controversy can be declared ended. Meanwhile the atomic weight as given above represents that of the thorium which is recognized as an element by all analysts.

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[1] Journ. Amer. Chem. Soc., 23, 761 and 26, 922.
[2] Proc. Chem. Soc., 17, 67.

PHOSPHORUS.

The material from which to calculate the atomic weight of phosphorus is by no means abundant. Berzelius, in his Lehrbuch,[1] adduces only his own experiments upon the precipitation of gold by phosphorus, and ignores all the earlier work relating to the composition of the phosphates. These experiments have been considered with reference to gold.

Pelouze,[2] in a single titration of phosphorus trichloride with a standard solution of silver, obtained a wholly erroneous result; and Jacquelain,[3] in his similar experiments, did even worse. Schrötter's criticism upon Jacquelain sufficiently disposes of the latter.[4]

Only the determinations made by Schrötter, Dumas, Van der Plaats, Ter Gazarian and Baxter and Jones remain to be considered.

Schrötter[5] burned pure amorphous phosphorus in dry oxygen, and weighed the pentoxide thus formed. One gramme of P yielded P_2O_5 in the following proportions:

2.28909
2.28783
2.29300
2.28831
2.29040
2.28788
2.28848
2.28856
2.28959
2.28872

Mean, 2.289186, ± .00033

Hence P=31.027.

Dumas[6] prepared pure phosphorus trichloride by the action of dry chlorine upon red phosphorus. The portion used in his experiments boiled between 76° and 78°. This was titrated with a standard solution of silver in the usual manner. Dumas publishes weights, from which I calculate the figures given in the third column, representing the quantity of trichloride proportional to 100 parts of silver:

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[1] 5th ed., 1188.

[2] Compt. Rend., 20, 1047.

[3] Compt. Rend., 33, 693.

[4] Journ. prakt. Chem., 57, 315.

[5] Journ. prakt. Chem., 53, 435. 1851.

[6] Ann. Chem. Pharm., 113, 29. 1860.

1.787 grm. PCl_3	= 4.208 grm. Ag.	42.4667
1.466 "	3.454 "	42.4435
2.056 "	4.844 "	42.4443
2.925 "	6.890 "	42.4528
3.220 "	7.582 "	42.4690

Mean, 42.4553, ± .0036

Hence P=31.027.

By Van der Plaats[1] three methods of determination were adopted, and all weights were reduced to a vacuum standard. First, silver was precipitated from a solution of the sulphate by means of phosphorus. The latter had been twice distilled in a current of nitrogen. The silver, before weighing, was heated to redness. The phosphorus equivalent to 100 parts of silver is given in the third column:

.9096 grm. P	gave 15.8865 Ag.	5.7256
.5832 "	10.1622 "	5.7389

Mean, 5.7322, ± .0045

Hence P=30.920.

The second method consisted in the analysis of silver phosphate; but the process is not given. Van der Plaats states that it is difficult to be sure of the purity of this salt.

6.6300 grm. Ag_3PO_4	gave 5.1250 Ag.	77.300 per cent.
12.7170 "	9.8335 "	77.326 "

Mean, 77.313, ± .0088

Hence P=30.970.

In the third set of determinations, yellow phosphorus was oxidized by oxygen at reduced pressure, and the resulting P_2O_5 was weighed:

10.8230 grm. P	gave 24.7925 P_2O_5.	Ratio, 2.29072
7.7624 "	17.7915 "	" 2.29201

Hence P=30.975.

As these figures fall within the range of Schrötter's, they may be averaged in with his series, the entire set of twelve determinations giving a mean of 2.28955, ± .00032.

Ter Gazarian[2] determined the density of gaseous phosphine, from which its molecular weight is deducible. For the weight of the normal litre, in grammes, he found—

1.52955
1.52907
1.52933
1.52944
1.52907
1.52933

Mean, 1.52930, ± .000054

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[1] Compt. Rend., 100, 52. 1885.

[2] Journ. Chim. Phys., 7, 337. 1909.

From these figures, reduced by means of the critical constants, Ter Gazarian finds $PH_3=33.931$. The probable error is 0.0012. Hence P=30.908.

Baxter and Jones[1] based their determinations of the atomic weight of phosphorus upon analyses of silver phosphate. This salt, was dissolved, and the silver precipitated and weighed as bromide, and in one experiment as chloride.

The weights, in vacuo, and the ratios are as follows:

Ag_3PO_4.	$AgBr$.	*Ratio.*
6.20166	8.34490	134.558
6.35722	8.55419	134.559
5.80244	7.80819	134.567
5.05845	6.80685	134.564
7.15386	9.62694	134.570
7.20085	9.68947	134.560
6.20182	8.34522	134.561
5.20683	7.00605	134.555
		Mean, 134.562, ± .0012

Hence P=31.051.

Ag_3PO_4.	$AgCl$.	*Ratio.*
3.34498	3.43544	102.704, ± .0034

Hence P=31.054.

The probable error assigned to the last ratio is that of one experiment in the bromide series.

From the following ratios the atomic weight of phosphorus is now to be computed.

(1). $2P:P_2O_5::1.0:2.28955$, ± .00032
(2). $3Ag:PCl_3::100:42.4553$, ± .0036
(3). $5Ag:P::100:5.7322$, ± .0045
(4). $Ag_3PO_4:3Ag::100:77.313$, ± .0088
(5). $Ag_3PO_4:3AgCl::100:102.704$, ± .0034
(6). $Ag_3PO_4:3AgBr::100:134.562$, ± .0012
(7). $PH_3=33.931$, ± .0012

To reduce these we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
H = 1.00779, ± .00001

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[1] Journ. Amer. Chem. Soc., 32, 298. 1910.

Hence,

From ratio 7	$P = 30.908$, ± .0012
" " 3	30.920, ± .0243
" " 4	30.970, ± .0384
" " 1	31.018, ± .0077
" " 2	31.027, ± .0117
" " 6	31.051, ± .0039
" " 5	31.054, ± .0148
General mean,	$P = 30.925$, ± .0011

Ratio 7 is here evidently overweighted to an enormous extent. It outweighs all the others collectively, which is a highly improbable condition. If we reject it altogether, the remaining six ratios give a general mean of $P = 31.041, \pm .0032$, which appears to be more probable than the much lower value given above. The arithmetic average of the seven values is $P = 30.993$. The true value is probably near 31, as is commonly assumed.

VANADIUM.

Roscoe's determination of the atomic weight of vanadium was the first to have any scientific value. The results obtained by Berzelius[1] and by Czudnowicz[2] were unquestionably too high, the error being probably due to the presence of phosphoric acid in the vanadic acid employed. This particular impurity, as Roscoe has shown, prevents the complete reduction of V_2O_5 to V_2O_3 by means of hydrogen. All vanadium ores contain small quantities of phosphorus, which can only be detected with ammonium molybdate—a reaction unknown in Berzelius' time. Furthermore, the complete purification of vanadic acid from all traces of phosphoric acid is a matter of great difficulty, and probably never was accomplished until Roscoe undertook his researches.

In his determination of the atomic weight, Roscoe[3] studied two compounds of vanadium, namely, the pentoxide, V_2O_5, and the oxychloride, $VOCl_3$. The pentoxide, absolutely pure, was reduced to V_2O_3 by heating in hydrogen, with the following results:

7.7397 grm. V_2O_5	gave	6.3827 grm. V_2O_3.	17.533 per cent. loss.	
6.5819	"	5.4296 "	17.507	"
5.1895	"	4.2819 "	17.489	"
5.0450	"	4.1614 "	17.515	"
5.4296 grm. V_2O_3, reoxidized,	gave	6.5814 grm. V_2O_5.	17.501 per cent. difference.	
		Mean,	17.509, ± .005	

Hence $V = 51.381, \pm .0220$.

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[1] Poggend. Annal., 22, 14. 1831.

[2] Poggend. Annal., 120, 17. 1863.

[3] Journ. Chem. Soc., 6, pp. 330 and 344. 1868.

Upon the oxychloride, VOCl$_3$, two series of experiments were made—one volumetric, the other gravimetric. In the volumetric series the compound was titrated with solutions containing known weights of silver, which had been purified according to the methods recommended by Stas. Roscoe publishes his weighings, and gives percentages deduced from them; his figures, reduced to a common standard, make the quantities of VOCl$_3$ given in the third column proportional to 100 parts of silver. He was assisted by two analysts:

Analyst A.

2.4322	grm. VOCl$_3$ =	4.5525	grm. Ag.	53.425
4.6840	"	8.7505	"	53.528
4.2188	"	7.8807	"	53.533
3.9490	"	7.3799	"	53.510
.9243	"	1.7267	"	53.530
1.4330	"	2.6769	"	53.532

Analyst B.

2.8530	grm. VOCl$_3$ =	5.2853	grm. Ag.	53.980
2.1252	"	3.9535	"	53.755
1.4248	"	2.6642	"	53.479

Mean, 53.586, ± .039

The gravimetric series, of course, fixes the ratio between VOCl$_3$ and AgCl. If we put the latter at 100 parts, the proportion of VOCl$_3$ is as given in the third column:

Analyst A.

1.8521	grm. VOCl$_3$ gave	4.5932	grm. AgCl.	40.323
.7013	"	1.7303	"	40.531
.7486	"	1.8467	"	40.537
1.4408	"	3.5719	"	40.337
.9453	"	2.3399	"	40.399
1.6183	"	4.0282	"	40.174

Analyst B.

2.1936	grm. VOCl$_3$ gave	5.4039	grm. AgCl.	40.391
2.5054	"	6.2118	"	40.333

Mean, 40.378, ± .028

These two series give us two values for the molecular weight of VOCl$_3$:

From volumetric series...... VOCl$_3$ = 173.426, ± .1262
From gravimetric series..... " = 173.631, ± .1204
General mean .... VOCl$_3$ = 173.532, ± .0871

Hence V = 51.157, ± .0872, when Ag = 107.880 and Cl = 35.4584.

From the oxide, V = 51.381, ± .0220. The two values combined give V = 51.367, ± .0214.

Addendum. Since the manuscript of the volume went to the printer the determinations made by Prandtl and Bleyer[1] have been published. They made two series of analyses of vanadium oxychloride, as was done gravimetrically by Roscoe. The data, with vacuum weights, are as follows:

I.

$VOCl_3$.	*AgCl.*	*Ratio.*
5.47218	13.54724	40.393
5.85234	14.50771	40.346
3.23175	8.00636	40.365
5.24732	13.01359	40.322
3.56589	8.83375	40.367
		Mean, 40.359, ± .0080

Hence V = 51.175.

II.

$VOCl_3$.	*AgCl.*	*Ratio.*
4.91432	12.18494	40.331
3.64470	9.04685	40.286
4.96088	12.30438	40.318
6.46766	16.04232	40.315
4.33158	10.74624	40.308
4.05060	10.04498	40.325
		Mean, 40.314, ± .0043

Hence V = 50.977.

These series, combined with Roscoe's similar series, give a general mean of $3AgCl : VOCl_3 :: 100 : 40.3245, \pm .0037$. Hence V = 51.027, ± .0160. Combining this with the value from Roscoe's oxide series, the final, general mean becomes V = 51.037, ± .0036.

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[1] Zeitsch. anorg. Chem., 65, 152. 1909.

ARSENIC.

For the determination of the atomic weight of arsenic five compounds have been studied—the chloride, the trioxide and three arsenates. The bromide may also be considered, since it was analyzed by Wallace in order to establish the atomic weight of bromine. His series, in the light of more recent knowledge, may properly be inverted, and applied to the determination of arsenic.

In 1826 Berzelius[1] heated arsenic trioxide with sulphur in such a way that only SO_2 could escape. 2.203 grammes of As_2O_3, thus treated, gave a loss of 1.069 of SO_2. Hence As = 75.02.

In 1845 Pelouze[2] applied his method of titration with known quantities of pure silver to the analysis of the trichloride of arsenic, $AsCl_3$. Using the old Berzelian atomic weights, and putting Ag = 1349.01 and Cl = 443.2, he found in three experiments for As the values 937.9, 937.1, and 937.4. Hence 100 parts of silver balance the following quantities of $AsCl_3$:

56.029
56.009
56.016

Mean, 56.018, ± .004

Hence As = 74.92.

Later, the same method was employed by Dumas,[3] whose weighings, reduced to the foregoing standard, give the following results:

4.298	grm. $AsCl_3$ =	7.673	grm. Ag.	Ratio,	56.015
5.535	"	9.880	"	"	56.022
7.660	"	13.686	"	"	55.970
4.680	"	8.358	"	"	55.993

Mean, 56.000, ± .008

Hence As = 74.86.

The two series of Pelouze and Dumas, combined, give a general mean of 56.014, ± .0035, as the amount of $AsCl_3$ equivalent to 100 parts of silver. Hence As = 74.91, a value closely agreeing with that deduced from the single experiment of Berzelius.

The same process of titration with silver was applied by Wallace[4] to the analysis of arsenic tribromide, $AsBr_3$. This compound was repeatedly distilled to ensure purity, and was well crystallized. His weighings

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[1] Poggend. Annalen, 8, 1.

[2] Compt. Rend., 20, 1047.

[3] Ann. Chim. Phys. (3), 55, 174. 1859.

[4] Phil. Mag. (4), 18, 270.

show that the quantities of bromide given in the third column are proportional to 100 parts of silver:

8.3246 grm. $AsBr_3$ =	8.58 grm. Ag.	97.023
4.4368 "	4.573 "	97.022
5.098 "	5.257 "	96.970
		Mean, 97.005, ± .012

Hence As = 74.19. Why this value should be so much lower than that from the chloride is unexplained.

The volumetric work done by Kessler,[1] for the purpose of establishing the atomic weights of chromium and of arsenic, is described in the chromium chapter. In that investigation the amount of potassium dichromate required to oxidize 100 parts of As_2O_3 to As_2O_5 was determined and compared with the quantity of potassium chlorate necessary to produce the same effect. From the molecular weight of $KClO_3$, that of $K_2Cr_2O_7$ was then calculable.

From the same figures, the molecular weights of $KClO_3$ and of $K_2Cr_2O_7$ being both known, that of As_2O_3 may be easily determined. The quantities of the other compounds proportional to 100 parts of As_2O_3 are as follows:

$K_2Cr_2O_7$.	$KClO_3$.
98.95	41.156
98.94	41.116
99.17	41.200
98.98	41.255
99.08	41.201
99.15	41.086
	41.199
Mean, 99.045, ± .028	41.224
	41.161
	41.193
	41.149
	41.126
	Mean, 41.172, ± .009

Another series with the dichromate gave the following figures:

99.08
99.06
99.10
98.97
98.97

Mean, 99.036, ± .019
Previous series, 99.045, ± .028

General mean, 99.039, ± .016

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[1] Poggend. Annal., 95, 204. 1855. Also 113, 134. 1861.

Other defective series are given to illustrate the partial oxidation of the As_2O_3 by the action of the air. From Kessler's data we get two values for the atomic weight of As, thus:

From $KClO_3$ series.............. As = 75.225
From $K_2Cr_2O_7$ series............. " = 75.032

The determinations made by Hibbs[1] are based upon an altogether different process from any of the preceding measurements. Sodium pyroarsenate was heated in gaseous hydrochloric acid, yielding sodium chloride. The latter was perfectly white, completely soluble in water, unfused, and absolutely free from arsenic. The vacuum weights are subjoined, with a column giving the percentage of chloride obtained from the pyroarsenate:

$Na_4As_2O_7$.	$NaCl$.	*Percentage.*
.02177	.01439	66.100
.04713	.03115	66.094
.05795	.03830	66.091
.40801	.26981	66.128
.50466	.33345	66.092
.77538	.51249	66.095
.82897	.54791	66.095
1.19124	.78731	66.092
1.67545	1.10732	66.091
3.22637	2.13267	66.101
		Mean, 66.098, ± .0030

Hence As = 74.895.

The determinations by Ebaugh[2] are analogous to those of Hibbs. First, silver arsenate was converted into silver chloride by heating in gaseous hydrochloric acid, and the chloride was afterwards reduced to metal in a stream of hydrogen. The data obtained are as follows:

Ag_3AsO_4.	$AgCl$.	Ag.	*Per cent.* $AgCl$.	*Per cent.* Ag.
.23182	.21547	.162175	92.947	69.957
.47996	.44615	.33583	92.956	69.970
.52521	.48820	.367525	92.953	69.977
.80173	.74517	.56099	92.945	69.972
.94782	.88083	.66318	92.932	69.969
1.02047	.94836	.71400	92.928	69.968
1.03558	.96258	......	92.951	.....
1.05462	.98014	.73771	92.938	69.950
			Mean, 92.944,	69.966,
			± .0025	± .0024

From Ag series, As = 74.928.

From AgCl series, As = 75.000.

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[1] Doctoral thesis, University of Pennsylvania, 1896. Work done under the direction of Professor E. F. Smith. In the fifth experiment the weight of NaCl is printed .33045. This is evidently a misprint, which I have corrected by comparison with the other data. The rejection of this experiment would not affect the final result appreciably.

[2] Doctoral thesis, University of Pennsylvania, 1901.

A similar series of experiments with lead arsenate gave the subjoined figures:

$Pb_3(AsO_4)_2$.	$PbCl_2$.	Per cent. $PbCl_2$.
.38152	.35381	92.737
.436197	.40449	92.731
.57218	.53065	92.742
.60085	.55717	92.730
.74123	.68736	92.732
.77107	.71494	92.721
.88282	.81858	92.723
.97779	.90674	92.734

Mean, 92.731, ± .0019

Hence As = 75.05.

Lead arsenate was also transformed into lead bromide, by heating in a stream of hydrobromic acid:

$Pb_3(AsO_4)_2$.	$PbBr_2$.	Per cent. $PbBr_2$.
.59704	.73092	122.424
.61712	.75567	122.451
.65799	.80569	122.447

Mean, 122.441, ± .0076

Hence As = 74.916.

All of Ebaugh's weights are reduced to a vacuum.

Silver arsenate was also chosen by Baxter and Coffin[1] for their determinations of the atomic weight of arsenic. In some experiments Ebaugh's method of heating in gaseous hydrochloric acid was adopted; in others the arsenate was dissolved in nitric acid, and the silver then precipitated as chloride or bromide. Corrections were applied, not only for weighing in air, but also for traces of moisture in the initial substance. Different samples of the arsenate were prepared, which gave slightly varying results for the atomic weight, and the determinations, for that reason, fall into two groups. In series 1 and 2, which may be treated as one here, the first five determinations were made by Ebaugh's method, and the last two by solution and precipitation. The figures thus obtained are as follows:

Ag_3AsO_4.	$AgCl$.	Ratio.
3.17276	2.94922	92.9544
2.65042	2.46367	92.9539
3.51128	3.26396	92.9564
5.83614	5.42503	92.9558
5.72252	5.31947	92.9568
4.59149	4.26796	92.9537
3.38270	3.14436	92.9542

Mean, 92.9550, ± .00036

Hence As = 74.956.

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[1] Journ. Amer. Chem. Soc., 31, 297. 1909.

Series 4 and 5, with silver arsenate of different origin from that previously used, gave the subjoined figures. Only the last experiment was conducted by the precipitation method:

Ag_3AsO_4.	*AgCl.*	*Ratio.*
4.67268	4.34389	92.9636
7.71882	7.17597	92.9672
5.28049	4.90908	92.9664
4.25346	3.95424	92.9652
3.47340	3.22893	92.9616
5.17269	4.80879	92.9650
4.10766	3.81858	92.9624
5.47133	5.08643	92.9646

Mean, 92.9681, ± .00044

Hence As = 74.901.

These series combine with Ebaugh's thus:

Ebaugh	92.944, ± .0025
Baxter and Coffin, 1	92.9550, ± .00036
Baxter and Coffin, 2	92.9681, ± .00044
General mean	92.9614, ± .00028

Baxter and Coffin also determined the ratio between silver arsenate and silver bromide by the solution and precipitation method. Here again two series of analyses are given, numbered 3 and 6, representing different preparations of the arsenate. The two series are as follows:

Series 3.

Ag_3AsO_4.	*AgBr.*	*Ratio.*
8.75751	10.66553	121.787
6.76988	8.24545	121.796
5.19424	6.32590	121.787
5.33914	6.50258	121.791
8.24954	10.03552	121.782
7.57962	9.23147	121.793
6.05230	7.37106	121.789

Mean, 121.789, ± .0016

Series 6.

Ag_3AsO_4.	*AgBr.*	*Ratio.*
4.96261	6.04440	121.7988
5.31743	6.47658	121.7991
4.46882	5.44300	121.7995
4.16702	5.07539	121.7990

Mean, 121.7991, ± .00015

In the last mean the probable error is so low as to give it inordinate weight, especially as Baxter and Coffin suspect the presence of basic impurities in the arsenate. It is better, therefore, to treat both series as one, giving in mean $Ag_3AsO_4 : 3AgBr :: 100 : 121.793$, ± .0012. Hence As = 74.947.

There are now the following ratios from which to compute the atomic weight of arsenic. The single determination by Berzelius has been arbitrarily assigned equal weight with that of Wallace's series:

(1). $2As_2O_3 : 3SO_2 :: 100 : 48.525$, ± .012
(2). $3Ag : AsCl_3 :: 100 : 56.014$, ± .0035
(3). $3Ag : AsBr_3 :: 100 : 97.005$, ± .012
(4). $3As_2O_3 : 2K_2Cr_2O_7 :: 100 : 99.039$, ± .016
(5). $3As_2O_3 : 2KClO_3 :: 100 : 41.172$, ± .009
(6). $Na_4As_2O_7 : 4NaCl :: 100 : 66.098$, ± .0030
(7). $Ag_3AsO_4 : 3Ag :: 100 : 69.966$, ± .0024
(8). $Ag_3AsO_4 : 3AgCl :: 100 : 92.9614$, ± .00028
(9). $Ag_3AsO_4 : 3AgBr :: 100 : 121.793$, ± .0012
(10). $Pb_3As_2O_8 : 3PbCl_2 :: 100 : 92.731$, ± .0019
(11). $Pb_3As_2O_8 : 3PbBr_2 :: 100 : 122.441$, ± .0076

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
Na = 23.0108, ± .00024
K = 39.0999, ± .0002
Cr = 52.0193, ± .0013
Pb = 206.970, ± .0017

Hence,

From ratio 3 .... As = 74.188, ± .0389
" " 6 .... 74.895, ± .0066
" " 11 .... 74.916, ± .0286
" " 7 .... 74.928, ± .0160
" " 8 .... 74.934, ± .0018
" " 9 .... 74.947, ± .0049
" " 2 .... 75.008, ± .0108
" " 1 .... 75.021, ± .0245
" " 4 .... 75.032, ± .0160
" " 10 .... 75.050, ± .0099
" " 5 .... 75.225, ± .0217

General mean, As = 74.957, ± .0016

This final mean is identical with the value found by Baxter and Coffin as the result of their determinations.

ANTIMONY.

After some earlier, unsatisfactory determinations, Berzelius,[1] in 1826, published his final estimation of the atomic weight of antimony. He oxidized the metal by means of nitric acid, and found that 100 parts of antimony gave 124.8 of Sb_2O_4. Hence Sb = 129.03. The value 129 remained in general acceptance until 1855, when Kessler,[2] by special volumetric methods, showed that it was certainly much too high. Kessler's results will be considered more fully further along, in connection with a later paper; for present purposes a brief statement of his earlier conclusions will suffice. Antimony and various compounds of antimony were oxidized partly by potassium dichromate and partly by potassium chlorate, and from the amounts of oxidizing agent required the atomic weight in question was deduced:

By oxidation of Sb_2O_3 from 100 parts of Sb........	Sb = 123.84
By oxidation of Sb with $K_2Cr_2O_7$.................	" = 123.61
By oxidation of Sb with KClO + $K_2Cr_2O_7$.........	" = 123.72
By oxidation of Sb_2O_3 with $KClO_3 + K_2Cr_2O_7$......	" = 123.80
By oxidation of Sb_2S_3 with $K_2Cr_2O_7$................	" = 123.58
By oxidation of tartar emetic....................	" = 119.80

The figures given are those calculated by Kessler himself. A recalculation with our newer atomic weights for O, K, Cl, Cr, S and C would yield slightly different values. It will be seen that five of the estimates agree closely, while one diverges widely from the others. It will be shown hereafter that the concordant values are all vitiated by constant errors, and that the exceptional figure is also worthless.

Shortly after the appearance of Kessler's first paper, Schneider[3] published some results obtained by the reduction of antimony sulphide in hydrogen. The material chosen was a very pure stibnite from Arnsberg, of which the gangue was only quartz. This was corrected for, and corrections were also applied for traces of undecomposed sulphide carried off mechanically by the gas stream, and for traces of sulphur retained by the reduced antimony. The latter sulphur was estimated as barium sulphate. From 3.2 to 10.6 grammes of material were taken in each experiment. The final corrected percentages of S in Sb_2S_3 were as follows:

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[1] Poggend. Annalen, 8, 1.

[2] Poggend. Annalen, 95, 215.

[3] Poggend. Annalen, 98, 293. 1856. Preliminary note in Bd. 97.

28.559
28.557
28.501
28.554
28.532
28.485
28.492
28.481

Mean, 28.520, ± .008

Hence Sb=120.55.

Immediately after the appearance of Schneider's memoir, Rose[1] published the result of a single analysis of antimony trichloride, previously made under his supervision by Weber. This analysis, if Cl=35.5, makes Sb=120.7, a value of no great weight, but in a measure confirmatory of that obtained by Schneider.

The next research upon the atomic weight of antimony was that of Dexter,[2] published in 1857. This chemist, having tried to determine the amount of gold precipitable by a known weight of antimony, and having obtained discordant results, finally resorted to the original method of Berzelius. Antimony, purified with extreme care, was oxidized by nitric acid, and the gain in weight was determined. From 1.5 to 3.3 grammes of metal were used in each experiment. The reduction of the weights to a vacuum standard was neglected as being superfluous. From the data obtained, we get the following percentages of Sb in Sb_2O_4:

79.268
79.272
79.255
79.266
79.253
79.271
79.264
79.260
79.286
79.274
79.232
79.395
79.379

Mean, 79.283, ± .009

Hence Sb=122.46.

The determinations of Dumas[3] were published in 1859. This chemist

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[1] Poggend. Annalen, 98, 455. 1856.
[2] Poggend. Annalen, 100, 363. 1857.
[3] Ann. Chim. Phys. (3), 55, 175.

sought to fix the ratio between silver and antimonious chloride, and obtained results for the atomic weight of antimony quite near to those of Dexter. The $SbCl_3$ was prepared by the action of dry chlorine upon pure antimony; it is was distilled several times over antimony powder, and it seemed to be perfectly pure. Known weights of this preparation were added to solutions of tartaric acid in water, and the silver chloride was precipitated without previous removal of the antimony. Here, as Cooke has since shown, is a possible source of error, for under such circumstances the crystalline argento-antimonious tartrate may also be thrown down and contaminate the chloride of silver. But be that as it may, Dumas' weighings, reduced to a common standard, give as proportional to 100 parts of silver, the quantities of $SbCl_3$ which are stated in the third of the subjoined columns:

1.876 grm. $SbCl_3$	= 2.660 grm. Ag.	70.526
4.336 "	6.148 "	70.527
5.065 "	7.175 "	70.592
3.475 "	4.930 "	70.487
3.767 "	5.350 "	70.411
5.910 "	8.393 "	70.416
4.828 "	6.836 "	70.626
	Mean,	70.512, ± .021

Hence Sb = 121.83.

In 1861 Kessler's second paper[1] relative to the atomic weight of antimony appeared. Kessler's methods were somewhat complicated, and for full details the original memoirs must be consulted. A standard solution of potassium dichromate was prepared, containing 6.1466 grammes to the litre. With this, solutions containing known quantities of antimony or of antimony compounds were titrated, the end reaction being adjusted with a standard solution of ferrous chloride. In some cases the titration was preceded by the addition of a definite weight of potassium chlorate, insufficient for complete oxidation; the dichromate then served to finish the reaction. The object in view was to determine the amount of oxidizing agent, and therefore of oxygen, necessary for the conversion of known quantities of antimonious into antimonic compounds.

In the later paper Kessler refers to his earlier work, and shows that the values then found for antimony were all too high, except in the case of the series made with tartar emetic. That series he merely states, and subsequently ignores, evidently believing it to be unworthy of further consideration. For the remaining series he points out the sources of

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[1] Poggend. Annalen, 113, 145. 1861.

error. These need not be rediscussed here, as the discussion would have no value for present purposes; suffice it to say that in the series representing the oxidation of Sb_2O_3 with the dichromate and chlorate, the material used was found to be impure. Upon estimating the impurity and correcting for it, the earlier value of Sb = 123.80 becomes Sb = 122.36, according to Kessler's calculations.

In the paper now under consideration four series of results are given. The first represents experiments made upon a pure antimony trioxide which had been sublimed, and which consisted of shining colorless needles. This was dissolved, together with some potassium chlorate, in hydrochloric acid, and titrated with dichromate solution. Six experiments were made, but Kessler rejects the first and second as untrustworthy. The data for the others are as follows:

Sb_2O_3.	$KClO_3$.	$K_2Cr_2O_7$ *sol. in cc.*
1.7888 grm.	.4527 grm.	19.2 cc.
1.6523 "	.4506 "	3.9 "
3.2998 "	.8806 "	16.5 "
1.3438 "	.3492 "	10.2 "

From these figures Kessler deduces Sb = 122.16.

These data, reduced to a common standard, give the following quantities of oxygen needed to oxidize 100 parts of Sb_2O_3 to Sb_2O_5. Each cubic centimetre of the $K_2Cr_2O_7$ solution corresponds to one milligramme of O:

10.985
10.939
10.951
10.936

Mean, 10.953, ± .0075

Hence Sb = 122.08.

In the second series of experiments pure antimony was dissolved in hydrochloric acid with the aid of an unweighed quantity of potassium chlorate. The solution, containing both antimonious and antimonic compounds, was then reduced entirely to the antimonious condition by means of stannous chloride. The excess of the latter was corrected with a strong hydrochloric acid solution of mercuric chloride, then, after diluting and filtering, a weighed quantity of potassium chlorate was added, and the titration with dichromate was performed as usual. Calculated as above, the percentages of oxygen given in the last column correspond to 100 parts of antimony:

Sb.	*$KClO_3$.*	*$K_2Cr_2O_7$ sol. cc.*	*Per cent. O.*
1.636 grm.	.5000 grm.	18.3	13.088
3.0825 “	.9500 “	30.2	13.050
4.5652 “	1.4106 “	45.5	13.098

Mean, 13.079, ± .0096

Hence Sb = 122.33.

The third and fourth series of experiments were made with pure antimony trichloride, $SbCl_3$, prepared by the action of mercuric chloride upon metallic antimony. This preparation, in the third series, was dissolved in hydrochloric acid, and titrated. In one experiment solid $K_2Cr_2O_7$ in weighed amount was added before titration; in the other two estimations $KClO_3$ was taken as usual. The third column gives the percentages of oxygen corresponding to 100 parts of $SbCl_3$:

				Per cent. O.
1.8576 grm. $SbCl_3$ needed	.5967 grm.	$K_2Cr_2O_7$ and	33.4 cc. sol.	7.0338
1.9118 “	.3019 “	$KClO_3$ “	16.2 “	7.0321
4.1235 “	.6801 “	$KClO_3$ “	23.2 “	7.0222

Mean, 7.0294, ± .0024

Hence Sb = 121.24.

The fourth set of experiments was gravimetric. The solution of $SbCl_3$, mixed with tartaric acid, was first precipitated by hydrogen sulphide, in order to remove the antimony. The excess of H_2S was corrected by copper sulphate, and then the chlorine was estimated as silver chloride in the ordinary manner. 100 parts of AgCl correspond to the amounts of $SbCl_3$ given in the third column:

1.8662 grm. $SbCl_3$ gave	3.483 grm. AgCl.	53.580
1.6832 “	3.141 “	53.588
2.7437 “	5.1115 “	53.677
2.6798 “	5.0025 “	53.569
5.047 “	9.411 “	53.629
3.8975 “	7.2585 “	53.696

Mean, 53.623, ± .015

The volumetric series with $SbCl_3$ gave Kessler values for Sb ranging from 121.16 to 121.47. The gravimetric series, on the other hand, yielded results from Sb = 124.12 to 124.67. This discrepancy Kessler rightly attributes to the presence of oxygen in the chloride; and, ingeniously correcting for this error, he deduces from both sets combined the value of Sb = 122.37.

The several mean results for antimony agree so fairly with each other, and with the estimates obtained by Dexter and Dumas, that we cannot wonder that Kessler felt satisfied of their general correctness, and of the inaccuracy of the figures published by Schneider. Still, the old series of data obtained by the titration of tartar emetic with dichromate contained no evident errors, and was not accounted for. This series,[1] if we reduce all of Kessler's figures to a single common standard, gives a ratio between $K_2Cr_2O_7$ and $C_4H_4KSbO_7.\frac{1}{2}H_2O$. 100 parts of the former will oxidize of the latter:

336.64
338.01
336.83
337.93
338.59
335.79

Mean, 337.30, ± .29

From this Sb=118.68.

The newer atomic weights found in other chapters of this work will be applied to the discussion of all these series further along. It may, however, be properly noted at this point that the probable errors assigned to the percentages of oxygen in three of Kessler's series are too low. These percentages are calculated from the quantities of $KClO_3$ involved in the several reactions, and their probable errors should be increased with reference to the probable error of the molecular weight of that salt. The necessary calculations would be more laborious than the importance of the figures would warrant, and accordingly, in computing the final general mean for antimony, Kessler's figures will receive somewhat higher weight than they are legitimately entitled to.

Naturally, the concordant results of Dexter, Kessler and Dumas led to the general acceptance of the value of 122 for antimony as against the lower figure, 120, of Schneider. Still, in 1871, Unger[2] published the results of a single analysis of Schlippe's salt, $Na_3SbS_4.9H_2O$. This analysis gave Sb=119.76, if S=32 and Na=23, but no great weight could be attached to the determination. It served, nevertheless, to show that the controversy over the atomic weight of antimony was not finally settled.

More than ten years after the appearance of Kessler's second paper the subject of the atomic weight of antimony was again taken up, this time by Professor Cooke. His results appeared in the autumn of 1877[3] and were conclusive in favor of the lower value, approximately 120. For full details the original memoir must be consulted; only a few of the leading points can be cited here.

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[1] Poggend. Annalen, 95, 217.

[2] Archiv der Pharmacie, 197, 194. Quoted by Cooke.

[3] Proc. Amer. Acad., 5, 13.

Schneider analyzed a sulphide of antimony which was already formed. Cooke, reversing the method, effected the synthesis of this compound. Known weights of pure antimony were dissolved in hydrochloric acid containing a little nitric acid. In this solution weighed balls of antimony were boiled until the liquid became colorless; subsequently the weight of metal lost by the balls was ascertained. To the solution, which now contained only antimonious compounds, tartaric acid was added, and then, with a supersaturated aqueous sulphhydric acid, antimony trisulphide was precipitated. The precipitate was collected by an ingenious process of reverse filtration, converted into the black modification by drying at 210°, and weighed. After weighing, the Sb_2S_3 was dissolved in hydrochloric acid, leaving a carbonaceous residue unacted upon. This was carefully estimated and corrected for. About two grammes of antimony were taken in each experiment and thirteen syntheses were performed. In two of these, however, the antimony trisulphide was weighed only in the red modification, and the results were uncorrected by conversion into the black variety and estimation of the carbonaceous residue. In fact, every such conversion and correction was preceded by a weighing of the red modification of the Sb_2S_3. The mean result of these weighings, if $S=32$, gave $Sb=119.994$. The mean result of the corrected syntheses gave $Sb=120.295$. In these eleven experiments the following percentages of S in Sb_2S_3 were established:

28.57
28.60
28.57
28.43
28.42
28.53
28.50
28.49
28.58
28.50
28.51

Mean, 28.5182, ± .0120

Hence $Sb=120.55$.

These results, confirmatory of the work of Schneider, were presented to the American Academy in 1876. Still, before publication, Cooke thought it best to repeat the work of Dumas, in order to detect the cause of the old discrepancy between the values $Sb=120$ and $Sb=122$. Accordingly, various samples of antimony trichloride were taken, and purified by repeated distillations. The final distillate was further subjected to several recrystallizations from the fused state; or, in one case, from a saturated solution in bisulphide of carbon. The portions analyzed were dissolved in concentrated aqueous tartaric acid, and precipitated by silver nitrate, many precautions being observed. The silver chloride was collected by reverse filtration, and dried at temperatures from 110° to 120°. In one experiment the antimony was first removed by H_2S. Seventeen experiments were made as follows. If we reduce to a common standard, Cooke's analyses give, as proportional to 100 parts of AgCl, the quantities of $SbCl_3$ stated in the third column:

1.5974	grm. $SbCl_3$ gave	3.0124	grm. AgCl.	53.028
1.2533	"	2.3620	"	53.061
.8876	"	1.6754	"	52.978
.8336	"	1.5674	"	53.184
.5326	"	1.0021	"	53.148
.7270	"	1.3691	"	53.101
1.2679	"	2.3883	"	53.088
1.9422	"	3.6646	"	52.999
1.7702	"	3.3384	"	53.025
2.5030	"	4.7184	"	53.048
2.1450	"	4.0410	"	53.081
1.7697	"	3.3281	"	53.175
2.3435	"	4.4157	"	53.072
1.3686	"	2.5813	"	53.020
1.8638	"	3.5146	"	53.030
2.0300	"	3.8282	"	53.028
2.4450	"	4.6086	"	53.053
			Mean,	53.066, ± .0096

Hence Sb = 121.82.

This mean may be combined with that of Kessler's series, as follows:

Kessler	53.623, ± .015
Cooke	53.066, ± .0096
General mean	53.2311, ± .008

The results thus obtained with $SbCl_3$ confirmed Dumas' determination of the atomic weight of antimony as remarkably as the syntheses of Sb_2S_3 had sustained the work of Schneider. Evidently, in one or the other series a constant error must be hidden, and much time was spent by Cooke in searching for it. It was eventually found that the chloride of antimony invariably contained traces of oxychloride, an impurity which tended to increase the apparent atomic weight of the metal under consideration. It was also found, in the course of the investigation, that hydrochloric acid solutions of antimonious compounds oxidize in the air during boiling as rapidly as ferrous compounds, a fact which explains the high values for antimony found by Kessler.[1]

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[1] In Amer. Journ. Sci. (3), 21, 220, Cooke pointed out the errors due to the solubility of silver chloride, and gave two series of analyses of $SbCl_3$ to illustrate their magnitude.

In order to render "assurance doubly sure," Professor Cooke also undertook the analysis of the bromide and the iodide of antimony. The bromide, $SbBr_3$, was prepared by adding the finely powdered metal to a solution of bromine in carbon disulphide. It was purified by repeated distillation over pulverized antimony, and by several recrystallizations from bisulphide of carbon. The bromine determinations resemble those of chlorine. Reduced to a common standard, the fifteen analyses give the subjoined quantities of $SbBr_3$ proportional to 100 parts of silver bromide:

1.8621	grm. $SbBr_3$ gave	2.9216	grm. AgBr.	63.736
.9856	"	1.5422	"	63.909
1.8650	"	2.9268	"	63.721
1.5330	"	2.4030	"	63.795
1.3689	"	2.1445	"	63.833
1.2124	"	1.8991	"	63.841
.9417	"	1.4749	"	63.848
2.5404	"	3.9755	"	63.901
1.5269	"	2.3905	"	63.874
1.8604	"	2.9180	"	63.756
1.7298	"	2.7083	"	63.870
3.2838	"	5.1398	"	63.890
2.3589	"	3.6959	"	63.825
1.3323	"	2.0863	"	63.859
2.6974	"	4.2285	"	63.791
			Mean,	63.830, ± .008

Hence Sb = 119.86.

The iodide of antimony was prepared like the bromide, and analyzed in the same way. At first, discordant results were obtained, due to the presence of oxyiodide in the iodide studied. The impurity, however, was removed by subliming the iodide in an atmosphere of dry carbon dioxide. With this purer material, seven estimations of iodine were made. Reduced to a uniform standard, Cooke's weighings give the following quantities of SbI_3 proportional to 100 parts of silver iodide:

1.1877	grm. SbI_3 gave	1.6727	grm. AgI.	71.005
.4610	"	.6497	"	70.956
3.2527	"	4.5716	"	71.150
1.8068	"	2.5389	"	71.165
1.5970	"	2.2456	"	71.117
2.3201	"	3.2645	"	71.071
.3496	"	.4927	"	70.956
			Mean,	71.060, ± .023

Hence Sb = 119.79.

Although Cooke's work was practically conclusive, as between the rival values for antimony, his results were severely criticised by Kessler,[1] who evidently had read Cooke's paper in a very careless way. On the other hand, Schneider published in Poggendorff's Annalen a friendly review of the new determinations, which so well vindicated his own accuracy. In reply to Kessler, Cooke undertook still another series of experiments with antimony bromide,[2] and obtained absolute confirmation of his previous results. To a solution of antimony bromide was added a solution containing a known weight of silver not quite sufficient to precipitate all the bromine. The excess of the latter was estimated by titration with a normal silver solution. Five analyses gave values for antimony ranging from 119.98 to 120.02, when Ag = 108 and Br = 80. Reduced to a common standard, the weights obtained gave the amounts of $SbBr_3$ stated in the third column as proportional to 100 parts of silver:

2.5032 grm. $SbBr_3$ =	2.2528 grm. Ag.	111.115
2.0567 "	1.8509 "	111.119
2.6512 "	2.3860 "	111.115
3.3053 "	2.9749 "	111.106
2.7495 "	2.4745 "	111.113
	Mean,	111.114, ± .0014

Hence Sb = 119.85.

Schneider,[3] also, in order to more fully answer Kessler's objections, repeated his work upon the Arnsberg stibnite. This he reduced in hydrogen as before, correcting scrupulously for impurities. The following percentages of sulphur were found:

28.546
28.534
28.542

Mean, 28.541, ± .0024

Hence Sb = 120.43.

These figures confirm his old results, and may be fairly combined with them and with the percentages found by Cooke, as follows:

Schneider, early series	28.520,	± .008
Schneider, late series	28.541,	± .0024
Cooke	28.5182,	± .0120
General mean	28.5385,	± .0023

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[1] Berichte Deutsch. chem. Gesell., 12, 1044. 1879.
[2] Amer. Journ. Sci., May, 1880. Berichte, 13, 951.
[3] Journ. prakt. Chem. (2), 22, 131.

In 1881 Pfeifer[1] determined electrolytically the direct ratios between silver and antimony, and copper and antimony. With copper the following data were obtained:

		$3Cu:2Sb::100:x$.
1.412 grm. Sb	= 1.1008 Cu.	128.270
1.902 "	1.4832 "	128.236
3.367 "	2.6249 "	128.272
		Mean, 128.259, ± .0077

Hence Sb = 122.27.

With silver he found—

		$3Ag:Sb::100:x$.
5.925 grm. Sb	= 15.774 Ag.	37.562
6.429 "	17.109 "	37.577
10.116 "	26.972 "	37.506
4.865 "	13.014 "	37.383
4.390 "	11.697 "	37.531
9.587 "	25.611 "	37.433
4.525 "	12.097 "	37.406
		Mean, 37.485, ± .0198

Hence Sb = 121.32.

The latter ratio was also determined by Popper,[2] several years afterwards. The two metals were precipitated simultaneously by the same current; and in some experiments two portions of antimony were thrown down against one of silver. These are indicated in the subjoined table by suitable bracketing, and the ratio is given in the third column:

Sb.	*Ag.*	*Ratio.*
1.4856	3.9655	37.463
1.4788		37.292
2.0120	5.3649	37.503
2.0074		37.417
3.8882	10.3740	37.480
3.8903		37.500
4.1893	11.1847	37.455
4.1885		37.447
4.2710	11.3868	37.507
4.2752		37.545
5.6860	15.1786	37.460
5.6901		37.487
4.4117	11.8014	37.383
4.9999	13.3965	37.322
5.2409	14.0679	37.250

Mean, 37.434, ± .0149
Pfeifer found, 37.485, ± .0198

General mean, 37.452, ± .0119

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[1] Ann. Chem. Pharm., 209, 161.

[2] Ann. Chem., 233, 153.

Popper's figures give in mean Sb=121.15.

The recent investigations by Cohen and Strengers[1] seem to prove that these electrolytic determinations are worthless. They effected the simultaneous precipitation of silver and antimony, using solutions of $SbCl_3$, and found that the apparent atomic weight of antimony increased with the concentration of the solutions. They give the results of 24 determinations, with full details, but only the end results need be cited here:

With 2.3 grm. $SbCl_3$ in 100 cc. of solution, Sb=120.84 to 120.87
" 83.3 " Sb=121.81 to 121.92

These values are calculated with old values for Cl and Ag, but they show the failure of the process to yield trustworthy figures. In any final discussion of the atomic weight of antimony, therefore, the work of Pfeifer and Popper must be disregarded.

The work done by Bongartz[2] in 1883 was quite different from any of the determinations which had preceded it. Carefully purified antimony was weighed as such, and then dissolved in a concentrated solution of potassium sulphide. From this, after strong dilution, antimony trisulphide was thrown down by means of dilute sulphuric acid. After thorough washing, this sulphide was oxidized by hydrogen peroxide, by Classen's method, and the sulphur in it was weighed as barium sulphate. The ratio measured, therefore, was $2Sb:3BaSO_4$, and the data were as follows. The $BaSO_4$ equivalent to 100 parts of Sb is the ratio stated:

Sb taken.	*$BaSO_4$ found.*	*Ratio.*
1.4921	4.3325	290.362
.6132	1.7807	290.394
.5388	1.5655	290.553
1.2118	3.5205	290.518
.9570	2.7800	290.491
.6487	1.8855	290.349
.7280	2.1100	289.835
.9535	2.7655	290.036
1.0275	2.9800	290.024
.9635	2.7980	290.399
.9255	2.6865	290.275
.7635	2.2175	290.438
		Mean, 290.306, ± .0436

Hence Sb=120.61.

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[1] Proc. Amsterdam Acad., Section of Sciences, 5 (2), 543. 1903. See also Cohen, Collins and Strengers, Zeitsch. phys. Chem., 50, 291.

[2] Ber. Deutsch. chem. Ges., 16, 1942. 1883.

Still another method of determination was adopted by Friend and Smith.[1] Potassium tartrylantimonite, $KSbC_4H_4O_7$, was heated in a stream of dry, gaseous hydrochloric acid, and so converted into potassium chloride. The results obtained, with vacuum weights, are subjoined:

$KSbC_4H_4O_7$.	KCl.	Per cent. KCl.
1.19481	.27539	23.049
1.57004	.36186	23.048
2.00912	.46307	23.048
2.04253	.47073	23.046
2.16646	.49935	23.049
2.25558	.51982	23.046
2.61255	.60215	23.048
2.95272	.68064	23.051

Mean, 23.048, ± .0006

Hence Sb = 120.345.

We have now before us the following ratios, good and bad, from which to calculate the atomic weight of antimony. The single analyses by Weber and Unger, being unimportant, are not included:

(1). Percentage of S in Sb_2S_3, 28.5385, ± .0023
(2). Percentage of Sb in Sb_2O_4, 79.283, ± .009
(3). O needed to oxidize 100 parts $SbCl_3$, 7.0294, ± .0024
(4). O needed to oxidize 100 parts Sb_2O_3, 10.953, ± .0075
(5). O needed to oxidize 100 parts Sb, 13.079, ± .0096
(6). $K_2Cr_2O_7$:tartar emetic::100:337.30, ± .29
(7). $3Ag:SbCl_3::100:70.512$, ± .021
(8). $3AgCl:SbCl_3::100:53.2311$, ± .008
(9). $3Ag:SbBr_3::100:111.114$, ± .0014
(10). $3AgBr:SbBr_3::100:63.830$, ± .008
(11). $3AgI:SbI_3::100:71.060$, ± .023
(12). $3Cu:2Sb::100:128.259$, ± .0077
(13). $3Ag:Sb::100:37.452$, ± .0119
(14). $2Sb:3BaSO_4::100:290.306$, ± .0436
(15). $KSbC_4H_4O_7:KCl::100:23.048$, ± .0006

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
I = 126.9204, ± .00033
S = 32.0667, ± .00075
C = 12.0038, ± .0002
K = 39.0999, ± .0002
Ba = 137.363, ± .0025
Cr = 52.0193, ± .0013
Cu = 63.555, ± .00063
H = 1.00779, ± .00001

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[1] Journ. Amer. Chem. Soc., 23, 502, 1901.

Hence,

From ratio 6	Sb = 118.678, ± .2844
“ “ 11	119.786, ± .1621
“ “ 9	119.850, ± .0047
“ “ 10	119.858, ± .0451
“ “ 15	120.345, ± .0086
“ “ 1	120.444, ± .0108
“ “ 14	120.612, ± .0182
“ “ 13	121.210, ± .0386
“ “ 3	121.240, ± .0777
“ “ 7	121.830, ± .0680
“ “ 4	122.078, ± .0100
“ “ 12	122.272, ± .0075
“ “ 5	122.333, ± .0898
“ “ 2	122.462, ± .0550
“ “ 8	122.527, ± .0345
General mean,	Sb = 120.684, ± .0031

This mean has obviously very little significance except in so far as it shows the relatively low weight attaching to the higher values. The latter, say all over 121, are almost certainly in error, and ought to be rejected. Taking only the seven lowest values, they give a general mean of Sb = 120.048, ± .0038. Even this figure, however, is not quite satisfactory, for the values derived from ratios 1 and 15, which seem to be good, are not adequately accounted for. It is highly desirable that more work should be done upon the atomic weight of antimony, by modern methods, and for the purpose, in part at least, of explaining some of the evident discrepancies which appear in the foregoing table.

BISMUTH.

Early in the last century the combining weight of bismuth was approximately fixed through the experiments of Lagerhjelm.[1] Effecting the direct union of bismuth and sulphur, he found that ten parts of the metal yield the following quantities of trisulphide:

12.2520
12.2065
12.2230
12.2465

Mean, 12.2320

Hence Bi = 215 in round numbers, a value now known to be much too high. Lagerhjelm also oxidized bismuth with nitric acid, and, after ignition, weighed the trioxide thus formed. Ten parts of metal gave the following quantities of Bi_2O_3:

11.1382
11.1275

Mean, 11.13285

Hence Bi = 211.85, a figure still too high.

In 1851 the subject of the atomic weight of bismuth was taken up by Schneider,[2] who, like Lagerhjelm, studied the oxidation of the metal with nitric acid. The work was executed with a variety of experimental refinements, by means of which every error due to possible loss of material was carefully avoided. For full details the original paper must be consulted; there is only room in these pages for the actual results, as follows. The figures represent the percentages of Bi in Bi_2O_3:

89.652
89.682
89.644
89.634
89.656
89.666
89.655
89.653

Mean, 89.6552, ± .0034

Hence Bi = 208.05.

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[1] Annals of Philosophy, 4, 358. 1814. Adopted by Berzelius.

[2] Poggend. Annalen, 82, 303. 1851.

Next in order are the results obtained by Dumas.[1] Bismuth trichloride was prepared by the action of dry chlorine upon bismuth, and repeatedly rectified by distillation over bismuth powder. The product was weighed in a closed tube, dissolved in water, and precipitated with sodium carbonate. In the filtrate, after strongly acidulating with nitric acid, the chlorine was precipitated by a known amount of silver. The figures in the third column show the quantities of $BiCl_3$ proportional to 100 parts of silver:

3.506	grm. $BiCl_3$ =	3.545	grm. Ag.	98.900
1.149	"	1.168	"	98.373
1.5965	"	1.629	"	98.005
2.1767	"	2.225	"	97.829
3.081	"	3.144	"	97.996
2.4158	"	2.470	"	97.806
1.7107	"	1.752	"	97.643
3.523	"	3.6055	"	97.712
5.241	"	5.361	"	97.762
				Mean, 98.003, ± .090

Hence, with Ag = 108 and Cl = 35.5, Bi = 211.03.

The first three of the foregoing experiments were made with slightly discolored material. The remaining six percentages give a mean of 97.791, whence, on the same basis as before, Bi = 110.79. Evidently these results are now of slight value, for it is probable that the chloride of bismuth, like the corresponding antimony compound, contained traces of oxychloride. This assumption fully accounts for the discordance between Dumas' determination and the determinations of Schneider and still more recent investigators.

In 1883 Marignac[2] took up the subject, attacking the problem by two methods. His point of departure was commercial subnitrate of bismuth, which was purified by re-solution and reprecipitation, and from which he prepared the oxide. First, bismuth trioxide was reduced by heating in hydrogen, beginning with a moderate temperature and closing the operation at redness. The results were as follows, with the percentage of Bi in Bi_2O_3 added:

2.6460	grm. Bi_2O_3 lost	.2730	grm. O.	89.683	per cent.
6.7057	"	.6910	"	89.696	"
3.6649	"	.3782	"	89.681	"
5.8024	"	.5981	"	89.692	"
5.1205	"	.5295	"	89.658	"
5.5640	"	.5742	"	89.680	"
				Mean, 89.682, ± .0036	

Hence Bi = 208.60.

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[1] Ann. Chim. Phys. (3), 55, 176. 1859.

[2] Arch. Sci. Phys. Nat. (3), 10, 10. Oeuvres Complètes, 2, 717.

Marignac's second method of determination was by conversion of the oxide into the sulphate. The oxide was dissolved in nitric acid, and then sulphuric acid was added in slight excess from a graduated tube. The mass was evaporated to dryness with great care, and finally heated over a direct flame until fumes of SO_3 no longer appeared. The third column gives the sulphate formed from 100 parts of oxide:

2.6503 Bi_2O_3	gave	4.0218 $Bi_2(SO_4)_3$.	Ratio,	151.749
2.8025	"	4.2535 "	"	151.775
2.710	"	4.112 "	"	151.734
2.813	"	4.267 "	"	151.688
2.8750	"	4.3625 "	"	151.739
2.7942	"	4.2383 "	"	151.682
			Mean,	151.728, ± .0099

Hence Bi = 208.16.

This result needs to be studied in the light of Bailey's observation,[1] that bismuth sulphate has a very narrow range of stability. It loses the last traces of free sulphuric acid at 405°, and begins to decompose at 418°, so that the foregoing ratio is evidently uncertain. The concordance of the data, however, is favorable to it.

Two analyses of bismuth sulphate, rather vaguely stated, are given by Bailey. The weights found, and the ratio derived from them are as follows:

$Bi_2(SO_4)_3$.	*Bi_2O_3.*	*Ratio.*
2.2155	1.4615	151.591
1.5635	1.0267	152.284
		Mean, 151.937, ± .231

Hence Bi = 207.25. Combined with Marignac's series, the general mean becomes 151.729, ± .0099. Bailey's figures practically disappear.

The next determination of this atomic weight was by Löwe,[2] who oxidized the metal with nitric acid, and reduced the nitrate to oxide by ignition. Special care was taken to prepare bismuth free from arsenic, and the oxide was fused before weighing. In the paper just quoted Bailey calls attention to the volatility of bismuth oxide, which doubtless accounts for the low results found in this investigation. The data are as follows:

Bi taken.	*Bi_2O_3 found.*	*Per cent. Bi.*
11.309	12.616	89.640
12.2776	13.694	89.656
		Mean, 89.648, ± .0040

Hence Bi = 207.84.

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[1] Journ. Chem. Soc., 51, 676. 1887. Bailey deduces from his analyses Bi = 208.33 and 208.43. There may be some error in his printed figures, for his deductions do not agree with the data as given.

[2] Zeit. anal. Chem., 22, 498.

In Classen's[1] work upon the atomic weight of bismuth, the metal itself was first carefully investigated. Commercial samples, even those which purported to be pure, were found to be contaminated with lead and other impurities, and these were not entirely removable by many successive precipitations as subnitrate. Finally, pure bismuth was obtained by an electrolytic process, and this was converted into oxide by means of nitric acid and subsequent ignition to incipient fusion. Results as follows, with the percentage of Bi in Bi_2O_3 added:

Bi taken.	*Bi_2O_3 found.*	*Per cent. Bi.*
25.0667	27.9442	89.703
21.0691	23.4875	89.7035
27.2596	30.3922	89.693
36.5195	40.7131	89.700
27.9214	31.1295	89.6944
32.1188	35.8103	89.692
30.1000	33.5587	89.694
26.4825	59.5257	89.693
19.8008	22.0758	89.695

Mean, 89.696, ± .0009

Hence Bi=208.92, or, reduced to a vacuum standard, 208.90.

Classen's paper was followed by a long controversy between Schneider and Classen,[2] in which the former upheld the essential accuracy of the work done by Marignac and himself. Schneider had started out with commercial bismuth, and Classen found that the commercial bismuth which he met with was impure. Schneider, by various analyses, showed that other samples of bismuth were so nearly pure that the common modes of purification were adequate; but Classen replied that the original sample used by Schneider in his atomic weight investigation had not been reëxamined. Accordingly, Schneider published a new series of determinations[3] made by the old method, but with metal which had been scrupulously purified. Results as follows:

Bi.	*Bi_2O_3.*	*Per cent. Bi.*
5.0092	5.5868	89.661
3.6770	4.1016	89.648
7.2493	8.0854	89.659
9.2479	10.3142	89.662
6.0945	6.7979	89.653
12.1588	13.5610	89.660

Mean, 89.657, ± .0015

Hence with O=16, Bi=208.05, a confirmation of the earlier determinations.

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[1] Ber. Deutsch. chem. Ges., 23, 938. 1890.

[2] Journ. prakt. Chem., 42, 563; 43, 133; 44, 23 and 411.

[3] Journ. prakt. Chem., 50, 461. 1894.

According to Adie[1] the differences between the low and high values for bismuth are due to the presence of silicon in the metal. A preliminary determination of the atomic weight, made with pure bismuth, gave Bi=208.8, approximately. Adie's explanation of the discrepancies remains to be substantiated by others.

Birckenbach,[2] working under the direction of Gutbier, effected the synthesis of bismuth oxide, and also studied its reduction. Bismuth from three distinct sources was employed in the investigation. First, the metal was converted into nitrate, and then calcined to oxide, which latter was proved to be free from occluded gases. The data obtained were as follows:

Preliminary Series.

Bi.	*Bi_2O_3.*	*Per cent. Bi.*
10.2899	11.4782	89.647
8.1023	9.0372	89.655

Mean, 89.651, ± .0027

Hence Bi=207.905.

Final Series.

Bi.	*Bi_2O_3.*	*Per cent. Bi.*
9.63289	10.74328	89.664
10.41101	11.61288	89.651
10.97914	12.24528	89.661
10.11990	11.28800	89.653
18.96770	21.15541	89.659
11.99601	13.38001	89.654
27.23022	30.37392	89.651
24.98170	27.86431	89.655
10.11284	11.27998	89.653
28.35991	31.63053	89.660

Mean, 89.656, ± .0010

Hence Bi=208.02.

The reduction of bismuth oxide to bismuth gave Birckenbach the following results:

Preliminary Series.

Bi_2O_3.	*Bi.*	*Per cent. Bi.*
2.43105	2.17994	89.671
2.9547	2.6488	89.647
1.65199	1.4810	89.671
2.4103	2.1609	89.653

Mean, 89.660, ± .0041

Hence Bi=208.11.

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[1] Proc. Cambridge Phil. Soc., 12, 240. 1903.

[2] Inaug. Diss., Erlangen, 1905. The oxidation series also appears under the authorship of Gutbier and Birckenbach, in Journ. prakt. Chem. (2), 47, 457. 1908.

Final Series.

Bi_2O_3.	Bi.	Per cent. Bi.
1.45827	1.30751	89.662
2.12432	1.90461	89.657
3.0021	2.6918	89.664
2.1012	1.8840	89.663
3.0182	2.70620	89.663
1.9091	1.71171	89.661
		Mean, 89.662, ± .0007

Hence Bi = 208.153.

Rejecting the work of Lagerhjelm, which has so high a probable error as to count for almost nothing, the data for the percentage of Bi in Bi_2O_3 combine as follows:

Schneider, 1851	89.655,	± .0034
Marignac	89.682,	± .0036
Löwe	89.648,	± .0040
Classen	89.696,	± .0009
Schneider, 1894	89.657,	± .0015
Birckenbach, preliminary oxidations	89.651,	± .0027
Birckenbach, final oxidations	89.656,	± .0010
Birckenbach, preliminary reductions	89.660,	± .0041
Birckenbach, final reductions	89.662,	± .0007
General mean	89.6683,	± .00044

If we omit the high value found by Classen, the general mean becomes 89.6594, ± .00052.

Mehler,[1] also under Gutbier's direction, studied the composition of bismuth tribromide, which was prepared by direct union of the metal with bromine, and afterwards sublimed. The bromine was precipitated with silver solution, and the silver bromide was weighed. The weights are given as reduced to a vacuum. In the third column I give the ratio $3AgBr : BiBr_3 :: 100 : x$:

$BiBr_3$.	AgBr.	Ratio
3.77071	4.74323	79.497
4.37676	5.50932	79.443
3.64088	4.58160	79.467
4.57894	5.76183	79.470
4.53204	5.70410	79.452
2.85054	3.58682	79.473
4.58310	5.76618	79.482
6.47910	8.15465	79.453
		Mean, 79.467, ± .0042

Hence Bi = 207.92.

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[1] Inaug. Diss., Erlangen, 1905. Sitzungsb. phys. med. Soz. Erlangen, 37, 343.

Another research, carried out under Gutbier by Janssen,[1] involved the synthesis of bismuth sulphate. Bismuth was first dissolved in nitric acid, and then, with sulphuric acid, converted into sulphate. The latter compound was freed from moisture and excess of acid by heating to 380°, at which temperature its weight was constant. The results obtained were as follows:

Bi.	*$Bi_2(SO_4)_3$.*	*Per cent. Bi.*
2.4045	4.0706	59.070
2.41900	4.09445	59.081
2.20280	3.72745	59.096
2.57206	4.35444	59.066
5.79241	3.79987	59.106
3.65233	6.18143	59.086

Mean, 59.084, ± .0042

Hence Bi = 208.085.

The subjoined ratios are now available for discussion:

(1). $Bi_2O_3:2Bi::100:89.6683$, ± .00044
(2). $Bi_2(SO_4)_3:2Bi::100:59.084$, ± .0042
(3). $Bi_2O_3:Bi_2(SO_4)_3::100:151.729$, ± .0099
(4). $3Ag:BiCl_3::100:98.003$, ± .090
(5). $3AgBr:BiBr_3::100:79.467$, ± .0042

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075

Hence,

From ratio 5 .... Bi = 207.921, ± .0239
" " 2 .... 208.085, ± .0260
" " 3 .... 208.171, ± .0445
" " 1 .... 208.295, ± .0095
" " 4 .... 210.802, ± .2914

General mean. Bi = 208.224, ± .0082

This value is probably too high, mainly because of Classen's determinations. Rejecting them, and also the worthless determination by Dumas, the general mean becomes

Bi = 208.062, ± .0096

which value is to be accepted. It is also sustained by Brauner's[2] statement that Kužma, by syntheses of bismuth sulphate from bismuth oxide, has obtained the value Bi = 208.0, ± .1. The details of Kužma's work are yet to be published.

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[1] Inaug. Diss., Erlangen, 1906.
[2] In Abegg's "Handbuch," 3 (3), 634.

COLUMBIUM.[1]

The atomic weight of this metal has been determined by several investigators. Rose[2] analyzed a compound which he supposed to be chloride, but which, according to Rammelsberg,[3] must have been nearly pure oxychloride. If it was chloride, then the widely varying results give approximately Cb=122; if it was oxychloride, the value becomes nearly 94. If it was chloride, it was doubtless contaminated with tantalum compounds.

Hermann's[4] results seem to have no present value, and Blomstrand's[5] are far from concordant. The latter chemist studied columbium pentachloride and sodium columbate. In the first case he weighed the columbium as columbium pentoxide, and the chlorine as silver chloride, the oxide being determined by several distinct processes. In some cases it was thrown down by water, in others by sulphuric acid, and in still others by sodium carbonate or ammonia jointly with sulphuric acid. The weights given are as follows:

$CbCl_5$.	Cb_2O_5.	$AgCl$.
.591	.294	....
.8085	.401	2.085
.633	.317	....
.195	.0974	.500
.507	.2505	1.302
.9415	.472	2.454
.563	.2796	....
.9385	.4675	2.465
.4788	.2378	....
.408	.204	1.067
.9065	.4515	....

Hence the subjoined percentages, and the ratios $5AgCl : CbCl_5 :: 100 : x$, and $10AgCl : Cb_2O_5 :: 100 : x$:

Per cent. Cb_2O_5.	$5AgCl:CbCl_5$.	$10AgCl:Cb_2O_5$.
49.788	.....	.....
49.598	38.777	19.233
50.079	.....	.....
49.949	39.000	19.435
49.408	38.940	19.240

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[1] This name has forty years priority over "niobium," and therefore deserves preference.

[2] Poggend. Annal., 104, 439. 1858.

[3] Poggend. Annal., 136, 353. 1869.

[4] Journ. prakt. Chem., 68, 73. 1856.

[5] Acta Univ. Lund, 1864.

50.135	38.366	19.234
49.662	.....	.....
49.813	38.073	18.966
49.666	.....	.....
50.000	38.238	19.119
49.807	.....	.....
Mean, 49.806, ± .045	Mean, 38.566, ± .108	Mean, 19.205, ± .043

From these means the atomic weight of columbium may be computed, thus:

From $2CbCl_5 : Cb_2O_5$ .... $Cb = 96.231$
From $CbCl_5 : 5AgCl$ .... " $= 99.107$
From $5AgCl : Cb_2O_5$ .... " $= 97.641$

when $Ag = 107.88$, and $Cl = 35.4584$.

The series upon sodium columbate, which salt was decomposed with sulphuric acid, both Cb_2O_5 and Na_2SO_4 being weighed, is too discordant for discussion. The exact nature of the salt studied is not clear, and the data given, when transformed into the ratio $Na_2SO_4 : Cb_2O_5 :: 100 : x$, give values for x ranging from 151.65 to 161.20. Further consideration of this series would therefore be useless. It seems highly probable that Blomstrand's materials were not entirely free from tantalum, since the atomic weight of columbium derived from his analyses of the chloride is evidently too high.

Marignac[1] made about twenty analyses of the potassium fluoxycolumbate, $CbOF_3.2KF.H_2O$. One hundred parts of this salt give the following percentages:

Cb_2O_5	Extremes	44.15	to	44.60	Mean, 44.36
K_2SO_4	"	57.60	"	58.05	
H_2O	"	5.75	"	5.98	
F	"	30.62	"	32.22	

From the mean percentage of Cb_2O_5, $Cb = 93.478$.

From the mean between the extremes given for K_2SO_4, $Cb = 93.95$.

The recent determinations by Balke and Smith[2] are much more satisfactory than those already cited. Their material was certainly purer, and the results obtained were highly concordant. Columbium pentachloride was decomposed by water, with the aid of a little nitric acid,

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[1] Arch. Sci. Phys. Nat. (2), 23. 1865. Oeuvres Complètes, 2, 259.

[2] Journ. Amer. Chem. Soc., 30, 1644. 1908.

and the oxide so produced was finally ignited and weighed. Their data, with vacuum weights, are as follows:

$CbCl_5$.	Cb_2O_5.	Per cent. Cb_2O_5.
9.56379	4.71539	49.305
5.42742	2.65730	49.292
5.15992	2.54364	49.296
9.64854	4.75641	49.297
7.24572	3.57222	49.301
8.00559	3.94746	49.309
9.60763	4.73852	49.324
9.19732	4.53638	49.323
4.27456	2.10734	49.300
		Mean, 49.305, ± .0026

Hence, if Cl=35.4584, Cb=93.528. It is not necessary to combine this value with the earlier determinations, for the reason that it supplants them. It is, however, near one of Marignac's values, which has confirmatory significance. The atomic weight of columbium appears to be quite near 93.5. The results obtained by Deville and Troost[1] for the vapor densities of columbium chloride and oxychloride are in harmony with this conclusion.

TANTALUM.

The results obtained for the atomic weight of this metal by Berzelius,[2] Rose,[3] and Hermann[4] may be fairly left out of account as valueless. These chemists could not have worked with pure preparations, and their data are sufficiently summed up in Becker's "Digest."

Blomstrand's determinations,[5] as in the case of columbium, were made upon the pentachloride. His weights are as follows:

$TaCl_5$.	Ta_2O_5.	$AgCl$.
.9808	.598	.....
1.4262	.867	2.906
2.5282	1.5375	5.0105
1.0604	.6455	2.156
2.581	1.577	.....
.8767	.534	.....

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[1] Compt. Rend., 56, 891. 1863.

[2] Poggend. Annalen, 4, 14. 1825.

[3] Poggend. Annalen, 99, 80. 1856.

[4] Journ. prakt. Chem., 70, 193. 1857.

[5] Acta Univ. Lund, 1864.

Hence the subjoined percentages of Ta_2O_5 from $TaCl_5$, and the ratios $5AgCl : TaCl_5 :: 100 : x$, and $10AgCl : Ta_2O_5 :: 100 : x$:

Per cent. Ta_2O_5.	*$5AgCl : TaCl_5$.*	*$10AgCl : Ta_2O_5$.*
60.971	.....	.....
60.791	49.078	29.835
60.814	50.458	30.685
60.873	49.297	29.940
60.960	.....	.....
60.924	.....	.....
Mean, 60.889, ± .0208	49.611, ± .289	30.153, ± .180

From these ratios we get for the atomic weight of tantalum:

From per cent. Ta_2O_5....................	$Ta = 173.74$
From $5AgCl : TaCl_5$......................	" $= 178.27$
From $10AgCl : Ta_2O_5$....................	" $= 176.10$

These results are too low, and their "probable errors" are not worth computing. Probably Blomstrand's material still contained some columbium.

In 1866 Marignac's determinations appeared.[1] He made four analyses of a pure potassium fluotantalate, and four more experiments upon the ammonium salt. The potassium compound, K_2TaF_7, was treated with sulphuric acid, and the mixture was then evaporated to dryness. The potassium sulphate was next dissolved out by water, while the residue was ignited and weighed as Ta_2O_5. One hundred parts of the salt gave the following quantities of Ta_2O_5 and K_2SO_4:

Ta_2O_5.	*K_2SO_4.*
56.50	44.37
56.75	44.35
56.55	44.22
56.56	44.24
Mean, 56.59, ± .037	Mean, 44.295, ± .026

From these figures, 100 parts of K_2SO_4 correspond to the subjoined quantities of Ta_2O_5:

127.338
127.960
128.178
127.848

Mean, 127.831, ± .120

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[1] Arch. Sci. Phys. Nat. (2), 26, 89. 1866. Oeuvres Complètes, 2, 314.

The ammonium salt, $(NH_4)_2TaF_7$, ignited with sulphuric acid, gave these percentages of Ta_2O_5. The figures are corrected for a trace of K_2SO_4 which was always present:

63.08
63.24
63.27
63.42

Mean, 63.25, ± .047

Hence we have four values for Ta:

From potassium salt, per cent. Ta_2O_5..... Ta = 183.55
From potassium salt, per cent. K_2SO_4.... " = 182.93
From potassium salt, K_2SO_4:Ta_2O_5....... " = 182.76
From ammonium salt, per cent. Ta_2O_5.... " = 182.66

Average .... Ta = 182.975

The determinations by Hinrichsen and Sahlbom[1] were much simpler. Metallic tantalum was converted into pentoxide by heating in oxygen, and the composition of the oxide was so ascertained. The weights and percentages of tantalum are as follows:

Ta.	*Ta_2O_5.*	*Per cent. Ta.*
.37200	.45437	81.872
.41278	.50364	81.959
.33558	.40975	81.899
.35883	.43807	81.912
.47554	.58087	81.868
		Mean, 81.902, ± .0111

Hence Ta = 181.019.

In this instance, as in the case of columbium, the latest determination supplants the others. Until further evidence is available the atomic weight of tantalum may be taken as 181. The uncertainty probably amounts to as much as a unit.

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[1] Ber. Deutsch. chem. Ges., 39, 2600. 1906.

CHROMIUM.

Concerning the atomic weight of chromium there has been much discussion, and many experimenters have sought to establish the true value. The earliest work upon it having any importance was that of Berzelius,[1] in 1818 and 1826, which led to results much in excess of the correct figure. His method consisted in precipitating a known weight of lead nitrate with an alkaline chromate and weighing the lead chromate thus produced. The error in his determination arose from the fact that lead chromate, except when thrown down from very dilute solutions, carries with it minute quantities of alkaline salts, and so has its apparent weight notably increased. When dilute solutions are used, a trace of the precipitate remains dissolved, and the weight obtained is too low. In neither case is the method trustworthy.

In 1844 Berzelius' results were first seriously called in question. The figure for chromium deduced from his experiments was somewhat over 56; but Peligot[2] now showed, by his analyses of chromous acetate and of the chlorides of chromium, that the true number was near 52.5. Unfortunately, Peligot's work, although good, was published with insufficient details to be useful here. For chromous acetate he gives the percentages of carbon and hydrogen, but not the actual weights of salt, carbon dioxide, and water from which they were calculated. His figures vary considerably, moreover—enough to show that their mean would carry but little weight when combined with the more explicit data furnished by other chemists.

Jacquelain's[3] work we may omit entirely. He gives an atomic weight for chromium which is notoriously too low (50.1), and prints none of the numerical details upon which his result rests. The researches which particularly command our attention begin with those of Berlin.[4] His starting point was normal silver chromate; but in one experiment the dichromate $Ag_2Cr_2O_7$ was used. These salts, which are easily obtained in a pure condition, were reduced in a large flask by means of hydrochloric acid and alcohol. The chloride of silver thus formed was washed by decantation, dried, fused and weighed without transfer. The united washings were supersaturated with ammonia, evaporated to dryness, and the residue treated with hot water. The resulting chromic oxide was

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[1] Schweigg. Journ., 22, 53, and Poggend. Annal., 8, 22.

[2] Compt. Rend., 19, 609 and 734; 20, 1187; 21, 74.

[3] Compt. Rend., 24, 679. 1847.

[4] Journ. prakt. Chem., 37, 509, and 38, 149. 1846.

then collected upon a filter, dried, ignited and weighed. The results were as follows:

4.6680	grm. Ag_2CrO_4 gave	4.027	grm. AgCl and	1.0754	grm. Cr_2O_3.
3.4568	"	2.983	"	.7960	"
2.5060	"	2.1605	"	.5770	"
2.1530	"	1.8555	"	.4945	"
4.3335	grm. $Ag_2Cr_2O_7$ gave	2.8692	"	1.5300	"

From these weighings three values are calculable for the atomic weight of chromium. The three ratios upon which these values depend we will consider separately, taking first that between the chromic oxide and the original silver salt. In the four analyses of the normal chromate the percentages of Cr_2O_3 deducible from Berlin's weighings are as follows:

23.037
23.027
23.025
22.968

Mean, 23.014, ± .011

Hence Cr = 52.46.

And from the single experiment with $Ag_2Cr_2O_7$ the percentage of Cr_2O_3 was 35.306. Hence Cr = 52.34.

For the ratio between Ag_2CrO_4 and AgCl, putting the latter at 100, we have for the former:

115.917
115.883
115.992
116.033

Mean, 115.956, ± .023

Hence Cr = 52.67.

In the single experiment with dichromate 100AgCl is formed from 151.035$Ag_2Cr_2O_7$. Hence Cr = 52.61.

Finally, for the ratio between AgCl and Cr_2O_3, the five experiments of Berlin give, for 100 parts of the former, the following quantities of the latter:

26.705
26.685
26.707
26.650
26.662

Mean, 26.682, ± .0076

Hence Cr = 52.49.

These results will be discussed, in connection with the work of other investigators, at the end of this chapter.

In 1848 the researches of Moberg[1] appeared. His method simply consisted in the ignition of anhydrous chromic sulphate and of ammonium chrome alum, and the determination of the amount of chromic oxide thus left as residue. In the sulphate, $Cr_2(SO_4)_3$, the subjoined percentages of Cr_2O_3 were found. The braces indicate two different samples of material, to which, however, we are justified in ascribing equal value:

.542	grm. sulphate gave	.212	grm. Cr_2O_3.	39.114	per cent.
1.337	"	.523	"	39.117	"
.5287	"	.207	"	39.153	"
1.033	"	.406	"	39.303	"
.868	"	.341	"	39.286	"

Mean, 39.1946, ± .0280

Hence Cr = 53.42.

From the alum, $NH_4.Cr(SO_4)_2.12H_2O$, we have these percentages of Cr_2O_3. The first series represents a salt long dried under a bell jar at a temperature of 18°. The crystals taken were clear and transparent, but may possibly have lost traces of water,[2] which would tend to increase the atomic weight found for chromium. In the second series the salt was carefully dried between folds of filter paper, and results were obtained quite near those of Berlin. Both of these series are discussed together, neither having any present value:

1.3185	grm. alum gave	.213	grm. Cr_2O_3.	16.155	per cent.
.7987	"	.129	"	16.151	"
1.0185	"	.1645	"	16.151	"
1.0206	"	.1650	"	16.167	"
.8765	"	.1420	"	16.201	"
.7680	"	.1242	"	16.172	"
1.6720	"	.2707	"	16.190	"
.5410	"	.0875	"	16.174	"
1.2010	"	.1940	"	16.153	"
1.0010	"	.1620	"	16.184	"
.7715	"	.1235	"	16.007	"
1.374	"	.2200	"	16.012	"

Mean, 16.143, ± .0125

Hence Cr = 53.46.

The determinations made by Lefort[3] are even less valuable than those by Moberg. This chemist started out from barium chromate, which,

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[1] Journ. prakt. Chem., 43, 114.

[2] This objection is suggested by Berlin in a note upon Lefort's paper. Journ. prakt. Chem., 71, 191.

[3] Journ. prakt. Chem., 51, 261. 1850.

to thoroughly free it from moisture, had been dried for several hours at 250°. The chromate was dissolved in nitric acid, the barium thrown down by sulphuric acid, and the precipitate collected upon a filter, dried, ignited and weighed in the usual manner. The natural objection to the process is that traces of chromium may be carried down with the sulphate, thus increasing its weight. In fact, Lefort's results are certainly too high. Calculated from his weighings, 100 parts of $BaSO_4$ correspond to the amounts of $BaCrO_4$ given in the third column:

1.2615	grm. $BaCrO_4$ gave	1.1555	grm. $BaSO_4$.	109.174
1.5895	"	1.4580	"	109.019
2.3255	"	2.1340	"	108.974
3.0390	"	2.7855	"	109.101
2.3480	"	2.1590	"	108.754
1.4230	"	1.3060	"	108.708
1.1975	"	1.1005	"	108.814
3.4580	"	3.1690	"	109.119
2.0130	"	1.8430	"	109.224
3.5570	"	3.2710	"	108.744
1.6470	"	1.5060	"	109.363
1.8240	"	1.6725	"	109.058
1.6950	"	1.5560	"	108.933
2.5960	"	2.3870	"	108.756

Mean, 108.9815, ± .0369

Hence Cr = 53.03.

Wildenstein,[1] in 1853, also made barium chromate the basis of his researches. A known weight of barium chloride was precipitated by a neutral alkaline chromate, and the precipitate allowed to settle until the supernatant liquid was perfectly clear. The barium chromate was then collected on a filter, washed with hot water, dried, gently ignited, and weighed. Here again arises the objection that the precipitate may have retained traces of alkaline salts, and again we find deduced an atomic weight which is too high. One hundred parts of $BaCrO_4$ correspond to $BaCl_2$ as follows:

81.87
81.80
81.61
81.78
81.52
81.84
81.85
81.70
81.68
81.54

81.57
81.75
81.66
81.83
81.66
81.80
81.66
81.85
81.57
81.83

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[1] Journ. prakt. Chem., 59, 27.

81.66
81.55
81.81
81.86
81.54
81.68

81.71
81.63
81.56
81.58
81.67
81.84

Mean, 81.702, ± .014

Hence Cr = 53.56.

Next in order we have to consider two papers by Kessler, who employed a peculiar volumetric method entirely his own. In brief, he compared the oxidizing power of potassium dichromate with that of the chlorate, and from his observations deduced the ratio between the molecular weights of the two salts.

In his earlier paper[1] the mode of procedure was about as follows: The two salts, weighed out in quantities having approximate chemical equivalency, were placed in two small flasks, and to each was added 100 cc. of a ferrous chloride solution and 30 cc. hydrochloric acid. The ferrous chloride was added in trifling excess, and, when action ceased, the amount unoxidized was determined by titration with a standard solution of dichromate. As in each case the quantity of ferrous chloride was the same, it became easy to deduce from the data thus obtained the ratio in question. I have reduced all of his somewhat complicated figures to a simple common standard, and give below the amount of chromate equivalent to 100 of chlorate:

120.118
120.371
120.138
120.096
120.241
120.181

Mean, 120.191, ± .028

Hence Cr = 52.20.

In his later paper[2] Kessler substituted arsenic trioxide for the iron solution. In one series of experiments the quantity of dichromate needed to oxidize 100 parts of the arsenic trioxide was determined, and in another the latter substance was similarly compared with the chlorate.

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[1] Poggend. Annalen, 95, 208. 1855.

[2] Poggend. Annalen, 113, 137. 1861.

The subjoined columns give the quantity of each salt proportional to 100 of As_2O_3:

$K_2Cr_2O_7$.	$KClO_3$.
98.95	41.156
98.94	41.116
99.17	41.200
98.98	41.255
99.08	41.201
99.15	41.086
	41.199
Mean, 99.045, ± .028	41.224
	41.161
	41.193
	41.149
	41.126
	Mean, 41.172, ± .009

Hence $Cr = 52.31$.

Reducing the later series to the standard of the earlier, the two combine as follows:

(1). $2KClO_3 : K_2Cr_2O_7 :: 100 : 120.191$, ± .028
(2). $2KClO_3 : K_2Cr_2O_7 :: 100 : 120.282$, ± .043

General mean....120.216, ± .0235

Siewert's determinations, which do not seem to have attracted general attention, were published in 1861.[1] He, reviewing Berlin's work, found that upon reducing silver chromate with hydrochloric acid and alcohol, the chromic chloride solution always retained traces of silver chloride dissolved in it. These could be precipitated by dilution with water; but, in Berlin's process, they naturally came down with the chromium hydroxide, making the weight of the latter too high: hence too large a value for the atomic weight of chromium. In order to find a more correct value Siewert resorted to the analysis of sublimed, violet, chromic chloride. This salt he fused with sodium carbonate and a little nitre, treated the fused mass with water, and precipitated from the resulting solution the chlorine by silver nitrate in presence of nitric acid. The weight of the silver chloride thus obtained, estimated after the usual manner, gave means for calculating the atomic weight of chromium. His figures, reduced to a common standard, give, as proportional to 100

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[1] Zeit. gesammt. Wissenschaften, 17, 530.

parts of chloride of silver, the quantities of chromic chloride stated in the third of the subjoined columns:

.2367	grm. $CrCl_3$ gave	.6396	grm. AgCl.	37.007
.2946	"	.7994	"	36.853
.2593	"	.7039	"	36.838
.4935	"	1.3395	"	36.842
.5850	"	1.5884	"	36.830
.6511	"	1.76681	"	36.852
.5503	"	1.49391	"	36.836

Mean, 36.865, ± .0158

The first of these figures varies so widely from the others that we are justified in rejecting it, in which case the mean becomes 36.842, ± .0031. Hence Cr = 52.046.

Siewert also made two analyses of silver dichromate by the following process. The salt, dried at 120°, was dissolved in nitric acid. The silver was then thrown down by hydrochloric acid, and, in the filtrate, chromium hydroxide was precipitated by ammonia. Reduced to a uniform standard, we find from his results, corresponding to 100 parts of AgCl, $Ag_2Cr_2O_7$ as in the last column:

.7866 grm. $Ag_2Cr_2O_7$ gave	.52202 AgCl and	.2764	Cr_2O_3.	150.684
1.089 "	.72249 "	.3840	"	150.729

Hence Cr = 52.14.

Berlin's single determination of this ratio gave 151.035. Taking all three values together as one series, they give a mean of 150.816, ± .074.

Siewert's percentages of Cr_2O_3 obtained from $Ag_2Cr_2O_7$ are as follows, calculated from the above weighings:

35.139
35.262

Mean, 35.2005, ± .0415

Hence Cr = 51.983.

Combining, as before, with Berlin's single result, giving the latter equal weight with one of these, we have a general mean of 35.236, ± .0335.

For the ratio between silver chloride and chromic oxide, Siewert's two analyses of the dichromate give as follows. For 100 parts of AgCl we have of Cr_2O_3:

52.948
53.150

Mean, 53.049, ± .068

Hence Cr = 52.041.

This figure, reduced to the standard of Berlin's work on the monochromate, becomes 26.525, ±.034. Berlin's mean was 26.682, ±.0076. The two means, combined, give a general mean of 26.676, ±.074.

By Baubigny[1] we have only three experiments upon the calcination of anhydrous chromic sulphate, as follows:

1.989 grm. $Cr_2(SO_4)_3$	gave .7715 grm. Cr_2O_3.	38.788	per cent.
3.958 "	1.535 "	38.782	"
2.6052 "	1.0115 "	38.826	"

Mean, 38.799, ± .0092

Hence Cr = 52.14.

Moberg found for the same ratio the percentage 39.195, ±.028. The general mean of both series, Moberg's and Baubigny's, is 38.838, ±.0087.

In Rawson's work[2] ammonium dichromate was the substance studied. Weighed quantities of this salt were dissolved in water, and then reduced by hydrochloric acid and alcohol. After evaporation to dryness the mass was treated with water and ammonia, reëvaporated, dried five hours at 140°, and finally ignited in a muffle. The residual chromic oxide was bright green, and was tested to verify its purity. The corrected weights are as follows:

$Am_2Cr_2O_7$.	Cr_2O_3.	*Per cent.* Cr_2O_3.
1.01275	.61134	60.365
1.08181	.65266	60.330
1.29430	.78090	60.334
1.13966	.68799	60.368
.98778	.59595	60.332
1.14319	.68987	60.346

Mean, 60.346, ± .0046

Hence Cr = 52.15.

Still later and most elaborate of all, we come to the determinations of the atomic weight of chromium made by Meineke,[3] who studied the chromate and ammonio-chromate of silver, and also the dichromates of potassium and ammonium. For the latter salt he measured the same ratio that Rawson determined, but by a different method. He precipi-

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[1] Compt. Rend., 98, 146.

[2] Journ. Chem. Soc., 55, 213.

[3] Ann. Chem., 261, 339. 1891.

tated its solution with mercurous nitrate, and ignited the precipitate, with the subjoined results. Vacuum weights are given:

$Am_2Cr_2O_7$.	Cr_2O_3.	Per cent. Cr_2O_3.
2.0416	1.2316	60.325
2.1618	1.3040	60.320
2.0823	1.2562	60.328
2.1913	1.3221*	60.335
2.0970	1.2656	60.353
		Mean, 60.332, ± .0037
		Rawson found, 60.346, ± .0046
		General mean, 60.337, ± .0029

From Meineke's figures Cr = 52.11.

The chromate of silver, Ag_2CrO_4, and the ammonio-chromate, $Ag_2CrO_4 . 4NH_3$, both prepared with all necessary precautions to insure purity, were first treated essentially as in Berlin's experiments, except that the traces of silver chloride held in solution by the chromic chloride were thrown out by sulphuretted hydrogen, estimated, and their amount added to the main portion. Thus the chief error in Berlin's work was avoided. I subjoin the data obtained, with vacuum standards, as usual. All of Meineke's results are so corrected:

Ag_2CrO_4.	$AgCl$.	Cr_2O_3.
2.7826	2.4047	.6384
3.2627	2.8199	.7480
3.6362	3.1416	.8338
4.6781	4.0414	1.0726
3.2325	2.7930	.7411
3.9137	3.3805	.8976

Hence we have the following ratios, as in the case of Berlin's data:

Per cent. Cr_2O_3.	$100AgCl : Ag_2CrO_4$.	$100AgCl : Cr_2O_3$.
22.943	115.715	26.548
22.926	115.703	26.526
22.931	115.744	26.602
22.928	115.754	26.601
22.924	115.736	26.531
22.935	115.773	26.552
Mean, 22.931, ±.0019	Mean, 115.737, ±.0072	Mean, 26.560, ±.0093
Berlin, 23.014, ±.0110		
General mean, 22.934, ±.0018		

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* Calculated back from Meineke's value for Cr, to replace an evident misprint in the original.

From Meineke's figures Cr = 52.10, 52.04 and 52.14.

With the ammonio-chromate Meineke found as follows:

$Ag_2CrO_4.4NH_3$.	$AgCl$.	Cr_2O_3.
4.1518	2.9724	.7904
4.2601	3.0592	.8125
5.9348	4.2654	1.1317

And the ratios become—

Per cent. Cr_2O_3.	$100AgCl:Salt$.	$100AgCl:Cr_2O_3$.
19.037	139.679	26.591
19.072	139.255	26.559
19.059	139.138	26.532
Mean, 19.059, ± .0074	Mean, 139.357, ± .1109	Mean, 26.561, ± .0115
Cr = 52.27	Cr = 51.61	Cr = 52.144

The first of these three analyses is rejected by Meineke as suspicious, but for the present I shall allow it to remain. The data in the third column may now be combined with the corresponding figures from the normal chromate, as found by Meineke and his predecessors:

Berlin	26.682, ± .0076
Siewert, from $Ag_2Cr_2O_7$	26.525, ± .0340
Meineke, from Ag_2CrO_4	26.560, ± .0093
Meineke, from $Ag_2CrO_4.4NH_3$	26.561, ± .0115
General mean	26.620, ± .0052

$4AgCl:Cr_2O_3::100:26.620, \pm .0052$

Obviously, this mean is vitiated by the known error in Berlin's work, the ultimate effect of which is serious.

In all four of the salts studied by Meineke he determined volumetrically the oxygen in excess of the normal oxides by measuring the amount of iodine liberated in acid solutions. With the silver salts the process was essentially as follows: A weighed quantity of the chromate was dissolved in weak ammonia, and the solution was precipitated with potassium iodide. After the silver iodide had been filtered off, five or six grammes of potassium iodide were added to the filtrate, which was then acidulated with phosphoric acid and a little sulphuric. The liberated iodine was then titrated with sodium thiosulphate solution, which had been standardized by means of pure iodine, prepared by Stas' method. From the iodine thus measured the excessive oxygen was computed, and from that datum the atomic weight of chromium was found. For present purposes, however, the data may be used more directly, as giving the ratios $3I : Ag_2CrO_4$ and $3I : Ag_2CrO_4.4NH_3$. Thus treated, the weights are as follows, reduced to a vacuum. Reckoning the salt as 100, the third column gives the percentage of iodine liberated:

Ag_2CrO_4.	*I Set Free.*	*Percentage.*
.43838	.50251	114.628
.90258	1.03432	114.595
.89858	1.02980	114.603
.89868	1.03072	114.693
		Mean, 114.630, ± .015

Hence Cr = 52.40.

The next series, obviously, gives the ratio $3I : Ag_2CrO_4.4NH_3$:

$Ag_2CrO_4.4NH_3$.	*I Set Free.*	*Percentage.*[1]
.54356	.51784	95.267
.54856	.52046	94.877
.54926	.52322	95.258
.54906	.52376	95.392
.54466	.51910	95.307
.54536	.51891	95.150
		Mean, 95.208, ± .0497

Hence Cr = 52.02.

In dealing with the two dichromates Meineke used the acid potassium iodate in place of potassium iodide, the chromate and the iodate reacting in the molecular ratio of 2 : 1. The thiosulphate was standardized by means of the acid iodate, so that we have direct ratios between the latter and the two chromates. The data are as follows, with the amount of iodate proportional to one hundred parts of the dichromate in the third column:

$K_2Cr_2O_7$.	KHI_2O_6.	*Percentage.*
.25090	.16609	66.198
.25095	.16613	66.200
.25078	.16601	66.197
.24979	.16541	66.220
.24987	.16540	66.192
.24966	.16543	66.262
.25015	.16559	66.196
.25012	.16559	66.204
.24977	.16546	66.245
.25034	.16572	66.198
.25025	.16567	66.202
.25015	.16568	66.234
		Mean, 66.212, ± .0044

Hence Cr = 52.14.

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[1] These figures are not wholly in accord with the percentages of oxygen computed by Meineke. I suspect that there is a misprint among his data as published, probably in the second experiment, but I cannot trace it with certainty.

$Am_2Cr_2O_7$.	KHI_2O_6.	*Percentage.*
.21457	.16584	77.290
.21465	.16588	77.279
.21464	.16584	77.264
.21416	.16543	77.246
.21447	.16564	77.232
.21427	.16559	77.281
.22196	.17152	77.272
.22194	.17151	77.278
.22180	.17139	77.272

Mean, 77.268, ± .0041

Hence Cr = 52.13.

Baxter, Mueller and Hines[1] determined the atomic weight of chromium through the analysis of silver chromate. The weighed salt was dissolved in nitric acid and reduced either by sulphurous acid or hydrazine sulphate. The silver was then precipitated as chloride or bromide, by weak hydrochloric or hydrobromic acid, and the halide compound was weighed. All modern precautions were taken in this work, such as determining traces of moisture in the chromate, and also the traces of silver chloride or bromide remaining in solution. The chloride series, with vacuum weights, was as follows:

Ag_2CrO_4.	$AgCl$.	*Ratio.*
10.30985	8.90908	115.723
8.26920	7.14492	115.735
5.56679	5.67444	115.726

Mean, 115.728, ± .0024

Hence Cr = 52.005.

This combines with previous series as follows:

Berlin	115.956, ± .023
Meineke	115.737, ± .0072
Baxter *et al*	115.728, ± .0024
General mean	115.731, ± .0023

For the bromide series, all corrections applied, the data are these:

Ag_2CrO_4.	$AgBr$.	*Ratio.*
2.63788	2.98621	88.336
2.82753	3.20084	88.337
2.33454	2.64268	88.340
1.77910	2.01402	88.336
2.33198	2.63994	88.335
3.10402	3.51390	88.336

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[1] Journ. Amer. Chem. Soc., 31, 529. 1909.

2.92751	3.31427	88.330
4.21999	4.77762	88.328
5.24815	5.94104	88.337
6.24014	7.06484	88.327
7.92313	8.96982	88.331
		Mean, 88.334, ± .0009

Hence Cr = 51.987.

Similar determinations of the atomic weight of chromium were made by Baxter and Jesse,[1] who analyzed silver dichromate. In a single experiment 6.26657 grammes of $Ag_2Cr_2O_7$ gave 4.16076 of AgCl. Ratio, 150.611. Giving this the weight of a single determination in the following bromide series, ±.0024, it combines with the earlier measurements by Berlin and Siewert to a general mean of 150.612, ±.0024. The older work vanishes. Hence Cr = 52.003.

The dichromate-bromide series of determinations gave the subjoined data:

$Ag_2Cr_2O_7$.	*AgBr.*	*Ratio.*
5.71554	4.97149	114.966
4.87301	4.23888	114.960
7.45476	6.48425	114.967
4.75269	4.13420	114.960
8.15615	7.09495	114.957
6.15412	5.35309	114.964
6.83662	5.94768	114.963
5.39883	4.69631	114.959
		Mean, 114.962, ± .0008

Hence Cr = 51.995.

The following ratios are now available for computing the atomic weight of chromium:

(1). Percentage Cr_2O_3 from Ag_2CrO_4, 22.934, ± .0018
(2). Percentage Cr_2O_3 from $Ag_2Cr_2O_7$, 35.236, ± .0335
(3). $2AgCl : Ag_2CrO_4 : : 100 : 115.731$, ± .0023
(4). $2AgCl : Ag_2Cr_2O_7 : : 100 : 150.612$, ± .0024
(5). $4AgCl : Cr_2O_3 : 100 : 26.620$, ± .0052
(6). Percentage Cr_2O_3 in $Cr_2(SO_4)_3$, 38.838, ± .0087
(7). Percentage Cr_2O_3 in $AmCr(SO_4)_2.12H_2O$, 16.143, ± .0125
(8). $BaSO_4 : BaCrO_4 : : 100 : 108.9815$, ± .0369
(9). $BaCrO_4 : BaCl_2 : : 100 : 81.702$, ± .014
(10). $3AgCl : CrCl_3 : : 100 : 36.842$, ± .0031
(11). $2KClO_3 : K_2Cr_2O_7 : : 100 : 120.216$, ± .0235
(12). Percentage Cr_2O_3 in $Ag_2CrO_4.4NH_3$, 19.059, ± .0074
(13). $2AgCl : Ag_2CrO_4.4NH_3 : : 100 : 139.357$, ± .1109
(14). Percentage Cr_2O_3 in $Am_2Cr_2O_7$, 60.337, ± .0029

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[1] Journ. Amer. Chem. Soc., 31, 541. 1909.

(15). $Ag_2CrO_4 : 3I :: 100 : 114.630$, ± .015
(16). $Ag_2CrO_4.4NH_3 : 3I :: 100 : 95.208$, ± .0497
(17). $2K_2Cr_2O_7 : KHI_2O_6 :: 100 : 66.212$, ± .0044
(18). $2Am_2Cr_2O_7 : KHI_2O_6 :: 100 : 77.268$, ± .0041
(19). $2AgBr : Ag_2CrO_4 :: 100 : 88.334$, ± .0009
(20). $2AgBr : Ag_2Cr_2O_7 :: 100 : 114.962$, ± .0008

To reduce these we have the following atomic weights:

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0005
I = 126.9204, ± .00033
S = 32.0667, ± .00075
N = 14.0101, ± .0001
K = 39.0999, ± .0002
Ba = 137.363, ± .0025
H = 1.00779, ± .00001

Hence,

From ratio 13	Cr = 51.610,	± .3180
“ “ 19	51.987,	± .0035
“ “ 20	51.995,	± .0016
“ “ 4	52.010,	± .0069
“ “ 3	52.014,	± .0067
“ “ 16	52.020,	± .2088
“ “ 10	52.046,	± .0134
“ “ 2	52.104,	± .0824
“ “ 1	52.111,	± .0063
“ “ 14	52.125,	± .0066
“ “ 18	52.126,	± .0067
“ “ 17	52.137,	± .0098
“ “ 11	52.235,	± .0285
“ “ 6	52.264,	± .0203
“ “ 12	52.267,	± .0240
“ “ 5	52.312,	± .0149
“ “ 15	52.404,	± .0499
“ “ 8	53.031,	± .0862
“ “ 7	53.456,	± .0611
“ “ 9	53.563,	± .0437
General mean,	Cr = 52.0193,	± .0013

In this combination the work of Baxter and his colleagues carries overwhelming weight, and yet the good work of Siewert, Baubigny, Rawson, and, in part, Meineke's, is not entirely ignored. The high values, with their large probable errors, practically vanish from the general mean. The ten lowest values give a mean of Cr = 52.007, ± .0013; the ten highest give Cr = 52.194, ± .0050. The unimportance of the last value is perfectly evident.

MOLYBDENUM.

If we leave out of account the inaccurate determination made by Berzelius,[1] we shall find that the data for the atomic weight of molybdenum lead to two independent estimates of its value—one near 92, the other near 96. The earlier results found by Berlin and by Svanberg and Struve lead to the lower number; the more recent investigations, together with considerations based upon the periodic classification, point conclusively to the higher.

The earliest investigation which we need especially to consider is that of Svanberg and Struve.[2] These chemists tried a variety of different methods, but finally based their conclusions upon the two following: First, molybdenum trioxide was fused with potassium carbonate, and the carbon dioxide which was expelled was estimated; secondly, molybdenum disulphide was converted into the trioxide by roasting, and the ratio between the weights of the two substances was determined.

By the first method it was found that 100 parts of MoO_3 will expel the following quantities of CO_2:

31.4954
31.3749
31.4705

Mean, 31.4469, ± .0248

The carbon dioxide was determined simply from the loss of weight when the weighed quantities of trioxide and carbonate were fused together. It is plain that if, under these circumstances, a little of the trioxide should be volatilized, the total loss of weight would be slightly increased. A constant error of this kind would tend to bring out the atomic weight of molybdenum too low.

By the second method, the conversion by roasting of MoS_2 into MoO_3, Svanberg and Struve obtained these results. Two samples of artificial disulphide were taken, A and B, and yielded for each hundred parts the following of trioxide:

$$\left.\begin{array}{r}89.7919\\89.7291\end{array}\right\}A$$
$$\left.\begin{array}{r}89.6436\\89.7082\\89.7660\\89.7640\\89.8635\end{array}\right\}B$$

Mean, 89.7523, ± .0176

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[1] Poggend. Annalen, 8, 1. 1826.
[2] Journ. prakt. Chem., 44, 301. 1848.

Three other experiments in series B gave divergent results, and, although published, are rejected by the authors themselves. Hence it is not necessary to cite them in this discussion. We again encounter in these figures the same source of constant error which apparently vitiates the preceding series, namely, the possible volatilization of the trioxide. Here, also, such an error would tend to reduce the atomic weight of molybdenum.

From the CO_2 series.............. Mo = 91.93
From the MoS_2 series............ Mo = 93.30

Berlin,[1] a little later than Svanberg and Struve, determined the atomic weight of molybdenum by igniting a molybdate of ammonium and weighing the residual MoO_3. Here, again, a loss of the latter by volatilization may (and probably does) lead to too low a result. The salt used was $(NH_4)_4Mo_5O_{17}.3H_2O$, and in it these percentages of MoO_3 were found:

81.598
81.612
81.558
81.555

Mean, 81.581, ± .0095

Hence Mo = 92.16.

Until 1859 the value 92 was generally accepted on the basis of the foregoing researches, but in this year Dumas[2] published some figures tending to sustain a higher number. He prepared molybdenum trioxide by roasting the disulphide, and then reduced it to metal by ignition in hydrogen. At the beginning the hydrogen was allowed to act at a comparatively low temperature, in order to avoid volatilization of trioxide; but at the end of the operation the heat was raised sufficiently to insure a complete reduction. From the weighings I calculate the percentages of metal in MoO_3:

.448 grm. MoO_3	gave .299	grm. Mo.	66.741	per cent.
.484	" .323	"	66.736	"
.484	" .322	"	66.529	"
.498	" .332	"	66.667	"
.559	" .373	"	66.726	"
.388	" .258	"	66.495	"

Mean, 66.649, ± .030

Hence Mo = 95.924.

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[1] Journ. prakt. Chem., 49, 444. 1850.
[2] Ann. Chem. Pharm., 105, 84; and 113, 23.

In 1868 the same method was employed by Debray.[1] His trioxide was purified by sublimation in a platinum tube. His data are as follows:

5.514 grm. MoO_3 gave	3.667 grm. Mo.	66.503 per cent.
7.910 "	5.265 "	66.561 "
9.031 "	6.015 "	66.604 "
	Mean,	66.556, ± .020

Hence Mo=95.524.

For the same ratio we have also a single experiment by Rammelsberg,[2] who, closely following Dumas' method, found in molybdenum trioxide 66.708 per cent. of metal. As this figure falls within the limits of Dumas' series, we may assign it equal weight with one experiment in the latter.

Debray also made two experiments upon the precipitation of molybdenum trioxide in ammoniacal solution by nitrate of silver. In his results, as published, there is curious discrepancy, which, I have no doubt, is due to a typographical error. These results I am therefore compelled to leave out of consideration. They could not, however, exert a very profound influence upon the final discussion.

In 1873, Lothar Meyer[3] discussed the analyses made by Liechti and Kemp[4] of four chlorides of molybdenum, and in the first edition of this work the same data were considered in detail. The analyses, however, were not intended as determinations of atomic weight, and since good determinations have been more recently published, the work on the chlorides will be omitted from further consideration. It is enough to state here that they gave values for Mo ranging near 96, both above and below that number, with an extreme range of over eight-tenths of a unit.

In 1893 the determinations by Smith and Maas appeared,[5] representing an entirely new method. Sodium molybdate, purified by many recrystallizations and afterwards dehydrated, was heated in a current of pure, dry, gaseous hydrochloric acid. The compound $MoO_3.2HCl$ was thus distilled off, and the sodium molybdate was quantitatively transformed into sodium chloride. The latter salt was afterwards carefully examined, and proved to be free from molybdenum. The data, with all weights reduced to a vacuum standard, are subjoined:

Na_2MoO_4.	NaCl.	Per cent. NaCl.
1.14726	.65087	56.733
.89920	.51023	56.743
.70534	.40020	56.739

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[1] Compt. Rend., 66, 734.

[2] Berlin Monatsbericht, 1877, p. 574.

[3] Ann. Chem. Pharm., 169, 365. 1873.

[4] Ann. Chem. Pharm., 169, 344.

[5] Journ. Amer. Chem. Soc., 15, 397 1893.

.70793	.40182	56.760
1.26347	.71695	56.745
1.15217	.65367	56.734
.90199	.51188	56.750
.81692	.46358	56.747
.65098	.36942	56.748
.80563	.45717	56.747

Mean, 56.745, ± .0017

Hence Mo = 96.055.

In 1895, Seubert and Pollard[1] determined the atomic weight of molybdenum by two methods. First, the carefully purified trioxide, in weighed amounts, was dissolved in an excess of a standard solution of caustic soda. This solution was standardized by means of hydrochloric acid, which in turn had been standardized gravimetrically as silver chloride. Hence, indirectly, the ratio $2AgCl : MoO_3$ was measured. Sulphuric acid and lime water were also used in the titrations, so that the entire process was rather complicated. Ignoring the intermediate data, the end results, in weights of MoO_3 and AgCl, were as follows. The third column gives the MoO_3 proportional to 100 parts of AgCl:

MoO_3.	*AgCl.*	*Ratio.*
3.6002	7.1709	50.206
3.5925	7.1569	50.196
3.7311	7.4304	50.214
3.8668	7.7011	50.211
3.9361	7.8407	50.201
3.8986	7.7649	50.208
3.9630	7.8941	50.202
3.9554	7.8806	50.192
3.9147	7.7999	50.189
3.8543	7.6767	50.208
3.9367	7.8437	50.190

Mean, 50.202, ± .0018

Hence Mo = 95.92.

The second method adopted by Seubert and Pollard was the old one of reducing the trioxide to metal by heating in a current of hydrogen. The weights and percentages of metal are subjoined:

MoO_3.	*Mo.*	*Per cent.*
1.8033	1.2021	66.661
1.9345	1.1564	66.670
3.9413	2.6275	66.666
1.5241	1.0160	66.662
4.0533	2.7027	66.679

Mean, 66.668, ± .0022

Hence Mo = 96.006.

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[1] Zeitsch. anorg. Chem., 8, 434. 1895.

Vandenberghe[1] prepared molybdenum dibromide, which was next reduced to metal by heating in hydrogen. The metal was then oxidized to trioxide by means of nitric acid. The data are as follows:

Mo.	*MoO_3.*	*Per cent. Mo.*
.7143	1.0711	66.689
.3453	.5177	66.699
.9693	1.4533	66.696
.5089	.7631	66.689
1.7212	2.5820	66.689
		Mean, 66.692, ± .0015

Corrected to a vacuum this becomes 66.687, ± .0015.

Hence Mo = 96.088.

This mean may be combined with former determinations thus:

Dumas	66.649, ± .0300
Debray	66.556, ± .0200
Rammelsberg	66.708, ± .0680
Seubert and Pollard	66.668, ± .0022
Vandenberghe	66.687, ± .0015
General mean	66.681, ± .0012

Neglecting all determinations made before 1859, there are now three ratios from which to compute the atomic weight of molybdenum, as follows:

(1). $MoO_3:Mo::100:66.681, \pm .0012$
(2). $2AgCl:MoO_3::100:50.202, \pm .0018$
(3). $Na_2MoO_4:2NaCl::100:56.745, \pm .0017$

Reducing these ratios with Ag = 107.880, ± .00029, Cl = 35.4584, ± .0002, and Na = 23.0108, ± .00024, we have—

From ratio 2	Mo = 95.917, ± .0052
" " 3	96.055, ± .0036
" " 1	96.062, ± .0039
General mean,	Mo = 96.029, ± .0024

In this combination the actual uncertainty is greater than the decimals. For practical purposes the round number 96 can be used.

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[1] Acad. Roy. Belge, Mém. Couronnés, T. 56.

TUNGSTEN.

The atomic weight of tungsten has been determined from analyses of the trioxide, the hexchloride, and the tungstates of iron, silver, sodium and barium.

The composition of the trioxide has been the subject of many investigations. Malaguti[1] reduced this substance to the blue oxide, and from the difference between the weights of the two compounds obtained a result now known to be considerably too high. In general, however, the method of investigation has been to reduce WO_3 to W in a stream of hydrogen at a white heat, and afterwards to reoxidize the metal, thus getting from one sample of material two results for the percentage of tungsten. This method is probably accurate, provided that the trioxide used be pure.

The first experiments which we need consider are, as usual, those of Berzelius.[2] 899 parts WO_3 gave, on reduction, 716 of metal. 676 of metal, reoxidized, gave 846 WO_3. Hence these percentages of W in WO_3:

79.644, by reduction
79.905, by oxidation

Mean, 79.7745, ± .0880

Hence W = 189.324.

These figures are far too high, the error being probably due to the presence of alkaline impurity in the trioxide employed.

Next in order of time comes the work of Schneider,[3] who, with characteristic carefulness, took every precaution to get pure material. His percentages of tungsten are as follows:

Reduction Series.

79.336
79.254
79.312
79.326
79.350

Mean, 79.3156

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[1] Journ. prakt. Chem., 8, 179. 1836.
[2] Poggend. Annalen, 8, 1. 1826.
[3] Journ. prakt. Chem., 50, 152. 1850.

Oxidation Series.

79.329
79.324
79.328

Mean, 79.327
Mean of all, 79.320, ± .0068

Hence W=184.108.

Closely agreeing with these figures are those of Marchand,[1] published in the following year:

Reduction Series.

79.307
79.302

Mean, 79.3045

Oxidation Series.

79.321
79.352

Mean, 79.3365
Mean of all, 79.3205, ± .0073

Hence W=184.114.

The figures obtained by v. Borch[2] agree approximately with the foregoing. They are as follows:

Reduction Series.

79.310
79.212
79.289
79.313
79.225
79.290
79.302

Mean, 79.277

Oxidation Series.

79.359
79.339

Mean, 79.349
Mean of all, 79.293, ± .0108

Hence W=183.806.

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[1] Ann. Chem. Pharm., 77, 261. 1851.

[2] Journ. prakt. Chem., 54, 254. 1851.

Dumas[1] gives only a reduction series, based upon trioxide obtained by the ignition of a pure ammonium tungstate. The reduction was effected in a porcelain boat, platinum being objectionable on account of the tendency of tungsten to alloy with it. Dumas publishes only weighings, from which I have calculated the percentages:

2.784 grm. WO_3 gave	2.208 grm. W.	79.310 per cent.
2.994 "	2.373 "	79.259 "
4.600 "	3.649 "	79.326 "
.985 "	.781 "	79.289 "
.917 "	.727 "	79.280 "
.917 "	.728 "	79.389 "
1.717 "	1.362 "	79.324 "
2.988 "	2.370 "	79.317 "
	Mean,	79.312, ± .009

Hence W = 184.019.

The data furnished by Bernoulli[2] differ widely from those just given. This chemist undoubtedly worked with impure material, the trioxide having a greenish tinge. Hence the results are too high. These are the percentages of W:

Reduction Series.

79.556
79.526
79.553
79.558
79.549
78.736

Mean, 79.413

Oxidation Series.

79.558
79.656
79.555
79.554

Mean, 79.581
Mean of all, 79.480, ± .056

Hence W = 185.918.

Two reduction experiments by Persoz[3] give the following results:

1.7999 grm. WO_3 gave	1.4274 grm. W.	79.304 per cent.
2.249 "	1.784 "	79.324 "
	Mean,	79.314, ± .007

Hence W = 184.041.

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[1] Ann. Chem. Pharm., 113, 23. 1860.
[2] Poggend. Annalen, 111, 573. 1860.
[3] Zeit. anal. Chem., 3, 260. 1864.

Next in order is the work done by Roscoe.[1] This chemist used a porcelain boat and tube, and made six weighings, after successive reductions and oxidations, with the same sample of 7.884 grammes of trioxide. These weighings give me the following five percentages which, for the sake of uniformity with foregoing series, I have classified under the usual, separate headings:

Reduction Series.

79.196
79.285
79.308

Mean, 79.263

Oxidation Series.

79.230
79.299

Mean, 79.2645
Mean of all, 79.264, ± .0146

Hence W = 183.482.

In Waddell's experiments[2] especial precautions were taken to procure tungstic oxide free from silica and molybdenum. Such oxide, elaborately purified, was reduced in hydrogen, with the following results:

1.4006 grm. WO_3	gave	1.1115 W.	79.359 per cent.
.9900	"	.7855 "	79.343 "
1.1479	"	.9110 "	79.362 "
.9894	"	.7847 "	79.311 "
4.5639	"	3.6201 "	79.320 "

Mean, 79.339, ± .0069

Hence W = 184.332.

The investigation by Pennington and Smith[3] started from the supposition that the tungsten compounds studied by their predecessors had not been completely freed from molybdenum. Accordingly, tungstic oxide, carefully freed from all other impurities, was heated in a stream of gaseous hydrochloric acid, so as to volatilize all molybdenum as the compound $MoO_3.2HCl$. The residual WO_3, was then reduced in pure hydrogen, and the tungsten so obtained was oxidized in porcelain crucibles. Care was taken to exclude reducing gases, and the trioxide was finally cooled in vacuum desiccators over sulphuric acid. The oxida-

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[1] Ann. Chem. Pharm., 162, 368. 1872.

[2] Amer. Chem. Journ., 8, 280. 1886.

[3] Read before the Amer. Philos. Soc., Nov. 2, 1894.

tion data are as follows, with the usual percentage column added. The weights are reduced to a vacuum:

Tungsten.	*Oxygen gained.*	*Percentage.*
.862871	.223952	79.394
.650700	.168900	79.392
.597654	.155143	79.390
.666820	.173103	79.391
.428228	.111168	79.390
.671920	.174406	79.392
.590220	.153193	79.394
.568654	.147588	79.394
1.080973	.280600	79.392
		Mean, 79.392, ± .0004

Hence W = 184.92.

The very high value for tungsten found by Pennington and Smith, nearly a unit higher than that which was commonly accepted, seems to have at once attracted the attention of Schneider,[1] who criticized the paper somewhat fully, and gave some new determinations of his own. The tungsten trioxide employed in this new investigation was heated in gaseous hydrochloric acid, and the absence of molybdenum was proved. The data obtained, both by reduction and by oxidation, are as follows:

Reduction Series.

2.0738 grm. WO_3 gave	1.6450 W.	79.323 per cent.
4.0853 "	3.2400 "	79.309 "
6.1547 "	4.8811 "	79.307 "

Oxidation Series.

1.5253 grm. W gave	1.9232 WO_3.	79.311 per cent.
3.1938 "	4.0273 "	79.304 "
4.7468 "	5.9848 "	79.314 "
		Mean of all, 79.311, ± .0018

Hence W = 184.007.

In order to account for the difference between this result and that of Pennington and Smith, an impurity of molybdenum trioxide amounting to about one per cent. would be necessary. Schneider suggests that the quantities of material used by Pennington and Smith were too small, and that there may have been mechanical loss of small particles during the long heatings. Such losses would tend to raise the atomic weight computed from the experiments. On the other hand, the losses could hardly have been uniform in extent, and the extremely low probable error of Pennington and Smith's series renders Schneider's supposition improbable. The error, if error exists, must be accounted for otherwise.

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[1] Journ. prakt. Chem. (2), 53, 288. 1896.

Soon after Schneider's paper appeared, another set of determinations by Shinn[1] was published from Smith's laboratory. Attempts to verify the results obtained by Smith and Desi having proved abortive, and other experiments having failed, Shinn resorted to the oxidation method and gives the subjoined data. The percentage column is added by myself:

.22297 grm. W	gave	.28090	WO_3.	79.377
.17200	"	.21664	"	79.394
.10989	"	.13844	"	79.377
.10005	"	.12598	"	79.417
			Mean,	79.391, ± .0066

Hence W = 184.908.

This figure is very close to that found in Pennington and Smith's series.

The great discordance between the determinations so far cited, led Hardin[2] to a very careful investigation of tungsten trioxide. The substance was prepared from various sources, and manipulated by various methods; and although concordant results were sometimes obtained in succession, the discordance between different series of experiments was very great. Hardin therefore concluded that a discussion of his figures, with reference to the atomic weight of tungsten, would be useless. Nevertheless, partly for the sake of completeness, and partly because this calculation is in great measure a study of the compensation of errors, I prefer to cite Hardin's determinations, in order that they may be compared with others. For this purpose I give his sixty-four determinations as one series. The letters o and r indicate oxidation and reduction experiments, respectively. The atomic weights found were as follows:

r 184.05
r 184.04
r 183.98
r 184.33
r 183.94
r 183.91
r 183.66
o 184.94
o 184.86
o 185.00
o 184.91
o 184.75
o 184.15
r 184.88
r 184.85
r 184.94
r 184.01
r 184.66
r 183.99
r 183.93
r 183.91
o 184.53
o 184.01
o 184.65
r 183.55
r 184.34
r 184.21
r 183.95
o 183.70
o 184.30
o 183.99
o 184.07
o 184.86
o 184.27
o 184.07
r 183.83
r 183.80
r 183.67
r 183.56
r 183.72
r 183.71
r 183.80
r 183.87
o 183.83
o 183.75
o 184.13
o 183.90
o 183.82
o 184.20
r 183.58
r 183.51
r 183.83
o 184.05
o 184.22
o 184.06
r 184.03
r 183.81
o 183.85
o 184.14
r 183.89
r 183.63
o 184.17
o 184.08
r 184.13

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[1] Thesis, University of Pennsylvania, 1896. "The atomic mass of tungsten."
[2] Journ. Amer. Chem. Soc., 19, 657. 1897.

The mean of all is W = 184.105, ± .0337. This gives a percentage of W in WO_3 of 79.320, ± .0185. The discordances were shown by Hardin to be due partly to impurities in his material, such as nitrogen retained by trioxide prepared from ammonium tungstate, and partly to volatility of the oxide at high temperatures. In a later memoir[1] he discusses these errors at some length, and gives a few other determinations which are even more discordant, and therefore not worth citation now.

Taylor's thesis,[2] representing work done in Smith's laboratory, is essentially a study of errors. He found that constant weight could not be secured during reduction experiments with the trioxide, and he also found, like Hardin, that the oxidations generally gave the higher values for the atomic weight of tungsten. Furthermore, he ascertained that tungstic oxide derived from colloidal ammonium tungstate gave different values dependent upon whether the latter compound was dialyzed or undialyzed. Oxide from the dialyzed salt gave the highest atomic weights. Some of the discrepancies were ultimately traced to the presence in the material studied, of a complex salt containing manganese and iron, and the influence of these impurities was studied. Iron, and also molybdenum, tend to lower the apparent atomic weight of tungsten; manganese, and in much greater measure, raises it. The errors are in opposite directions, but do not absolutely compensate one another.

One new method for measuring the atomic weight of tungsten was tested by Taylor, but the results were not satisfactory. Sodium carbonate was heated in a glass bulb with tungsten trioxide and water, the latter was distilled off after effervescence had ceased, and the residue was then heated to 300° in a vacuum. The weights of carbonate and oxide being known, the loss in weight represented carbon dioxide. The ratio between WO_3 and CO_2 was thus determined. I cite the weights, and also the values for the ratio $WO_3 : CO_2 :: 100 : x$:

Weight WO_3.	*Weight CO_2.*	*Ratio.*
2.0802	.3952	18.998
2.1937	.4173	19.023
4.0818	.7762	19.016
3.3629	.6394	19.013
		Mean, 19.0125, ± .0034

Hence W = 183.45; a determination which Taylor regards as worthless, while admitting that the method is one of some promise.

Several of the investigations so far described were carried out under the direction of, or in coöperation with Professor Edgar F. Smith. The

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[1] Journ. Amer. Chem. Soc., 21, 1017. 1899.

[2] Thesis, University of Pennsylvania, 1901. "Atomic weight of tungsten."

experience obtained in their conduct gave a sound basis for further researches, which were undertaken by Smith and Exner.[1] These authors discuss at length the sources of error in former determinations of the atomic weight of tungsten, and point out the difficulty of preparing pure material, a difficulty which was at last overcome. From a pure ammonium tungstate they prepared pure tungsten, the pure trioxide, and pure tungsten hexchloride, free from oxychloride, and with these substances their atomic weight determinations were made. At this point only their syntheses of the trioxide will be considered, their other series being discussed later. Their figures, with vacuum weights, and the usual percentage column are given below:

Weight W.	*Weight WO_3.*	*Per cent.*
2.24552	2.83144	79.306
1.78151	2.24619	79.313
1.63590	2.06270	79.309
1.38534	1.74665	79.314
1.29903	1.63774	79.318
2.01302	2.53781	79.321
2.18607	2.75632	79.311
2.36755	2.98478	79.323
1.94958	2.45781	79.322
4.43502	5.59141	79.318
2.37603	2.99548	79.321
2.58780	3.26260	79.314
2.58503	3.25886	79.322
2.38298	3.00441	79.316
2.05578	2.59169	79.322
3.60828	4.54915	79.318
6.22621	7.84949	79.320
5.28444	6.66239	79.317
3.99095	5.03138	79.321
7.30166	9.20647	79.309
3.44143	4.33870	79.319
2.67709	3.37541	79.312
4.96735	6.26229	79.322
		Mean, 79.3169, ± .0007

Hence W = 184.075, ± .0064.

There are still other experiments by Riche,[2] which I have not been able to get in detail. They cannot be of any value however, for they give to tungsten an atomic weight of about ten units too low. We may, therefore, neglect this series and go on to combine the others:

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[1] Proc. Amer. Phil. Soc., 43, 123. 1904.

[2] Journ. prakt. Chem., 69, 10. 1857.

1. Berzelius	79.7745,	± .0880
2. Schneider, 1850	79.320,	± .0068
3. Marchand	79.3205,	± .0073
4. Borch	79.293,	± .0108
5. Dumas	79.312,	± .0090
6. Bernoulli	79.480,	± .0560
7. Persoz	79.314,	± .0070
8. Roscoe	79.264,	± .0146
9. Waddell	79.339,	± .0069
10. Pennington and Smith	79.392,	± .0004
11. Schneider, 1896	79.311,	± .0018
12. Shinn	79.391,	± .0066
13. Hardin	79.320,	± .0185
14. Smith and Exner	79.3169,	± .0007
General mean	79.3706,	± .00034

In this combination only two values carry much weight; the tenth and the fourteenth. The series by Pennington and Smith is evidently much overvalued, and exerts an undue influence upon the general mean. In reality the series by Smith and Exner is by far the most trustworthy of all, and the figures given by Schneider, Marchand, Dumas and Persoz are in harmony with it. The other series are more doubtful. The weighted mean of twelve series, omitting Nos. 10 and 14, is 79.3160; a value almost identical with that of Smith and Exner. The latter, therefore, is abundantly confirmed.

In 1861 Scheibler[1] deduced the atomic weight of tungsten from analyses of barium metatungstate, $BaO.4WO_3.9H_2O$. In four experiments he estimated the barium as sulphate, getting closely concordant results, which were, however, very far too low. These, therefore, are rejected. But from the percentage of water in the salt a better result was attained. The percentages of water are as follows:

13.053
13.054
13.045
13.010
13.022

Mean, 13.0368, ± .0060

Hence W = 184.05.

The work of Zettnow,[2] published in 1867, was more complicated than any of the foregoing researches. He prepared the tungstates of silver and of iron, and from their composition determined the atomic weight of tungsten.

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[1] Journ. prakt. Chem., 83, 324.

[2] Poggend. Annalen, 130, 30.

In the case of the iron salt the method of working was this: The pure, artificial $FeWO_4$ was fused with sodium carbonate, the resulting sodium tungstate was extracted by water, and the thoroughly washed residual ferric oxide was dissolved in hydrochloric acid. This solution was then reduced by zinc, and titrated for iron with potassium permanganate. Corrections were applied for the drop in excess of permanganate needed to produce distinct reddening, and for the iron contained in the zinc. 11.956 grammes of the latter metal contained iron corresponding to 0.6 cc. of the standard solution. The permanganate was standardized by comparison with pure ammonium-ferrous sulphate, $Am_2Fe(SO_4)_2.6H_2O$, so that, in point of fact, Zettnow establishes directly only the ratio between that salt and the ferrous tungstate. From Zettnow's four experiments in standardizing I find that 1 cc. of his solution corresponds to 0.0365457 gramme of the double sulphate, with a probable error of ±.0000012.

Three sets of titrations were made. In the first a quantity of ferrous tungstate was treated according to the process given above; the iron solution was diluted to 500 cc., and four titrations made upon 100 cc. at a time. The second set was like the first, except that three titrations were made with 100 cc. each, and a fourth upon 150 cc. In the third set the iron solution was diluted to 300 cc., and only two titrations upon 100 cc. each were made. In sets one and two thirty grammes of zinc were used for the reduction of each, while in number three but twenty grammes were taken. Zettnow's figures, as given by him, are quite complicated; therefore I have reduced them to a common standard. After applying all corrections the following quantities of tungstate, in grammes, correspond to 1 cc. of permanganate solution:

.028301	First set
.028291	
.028311	
.028301	
.028367	Second set
.028368	
.028367	
.028367	
.028438	Third set
.028438	

Mean, .0283549, ± .0000115

Hence W = 184.41.

With the silver tungstate, Ag_2WO_4, Zettnow employed two methods. In two experiments the substance was decomposed by nitric acid, and the silver thus taken into solution was titrated with standard sodium chloride. In three others the tungstate was treated directly with common salt, and the residual silver chloride collected and weighed. Here again, on account of some complexity in Zettnow's figures, I am compelled to reduce his data to a common standard. To 100 parts of AgCl the following quantities of Ag_2WO_4 correspond:

By First Method.

161.665
161.603

Mean, 161.634, ± .021

By Second Method.

161.687
161.651
161.613

Mean, 161.650, ± .014
General mean from both series, 161.645, ± .012

Hence W = 183.64.

For tungsten hexchloride we have first, two analyses by Roscoe, published in the same paper with his results upon the trioxide. In one experiment the chlorine was determined as AgCl; in the other the chloride was reduced by hydrogen, and the residual tungsten estimated. By bringing both results into one form of expression we have for the percentage of chlorine in WCl_6:[1]

53.610
53.632

Mean, 53.621, ± .0074

Hence W = 184.02.

The investigation of tungsten hexchloride by Smith and Exner was much more elaborate. They prepared the substance from scrupulously pure materials, and further purified it by repeated sublimations. They decomposed the chloride by means of water, and weighed the residual tungsten trioxide. Their figures, with vacuum weights, are as follows, with a percentage column added by myself:

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[1] The actual figures are as follows:
19.5700 grm. WCl_6 gave 42.4127 grm. AgCl.
10.4326 grm. WCl_6 gave 4.8374 grm. tungsten.

Weight WCl_6.	*Weight* WO_3.	*Per cent.* WO_3.
3.18167	1.86085	58.487
2.66612	1.55903	58.476
3.52632	2.06244	58.487
1.52117	.88972	58.489
1.22299	.71523	58.482
2.28445	1.33603	58.484
3.25404	1.90237	58.493
3.37078	1.97133	58.483
7.76488	4.54082	58.479
2.08764	1.22114	58.494
2.80141	1.63859	58.492
3.24328	1.89681	58.484
4.97475	2.91262	58.489
3.04036	1.77838	58.492
4.31046	2.52133	58.493
3.21201	1.29381	58.490
2.70368	1.58135	58.489
3.60658	2.10934	58.486
2.63037	1.53835	58.484
3.41668	1.99808	58.480
3.49940	2.04675	58.489
3.86668	2.26145	58.486
3.40202	1.98970	58.486
3.20661	1.87533	58.483
3.26386	1.90909	58.492
6.73833	3.94031	58.476
7.37889	4.31643	58.497
	Mean,	58.4868, ± .0007

Hence W = 184.11.

The syntheses of WO_3 by Smith and Exner give W = 184.007, ± .0079. If we substitute that value in the hexchloride ratio we can compute an independent figure for the atomic weight of chlorine, namely, Cl = 35.454, ± .0028. This figure is good, and furnishes additional corroboration of Smith and Exner's determinations.

Smith and Exner also made a series of determinations of Taylor's ratio between WO_3 and CO_2. Their figures, with the ratio added, are as follows:

Weight WO_3.	*Weight* CO_2.	*Ratio.*
2.45645	.46775	19.041
2.72292	.51785	18.974
3.32953	.63288	19.008
3.97620	.75473	18.981
3.44944	.65489	18.985
3.41273	.64796	18.986
6.10309	1.16087	19.021

6.39735	1.21644	19.015
2.17450	.41332	19.008
1.57903	.29966	18.977
		Mean, 18.9996, ± .0046

Hence W = 183.60.

Combined with Taylor's mean, 19.0125, ± .0034, the general mean is 19.0073, ± .0027. This ratio, however, is affected by constant errors, as Smith and Exner have shown. There is not only a possibility of action of the sodium carbonate upon the glass bulb, but also a loss due to slight decomposition of the carbonate itself at the temperature employed in the experiments. Smith and Exner therefore discard the method as too inaccurate.

The work done by Smith and Desi[1] probably ought to be considered in connection with that of Pennington and Smith on the trioxide. Smith and Desi started with tungsten trioxide, freed from molybdenum by means of gaseous hydrochloric acid. This material was reduced in a stream of carefully purified hydrogen, and the water formed was collected in a calcium chloride tube and weighed. To the results found I add the percentage of water obtained from 100 parts of WO_3. Vacuum weights are given:

WO_3.	H_2O.	Per cent. H_2O.
.983024	.22834	23.228
.998424	.23189	23.226
1.008074	.23409	23.221
.911974	.21184	23.229
.997974	.23179	23.226
1.007024	.23389	23.226
		Mean, 23.226, ± .0008

Hence W = 184.70. This method is also criticized by Smith and Exner, and rejected.

Still another method for determining the atomic weight of tungsten was tested by Thomas,[2] also in Smith's laboratory. Sodium tungstate, $Na_2WO_4.2H_2O$, was dehydrated between 180° and 200°, and the percentage of water so determined. In this series of experiments the tungstate contained traces of carbonate and silicate. With purer material other determinations were made between 268° and 295°, and these were divided

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[1] Read before Amer. Phil. Soc., Nov. 2, 1894.

[2] Journ. Amer. Chem. Soc. 21, 373. 1899. Thomas cites some work on tungsten trioxide, but his figures appear in Hardin's series.

by the author into two series. I give below the percentage of water computed from Thomas' weights:

Preliminary.	*First Series.*	*Second Series.*
10.920	10.895	10.918
10.919	10.886	10.971
10.941	10.900	10.800
10.931	10.894	10.926
10.937	10.861	10.860
10.929	10.891	
10.926	10.878	Mean, 10.895, ± .0200
10.945		
10.924	Mean, 10.886, ± .0034	
10.935		
Mean, 10.931, ± .0020		

The general mean of the three series is 10.919, ± .0017. Hence W = 183.93, but with a very wide range of values in the individual experiments. This method, again, is rejected by the author himself as unsuited to exact atomic weight determinations.

The ratios, good and bad, rejecting nothing, from which to calculate the atomic weight of tungsten are now as follows:

(1). $WO_3:W::100:79.3706, \pm .00034$
(2). $BaW_4O_{13}.9H_2O:9H_2O::100:13.0368, \pm .0060$
(3). $WO_3:3H_2O::100:23.226, \pm .0008$
(4). $Am_2Fe(SO_4)_2.6H_2O:FeWO_4::0.0365457, \pm .0000012:0.0283549, \pm .0000115$
(5). $2AgCl:Ag_2WO_4::100:161.645, \pm .012$
(6). $WCl_6:6Cl::100:53.621, \pm .0074$
(7). $WCl_6:WO_3::100:58.4868, \pm .0007$
(8). $WO_3:CO_2::100:19.0073, \pm .0027$
(9). $Na_2WO_4.2H_2O:2H_2O::100:10.919, \pm .0017$

The values to use in reducing these ratios are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
S = 32.0667, ± .00075
N = 14.0101, ± .0001
C = 12.0038, ± .0002
Na = 23.0108, ± .00024
Ba = 137.363, ± .0025
Fe = 55.880, ± .0012
H = 1.00779, ± .00001

Hence,

From ratio 8	W = 183.510, ± .0329
" " 5	183.639, ± .0345
" " 9	183.934, ± .0491
" " 6	184.016, ± .0309
" " 2	184.052, ± .0819
" " 7	184.112, ± .0067
" " 4	184.409, ± .1236
" " 1	184.678, ± .0031
" " 3	184.700, ± .0080
General mean,	W = 184.575, ± .0026

This combination is evidently of very little significance. It includes data which are confessedly defective, and which do not tend to compensation of errors. The abnormally high value derived from ratio 1, which dominates the combination, is due to the excessive weight given to the determinations by Pennington and Smith, which Smith himself has discarded. If, in place of ratio 1 we take the determinations of Smith and Exner alone, namely, $WO_3 : W :: 100 : 79.3169, \pm .0007$, we have the more trustworthy value, $W = 184.075, \pm .0064$. This, combined with the value from ratio 7, also due to Smith and Exner, gives a general mean of $W = 184.092, \pm .0046$. This seems to be the most probable value now available, and it is checked by the fact, already pointed out, that the two ratios of Smith and Exner, combined, give a good value for the atomic weight of chlorine.

URANIUM.

The earlier attempts to determine the atomic weight of uranium were all vitiated by the erroneous supposition that uranous oxide was really the metal. The supposition, of course, does not affect the weighings and analytical data which were obtained, although these, from their discordance with each other and with later and better results, have now only a historical value.

For present purposes the determinations made by Berzelius,[1] by Arfvedson,[2] and by Marchand[3] may be left quite out of account. Berzelius employed various methods, while the others relied upon estimating the percentage of oxygen lost upon the reduction of U_3O_8 to UO_2. Rammelsberg's[4] results also, although very suggestive, need no full discussion. He analyzed the green chloride, UCl_4; effected the synthesis of uranyl sulphate from uranous oxide; determined the amount of residue left upon the ignition of the sodio and bario-uranic acetates; estimated the quantity of magnesium uranate formed from a known weight of UO_2, and attempted also to fix the ratio between the green and the black oxides. His figures vary so widely that they could count for little in the establishing of any general mean; and, moreover, they lead to estimates of the atomic weight which are mostly below the true value. For instance, twelve lots of U_3O_8 from several different sources were reduced to UO_2 by heating in hydrogen. The percentages of loss varied from 3.83 to 4.67, the mean being 4.121. These figures give values for the atomic

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[1] Schweigg. Journ., 22, 336. 1818. Poggend. Annalen, 1, 359. 1825.

[2] Poggend. Annalen, 1, 245. Berz. Jahr., 3, 120. 1822.

[3] Journ. prakt. Chem., 23, 497. 1841.

[4] Poggend. Annalen, 55, 318, 1842; 56, 125, 1842; 59, 9, 1843; 66, 91, 1845. Journ. prakt. Chem., 29, 324.

weight of uranium ranging from 185.74 to 235.84, or, in mean, 216.17. Such discordance is due partly to impurity in some of the material studied, and illustrates the difficulties inherent in the problem to be solved. Some of the uranoso-uranic oxide was prepared by calcining the oxalate, and retained an admixture of carbon. Many such points were worked up by Rammelsberg with much care, so that his papers should be scrupulously studied by any chemist who contemplates a redetermination of the atomic weight of uranium.

In 1841 and 1842 Peligot published certain papers[1] showing that the atomic weight of uranium must be somewhere near 240. A few years later the same chemist published fuller data concerning the constant in question, but in the time intervening between his earlier and his final researches other determinations were made by Ebelmen and by Wertheim. These investigations we may properly discuss in chronological order. For present purposes the early work of Peligot may be dismissed as only preliminary in character. It showed that what had been previously regarded as metallic uranium was in reality an oxide, but gave figures for the atomic weight of the metal which were merely approximations.

Ebelmen's[2] determinations of the atomic weight of uranium were based upon analyses of uranic oxalate. This salt was dried at 100°, and then, in weighed amount, ignited in hydrogen. The residual uranous oxide was weighed, and in some cases converted into U_3O_8 by heating in oxygen. The following weights are reduced to a vacuum standard:

10.1644	grm. oxalate gave	7.2939	grm. UO_2.			
12.9985	"	9.3312	"	Gain on oxidation,		.3685
11.8007	"	8.4690	"		"	.3275
9.9923	"	7.1731	"		"	.2812
11.0887	"	7.9610	"		"	.3105
10.0830	"	7.2389	"			
6.7940	"	4.8766	"			
16.0594	"	11.5290	"		"	.4531

Reducing these figures to percentages, we may present the results in two columns. Column A gives the percentages of UO_2 in the oxalate, while B represents the amount of U_3O_8 formed from 100 parts of UO_2:

A.	*B.*
71.924	......
71.787	103.949
71.767	103.867
71.621	103.920

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[1] Compt. Rend., 12, 735. 1841. Ann. Chim. Phys. (3), 55. 1842.

[2] Journ. prakt. Chem., 27, 385. 1842.

71.794	103.900
71.793	......
71.778	......
71.790	103.930
Mean, 71.782, ± .019	Mean, 103.913, ± .009

Hence U=237.70. Hence U=240.25.

Wertheim's[1] experiments were even simpler in character than those of Ebelmen. Sodio-uranic acetate, carefully dried at 200°, was ignited, leaving the following percentages of sodium uranate:

67.51508
67.54558
67.50927

Mean, 67.52331, ± .0076

Hence U=239.29.

The final results of Peligot's[2] investigations appeared in 1846. Both the oxalate and the acetate of uranium were studied and subjected to combustion analysis. The oxalate was scrupulously purified by repeated crystallizations, and thirteen analyses, representing different fractions, were made. Seven of these gave imperfect results, due to incomplete purification of the material; six only, from the later crystallizations, need to be considered. In these the uranium was weighed as U_3O_8, and the carbon as CO_2. From the ratio between the CO_2 and U_3O_8 the atomic weight of uranium may be calculated without involving any error due to traces of moisture possibly present in the oxalate. I subjoin Peligot's weighings, and give, in the third column, the U_3O_8 proportional to 100 parts of CO_2:

CO_2.	U_3O_8.	*Ratio.*
1.456	4.649	319.299
1.369	4.412	322.279
2.209	7.084	320.688
1.019	3.279	321.786
1.069	3.447	322.461
1.052	3.389	322.148
		Mean, 321.443, ± .338

Hence U=240.23.

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[1] Journ. prakt. Chem., 29, 209. 1843.

[2] Compt. Rend., 22, 487. 1846.

From the acetate, $UO_2(C_2H_3O_2)_2.2H_2O$, the following percentages of U_3O_8 were obtained:

5.061	grm. acetate gave	3.354	grm. U_3O_8.	66.2715 per cent.
4.601	"	3.057	"	66.4421 "
1.869	"	1.238	"	66.2386 "
3.817	"	2.541	"	66.5706 "
10.182	"	6.757	"	66.3622 "
4.393	"	2.920	"	66.4694 "
2.868	"	1.897	"	66.1437 "
				Mean, 66.3569, ± .038

Hence U = 239.73.

The acetate also yielded the subjoined percentages of carbon and of water. Assuming that the figures for carbon were calculated from known weights of dioxide, with C = 12 and O = 16, I have added a third column, in which the carbon percentages are converted into percentages of CO_2:

H_2O.	*C.*	*CO_2.*
21.60	11.27	41.323
21.16	11.30	41.433
21.10	11.30	41.433
21.20	11.10	40.700
Mean, 21.265, ± .187	Mean, 11.24	Mean, 41.222, ± .092

From these data we get the following values for the molecular weight of uranyl acetate:

From percentage of U_3O_8............	425.827, ± .1678
From percentage of CO_2............	426.993, ± .9530
From percentage of H_2O............	423.603, ± 3.7250
General mean	425.861, ± .1651

Hence U = 239.77.

In the posthumous paper of Zimmermann, edited by Krüss and Alibegoff,[1] the atomic weight of uranium is determined by two methods. First, UO_2, prepared by several methods, is converted into U_3O_8 by heating in oxygen. To begin with, U_3O_8 was prepared, and reduced to UO_2 by ignition in hydrogen. When the reduction takes place at moderate temperatures, the UO_2 is somewhat pyrophoric, but if the operation is performed over the blast lamp this difficulty is avoided. After weighing the UO_2, the oxidation is effected, and the gain in weight observed. The preliminary U_3O_8 was derived from the following sources: A, from uranium tetroxide; B, from the oxalate; C, from uranyl nitrate; D, by

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[1] Ann. Chem., 232, 299. 1886.

precipitation with mercuric oxide. The full data, lettered as indicated above, are subjoined:

	UO_2.	U_3O_8.	*Per cent. of Gain.*
A	8.9363	9.2872	3.927
	7.9659	8.2789	3.929
	12.4385	12.9270	3.927
B	12.8855	13.3913	3.925
	5.7089	5.9331	3.927
	9.6270	10.0051	3.928
C	13.1855	13.7036	3.929
	9.9973	10.3901	3.929
D	15.8996	16.5242	3.928
	7.4326	7.7245	3.927

Mean, 3.9276, ± .0003
Ebelmen found, 3.913, ± .009

General mean, 3.9276, ± .0003

In short, Ebelmen's mean vanishes when combined with Zimmermann's. From Zimmermann's mean U = 239.58.

Zimmermann's second method was essentially that of Wertheim, namely, the ignition of the double acetate $UO_2(C_2H_3O_2)_2.NaC_2H_3O_2$, the residue being sodium uranate, $Na_2U_2O_7$.

Double Acetate.	*Uranate.*	*Per cent. Uranate.*
4.272984	2.886696	67.557
5.272094	3.560770	67.540
2.912283	1.967428	67.556
3.181571	2.149309	67.555

Mean, 67.552, ± .0027
Wertheim found, 67.523, ± .0076

General mean, 67.549, ± .0025

From Zimmermann's figures U = 239.71.

An entirely different method for determining the atomic weight of uranium was adopted by Aloy.[1] Pure uranyl nitrate was ignited in a suitable apparatus, and the nitrogen evolved was collected and measured. The residual green oxide of uranium was reduced to uranous oxide, which was weighed. From this weight and that of the nitrogen, as computed from its volume, the atomic weight of the metal was calculated. Unfortunately, Aloy gives only the volumes of gas and the corresponding atomic weight, but not the weight of the oxide. His data, therefore, as

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[1] Ann. Chim. Phys. (7), 24, 418. Preliminary paper in Compt. Rend., 182, 551. 1901.

published, are radically defective. Assuming N = 14.04, Aloy gives the following values for uranium:

239.3
239.4
239.6
239.3
239.4
239.5
239.4
239.4

Mean, 239.412, ± .0235

If N = 14.0101, this reduces to U = 238.902.

The important memoir by Richards and Merigold[1] begins with a careful criticism of former determinations. In Aloy's work, they show that the residual oxide probably contained some unexpelled nitrogen, and they also point out the difficulty of exactly measuring small volumes of gas. Their own work was based upon careful analyses of uranous bromide by the best established methods, and their results, with vacuum weights, are as follows. First, analyses to determine the ratio $4AgBr : UBr_4$:

Preliminary Series.

Weight UBr_4.	*Weight AgBr.*	*Ratio.*
2.2058	2.9699	74.272
1.4418	1.9401	74.316
1.4050	1.8910	74.299
1.1749	1.5818	74.276

Mean, 74.291, ± .0070

Second Series.

Weight UBr_4.	*Weight AgBr.*	*Ratio.*
1.7999	2.4226	74.296
1.0662	1.4352	74.290
1.8551	2.4967	74.302

Mean, 74.296, ± .0029

General mean of both series, 74.295, ± .0027. Hence U = 238.424.

Second, measurements of the ratio $4Ag : UBr_4$:

Weight UBr_4.	*Weight Ag.*	*Ratio.*
1.7999	1.3918	129.322
1.0662	.8245	129.315
1.8551	1.4342	129.347

Mean, 129.328, ± .0066

Hence U = 239.397.

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[1] Proc. Amer. Acad., 37, 365. 1902.

The two ratios, combined, give the cross ratio Ag : Br : : 100 : 74.074.

Oechsner de Coninck,[1] in order to establish the molecular weight of uranyl oxide, reduced UO_2Br_2 by heating. His results were as follows:

UO_2Br_2.	UO_2.	Per cent. UO_2.
.737	.466	63.229
.900	.566	62.889
.720	.452	62.778
.818	.519	63.447
1.080	.681	63.056
		Mean, 63.080, ± .0805

Hence U = 241.1.

In a second brief paper[2] he gives three reductions of the chloride, UO_2Cl_2, by heating in hydrogen:

UO_2Cl_2.	UO_2.	Per cent. UO_2.
.523	.414	79.159
.5763	.456	79.098
1.048	.830	79.198
		Mean, 79.152, ± .0197

Hence U = 237.24.

These determinations are of no real importance, and are included in this discussion merely for the sake of completeness. Summing up, the following ratios are now available for uranium:

(1). Per cent. UO_2 from uranyl oxalate, 71.782, ± .019
(2). $6CO_2 : U_3O_8 : : 100 : 321.443$, ± .338
(3). Molecular weight of uranyl acetate, 425.861, ± .1650
(4). $3UO_2 : U_3O_8 : : 100 : 103.9276$, ± .0003
(5). Per cent. $Na_2U_2O_7$, from $UO_2.Na(C_2H_3O_2)_3$, 67.549, ± .0025
(6). N : U : : 14.04 : 239.412, ± .0235
(7). $4AgBr : UBr_4 : : 100 : 74.295$, ± .0027
(8). $4Ag : UBr_4 : : 100 : 129.328$, ± .0066
(9). $UO_2Br_2 : UO_2 : : 100 : 63.080$, ± .0805
(10). $UO_2Cl_2 : UO_2 : : 100 : 79.152$, ± .0197

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
C = 12.0038, ± .0002
N = 14.0101, ± .0001
Na = 23.0108, ± .00024
H = 1.00779, ± .00001

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[1] Bull. Acad. Belg., Classe des Sciences, 1907, 1041.

[2] *Ibid.*, 1908, 163. In another paper, Compt. Rend., 147, 1477, Oechsner de Coninck describes attempts to determine the atomic weight of chlorine by reductions of UO_2Cl_2. His results have no value and need not be considered here.

Hence,

From ratio	10	U = 237.244, ± .2665
" "	1	237.705, ± .1951
" "	8	238.397, ± .0285
" "	7	238.424, ± .0203
" "	6	238.902, ± .0235
" "	4	239.582, ± .0207
" "	5	239.663, ± .0279
" "	3	239.768, ± .1651
" "	2	240.228, ± .2975
" "	9	241.094, ± .6900
General mean,		U = 238.977, ± .0104

Ratios 1, 2, 3, 9 and 10 are evidently worthless; but their omission would only change the general mean by about 0.001, a negligible quantity. The final result is higher than the values obtained by Richards and Merigold, which are probably the best of all the separate determinations. It would hardly be safe, however, to reject the work of Zimmermann, at least until more evidence is available. The radio-active properties of uranium may possibly affect its atomic weight, but that possibility remains to be tested.

SELENIUM.

The atomic weight of this element was first determined by Berzelius,[1] who, saturating 100 parts of selenium with chlorine, found that 179 of chloride were produced. Hence Se=79.24. Further on these figures will be combined with similar results by Dumas.

We may omit, as unimportant for present purposes, the analyses of alkaline selenates made by Mitscherlich and Nitzsch,[2] and pass on to the experiments published by Sacc[3] in 1847. This chemist resorted to a variety of methods, some of which gave good results, while others were unsatisfactory. First, he sought to establish the exact composition of SeO_2, both by synthesis and by analysis. The former plan, according to which he oxidized pure selenium by nitric acid, gave poor results; better figures were obtained upon reducing SeO_2 with ammonium bisulphite and hydrochloric acid, and determining the percentage of selenium set free:

.6800 grm. SeO_2 gave	.4828 grm. Se.	71.000 per cent.
3.5227 "	2.5047 "	71.102 "
4.4870 "	3.1930 "	71.161 "
	Mean,	71.088, ± .032

Hence Se=78.68.

In a similar manner Sacc also reduced barium selenite, and weighed the resulting mixture of barium sulphate and free selenium. This process gave discordant results, and a better method was found in calcining $BaSeO_3$ with sulphuric acid, and estimating the resulting quantity of $BaSO_4$. In the third column I give the amounts of $BaSO_4$ equivalent to 100 of $BaSeO_3$:

.5573 grm. $BaSeO_3$ gave	.4929 grm. $BaSO_4$.	88.444
.9942 "	.8797 "	88.383
.2351 "	.2080 "	88.473
.9747 "	.8621 "	88.448
	Mean.	88.437, ± .013

Hence Se=78.59.

Still other experiments were made with the selenites of silver and lead; but the figures were subject to such errors that they need no further discussion here.

A few years after Sacc's work was published, Erdmann and Marchand made with their usual care a series of experiments upon the atomic

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[1] Poggend. Annalen, 8, 1. 1826.

[2] Poggend. Annalen, 9, 623. 1827.

[3] Ann. Chim. Phys. (3), 21, 119.

weight under consideration.[1] They analyzed pure mercuric selenide, which had been repeatedly sublimed and was well crystallized. Their method of manipulation has already been described in the chapter upon mercury. These percentages of Hg in HgSe were found:

71.726
71.731
71.741

Mean, 71.7327, ± .003

Hence Se=78.88.

The next determinations were made by Dumas,[2] who returned to the original method of Berzelius. Pure selenium was converted by dry chlorine into $SeCl_4$, and from the gain in weight the ratio between Se and Cl was easily deducible. I include Berzelius' single experiment, which I have already cited, and give in a third column the quantity of chlorine absorbed by 100 parts of selenium:

1.709	grm. Se absorb	3.049	grm. Cl.	178.409
1.810	"	3.219	"	177.845
1.679	"	3.003	"	178.856
1.498	"	2.688	"	179.439
1.944	"	3.468	"	178.395
1.887	"	3.382	"	179.226
1.935	"	3.452	"	178.398
				179.000—Berzelius
			Mean,	178.696, ± .125

Dumas' figures alone give Se=79.39.

The question may here be properly asked, whether it would be possible thus to form $SeCl_4$, and be certain of its absolute purity? A trace of oxychloride, if simultaneously formed, would increase the apparent atomic weight of selenium. In point of fact, this method gives a higher value for Se than any of the other processes which have been adopted, and that value has the largest probable error of any one in the entire series. A glance at the table which summarizes the discussion at the end of this chapter will render this point sufficiently clear.

Still later, Ekman and Pettersson[3] investigated several methods for the determination of this atomic weight, and finally decided upon the two following:

First, pure silver selenite, Ag_2SeO_3, was ignited, leaving behind metallic

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[1] Journ. prakt. Chem., 55, 202. 1852.

[2] Ann. Chem. Pharm., 113, 32. 1860.

[3] Ber. Deutsch. chem. Gesell., 9, 1210, 1876. Published in detail by the society at Upsala.

silver, which, however, sometimes retained minute traces of selenium. The data obtained were as follows:

Ag_2SeO_3.	Ag.	Per cent. Ag.
5.2102	3.2787	62.929
5.9721	3.7597	62.954
7.2741	4.5803	62.967
7.5390	4.7450	62.939
6.9250	4.3612	62.978
7.3455	4.6260	62.978
6.9878	4.3992	62.955
		Mean, 62.957, ± .0048

Hence Se = 78.95.

Secondly, a warm aqueous solution of selenious acid was mixed with HCl and reduced by a current of SO_2. The reduced Se was collected upon a glass filter, dried and weighed.

SeO_2.	Se.	Per cent. Se.
11.1760	7.9573	71.199
11.2453	8.0053	71.185
24.4729	17.4232	71.193
20.8444	14.8383	71.187
31.6913	22.5600	71.191
		Mean, 71.191, ± .0016
		Sacc found, 71.088, ± .0320
		General mean, 71.1907, ± .0016

Ekman and Pettersson's series alone give Se = 79.076.

Lenher,[1] in order to determine the atomic weight of selenium, studied two of its compounds. First, silver selenite was heated in a stream of gaseous hydrochloric acid, and so transformed into silver chloride. In a second series of experiments the silver chloride was afterwards reduced to metal by heating in hydrogen. Two ratios were thus determined. For convenience I now treat the two series as one. Lenher's data, with vacuum weights, and with the corresponding percentages added by myself, are as follows:

Ag_2SeO_3.	AgCl.	Ag.	Per cent. Ag.	Per cent. AgCl.
.98992	.82715	......	.....	83.557
1.59912	1.33600	......	.....	83.560
2.70573	2.26087	......	.....	83.559
.26204	.21897	.16480	62.891	83.564
.58078	.48522	.36534	62.906	83.546
.70614	.58999	.44417	62.901	83.551

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[1] Journ. Amer. Chem. Soc., 20, 355. 1898. Thesis, University of Pennsylvania.

.80811	.67532	.50821	62.889	83.568
.98396	.82232	.61882	62.891	83.572
1.29685	1.08350	.81562	62.892	83.549
1.63103	1.36288	1.02588	62.898	83.559
2.00162	1.67234	1.25884	62.891	83.549
			Mean, 62.895,	83.558,
			± .0014	± .0017

From Ag ratio, Se = 79.288.
From AgCl ratio, Se = 79.328.

Secondly, ammonium bromoselenate was studied. From this salt the selenium was precipitated by hydroxylamine hydrochloride, and then collected and weighed in a Gooch crucible. The vacuum weights and percentages of selenium follow:

Am_2SeBr_6.	*Se.*	*Per cent. Se.*
1.00059	.13324	13.3161
1.50153	.20022	13.3344
2.00059	.26649	13.3209
2.00126	.26657	13.3201
3.00125	.39958	13.3138
4.00216	.53346	13.3293
5.00218	.66656	13.3254
5.03001	.66998	13.3196
		Mean, 13.3224, ± .0017

Hence Se = 79.25.

Steiner's[1] determinations, two in number, were made incidentally to his work on tellurium. Phenyl selenide was burned in a combustion tube, and the carbon dioxide so produced was weighed. To his figures I add the ratio $(C_6H_5)_2Se : 12CO_2 :: 100 : x$:

Selenide.	CO_2.	*Ratio.*
.2812	.6375	226.707
.5371	1.2158	226.365
		Mean, 226.536, ± .0485

Hence Se = 78.97. This determination is of trifling significance.

Julius Meyer[2] analyzed silver selenite electrolytically. The silver was precipitated from a solution of the salt in potassium cyanide. With vacuum weights the data are as follows:

Ag_2SeO_3.	*Ag.*	*Per cent. Ag.*
.5152	.3241	62.907
.5237	.3295	62.915
1.6964	1.0672	62.910
1.8793	1.1826	62.928
2.1460	1.3503	62.922
		Mean, 62.9164, ± .0082

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[1] Ber. Deutsch. chem. Ges., 34, 570. 1901.

[2] Ber. Deutsch. chem. Ges., 35, 1591. 1902.

From the solutions 0.0002 gramme of silver was recovered, to be added to the sum of the silver weights given above. This raises the percentage to 62.9193. Hence Se = 79.155.

Combining Meyer's series with its predecessors we have—

Ekman and Pettersson	62.957, ± .0048
Lenher	62.895, ± .0014
Meyer	62.9193, ± .0082
General mean	62.9003, ± .0013

There are now eight ratios from which to deduce the atomic weight of selenium:

(1). SeO_2 : Se : : 100 : 71.1907, ± .0016
(2). $BaSeO_3$: $BaSO_4$: : 100 : 88.437, ± .013
(3). HgSe : Hg : : 100 : 71.7327, ± .003
(4). Se : 4Cl : : 100 : 178.696, ± .125
(5). Ag_2SeO_3 : 2Ag : : 100 : 62.9003, ± .0013
(6). Ag_2SeO_3 : 2AgCl : : 100 : 83.558, ± .0017
(7). Am_2SeBr_6 : Se : : 100 : 13.3224, ± .0017
(8). $C_{12}H_{10}Se$: $12CO_2$: : 100 : 226.536, ± .0486

The atomic weights used in reducing these ratios are as follows:

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
N = 14.0101, ± .0001
C = 12.0038, ± .0002
Ba = 137.363, ± .0025
Hg = 200.054, ± .0017
H = 1.00779, ± .00001

Hence,

From ratio 2	Se = 78.587, ± .0388
" " 3	78.883, ± .0124
" " 8	78.972, ± .0501
" " 1	79.075, ± .0047
" " 7	79.248, ± .0102
" " 5	79.259, ± .0052
" " 6	79.328, ± .0070
" " 4	79.373, ± .0555
General mean,	Se = 79.176, ± .0029

This mean is slightly lower than the values obtained by Lenher, but near that given by Meyer. In default of more evidence it seems to be as trustworthy as any value which might be arbitrarily chosen.

TELLURIUM.

Particular interest attaches to the atomic weight of tellurium on account of its relations to the periodic system. According to that system, tellurium should lie between antimony and iodine, having an atomic weight greater than 120 and less than 126. Theoretically, Mendeléef assigns it a value of Te=125, but all of the best determinations lead to a mean number higher than is admissible under the currently accepted hypotheses. Whether theory or experiment is at fault remains to be discovered.

The first, and for many years the only, determinations of the constant in question were made by Berzelius.[1] By means of nitric acid he oxidized tellurium to the dioxide, and from the increase in weight deduced a value for the metal. He published only his final results, from which, if O=100, Te=802.121. The three separate experiments give Te=801.74, 801.786 and 802.838, whence we can calculate the following percentages of metal in the dioxide:

80.057
80.036
80.034

Mean, 80.042, ± .005

Hence Te=128.34.

The next determinations were made by von Hauer,[2] who resorted to the analysis of the well crystallized double salt $TeBr_4.2KBr$. In this compound the bromine was estimated as silver bromide, the values assumed for Ag and Br being respectively 108.1 and 80. Recalculating, we get from von Hauer's analyses, for 100 parts of the salt, the quantities of AgBr which are put in the third column:

2.000	grm. K_2TeBr_6 gave	69.946	per cent. Br.	164.460
6.668	"	69.8443	"	164.221
2.934	"	69.9113	"	164.379
3.697	"	70.0163	"	164.626
1.000	"	69.901	"	164.355
				Mean, 164.408, ± .045

Hence Te=127.64.

Dumas,[3] by a method for which he gives absolutely no particulars, found Te=129.

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[1] Poggend. Annalen, 28, 395. 1833.

[2] Sitzungsb. Wien. Akad., 25, 142.

[3] Ann. Chim. Phys. (3), 55, 129. 1859.

In 1879, with direct reference to Mendeléef's theory, the subject of the atomic weight of tellurium was taken up by Wills.[1] The methods of Berzelius and von Hauer were employed, with various rigid precautions in the way of testing balance and weights, and to ensure purity of material. In the first series of experiments tellurium was oxidized by nitric acid to form TeO_2. The results gave figures ranging from Te = 126.64 to 129.66:

2.21613	grm. Te gave	2.77612	grm. TeO_2.	79.828	per cent. Te.
1.45313	“	1.81542	“	80.044	“
2.67093	“	3.33838	“	80.007	“
4.77828	“	5.95748	“	80.207	“
2.65029	“	3.31331	“	79.989	“
				Mean, 80.015, ± .041	

Hence Te = 128.12.

In the second series tellurium was oxidized by aqua regia to TeO_2, with results varying from Te = 128.10 to 128.32:

2.85011	grm. Te gave	3.56158	grm. TeO_2.	80.024	per cent. Te.
3.09673	“	3.86897	“	80.040	“
5.09365	“	6.36612	“	80.012	“
3.26604	“	4.08064	“	80.037	“
				Mean, 80.028, ± .004	

Hence Te = 128.22.

By von Hauer's process, the analysis of $TeBr_4.2KBr$, Wills' figures give results ranging from Te = 126.36 to 127.90. Reduced to a common standard, 100 parts of the salt yield the quantities of AgBr given in the third column:

1.70673	grm. K_2TeBr_6 gave	2.80499	grm. AgBr.	164.349
1.75225	“	2.88072	“	164.398
2.06938	“	3.40739	“	164.657
3.29794	“	5.43228	“	164.717
2.46545	“	4.05742	“	164.571
				Mean, 164.538, ± .048

Hence Te = 127.10.

Combined with von Hauer's mean, 164.408, ± .045, this gives a general mean of 164.468, ± .0324.

The next determinations in order of time were those of Brauner.[2] This chemist tried various unsuccessful methods for determining the

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[1] Journ. Chem. Soc., Oct., 1879, p. 704.

[2] Journ. Chem. Soc., 55, 382. 1889.

atomic weight of tellurium, among them being the synthetic preparation of silver, copper and gold tellurides, and the basic sulphate, Te_2SO_7. None of these methods gave sufficiently concordant results, and they were therefore abandoned. The oxidation of tellurium to dioxide by means of nitric acid was also unsatisfactory, but a series of oxidations with aqua regia gave data as follows. The third column contains the percentage of tellurium in the dioxide:

Te.	*TeO_2.*	*Per cent. Te.*
2.3092	2.9001	79.625
2.8153	3.5332	79.681
4.0176	5.0347	79.798
3.1613	3.9685	79.660
.8399	1.0526	79.793

Mean, 79.711, ± .0239

Hence Te = 125.72.

In a single analysis of the dioxide, by reduction with SO_2, 2.5489 grammes TeO_2 gave 2.0374 of metal. If we give this experiment the weight of one observation in the synthetic series, the percentage of tellurium found by it becomes—

79.932, ± .0534

Hence Te = 127.46.

Brauner's best results were obtained from analyses of tellurium tetrabromide, prepared from pure tellurium and pure bromine, and afterwards sublimed in a vacuum. This compound was titrated with standard solutions of silver, and three series of experiments, made with samples of bromide of different origin, gave results as follows. The $TeBr_4$ equivalent to 100 parts of silver appears in the third column:

First Series.

$TeBr_4$.	*¼Ag.*	*Ratio.*
2.14365	2.06844	103.636
1.76744	1.70531	103.643
1.47655	1.42477	103.634
1.23354	1.19019	103.642

Second Series.

$TeBr_4$.	*¼Ag.*	*Ratio.*
3.07912	2.97064	103.651
5.47446	5.28157	103.652
3.30927	3.19313	103.637
7.26981	7.01414	103.645
3.52077	3.39667	103.654

Third Series.

$TeBr_4$.	$4Ag$.	*Ratio.*
2.35650	2.27363	103.645
1.51931	1.46564	103.662
1.43985	1.38942	103.630

Mean of all as one series, 103.644, ± .0018

Hence Te=127.57. A reduction of the weighings to a vacuum raises this by 0.07 to 127.64.

Still another series of analyses, made with fractionated material, gave values for tellurium running up to as high as 137. These experiments led Brauner to believe that he had found in tellurium a higher homologue of that element, a view which he has since abandoned.[1] Brauner also made a series of analyses of tellurium dibromide, but the results were unsatisfactory.

In the series of determinations by Gooch and Howland[2] an alkaline solution of tellurium dioxide was oxidized by means of standard solutions of potassium permanganate. This was added in excess, the excess being measured, after acidification with sulphuric acid, by back titration with oxalic acid and permanganate. Two series are given, varying in detail, but for present purposes they may be treated as one. The ratio $TeO_2 : O :: 100 : x$ is given in the third column:

TeO_2 taken.	*O required.*	*Ratio*
.1200	.01202	10.017
.0783	.00785	10.026
.0931	.00940	10.097
.1100	.01119	10.149
.0904	.00909	10.055
.1065	.01078	10.122
.0910	.00915	10.055
.0910	.00910	10.000
.0911	.00924	10.143
.0913	.00915	10.022
.0912	.00915	10.033
.0914	.00923	10.098

Mean, 10.068, ± .0100

Hence Te=126.92.

In Staudenmaier's[3] determinations of the atomic weight of tellurium.

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[1] Journ. Chem. Soc., 67, 549. 1895.

[2] Amer. Journ. Sci., 58, 375. 1894. Some misprints in the original publication have been kindly corrected by Professor Gooch; hence the differences between these data and the figures formerly given.

[3] Zeitsch. anorg. Chem., 10, 189. 1895.

crystallized telluric acid, H_6TeO_6 was the starting point. By careful heating in a glass bulb this compound can be reduced to TeO_2, and by heating in hydrogen, to metal. In the latter case finely divided silver was added to prevent volatilization of tellurium. The telluric acid was fractionally crystallized, but the different fractions gave fairly constant results. I therefore group Staudenmaier's data so as to bring them into series more suitable for the present discussion:

First.—H_6TeO_6 to TeO_2.

H_6TeO_6.	*Loss in Weight.*	*Per cent. TeO_2.*
1.7218	.5260	69.451
2.8402	.8676	69.453
4.0998	1.2528	69.442
3.0916	.9450	69.433
1.1138	.3405	69.429
4.9843	1.5236	69.432
4.6716	1.4278	69.437
		Mean, 69.440, ± .0024

Hence Te=127.16.

Second.—H_6TeO_6 to Te.

H_6TeO_6.	*Loss in Weight.*	*Per cent. Te.*
1.2299	.5471	55.517
1.0175	.4526	55.518
2.5946	1.1549	55.488
		Mean, 55.508, ± .0068

Hence Te=127.31.

Staudenmaier also gives four reductions of TeO_2 to Te, in presence of finely divided silver. The data are as follows:

TeO_2.	*Loss in Weight.*	*Per cent. Te.*
.9171	.1839	79.948
1.9721	.3951	79.966
2.4115	.4835	79.950
1.0172	.2041	79.935
		Mean, 79.950, ± .0043

Hence Te=127.60.

Chikashige[1] resorted to Brauner's method, giving the ratio between silver and $TeBr_4$. In all essential particulars the work resembles that of Brauner, except that the tellurium, instead of being extracted from metallic tellurides, was derived from Japanese native sulphur, in which

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[1] Journ. Chem. Soc., 69, 881. 1896.

it exists as an impurity. This difference of origin in the material studied gives the chief interest to the investigation. The data are as follows:

$TeBr_4$.	*Ag.*	*Ratio.*
4.1812	4.0348	103.628
4.3059	4.1547	103.639
4.5929	4.4319	103.633
	Mean,	103.633, ± .0023
	Brauner found,	103.644, ± .0018
	General mean,	103.640, ± .0014

From Chikashige's mean, Te = 127.42.

Metzner[1] determined the atomic weight of tellurium by two methods, using tellurium which had been prepared from the hydride. First, tellurium was treated with sulphuric acid and converted into the basic sulphate, Te_2SO_7. I give his weights, and also the percentage of tellurium in the compound:

Te.	*Sulphate.*	*Per cent. Te.*
790.2	1245.0	63.982
414.3	647.5	63.985
1098.3	1717.0	63.966
	Mean,	63.978, ± .0040

Hence Te = 127.94.

Secondly, Metzner prepared tellurium dioxide by hydrolysis of the tetrachloride, and reduced it to tellurium by heating in a current of carbon monoxide. The reduction was effected in presence of silver, in order to avoid volatilization. His data follow, with the usual percentage of Te in TeO_2 stated in the third column:

TeO_2.	*Loss.*	*Per cent. Te.*
743.2	148.8	79.978
1106.7	221.3	80.004
988.5	197.0	80.073
1312.5	262.5	79.962
	Mean,	80.004, ± .0165

Hence Te = 128.032.

The determinations by Heberlein[2] represent three distinct methods, starting with crystallized telluric acid, H_6TeO_6. First, the acid was treated with hydrochloric acid, by which chlorine was liberated. The

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[1] Compt. Rend., 126, 1716. 1898. Metzner fails to state what his weights mean. Are they milligrammes?

[2] Inaug. Diss., Basel. Printed at Strassburg, 1898.

latter was distilled off and collected in a solution of potassium iodide. Iodine was set free and determined by titration with a tenth normal thiosulphate solution. If W = the weight of telluric acid, and n the number of cubic centimetres of the thiosulphate solution, the atomic weight of tellurium is given by the subjoined formula:

$$Te = \frac{20000\,W}{n} - H_6O_6$$

The first term on the right of the equation obviously represents the molecular weight of H_6TeO_6. The figures are as follows:[1]

Weight H_6TeO_6.	*Vol. thiosulphate, cc.*	*Mol. W. H_6TeO_6.*
.22911	20.00	229.100
.5736	50.02	229.348
.4038	35.21	229.367
.4393	38.30	229.400
.32331	28.22	229.135
		Mean, 229.270, ± .0425

Hence Te = 127.223.

Secondly, Heberlein employed Staudenmaier's method of reducing H_6TeO_6 to TeO_2 by careful heating in a glass bulb:

Weight Acid.	*Loss.*	*Per cent. TeO_2.*
1.35236	.41431	69.364
1.76859	.54122	69.398
		Mean, 69.381, ± .0115

Hence Te = 126.72.

Finally, tellurium dioxide was reduced to tellurium by heating in a current of hydrogen in presence of silver. Heberlein's two experiments are as follows:

TeO_2.	*Loss.*	*Per cent. Te.*
1.35908	.27353	79.874
1.94038	.39050	79.875
		Mean, 79.8745, ± .0034

Hence Te = 127.002. Heberlein's determinations assign low values to the atomic weight of tellurium.

Steiner's determination[2] of the atomic weight was made by combustion

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[1] The formula given by Heberlein probably involves the old atomic weights of chlorine and iodine. With modern atomic weights the value for Te would be raised. The data as printed are, however, incomplete. A correction would be uncertain, and the probable error of the determinations is so high that the change could exert no appreciable effect upon the final combination of values.

[2] Ber. Deutsch. chem. Ges., 34, 570. 1901.

of phenyl telluride, $(C_6H_5)_2Te$. I give his weights, and also the ratio $C_{12}H_{10}Te : 12CO_2 :: 100 : x$:

Weight telluride.	*Weight CO_2.*	*Ratio.*
.2925	.5512	188.440
.2559	.4811	188.003
.23065	.4341	188.207
.2140	.4031	188.364
.2578	.4849	188.092
		Mean, 188.221, ± .0549

Hence Te=126.42, a very low value. This determination only claims a rough approximation to the truth, and is not entitled to much consideration.

Pellini,[1] in his determinations of this atomic weight, returned to the time-honored methods with the dioxide. First, carefully purified tellurium was oxidized by nitric acid. Secondly, tellurium dioxide was reduced by hydrogen in presence of metallic silver. The data are as follows:

Oxidation Series.

Weight Te.	*Weight TeO_2.*	*Per cent. Te.*
1.0679	1.3353	79.968
1.5469	1.9354	79.926
2.2386	2.7980	80.007
2.4522	3.0665	79.967
2.0977	2.6239	79.945
2.0442	2.5575	79.929
2.0434	2.5556	79.957
		Mean, 79.957, ± .0071

Hence Te=127.657.

Reduction Series.

Weight TeO_2.	*Loss.*	*Per cent. Te.*
1.4680	.2944	79.945
1.9968	.3993	80.000
1.9575	.3932	79.913
		Mean, 79.953, ± .0171

Hence Te=127.625.

Koethner,[2] after some preliminary, but inconclusive experiments with tellurium sulphate and telluric acid, finally resorted to analyses of the

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[1] Ber. Deutsch. chem. Ges., 34, 3807. 1901. Gazz. Chim. Ital., 32. 131. 1902. In Gazz. Chim. Ital., 33 (2), 35, Pellini discusses the possibility of an admixture in ordinary tellurium, of an element having a higher atomic weight.

[2] Liebig's Annalen, 319, 1. 1901. Thesis, Halle. 1901. For a critical discussion of the subject, see Koethner, Zeitsch. anorg. Chem., 34, 402, 1903. See also Seubert, Zeitsch. anorg. Chem., 33, 247; and 35, 206.

basic nitrate, Te_2HNO_7. This compound was reduced by careful heating to TeO_2. In series II, as given below, the tellurium was purified by distillation in a vacuum; in series I that precaution was not taken. Weights not reduced to a vacuum:

Series I.

Nitrate.	*TeO_2.*	*Per cent. TeO_2.*
2.9373	2.4522	83.485
2.7982	2.3361	83.486
2.8554	2.3840	83.491
		Mean, 83.487, ± .0014

Hence Te = 127.30.

Series II.

Nitrate.	*TeO_2.*	*Per cent. TeO_2.*
5.30270	4.42824	83.510
6.00600	5.01543	83.507
5.58039	4.65990	83.505
28.66904	23.94259	83.513
3.83859	3.20560	83.510
5.85449	4.88930	83.514
25.65029	21.412146	83.513
		Mean, 83.5103, ± .0009

Hence Te = 127.57.

In 1902 Scott[1] published a preliminary note on the atomic weight of tellurium. Analyses of trimethyl tellurium iodide and bromide were made, the ratio with silver iodide being determined in the first case and the titration ratio with silver in the second. I subjoin Scott's figures, with the ratio to 100AgI and 100Ag, respectively. Vacuum weights are given:

$(CH_3)_3TeI$.	*AgI.*	*Ratio.*
1.7461	1.3688	127.564
6.6425	5.20575	127.570
8.0628	6.3181	127.614
		Mean, 127.583, ± .0105

Hence Te = 127.56.

$(CH_3)_3TeBr$.	*Ag.*	*Ratio.*
2.4294	1.0373	234.204
6.8424	2.9201	234.321
		Mean, 234.263, ± .0391

Hence Te = 127.72.

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[1] Proc. Chem. Soc., 18, 112. 1902.

Gutbier's[1] determinations began with telluric acid, H_6TeO_6. First, the acid was dehydrated by heating in a stream of dry air, and the water was collected in a calcium chloride tube and weighed:

H_6TeO_6.	H_2O.	Per cent. H_2O.
.4937	.1162	23.537
.9910	.2335	23.562
		Mean, 23.550, ± .0083

Hence Te = 127.45.

Secondly, telluric acid was reduced to tellurium by precipitation with hydrazin hydrate. Gutbier's data are as follows:

H_6TeO_6.	Te.	Per cent. Te.
.9380	.5204	55.480
.4963	.2754	55.491
1.0485	.5829	55.594
.8865	.4915	55.443
.4339	.2411	55.566
.3492	.1937	55.470
		Mean, 55.507, ± .0165

Hence Te = 127.31. Staudenmaier found 55.508, ± .0068 per cent. The general mean of both series is 55.5079, ± .0067.

Finally, tellurium dioxide was reduced to tellurium by the same process:

TeO_2.	Te.	Per cent. Te.
.1662	.13287	79.946
.3136	.2507	79.942
.2799	.2238	79.957
		Mean, 79.948, ± .0031

Hence Te = 127.585. All of Gutbier's weights were reduced to a vacuum standard.

In a later memoir Gutbier[2] gives two more series of reductions of tellurium dioxide. In series I the oxide was reduced by hydrogen, and in series II by hydrazin. Vacuum weights are given:

Series I.

TeO_2.	Te.	Per cent. Te.
2.99688	2.39585	79.944
1.30740	1.04527	79.950
2.04325	1.63380	79.955
2.61725	2.09249	79.949
3.61725	2.89222	79.956
		Mean, 79.951, ± .0015

Hence Te = 127.609.

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[1] Liebig's Annalen, 320, 52. 1902.

[2] Liebig's Annalen, 342, 266. 1905.

Series II.

TeO_2.	*Te.*	*Per cent. Te.*
1.90601	1.52390	79.952
1.03532	.82784	79.959
2.2200	1.77480	79.945

Mean, 79.952, ± .0031

Hence Te = 127.617.

In a more extended memoir, which includes the results of the last mentioned investigation, Gutbier[1] gives a series of analyses of basic tellurium nitrate, like those of Koethner. His figures are as follows, not reduced to a vacuum standard:

Nitrate.	*TeO_2.*	*Per cent. TeO_2.*
4.70704	3.92380	83.360
6.23210	5.20285	83.484
5.65043	4.71132	83.379
2.86977	2.39211	83.355
4.43213	3.69833	83.443
9.25691	7.73205	83.505
7.09070	5.91930	83.481
12.2400	10.2216	83.508

Mean, 83.439, ± .0156

Hence Te = 126.74.

Gallo's[2] investigation was an attempt to determine the electrochemical equivalent of tellurium in terms of silver. Silver and tellurium were thrown down by the same current, but in different receptacles, and so were directly compared. In the third column I give the ratio 4Ag : Te : : 100 : x:

Weight Ag.	*Weight Te.*	*Ratio.*
.74117	.218412	29.469
1.03801	.304514	29.336
.91704	.27256	29.722
1.041101	.307117	29.499
1.09064	.321952	29.519
1.16302	.34582	29.666
.968903	.28646	29.565
1.518712	.44767	29.477
.906561	.26836	29.534
.995511	.29586	29.720
.86596	.25656	29.627
1.11282	.328318	29.503

Mean, 29.553, ± .0221

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[1] Sitzungsb. phys. med. Soz. Erlangen, 37, 270. 1906. Gutbier regards these determinations as unsatisfactory.

[2] Atti Acad. Lincei (5), 14, 23, 1905. Also Gazz. Chim. Ital., 35, 245. See also Pellini, Gazz. Chim. Ital., 34, 132, on the electrolytic determination of tellurium.

Hence Te=127.53. All of Gallo's weights are on a vacuum basis. Gallo also made a series of electrolytic analyses of tellurium dioxide as follows. The precipitation was effected in a hydrofluoric acid solution:

Weight TeO_2.	*Weight Te.*	*Per cent. Te.*
.4624	.3694	79.888
.7429	.5938	79.930
.7995	.6390	79.925
.9610	.7664	79.750
1.0043	.8025	79.906
1.9891	1.5890	79.885
		Mean, 79.881, ± .0220

Hence Te=127.053.

Two determinations by Lenher,[1] although not next in chronological order, may be conveniently inserted here. One was by reduction of TeO_2, the other by oxidation of Te:

TeO_2.	*Te.*	*Per cent. Te.*
.85635	.6845	79.932
.2119	.1694	79.943
		Mean, 79.938, ± .0037

Hence Te=127.50.

The percentage of tellurium in the dioxide is now fixed by 15 series of determinations, which, arranged in the order of ascending magnitude may be combined, as usual, into a general mean:

Brauner, oxidation	79.711,	± .0239
Heberlein	79.8745,	± .0034
Gallo	79.881,	± .0220
Brauner, reduction	79.932,	± .0534
Lenher	79.938,	± .0037
Gutbier, 1902	79.948,	± .0031
Staudenmaier	79.950,	± .0043
Gutbier, 1905, 1	79.951,	± .0015
Gutbier, 1905, 2	79.952,	± .0031
Pellini, reduction	79.953,	± .0171
Pellini, oxidation	79.957,	± .0071
Metzner	80.004,	± .0165
Wills, 1	80.015,	± .0410
Wills, 2	80.028,	± .0040
Berzelius	80.042,	± .0050
General mean,	79.9498,	± .0010

The determinations by Staudenmaier, Pellini and Gutbier are in close agreement, and very near the general mean of all.

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[1] Journ. Amer. Chem. Soc., 30, 741. 1908.

Norris,[1] in the course of an investigation which proved the unity of tellurium as an element, made a series of atomic weight determinations by Koethner's method. The element itself was purified by various modes of fractionation, and different fractions were found to be identical. The basic nitrate was reduced by heating to TeO_2, which was fused before weighing; a precaution which eliminated the possibility of contamination by enclosed gases. The uncorrected data are as follows:

Nitrate.	*TeO_2.*	*Per cent. TeO_2.*
2.28215	1.90578	83.508
2.35429	1.96615	83.513
1.86853	1.56042	83.512
1.77348	1.48110	83.514
2.31048	1.92938	83.506
2.14267	1.78936	83.511
2.35523	1.96676	83.506
2.18860	1.82780	83.515
3.29158	2.74881	83.510
3.27516	1.89993	83.508
2.53164	2.11410	83.507
2.01327	1.68121	83.506

Mean, 83.5097, ± .0006

A vacuum correction to the weights reduces this mean by 0.0074 to 83.5023. Hence Te = 127.48. Assuming the same correction to previous series of determinations, the final value for the percentage of TeO_2 is given by the subjoined combination:

Koethner, 1	83.480,	± .0014
Koethner, 2	83.5003,	± .0009
Gutbier	83.432,	± .0156
Norris	83.5023,	± .0006
General mean	83.5000,	± .00047

The investigation by Baker and Bennett[2] was also intended to determine the definiteness of tellurium as an element. Different preparations from different sources were studied by several methods, and all gave sensibly the same atomic weight. The results obtained by two methods are given in detail, with vacuum weights throughout. First, tellurium dioxide was heated with sulphur in tubes of glass, the two ends of the tube being packed with silver leaf to avoid loss of tellurium. Sulphur dioxide was expelled, and from its amount, as measured by the loss in weight of the apparatus, the atomic weight of tellurium was com-

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[1] Journ. Amer. Chem. Soc., 28, 1675. 1906.

[2] Journ. Chem. Soc., 91, 1849. 1907.

puted. The determinations by this method may be arranged in three principal series, representing differences in the source of the initial substance, as follows: 1. Fractional crystallization of telluric acid from barium tellurate. 2. Fractional crystallization of telluric acid produced by oxidation of the element. 3. Tellurium dioxide prepared from tellurium hydride.

Series I.

Weight TeO_2.	*Loss. SO_2.*	*Per cent. SO_2.*
1.51509	.60838	40.155
1.09875	.44074	40.113
1.02150	.40993	40.130
.90835	.36472	40.152
1.00702	.40451	40.169
1.01515	.40733	40.125

Series II.

Weight TeO_2.	*Loss. SO_2.*	*Per cent. SO_2.*
1.56837	.62938	40.130
1.07852	.43257	40.108
1.72627	.69246	40.142
2.09253	.83927	40.108
.83335	.33465	40.157
1.15372	.46284	40.117
.68618	.67661	40.127
.90835	.36472	40.152

Series III.

Weight TeO_2.	*Loss. SO_2.*	*Per cent. SO_2.*
1.02217	.41050	40.160
.80697	.32392	40.140
1.32003	.52992	40.145
1.05207	.42221	40.131
1.37043	.54969	40.111
.95944	.38511	40.139

Mean of all as one series, 40.136, ± .0028

Hence Te=127.62. Several other determinations, concordant with these, are cited, but without the detailed weighings.

Baker and Bennett also determined the atomic weight of tellurium by synthesis of the tetrabromide. Tellurium and bromine were directly combined in an atmosphere of nitrogen, and the excess of bromine was expelled by a current of nitrogen at a temperature of 50°. The determinations fall into several series, representing different samples of material, but they are given here as one series:

Weight Te.	*Weight* $TeBr_4$.	*Per cent. Te.*
.61273	2.14933	28.508
.56866	1.99354	28.525
.59884	2.09951	28.523
.57894	2.03040	28.514
.54743	1.91899	28.527
.33859	1.18732	28.517
.56866	1.99354	28.526
.47643	1.67025	28.525
.56622	1.98597	28.511
.44271	1.55205	28.524
.41671	1.46177	28.508
.50611	1.77489	28.515
.37382	1.31081	28.519
.31895	1.11868	28.512
.48931	1.71554	28.522
.47156	1.65404	28.510
.40748	1.42867	28.523
.62013	2.17449	28.518
.37382	1.31081	28.519
.50822	1.78207	28.518
.12928	.45354	28.505
.42926	1.50540	28.515
.80348	2.81715	28.511
.95309	3.34193	28.512
		Mean, 28.517, ± .0009

Hence Te = 127.53. If Br = 79.96, Te = 127.60, in accord with the SO_2 ratio.

Baker and Bennett furthermore report a few analyses of tellurium tetrachloride which gave values for Te ranging from 127.58 to 127.64, but without weights or details. These determinations, therefore, are unavailable for discussion here.

In the determinations by Marckwald[1] telluric acid was the starting point. This compound was reduced to TeO_2 by heating, as in several former investigations, with the following results:

Weight H_6TeO_6.	TeO_2.	*Per cent.* TeO_2.
8.6277	5.9884	69.409
12.2680	8.5135	69.396
13.0051	9.0244	69.390
8.6415	5.9947	69.371
8.4588	5.8696	69.390
8.0113	5.5599	69.401
		Mean, 69.393, ± .0035

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[1] Ber. Deutsch. chem. Ges., 40, 4730, 1907. For a criticism of Marckwald by Baker, see Chem. News, 97, 200.

Hence Te=126.81, a figure which falls below the atomic weight of iodine. The error which suggests itself is the possible retention of water or mother liquor by the telluric acid; but Marckwald obtained an acid of constant weight after prolonged drying over phosphorus pentoxide. Still, water may have been retained as an enclosure within the particles of acid, so enveloped as to be prevented from escaping. Marckwald's figures combine with other similar determinations thus:

Staudenmaier	69.440, ± .0024
Heberlein	69.381, ± .0115
Marckwald	69.393, ± .0035
General mean	69.424, ± .0020

Lenher's investigations,[1] like those of his recent predecessors, had special reference to the homogeneity of tellurium. The tellurium was obtained from three distinct sources; first, from the telluride ores of Colorado; second, from the residues of an electrolytic copper refinery; and third, from Bohemian material. From these the double bromide K_2TeBr_6 was prepared, and this, by heating first in chlorine and afterwards in gaseous hydrochloric acid, was converted into potassium chloride. That is, the ratio $K_2TeBr_6 : 2KCl$ was measured, all weights being reduced to a vacuum. In the following table I have treated the three series as one, for the results obtained are sensibly uniform:

	K_2TeBr_6.	*KCl.*	*Per cent. KCl.*
1	2.33360	.50779	21.7599
	1.27372	.27716	21.7599
	1.47573	.32111	21.7594
	1.65715	.36059	21.7596
	1.54006	.33513	21.7608
2	1.82810	.39778	21.7592
	1.87342	.40765	21.7595
	1.48045	.32214	21.7596
	2.24775	.48911	21.7600
3	2.37899	.51767	21.7601
	1.79926	.39146	21.7562
	.94102	.20476	21.7594
	1.55357	.33806	21.7602
	1.95038	.42440	21.7599
	1.73248	.37698	21.7596
	1.81923	.39586	21.7598
			Mean, 21.7596, ± .00017

Hence Te=127.57.

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[1] Journ. Amer. Chem. Soc., 31, 20. 1909. See also Lenher's figures for TeO_2, previously cited.

The ratios for tellurium are now as follows:

(1). $TeO_2:Te::100:79.9498$, ± .0010
(2). $TeO_2:O::100:10.068$, ± .0100
(3). $H_6TeO_6:3H_2O::100:23.550$, ± .0083
(4). $H_6TeO_6:TeO_2::100:69.424$, ± .0020
(5). $H_6TeO_6:Te::100:55.5079$, ± .0067
(6). Molecular weight H_6TeO_6, 229.270, ± .0425. (Heberlein)
(7). $4Ag:Te::100:29.553$, ± .0021
(8). $4Ag:TeBr_4::100:103.640$, ± .0014
(9). $K_2TeBr_6:6AgBr::100:164.468$, ± .0324
(10). $TeBr_4:Te::100:28.517$, ± .0009
(11). $TeO_2:SO_2::100:40.136$, ± .0028
(12). $Te_2SO_7:Te::100:63.978$, ± .0040
(13). $Te_2HNO_7:TeO_2::100:83.5000$, ± .00047
(14). $C_{12}H_{10}Te:12CO_2::100:188.221$, ± .0549
(15). $AgI:C_3H_9TeI::100:127.583$, ± .0105
(16). $Ag:C_3H_9TeBr::100:234.263$, ± .0391
(17). $K_2TeBr_6:2KCl::100:21.7596$, ± .00017

To reduce these ratios we have—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
I = 126.9204, ± .00033
N = 14.0101, ± .0001
C = 12.0038, ± .0002
S = 32.0667, ± .00075
K = 39.0999, ± .0002
H = 1.00779, ± .00001

Hence,

From ratio 14	Te = 126.418, ± .0820
" " 2	126.919, ± .1579
" " 4	127.044, ± .0114
" " 6	127.223, ± .0425
" " 5	127.313, ± .0247
" " 9	127.392, ± .1350
" " 3	127.451, ± .0583
" " 13	127.453, ± .0046
" " 7	127.527, ± .0954
" " 10	127.531, ± .0045
" " 8	127.548, ± .0063
" " 15	127.563, ± .0247
" " 17	127.572, ± .0064
" " 1	127.599, ± .0066
" " 11	127.614, ± .0113
" " 16	127.722, ± .0422
" " 12	127.937, ± .0163
General mean,	Te = 127.520, ± .0023

In short, the atomic weight of tellurium is near 127.5, at least so far as the element is now known. The general mean given above is between the values determined by Norris and Gallo.

It has already been stated that several of the more important investigations relative to the atomic weight of tellurium, have had for their purpose the establishment of its homogeneity. Up to this point all the evidence has gone to show that it is not a mixture of two elements. Tellurium from widely different sources, as in Lenher's recent work, gives one and the same value for its atomic weight. Fractionations by different methods have also given constant results, and it seemed as if the question had been definitely settled. Very recently, however, even since this chapter was in great part written, Browning and Flint[1] have secured evidence upon the other side, which deserves some attention. When tellurium tetrachloride is mixed with water and hydrolyzed, a large part of it is precipitated as tellurium dioxide. A part, however, remains in solution, from which it can be thrown down by ammonia and a slight excess of acetic acid. Carefully purified tellurium was treated by the process thus briefly suggested, and converted, with all due precautions, into the basic nitrate. The portion precipitated by hydrolysis gave, on analysis of the nitrate, a mean value of Te = 126.53. From the portion afterwards thrown down the value 128.97 was obtained. Other determinations, by other methods, gave similar results. The *alpha*, or first precipitate, gave mean values, in two additional series, of 126.64 and 126.31. The *beta* portion, that not precipitated during hydrolysis of the chloride, gave Te = 128.77 and 128.81. Browning and Flint intend to continue their research; but until that is finished it is not practicable to discuss their atomic weights in connection with previous determinations. Their fractionations are evidently not perfect, but preliminary; and their atomic weights are not given as being anything more than approximations. So far they have established a reasonable probability; nothing more.

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[1] Amer. Journ. Sci. (4), 28, 347. 1909.

FLUORINE.

The atomic weight of fluorine has been commonly determined by two general methods: namely, the conversion of fluorides into sulphates. There are, however, two exceptions, which will be considered in due time.

Excluding the early results of Davy,[1] we have to consider first the experiments of Berzelius, Louyet, Dumas, De Luca and Moissan with reference to the fluorides of calcium, sodium, potassium, barium and lead.

The ratio between calcium fluoride and sulphate has been determined by the five investigators above named, and by one general process. The fluoride is treated with strong sulphuric acid, the resulting sulphate is ignited, and the product weighed. In order to insure complete transformation special precautions are necessary, such, for instance, as repeated treatment with sulphuric acid, and so on. For details like these the original papers must be consulted.

The first experiments in chronological order are those of Berzelius,[2] who operated upon an artificial calcium fluoride. He found, in three experiments, for one part of fluoride the following of sulphate:

1.749
1.750
1.751

Mean, 1.750, ± .0004

Hence F = 18.85.

Louyet's researches[3] were much more elaborate than the foregoing. He began with a remarkably concordant series of results upon fluor spar, in which one gramme of the fluoride yielded from 1.734 to 1.737 of sulphate. At first he regarded these as accurate, but he soon found that particles of spar had been coated with sulphate, and had therefore escaped action. In the following series this source of error was guarded against.

Starting with fluor spar, Louyet found of sulphate as follows:

1.742
1.744
1.745
1.744
1.7435
1.7435

Mean, 1.7437, ± .0003

Hence F = 18.99.

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[1] Phil. Trans., 64. 1814.

[2] Poggend. Annalen, 8, 1. 1826.

[3] Ann. Chim. Phys. (3), 25, 300. 1849.

A second series, upon artificial fluoride, gave:

1.743
1.741
1.741

Mean, 1.7417, ± .0004

Hence F = 19.03.

Dumas[1] published but one result for calcium fluoride. .495 grm. gave .864 grm. sulphate, the ratio being 1 : 1.7455. Hence F = 18.95.

De Luca[2] worked with a very pure fluor spar, and published the following results. The ratio between $CaSO_4$ and one gramme of CaF_2 is given in the third column:

.9305 grm. CaF_2 gave	1.630 grm. $CaSO_4$.	1.7518
.836 "	1.459 "	1.7452
.502 "	.8755 "	1.7440
.3985 "	.6945 "	1.7428

Hence F = 18.97.

If we include Dumas' single result with these, we get a mean of 1.7459, ± .0011.

Moissan[3] unfortunately gives no details nor weighings, but merely states that four experiments with calcium fluoride gave values for F ranging from 19.02 to 19.08. To S he assigned the value 32.074, and probably Ca was taken as = 40. With these data his extreme values as given may be calculated back into uniformity with the ratio as stated above, becoming—

1.7444
1.7410

Mean, 1.7427

Hence F = 19.011.

If we assign this equal weight with Berzelius' series, the data for this ratio combine thus:

Berzelius	1.7500, ± .0004
Louyet, first series	1.7437, ± .0003
Louyet, second series	1.7417, ± .0004
De Luca with Dumas	1.7459, ± .0011
Moissan	1.7427, ± .0004
General mean	1.7444, ± .00018

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[1] Ann. Chem. Pharm., 113, 28. 1860.
[2] Compt. Rend., 51, 299. 1860.
[3] Compt. Rend., 111, 570. 1890.

For the ratio between the two sodium salts we have experiments by Dumas, Louyet and Moissan. According to Louyet, one gramme of NaF gives of Na_2SO_4—

1.686
1.683
1.685

Mean, 1.6847, ± .0006

Hence F = 19.06.

The weighings published by Dumas are as follows:

.777	grm. NaF gave	1.312	grm. Na_2SO_4.	Ratio,	1.689
1.737	"	2.930	"	"	1.687
				Mean,	1.688, ± .0007

Hence F = 19.08.

Moissan says only that five experiments with sodium fluoride gave F = 19.04 to 19.08. This was calculated with Na = 23.05 and S = 32.074. Hence, reckoning backward, the two values give for the standard ratio—

1.6889
1.6873

Mean, 1.6881

Hence F = 19.07.

Giving this equal weight with Dumas' mean, we have—

Louyet	1.6847, ± .0006
Dumas	1.688, ± .0007
Moissan	1.6881, ± .0007
General mean	1.6867, ± .00038

Dumas also gives experiments upon potassium fluoride. The quantity of sulphate formed from one gramme of fluoride is given in the last column:

1.483	grm. KF gave	2.225	grm. K_2SO_4.	1.5002
1.309	"	1.961	"	1.4981
			Mean.	1.4991. ± .0007

Hence F = 19.02.

The ratio between barium fluoride and barium sulphate was measured by Louyet and Moissan. According to Louyet, one gramme of BaF_2 gives of $BaSO_4$—

1.332
1.331
1.330

Mean, 1.331, ± .0004

Hence F = 19.01.

Moissan, in five experiments, found F = 19.05 to 19.09. Assuming that he put Ba = 137, and S = 32.074 as before, these two extremes become—

1.3311
1.3305

Mean, 1.3308

Hence F = 19.02.

Giving this equal weight with Louyet's mean, we get the subjoined combination:

Louyet	1.331, ± .0004
Moissan	1.3308, ± .0004
General mean	1.3309, ± .00028

The experiments with lead fluoride are due to Louyet, and a new method of treatment was adopted. The salt was fused, powdered, dissolved in nitric acid, and precipitated by dilute sulphuric acid. The evaporation of the fluid and the ignition of the sulphate was then effected without transfer. Five grammes of fluoride were taken in each operation, yielding of sulphate:

6.179
6.178
6.178

Mean, 6.1783, ± .0002

Hence F = 19.14.

In Christensen's determinations[1] we find a method adopted which is radically unlike anything in the work of his predecessors. He started out with the salt $(NH_4)_2MnF_5$. When this is added to a mixture, in solution, of potassium iodide and hydrochloric acid, iodine is set free, and may be titrated with sodium thiosulphate. One molecule of the salt (as written above) liberates one atom of iodine. In four experiments Christensen obtained the following data:

3.1199 grm. Am_2MnF_5 gave	2.12748 I.	68.191 per cent.
3.9190 "	2.67020 "	68.135 "
3.5005 "	2.38429 "	68.113 "
1.2727 "	.86779 "	68.185 "
		Mean, 68.156, ± .0128

Hence F = 19.038.

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[1] Journ. prakt. Chem. (2), 35, 541. Christensen assigns to the salt double the formula here given

Still another method for determining the atomic weight of fluorine was adopted by Julius Meyer.[1] Carefully purified calcium oxide was weighed, slaked with water and then converted into chloride by means of hydrochloric acid. The chloride solution was then repeatedly evaporated with pure hydrofluoric acid. The calcium fluoride so produced was finally ignited to constant weight. On a vacuum basis his weights were as follows. The third column gives the ratio $CaO : CaF_2 :: 100 : x$:

Weight CaO.	*Weight CaF_2.*	*Ratio.*
6.1883	8.6215	139.320
4.2736	5.9548	139.339
6.2931	8.7658	139.292
5.7767	8.0485	139.327
4.9836	6.9426	139.309
		Mean, 139.317, ± .0054

Hence F = 19.035.

The ratios from which to compute the atomic weight of fluorine are now—

(1). $CaO:CaF_2::100:139.317$, ± .0054
(2). $CaF_2:CaSO_4::1.0:1.7444$, ± .00018
(3). $2NaF:Na_2SO_4::1.0:1.6867$, ± .00038
(4). $2KF:K_2SO_4::1.0:1.4991$, ± .0007
(5). $BaF_2:BaSO_4::1.0:1.3309$, ± .00028
(6). $PbF_2:PbSO_4::5.0:6.1783$, ± .0002
(7). $Am_2MnF_5:I::100:68.156$, ± .0128

To reduce these ratios we have—

Ca = 40.1323, ± .0005
Ba = 137.363, ± .0025
Pb = 206.970, ± .0017
Mn = 54.947, ± .0005
K = 39.0999, ± .0002

Na = 23.0108, ± .00024
S = 32.0667, ± .00075
I = 126.9204, ± .00033
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 2 ... .F = 18.973, ± .0041
" " 5 ....19.015, ± .0185
" " 4 ....19.024, ± .0271
" " 1 ....19.035, ± .0016
" " 7 ....19.038, ± .0070
" " 3 ....19.109, ± .0085
" " 6 ....19.136, ± .0041

General mean, F = 19.041, ± .00135

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[1] Zeitsch. anorg. Chem., 36, 313. 1903.

This mean is near the values deduced from Meyer's and Christensen's determinations, which are presumably the best. If it is applied to Christensen's ratio, No. 7, it gives for the atomic weight of manganese Mn = 54.933, which agrees well with the results obtained by Baxter and Hines. From this we may fairly infer that the value for fluorine is not far from the truth.

MANGANESE.

The earliest experiments of Berzelius[1] and of Arfvedson[2] gave values for Mn ranging between 56 and 57, and therefore need no farther consideration here. The first determinations to be noticed are those of Turner[3] and a later measurement by Berzelius,[4] who both determined gravimetrically the ratio between the chlorides of manganese and silver. The manganese chloride was fused in a current of dry hydrochloric acid, and afterwards precipitated with a silver solution. I give the $MnCl_2$ equivalent to 100 parts of AgCl in the third column:

4.20775 grm. $MnCl_2$ =	9.575 grm. AgCl.	43.945	Berzelius
3.063 " =	6.96912 "	43.950	
12.47 grains $MnCl_2$ =	28.42 grains AgCl.	43.878	Turner
	Mean,	43.924, ± .015	

Hence Mn = 55.07, Berzelius; or 54.87, Turner.

Many years later Dumas[5] also made the chloride of manganese the starting point of some atomic weight determinations. The salt was fused in a current of hydrochloric acid, and afterwards titrated with a standard solution of silver in the usual way. One hundred parts of Ag are equivalent to the quantities of $MnCl_2$ given in the third column:

3.3672 grm. $MnCl_2$ =	5.774 grm. Ag.	58.317
3.0872 "	5.293 "	58.326
2.9671 "	5.0875 "	58.321
1.1244 "	1.928 "	58.320
1.3134 "	2.251 "	58.321
	Mean,	58.321, ± .001

Hence Mn = 54.916.

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[1] Poggend. Annalen, 8, 185. 1826.

[2] Berz. Jahresbericht, 9, 136. 1829.

[3] Trans. Roy. Soc. Edinb., 11, 143. 1831.

[4] Lehrbuch, 5 Aufl., 3, 1224.

[5] Ann. Chem. Pharm., 113, 25. 1860.

An entirely different method of investigation was followed by von Hauer,[1] who, as in the case of cadmium, ignited the sulphate in a stream of sulphuretted hydrogen, and determined the quantity of sulphide thus formed. I subjoin his weighings, and also the percentage of MnS in $MnSO_4$ as calculated from them:

4.0626	grm. $MnSO_4$ gave	2.3425	grm. MnS.	57.660	per cent.
4.9367	"	2.8442	"	57.613	"
5.2372	"	3.0192	"	57.649	"
7.0047	"	4.0347	"	57.600	"
4.9175	"	2.8297	"	57.543	"
4.8546	"	2.7955	"	57.585	"
4.9978	"	2.8799	"	57.625	"
4.6737	"	2.6934	"	57.629	"
4.7240	"	2.7197	"	57.572	"
			Mean,	57.608, ± .008	

Hence Mn = 54.915.

This method of von Hauer, which seemed to give good results with cadmium, is, according to Schneider,[2] inapplicable to manganese, for the reason that the sulphide of the latter metal is liable to be contaminated with traces of oxysulphide. Such an impurity would bring the atomic weight out too high. The results of two different processes, one carried out by himself and the other in his laboratory by Rawack, are given by Schneider in this paper.

Rawack reduced manganoso-manganic oxide to manganous oxide by ignition in a stream of hydrogen, and weighed the water thus formed. From his weighings I get the values in the third column, which represent the Mn_3O_4 equivalent to one gramme of water:

4.149	grm. Mn_3O_4 gave	.330	grm. H_2O.	12.5727
4.649	"	.370	"	12.5643
6.8865	"	.5485	"	12.5552
7.356	"	.5855	"	12.5636
8.9445	"	.7135	"	12.5361
11.584	"	.9225	"	12.5572
			Mean,	12.5582, ± .0034

Hence Mn = 54.08.

Here the most obvious source of error lies in the possible loss of water. Such a loss, however, would increase the apparent atomic weight of manganese; but we see that the value found is much lower than that obtained either by Dumas or von Hauer.

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[1] Journ. prakt. Chem., 72, 360. 1857.

[2] Poggend. Annalen, 107, 605.

Schneider himself effected the combustion of manganous oxalate with oxide of copper. The salt was not absolutely dry, so that it was necessary to collect both water and carbon dioxide. Then, upon deducting the weight of water from that of the original material, the weight of anhydrous oxalate was easily ascertained. Subtracting from this the CO_2, we get the weight of Mn. If we put $CO_2 = 100$, the quantities of manganese equivalent to it will be found in the last column:

1.5075 grm. oxalate gave	.306 grm. H_2O and	.7445 grm. CO_2.	61.3835
2.253 "	.4555 "	1.1135 "	61.4291
3.1935 "	.652 "	1.5745 "	61.4163
5.073 "	1.028 "	2.507 "	61.3482
			Mean, 61.3943, ± .0122

Hence Mn = 54.03.

Up to this point the data give two distinct values for Mn—one near 54, the other approximately 55—and with no sure guide to preference between them. The higher value, however, has been confirmed by later testimony.

In 1883 Dewar and Scott[1] published the results of their work upon silver permanganate. This salt is easily obtained pure by recrystallization, and has the decided advantage of not being hygroscopic. Two sets of experiments were made. First, the silver permanganate was heated to redness in a glass bulb, first in air, then in hydrogen. Before weighing, the latter gas was replaced by nitrogen. The data are as follows:

$AgMnO_4$.	$Ag + MnO$.	Per cent. $Ag + MnO$.
5.8696	4.63212	78.917
5.4988	4.33591	78.852
7.6735	6.05395	78.894
13.10147	10.31815	78.756
12.5799	9.91065	78.782
	9.91435	78.811
		Mean, 78.835, ± .0174

Hence Mn = 55.009.

The duplication of the last weighing is not explained.

In the second series the permanganate was dissolved in dilute nitric acid, reduced by sulphur dioxide, potassium nitrite, or sodium formate,

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[1] Proc. Roy. Soc., 35, 44. 1883.

and titrated with potassium bromide. The $AgMnO_4$ equivalent to 100 KBr appears in the third column:

$AgMnO_4$.	*KBr.*	*Ratio.*
6.5289	3.42385	190.686
7.5378	3.9553	190.575
6.1008	3.20166	190.559
5.74647	3.00677	191.117
6.16593	3.23602	190.540
5.11329	2.6828	190.596
5.07438	2.66204	190.624
13.4484	7.05603	190.604
12.5799	6.60065	190.588
12.27025	6.43808	190.584
		Mean, 190.647, ± .0361

Vacuum weights are given throughout. To the first series of experiments the authors attach little importance, and numbers 1 and 4 of the second series they also regard as questionable. These experiments represent the use of sulphur dioxide as the reducing agent, and were attended by the formation of an insoluble residue, apparently of a sulphide. Excluding them, the remaining eight experiments of the second series give in mean—

$KBr:AgMnO_4::100:190.584, \pm .0062$, and $Mn = 54.95$

which will be used for the present calculation. Dewar and Scott also made determinations with manganese chloride and bromide. With the first salt they found Mn = 54.91, and with the second, Mn = 54.97; but they give no details.

Marignac's work upon the atomic weight of manganese also appeared in 1883.[1] He prepared the oxide, MnO, by ignition of the oxalate and subsequent reduction of the resulting Mn_3O_4 in hydrogen. The oxide, with various precautions, was then converted into sulphate. The percentage of MnO in $MnSO_4$ is appended:

2.6587 grm. MnO	gave	5.6530	$MnSO_4$.	47.032 per cent.
2.5185	"	5.3600	"	46.987 "
2.5992	"	5.5295	"	47.006 "
2.8883	"	6.1450	"	47.002 "
				Mean, 47.007, ± .0025

Hence Mn = 55.022.

J. M. Weeren, in 1890,[2] published determinations made by two methods, the one Marignac's, the other von Hauer's. From manganese sulphate

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[1] Arch. Sci. Phys. Nat. (3), 10, 21. 1883.

[2] Atom-Gewichtsbestimmung des Mangans. Inaugural Dissertation. Halle, 1890.

he threw down the hydrated peroxide electrolytically, and the latter compound was then reduced in hydrogen which had been proved to be free from oxygen. The resulting monoxide was cooled in a stream of purified nitrogen. After the oxide had been treated with sulphuric acid, converted into sulphate, and weighed, a few drops of sulphuric acid and a little sulphurous acid were added to it, after which it was reheated and weighed again. This process was repeated until four successive weighings absolutely agreed. The results of this set of experiments were as follows, reduced to a vacuum standard:

15.2349	grm. MnO gave	32.4142	$MnSO_4$.	47.005	per cent.
13.9686	"	29.7186	"	47.004	"
13.7471	"	29.2493	"	47.000	"
15.5222	"	33.0246	"	47.001	"
14.9824	"	31.8755	"	47.002	"
14.6784	"	31.2304	"	47.000	"

Mean, 47.002, ± .0006

Hence Mn = 55.008.

Marignac's mean, combined with this, hardly affects either the percentage itself or its probable error. Fortunately, both Marignac and Weeren are completely in agreement as to the ratio, and either set of measurements would be valid without the other. In order, therefore, to give Marignac's work some proper recognition, we can assume a general mean of 47.004, ± .0006, without danger of serious error.

The manganese sulphate produced in the foregoing series of experiments was used, with many precautions, for the next series carried out by von Hauer's method. It was transferred to a porcelain boat, dried at 260° to avoid errors due to retention of water taken up in the process of transfer, and then heated to constant weight in a stream of hydrogen sulphide. Before weighing, the sulphide was heated to redness in hydrogen and cooled in the same gas. The results, with vacuum weights, were as follows:

16.0029	grm. $MnSO_4$ gave	9.2228	MnS.	57.632	per cent.
16.3191	"	9.4048	"	57.631	"
15.9307	"	9.1817	"	57.634	"
15.8441	"	9.1315	"	57.634	"
16.2783	"	9.3819	"	57.635	"
17.0874	"	9.8477	"	57.633	"

Mean, 57.633, ± .0004

von Hauer found, 57.608, ± .0080

Hence the general mean is identical with Weeren's to the third decimal place, which is unaffected by combination with von Hauer's data.

From Weeren's figures alone Mn=54.994.

The determinations by Baxter and Hines[1] were based upon analyses of manganese bromide and chloride, both fused in order to eliminate moisture. The usual Harvard methods were employed, giving two ratios for each salt. With vacuum weights the data obtained were as follows, first with the bromide:

$MnBr_2$.	$AgBr$.	Ag.	Ag ratio.	$AgBr$ ratio.
5.58416	9.76561	......	.....	57.181
5.63432	9.85345	......	.....	57.181
6.53738	11.43300	6.56765	99.539	57.180
4.81005	8.41206	4.83238	99.538	57.180
4.88097	8.53642	4.90354	99.540	57.178
5.63219	9.85008	5.65813	99.542	57.179
6.52626	11.41293	......	.....	57.183
5.79924	10.14206	5.82600	99.541	57.180
3.59809	6.29271	3.61478	99.538	57.179
5.16334	9.02959	5.18711	99.542	57.182
3.92226	6.85968	3.94042	99.539	57.178
4.49158	7.85571	4.51250	99.536	57.176
3.60071	6.29740	3.61736	99.540	57.178
4.77392	8.34915	4.79620	99.535	57.179
3.57660	6.25569	3.59319	99.538	57.174
5.69972	9.96840	5.72641	99.534	57.178
		Mean,	99.539,	57.179,
			± .0005	± .0004

From Ag ratio, Mn=54.926.
From AgBr ratio, Mn=54.925.
And Ag : Br : : 100 : 74.083.

Secondly, for the chloride series:

$MnCl_2$.	$AgCl$.	Ag.	Ag ratio.	$AgCl$ ratio.
4.62970	10.54641	7.93740	58.328	43.898
3.52899	8.03868	6.05041	58.326	43.900
3.30881	7.53731	5.67279	58.328	43.899
3.56843	8.12932	6.11818	58.325	43.896
3.45083	7.86129	5.91637	58.327	43.896
4.47948	10.20372	7.67995	58.327	43.900
3.92089	8.93140	6.72227	58.327	43.900
		Mean,	58.327,	43.898,
			± .0003	± .0005

From Ag ratio, Mn=54.928.
From AgCl ratio, Mn=54.928.
And Ag : Cl : : 100 : 32.869.

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[1] Journ. Amer. Chem. Soc., 28, 1560. 1906.

The chloride ratios, as determined by different chemists, combine thus:

Ag ratio.

Dumas	58.321, ± .0010
Baxter and Hines	58.327, ± .0003
General mean	58.3265, ± .0003

AgCl ratio.

Berzelius with Turner	43.924, ± .0150
Baxter and Hines	43.898, ± .0005
General mean	43.898, ± .0005

In this instance the early work does not even effect the fourth decimal place.

We have now to consider the following ratios for manganese:

(1). $2Ag:MnCl_2::100:58.3265$, ± .0003
(2). $2AgCl:MnCl_2::100:43.898$, ± .0005
(3). $2Ag:MnBr_2::100:99.539$, ± .0005
(4). $2AgBr:MnBr_2::100:57.179$, ± .0004
(5). $H_2O:Mn_3O_4::100:1255.82$, ± .3400
(6). $2CO_2:Mn::100:61.3943$, ± .0122
(7). $AgMnO_4:Ag+MnO::100:78.835$, ± .0174
(8). $KBr:AgMnO_4::100:190.584$, ± .0062
(9). $MnSO_4:MnO::100:47.004$, ± .0006
(10). $MnSO_4:MnS::100:57.633$, ± .0004

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
S = 32.0667, ± .00075
K = 39.0999, ± .0002
C = 12.0038, ± .0002
H = 1.00779, ± .00001

Hence,

From ratio 6	Mn = 54.032, ± .0108
" " 5	54.081, ± .0610
" " 4	54.925, ± .0020
" " 3	54.926, ± .0014
" " 1	54.928, ± .0008
" " 2	54.928, ± .0015
" " 8	54.953, ± .0074
" " 10	54.994, ± .0013
" " 7	55.009, ± .1522
" " 9	55.014, ± .0014
General mean,	Mn = 54.947, ± .0005

In this combination the best work is evidently that of Baxter and Hines, as shown by the concordant values derived from ratios 1 to 4. But Weeren's work also appears to be excellent, and ought not to be ignored. The general mean takes all the trustworthy determinations into account, and seems to be preferable to any selection among them.

IRON.

The atomic weight of iron has been mainly determined from the composition of ferric oxide, ferrous bromide and the two chlorides.

Most of the earlier data relative to the percentage of metal and oxygen in ferric oxide we may reject at once, as set aside by later investigations. Among this no longer valuable material there is a series of experiments by Berzelius, another by Döbereiner, and a third by Capitaine.[1] The first work deserving of present consideration is that of Wackenroder,[2] who reduced the oxide in hydrogen at a moderate red heat. The following percentages of iron were thus found:

69.62
69.954
69.98
69.98
69.99
70.04

Mean, 69.927, ± .0905

If we reject the first of these figures the mean becomes 69.988, ± .0099, which is more trustworthy. Hence Fe = 55.97.

In 1844 Berzelius[3] published two determinations of the ratio in question. He oxidized iron by means of nitric acid, and weighed the oxide thus formed. He thus found that when O = 100 Fe = 350.27 and 350.369.

Hence the following percentages of Fe in Fe_2O_3:

70.018
70.022

Mean, 70.020, ± .0013

Hence Fe = 56.05. The "probable error" assigned to this pair of measurements greatly overvalues them. It is better, therefore, to give the mean equal weight with Wackenroder's, making it 70.020, ± .0099.

About the same time Svanberg and Norlin[4] published two elaborate series of experiments; one relating to the synthesis of ferric oxide, the other to its reduction. In the first set pure piano-forte wire was oxidized

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[1] For details concerning these earlier researches, see Oudeman's monograph, pp. 140, 141.

[2] Arch. Pharm., 35, 279, and 36, 22. 1843.

[3] Berz. Jahresb., 25, 43. Ann. Chem. Pharm., 30, 432.

[4] Berz. Jahresb., 25, 42.

by nitric acid, and the amount of oxide thus formed was determined. The results were as follows:

1.5257 grm. Fe	gave	2.1803 grm. Fe_2O_3.	69.977 per cent.	Fe.
2.4051	"	3.4390 "	69.936	"
2.3212	"	3.3194 "	69.928	"
2.32175	"	3.3183 "	69.968	"
2.2772	"	3.2550 "	69.960	"
2.4782	"	3.5418 "	69.970	"
2.3582	"	3.3720 "	69.935	"

Mean, 69.9534, ± .0050

Hence Fe = 55.875.

In the second series ferric oxide was reduced by ignition in a current of hydrogen, yielding the subjoined percentages of metal:

2.98353 grm. Fe_2O_3	gave	2.08915 grm. Fe.	70.025 per	cent.
2.41515	"	1.6910 "	70.015	"
2.99175	"	2.09455 "	70.014	"
3.5783	"	2.505925 "	70.030	"
4.1922	"	2.9375 "	70.072	"
3.1015	"	2.17275 "	70.056	"
2.6886	"	1.88305 "	70.036	"

Mean, 70.0354, ± .0055

Hence Fe = 56.093.

It is evident that one or both of these series must be vitiated by constant errors, and that these probably arise from impurities in the materials employed. Impurities in the wire taken for the oxidation series could hardly have been altogether avoided.

In 1844 there was also published an important paper by Erdmann and Marchand.[1] These chemists prepared ferric oxide by the ignition of pure ferrous oxalate, and submitted it to reduction in a stream of hydrogen. Two sets of results were obtained with two different samples of ferrous oxalate, prepared by two different methods. For present purposes, however, it is not necessary to discuss these sets separately. The percentages of iron in Fe_2O_3 are as follows:

70.013	A
69.962	
69.979	
70.030	
69.977	
70.044	B
70.015	
70.055	

Mean, 70.0094, ± .0080

Hence Fe = 56.025.

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[1] Journ. prakt. Chem., 33, 1.

In 1850 Maumené's[1] results appeared. He dissolved pure iron wire in aqua regia, precipitated with ammonia, filtered off the precipitate, washed thoroughly, ignited and weighed after the usual methods of quantitative analysis. The percentages of Fe in Fe_2O_3 are given in the third column:

1.482	grm. Fe gave	2.117	grm. Fe_2O_3.	70.005	per cent.
1.452	"	2.074	"	70.010	"
1.3585	"	1.941	"	69.990	"
1.420	"	2.0285	"	70.002	"
1.492	"	2.1315	"	69.998	"
1.554	"	2.220	"	70.000	"
			Mean,	70.0008, ± .0019	

Hence Fe = 56.003.

The two determinations by Rivot[2] are quite unimportant. This chemist reduced ferric oxide in hydrogen, and obtained the subjoined percentages of iron:

69.31
69.35

Mean, 69.33, ± .0130

Hence Fe = 54.25.

Richards and Baxter[3] also reduced ferric oxide by hydrogen. Iron was purified electrolytically and then converted into oxide by two processes. First, by solution, precipitation as hydroxide, and ignition of the latter compound. With the oxide thus prepared, the two subjoined reductions were made:

Fe_2O_3.	*Fe.*	*Per cent. Fe.*
3.17485	2.22096	69.954
3.61235	2.52750	69.968
		Mean, 69.961, ± .0047

Hence Fe = 55.900.

Secondly, iron was converted into nitrate, and that into oxide by calcination. The oxide was free from occluded gases. The data, with vacuum weights for both series, are as follows:

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[1] Compt. Rend., Oct. 17, 1850.

[2] Ann. Chem. Pharm., 78, 214. 1851.

[3] Proc. Amer. Acad., 35, 253. Zeitsch. anorg. Chem., 23, 245. 1900.

Fe_2O_3.	*Fe.*	*Per cent. Fe.*
3.97557	2.78115	69.956
4.89655	3.42558	69.959
4.35955	3.04990	69.959
7.14115	4.99533	69.951
6.42021	4.49130	69.956
		Mean, 69.9562, ± .0010

Hence Fe = 55.883.

The nine series of figures for this ratio combine thus:

Wackenroder	69.988,	± .0099
Berzelius	70.020,	± .0099
Erdmann and Marchand	70.0094,	± .0080
Svanberg and Norlin, oxidation	69.9534,	± .0050
Svanberg and Norlin, reduction	70.0354,	± .0055
Maumené	70.0008,	± .0019
Rivot	69.33,	± .013
Richards and Baxter, 1	69.961,	± .0047
Richards and Baxter, 2	69.9562,	± .0010
General mean	69.9728,	± .00083

Although they are not in chronological order, the analyses of ferrous bromide by Baxter[1] may conveniently be considered here. He made two sets of analyses, fixing the two usual ratios, by the established Harvard methods. His figures, with all corrections and vacuum weights, follow:

$FeBr_2$.	*AgBr.*	*Ag.*	*Ag ratio.*	*AgBr ratio.*
3.55929	6.19873	......	.....	57.420
3.07448	5.35450	......	.....	57.419
2.96102	5.15696	2.96234	99.956	57.418
4.00791	6.97983	4.00937	99.964	57.421
		Mean,	99.960, ± .0027	57.4195, ± .00044

From Ag ratio, Fe = 55.836.

From AgBr ratio, Fe = 55.828.

And Ag : Br :: 100 : 74.087.

Dumas'[2] results, obtained from the chlorides of iron, are of so little weight that they might safely be omitted from our present discussion. For the sake of completeness, however, they must be included.

Pure ferrous chloride, ignited in a stream of hydrochloric acid gas, was dissolved in water and titrated with a silver solution in the usual

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[1] Proc. Amer. Acad., 39, 245. 1903.

[2] Ann. Chem. Pharm., 113, 26. 1860.

way. One hundred parts of silver are equivalent to the amounts of $FeCl_2$ given in the third column:

3.677 grm. $FeCl_2$ = 6.238 grm. Ag.		58.945
3.924 "	6.675 "	58.787
	Mean,	58.866, ± .053

Ferric chloride, titrated in the same way, gave these results:

1.179 grm. $FeCl_3$ = 2.3475 grm. Ag.		50.224
1.242 "	2.471 "	50.263
	Mean,	50.2435, ± .0132

These give us two additional values for Fe, as follows:

From $FeCl_2$ .... Fe = 56.092
From $FeCl_3$ .... " = 56.231

A series of determinations of the equivalent of iron, made by students by measuring the hydrogen evolved when the metal is dissolved in an acid, was published by Torrey in 1888.[1] The data have, of course, slight value, but may be considered as being in some measure confirmatory. They are as follows:

56.40
55.60
55.38
55.56
55.48
55.50
55.86
56.06
56.22
55.80
55.78
55.60
55.70
55.94

Mean, 55.777, ± .0532

These values undoubtedly depend on Regnault's value for the weight of hydrogen. Correcting by the later value, as found in the chapter of this work relating to the density ratio H : O, the mean becomes Fe = 55.608, ± .0532. With O = 16, Fe = 56.042. The probable error in the weight of the hydrogen is ignored as having no practical significance.

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[1] Am. Chem. Journ., 10, 74.

A few determinations of the atomic weight of iron by Winkler[1] still need to be mentioned, not as directly significant, but as relating to the validity of a method which he applied to nickel and cobalt. Iron, not absolutely pure, was dissolved in a solution of iodine and potassium iodide. The quantity of iodine was known, and after the reaction ended the amount unconsumed was measured by titration with thiosulphate solution. A ratio between iodine and iron was thus determined, which can be expressed as I_2 : Fe : : 100 : x. Two series are given, one with iron cleaned by scrubbing, the other with iron which had been heated in hydrogen. The weights of iron given below are corrected for known impurities.

Series I.

Fe.	*I.*	*Ratio.*
.5726	2.585609	22.146
.5778	2.608375	22.152
.5721	2.582935	22.149
		Mean, 22.149, ± .0012

Hence Fe = 56.223.

Series II.

Fe.	*I.*	*Ratio.*
.8252	3.727316	22.139
.8430	3.809144	22.131
.8349	3.771613	22.137
		Mean, 22.136, ± .0017

Hence Fe = 56.190. The weighted mean of both series is 22.145, ± .0010, which gives Fe = 56.213. This value is high, and so are the values found for cobalt and nickel by the same method. The process is probably affected by serious constant errors, and the results obtained by it are not good. For comparative purposes, however, the iodine ratio is included in the following tabulation of ratios:

(1). Fe_2O_3 : 2Fe : : 100 : 69.9728, ± .00083
(2). 2Ag : $FeBr_2$: : 100 : 99.960, ± .0027
(3). 2AgBr : $FeBr_2$: : 100 : 57.4195, ± .00044
(4). 2Ag : $FeCl_2$: : 100 : 58.866, ± .0530
(5). 3Ag : $FeCl_3$: : 100 : 50.2435, ± .0132
(6). I_2 : Fe : : 100 : 22.145, ± .0010
(7). H : Fe : : 1 : 55.608, ± .0532

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[1] Zeitsch. anorg. Chem., 8, 291. 1895.

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Br = 79.9197, ± .0003
Cl = 35.4584, ± .0002
I = 126.9204, ± .00033
H = 1.00779, ± .00001

Hence,

From ratio 3 ....Fe = 55.828, ± .0018
" " 2 ....55.836, ± .0062
" " 1 ....55.927, ± .0018
" " 7 ....56.042, ± .0532
" " 4 ....56.092, ± .1144
" " 6 ....56.213, ± .0026
" " 5 ....56.231, ± .0428

General mean, Fe = 55.943, ± .0011

The last four of these values are evidently not to be trusted. The first three, which are good, give a general mean of Fe = 55.880, ± .0012. This agrees well with the oxide series of Richards and Baxter, and is probably near the truth.

NICKEL AND COBALT.

On account of the close similarity of these metals to each other, their atomic weights, approximately if not actually identical, have received of late years much attention.

The first determinations, and the only ones up to 1852, were made by Rothhoff,[1] each with but a single experiment. For nickel 188 parts of the monoxide were dissolved in hydrochloric acid; the solution was evaporated to dryness, the residue was dissolved in water, and precipitated by silver nitrate. 718.2 parts of silver chloride were thus formed; whence Ni = 59.05. The same process was applied also to cobalt, 269.2 parts of the oxide being found equivalent to 1029.9 of AgCl; hence Co = 58.93. These values are so nearly equal that their differences were naturally ascribed to experimental errors. They are, however, entitled to no special weight at present, since it cannot be certain from any evidence recorded that the oxide of either metal was absolutely free from traces of the other.

In 1852 Erdmann and Marchand[2] published some figures, but without details, concerning the atomic weight of nickel. They reduced the oxide by heating in a current of hydrogen, and obtained values ranging from 58.2 to 58.6, when O = 16.

In 1856, incidentally to other work, Deville[3] found that 100 parts of pure metallic nickel yielded 262 of sulphate; whence Ni = 59.26.

To none of the foregoing estimations can any importance now be attached. The modern discussion of the atomic weights under consideration began with the researches of Schneider[4] in 1857. This chemist examined the oxalates of both metals, determining carbon by the combustion of the salts with copper oxide in a stream of dry air. The carbon dioxide thus formed was collected as usual in a potash bulb, which, in weighing, was counterpoised by a similar bulb, so as to eliminate errors due to the hygroscopic character of the glass. The metal in each oxalate was estimated, first by ignition in a stream of dry air, followed by intense heating in hydrogen. Pure nickel or cobalt was left behind in good condition for weighing. Four analyses of each oxalate were made, with the

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[1] Cited by Berzelius. Poggend. Annalen, 8, 184. 1826.

[2] Journ. prakt. Chem., 55, 202. 1852.

[3] Ann. Chim. Phys. (3), 46, 182. 1856.

[4] Poggend. Annalen, 101, 387. 1857.

results given below. The nickel salt contained three molecules of water, and the cobalt salt two molecules:

$NiC_2O_4.3H_2O$.

1.1945	grm. gave	.528	grm. CO_2.	44.203 per cent.
2.5555	"	1.12625	"	44.072 "
3.199	"	1.408	"	44.014 "
5.020	"	2.214	"	44.104 "

Mean, 44.098, ± .027

The following percentages of nickel were found in this salt:

29.107
29.082
29.066
29.082

Mean, 29.084, ± .006

$CoC_2O_4.2H_2O$.

1.6355	grm. gave	.781	grm. CO_2.	47.753 per cent.
1.107	"	.5295	"	47.832 "
2.309	"	1.101	"	47.683 "
3.007	"	1.435	"	47.722 "

Mean, 47.7475, ± .0213

The following were the percentages found for cobalt:

32.552
32.619
32.528
32.523

Mean, 32.5555, ± .0149

In a later paper[1] Schneider also gives some results obtained with a nickel oxalate containing but two molecules of water. This gave him 47.605 per cent. of CO_2, and the following percentages of nickel:

31.4115
31.4038

Mean, 31.4076, ± .0026

The conclusion at which Schneider arrived was that the atomic weights of cobalt and nickel are not identical, being about 60 and 58, respectively.

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[1] Poggend. Annalen, 107, 616.

The percentages given above will be discussed at the end of this chapter in connection with all the other data relative to the constants in question.

The next chemist to take up the discussion of these atomic weights was Marignac, in 1858.[1] He worked with the chlorides and sulphates of nickel and cobalt, using various methods, but publishing few details, as he did not consider the determinations final. The sulphates, taken as anhydrous, were calcined to oxides. From the ratio $NiSO_4 : NiO$, he found Ni = 58.4 to 59.0, and from five measurements of the ratio $CoSO_4 : Co$, Co = 58.64 to 58.76. If oxygen is taken as 16, these give for the percentages of oxide in sulphate:

CoO in $CoSO_4$.	*NiO in $NiSO_4$.*
48.267	48.187
48.307	48.387
Mean, 48.287, ± .0135	Mean, 48.287, ± .0675

Hence Co = 58.706. Hence Ni = 58.706.

The chlorides were dried at 100°, but found to retain water; and in most cases were then either fused in a stream of chlorine or of dry, gaseous hydrochloric acid, or else calcined gently with ammonium chloride. The determinations were then made by titration with a standard solution of silver in nitric acid. Five experiments with anhydrous $CoCl_2$ gave Co = 58.72 to 58.84. Three more with $CoCl_2$ dried at 100° gave Co = 58.84 to 59.02. Three with anhydrous $NiCl_2$ gave Ni = 58.80 to 59.00. If the calculations were made with Ag = 108 and Cl = 35.5, then these data give as proportional to 100 parts of silver:

$NiCl_2$.	*$CoCl_2$.*
60.093	60.056
60.185	60.111
	60.111
Mean, 60.139, ± .0310	60.194
	Mean, 60.118, ± .0192

Hence Ni = 58.84. Hence Co = 58.79.

In one more experiment $NiCl_2$ was precipitated with a known quantity of silver. The filtrate was calcined, yielding NiO: hence the ratio $2Ag : NiO$, giving Ni = 59.29. This experiment needs no farther attention.

In short, according to Marignac, and contrary to Schneider's views, the two atomic weights are approximately the same. Marignac criticises Schneider's earlier paper, holding that the nickel oxalate may have con-

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[1] Arch. Sci. Phys. Nat. (nouv. série), 1, 372. 1858. Oeuvres Complètes, 1, 575.

tained some free oxalic acid, and that the cobalt salt was possibly contaminated with carbonate or with basic compounds. In his later papers Schneider rejects these suggestions as unfounded, and in turn criticises Marignac. The purity of anhydrous $NiSO_4$ is not easy to guarantee, and, according to Schneider, the anhydrous chlorides of cobalt and nickel are liable to be contaminated with oxides. This is the case even when the chlorides are heated in chlorine, unless the gas is carefully freed from all traces of air and moisture.

Dumas'[1] determinations of the two atomic weights were made with the chlorides of nickel and cobalt. The pure metals were dissolved in aqua regia, the solutions were repeatedly evaporated to dryness, and the residual chlorides were ignited in dry hydrochloric acid gas. The last two estimations in the nickel series were made upon $NiCl_2$ formed by heating the spongy metal in pure chlorine. In the third column I give the $NiCl_2$ or $CoCl_2$ equivalent to 100 parts of silver:

.9123 grm. $NiCl_2$ =	1.515 grm. Ag.		60.218
2.295 "	3.8115	"	60.212
3.290 "	5.464	"	60.212
1.830 "	3.041	"	60.178
3.001 "	4.987	"	60.176

Mean, 60.1992, ± .0062

Hence Ni = 58.97.

2.352 grm. $CoCl_2$ =	3.9035 grm. Ag.		60.254
4.210 "	6.990	"	60.229
3.592 "	5.960	"	60.268
2.492 "	4.1405	"	60.186
4.2295 "	7.0255	"	60.202

Mean, 60.2278, ± .011

Hence Co = 59.03.

These values for Co and Ni differ by less than a tenth of a unit; here, as elsewhere, the figure for Ni being a trifle the lower.

Combining these data for nickel with Marignac's series, we have—

$2Ag:NiCl_2::100:x$.

Marignac	60.139, ± .0310
Dumas	60.199, ± .0062
General mean	60.194, ± .0061

The cobalt figures will be combined with others later.

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[1] Ann. Chem. Pharm., 113, 25. 1860.

In 1863[1] the idea that nickel and cobalt have equal atomic weights was strengthened by the researches of Russell. He found that the black oxide of cobalt, by intense heating in an atmosphere of carbon dioxide, became converted into a brown monoxide of constant composition. The ordinary oxide of nickel, on the other hand, was shown to be convertible into a definite monoxide by simple heating over the blast lamp. The pure oxides of the two metals, thus obtained, were reduced by ignition in hydrogen, and their exact composition thus ascertained. Several samples of each oxide were taken, yielding the following data. The separate samples are indicated by lettering:

Nickel.

	NiO.	*Ni.*	*Per cent. Ni.*
A	2.0820	1.6364	78.597
	2.0956	1.6468	78.584
	2.0148	1.5838	78.608
B	2.2069	1.7342	78.581
	2.2843	1.7952	78.589
	2.1329	1.6761	78.583
C	2.2783	1.7911	78.616
	2.1434	1.6845	78.590
	2.4215	1.9030	78.588
D	2.1859	1.7179	78.590
	2.0088	1.5788	78.594
	2.0839	1.6379	78.597
	2.6560	2.0873	78.588

Mean, 78.593, ± .0018

Cobalt.

	CoO.	*Co.*	*Per cent. Co.*
A	2.1211	1.6670	78.591
	2.0241	1.5907	78.588
	2.1226	1.6673	78.550
	1.9947	1.5678	78.598
	3.0628	2.4078	78.614
B	2.1167	1.6638	78.603
	1.7717	1.3924	78.591
	1.7852	1.4030	78.591
C	1.6878	1.3264	78.588
	2.2076	1.7350	78.592
D	2.6851	2.1104	78.597
	2.1461	1.6868	78.598
E	3.4038	2.6752	78.595
	2.2778	1.7901	78.589
	2.1837	1.7163	78.596

Mean, 78.592, ± .0023

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[1] Journ. Chem. Soc. (2), 1, 51. 1863.

These percentages are practically identical, and lead to essentially the same mean value for each atomic weight, namely,

Ni = 58.742
Co = 58.738

In a later paper Russell[1] confirmed the foregoing results by a different process. He dissolved metallic nickel and cobalt in hydrochloric acid and measured the hydrogen evolved. Thus the ratio between the metal and his ultimate standard was fixed without the intervention of any other element. About two-tenths of a gramme of metal, or less, was taken in each experiment. The data obtained were as follows; the last column giving the weight of hydrogen, computed from its volume, yielded by 100 parts of cobalt or nickel:

Nickel.

	Wt. Ni.	*Vol. H in cc.*	*Ratio.*
A	.0906	153.62	3.420
	.1017	172.32	3.418
	.1990	337.06	3.416
	.0997	168.93	3.417
	.1891	319.86	3.412
	.1859	314.75	3.415
	.1838	311.25	3.416
B	.1892	318.75	3.398
	.1806	305.28	3.409
	.2026	333.81	3.404
C	.1933	325.93	3.401
D	.1890	319.77	3.412
	.1942	328.15	3.408
	.1781	301.09	3.410

Mean, 3.411, ± .001

Cobalt.

	Wt. Co.	*Vol. H in cc.*	*Ratio.*
A	.1958	321.36	3.395
	.1905	312.95	3.398
	.1946	319.63	3.397
	.2002	328.96	3.398
B	.1996	328.43	3.403
	.2000	329.55	3.401
	.1721	290.17	3.401
C	.1877	308.97	3.404
	.1935	318.60	3.405
D	.1909	314.73	3.410
	.1834	305.40	3.407

Mean, 3.4017, ± .0009

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[1] Journ. Chem. Soc. (2), 7, 294. 1869.

The weight of the hydrogen in these determinations was doubtless computed from Regnault's figures for the density of that gas. Correcting by the new value for the weight of a litre of hydrogen. .089872 gramme, the ratios become:

For nickel .... 3.4211, ± .0010
For cobalt .... 3.4112, ± .0009

Hence Ni=58.92 and Co=59.09.

Some time after the publication of Russell's first paper, but before the appearance of his second, some other investigations were made known. Of these the first was by Sommaruga,[1] whose results, obtained by novel methods, closely confirmed those of Schneider and antagonized those of Dumas, Marignac and Russell. The atomic weight of nickel Sommaruga deduced from analyses of the nickel potassium sulphate, $K_2Ni(SO_4)_2.6H_2O$, which, dried at 100°, has a perfectly definite composition. In this salt the sulphuric acid was determined in the usual way as barium sulphate, a process to which there are obvious objections. In the third column are given the quantities of the nickel salt proportional to 100 parts of $BaSO_4$:

.9798	grm. gave	1.0462	grm. $BaSO_4$.	93.653
1.0537	"	1.1251	"	93.654
1.0802	"	1.1535	"	93.645
1.1865	"	1.2669	"	93.654
3.2100	"	3.4277	"	93.649
3.2124	"	3.4303	"	93.648
				Mean, 93.6505, ± .001

Hence Ni=58.79.

For cobalt Sommaruga used the purpureocobalt chloride of Gibbs and Genth. This salt, dried at 110°, is anhydrous and stable. Heated hotter, $CoCl_2$ remains. The latter, ignited in hydrogen, yields metallic cobalt. In every experiment the preliminary heating must be carried on cautiously until ammoniacal fumes no longer appear:

.6656	grm. gave	.1588	grm. Co.	23.858	per cent.
1.0918	"	.2600	"	23.814	"
.9058	"	.2160	"	23.846	"
1.5895	"	.3785	"	23.813	"
2.9167	"	.6957	"	23.847	"
1.8390	"	.4378	"	23.806	"
2.5010	"	.5968	"	23.808	"
				Mean, 23.827, ± .006	

Hence Co=59.91.

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[1] Sitzungsb. Wien. Akad., 54, 2 Abth., 50. 1866.

Further along this series will be combined with a similar one by Lee. It may here be said that Sommaruga's paper was quickly followed by a critical essay from Schneider,[1] endorsing the former's work and objecting to the results of Russell.

In 1867 still another new process for the estimation of these atomic weights was put forward by Winkler,[2] who determined the amount of gold which pure metallic nickel and cobalt could precipitate from a neutral solution of sodio-auric chloride.

In order to obtain pure cobalt Winkler prepared purpureocobalt chloride, which, having been four or five times recrystallized, was ignited in hydrogen. His nickel was repeatedly purified by precipitation with sodium hypochlorite. From material thus obtained pure nickel chloride was prepared, which, after sublimation in dry chlorine, was also reduced by hydrogen. One hundred parts of gold are precipitated by the quantities of nickel and cobalt given in the third columns, respectively. In the cobalt series I include one experiment by Weselsky, which was published by him in a paper presently to be cited:

.4360	grm. nickel precipitated	.9648	grm. gold.	45.191
.4367	"	.9666	"	45.179
.5189	"	1.1457	"	45.291
.6002	"	1.3286	"	45.175
			Mean,	45.209, ± .019

Hence Ni = 59.46.

.5890	grm. cobalt precipitated	1.3045	grm. gold.	45.151
.3147	"	.6981	"	45.080
.5829	"	1.2913	"	45.141
.5111	"	1.1312	"	45.182
.5821	"	1.2848	"	45.307
.559	"	1.241	"	45.044–Weselsky
			Mean,	45.151, ± .025

Hence Co = 59.38.

Weselsky's paper,[3] already quoted, relates only to cobalt. He ignited the cobalticyanides of ammonium and of phenylammonium in hydrogen, and from the determinations of cobalt thus made deduced its atomic weight. His results are as follows:

.7575	grm. $(NH_4)_6Co_2Cy_{12}$ gave	.166	grm. Co.	21.914 per cent.
.5143	"	.113	"	21.972 "
			Mean,	21.943, ± .029

Hence Co = 59.09.

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[1] Poggend. Annalen, 130, 310.

[2] Zeit. anal. Chem., 6, 18. 1867.

[3] Ber. Deutsch. chem. Gesell., 2, 592. 1868.

.8529 grm.	$(C_6H_8N)_6Co_2Cy_{12}$ gave	.1010 grm.	Co.	11.842	per cent.
.6112	"	.0723	"	11.829	"
.7140	"	.0850	"	11.905	"
.9420	"	.1120	"	11.890	"

Mean, 11.8665, ± .0124

Hence Co = 59.04.

Next in order is the work done by Lee[1] in the laboratory of Wolcott Gibbs. Like Weselsky, Lee ignited certain cobalticyanides and also nickelocyanides in hydrogen and determined the residual metal. The double cyanides chosen were those of strychnia and brucia, salts of very high molecular weight, in which the percentages of metal are relatively low. A series of experiments with purpureocobalt chloride was also carried out. In order to avoid admixture of carbon in the metallic residues, the salts were first ignited in air, and then in oxygen. Reduction by hydrogen followed. The salts were in each case covered by a porous septum of earthenware, through which the hydrogen diffused, and which served to prevent the mechanical carrying away of solid particles; furthermore, heat was applied from above. The results attained appeared to be satisfactory, and assign to nickel and cobalt atomic weights varying from each other by about a unit; Ni being nearly 58, and Co about 59, when O = 16. The cobalt results agree remarkably well with those of Weselsky. The following are the data obtained:

Brucia nickelocyanide, $Ni_3Cy_{12}(C_{23}H_{26}N_2O_4)_6H_6.10H_2O$.

Salt.	*Ni.*	*Per cent. Ni.*
.3966	.0227	5.724
.5638	.0323	5.729
.4000	.0230	5.750
.3131	.01795	5.733
.4412	.0252	5.712
.4346	.0249	5.729

Mean, 5.7295, ± .0034

Hence Ni = 58.027.

Strychnia nickelocyanide, $Ni_3Cy_{12}(C_{21}H_{22}N_2O_2)_6H_6.8H_2O$.

Salt.	*Ni.*	*Per cent. Ni.*
.5358	.0354	6.607
.5489	.0363	6.613
.3551	.0234	6.589
.4495	.0297	6.607
.2530	.0166	6.561
.1956	.0129	6.595

Mean, 6.595, ± .005

Hence Ni = 58.085.

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[1] Am. Journ. Sci. (3), 2, 44. 1871.

Brucia cobalticyanide, $Co_2Cy_{12}(C_{23}H_{26}N_2O_4)_6H_6.20H_2O$.

Salt.	*Co.*	*Per cent. Co.*
.4097	.0154	3.759
.3951	.0147	3.720
.5456	.0204	3.739
.4402	.0165	3.748
.4644	.0174	3.747
.4027	.0151	3.749
		Mean, 3.7437, ± .0036

Hence Co = 59.20.

Strychnia cobalticyanide, $Co_2Cy_{12}(C_{21}H_{22}N_2O_2)_6H_6.8H_2O$.

Salt.	*Co.*	*Per cent. Co.*
.4255	.0195	4.583
.4025	.0185	4.596
.3733	.0170	4.554
.4535	.0207	4.564
.2753	.0126	4.577
.1429	.0065	4.549
		Mean, 4.5705, ± .005

Hence Co = 59.10.

Purpureocobalt chloride, $Co_2(NH_3)_{10}Cl_6$.

Salt.	*Co.*	*Per cent. Co.*
.9472	.2233	23.575
.8903	.2100	23.587
.6084	.1435	23.586
.6561	.1547	23.579
.6988	.1647	23.569
.7010	.1653	23.581
		Mean, 23.5795, ± .0019

Hence Co = 59.10.

The last series may be combined with Sommaruga's, thus:

Sommaruga	23.827, ± .006
Lee	23.5795, ± .0019
General mean	23.6045, ± .0018

Baubigny's[1] determinations of the atomic weight of nickel are limited

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[1] Compt. Rend., 97, 951. 1883.

to two experiments upon the calcination of nickel sulphate, and his data are as follows:

6.2605 grm. $NiSO_4$ gave	3.0225 NiO.	48.279 per cent.
4.4935 "	2.1695 "	48.281 "

Mean, 48.280

Hence Ni = 58.741.

Zimmermann's work, published after his death by Krüss and Alibegoff,[1] was based, like Russell's, upon the reduction of cobalt and nickel oxides in hydrogen. The materials used were purified with great care, and the results were as follows:

Nickel.

NiO.	*Ni.*	*Per cent. Ni.*
6.0041	4.7179	78.578
6.4562	5.0734	78.582
8.5960	6.7552	78.585
4.7206	3.7096	78.583
8.2120	6.4536	78.587
9.1349	7.1787	78.585
10.0156	7.8702	78.579
4.6482	3.6526	78.580
8.9315	7.0184	78.580
10.7144	8.4196	78.582
3.0036	2.3602	78.579

Mean, 78.582, ± .0006

Hence Ni = 58.704.

Cobalt.

CoO.	*Co.*	*Per cent. Co.*
6.3947	5.0284	78.634
6.6763	5.2501	78.638
5.6668	4.4560	78.633
2.9977	2.3573	78.637
8.7446	6.8763	78.635
3.2625	2.5655	78.636
6.3948	5.0282	78.630
8.2156	6.4606	78.638
9.4842	7.4580	78.636
9.9998	7.8630	78.632

Mean, 78.635, ± .0002

Hence Co = 58.889.

Shortly after the discovery of nickel carbonyl, NiC_4O_4, Mond, Langer and Quincke[2] made use of it with reference to the atomic weight of

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[1] Ann. Chem., 232, 324. 1886.

[2] Journ. Chem. Soc., 57, 753. 1890.

nickel. The latter was purified by distillation as nickel carbonyl, then converted into oxide, and that was reduced by hydrogen in the usual way.

NiO.	*Ni.*	*Per cent. Ni.*
.2414	.1896	78.542
.3186	.2503	78.562
.3391	.2663	78.531
		Mean, 78.545, ± .0061

Hence Ni=58.575.

Schutzenberger's experiments,[1] published in 1892, were also few in number. First, nickel sulphate, dehydrated at 440°, was calcined to oxide.

3.505 grm. $NiSO_4$ gave 1.690 NiO.		48.217 per cent.
2.6008 "	1.2561 "	48.297 "
		Mean, 48.257, ± .027

Hence Ni=58.672.

Secondly, nickel oxide was reduced in hydrogen, as follows:

1.6865 grm. NiO gave 1.3245 Ni.		78.535 per cent.
1.2527 "	.9838 "	78.533 "
		Mean, 78.534

Hence Ni=58.536.

In one experiment with cobalt oxide, 3.491 grm. gave 2.757 Co, or 78.975 per cent. Hence Co=60.1. In view of the many determinations of this ratio by other observers, this single estimation may be neglected. The experiments on nickel sulphate, however, should be combined with those of Marignac and Baubigny, giving the latter equal weight with Schutzenberger's, thus:

Marignac	48.287, ± .0675
Baubigny	48.280, ± .027
Schutzenberger	48.257, ± .027
General mean	48.269, ± .018

From this point on the determination of these atomic weights was temporarily complicated by the questions raised by Krüss as to the truly elementary character of nickel and cobalt. If that which has been called nickel really contains an admixture of some other hitherto unknown element, then all the determinations made so far are worthless, and the investigations now to be considered bear directly upon that question.

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[1] Compt. Rend., 114, 1149. 1892.

First in order comes Remmler's research upon cobalt.[1] This chemist, asking whether cobalt is homogeneous, prepared cobaltic hydroxide in large quantity, and made a series of successive ammoniacal extracts from it, twenty-five in all. Each extract represented a fraction, from which, by a long series of operations, cobalt monoxide was prepared, and the latter was reduced in hydrogen after the manner of Russell. The actual determinations began with the second fraction, and the data are subjoined, the number of the fraction being given with each experiment:

	CoO.	*Co.*	*Per cent. Co.*
2	.09938	.07837	78.859
3	.15021	.11814	78.650
4	.22062	.17360	78.687
5	.39011	.30681	78.647
6	.28820	.22661	78.629
7	.34304	.26968	78.615
8	.43703	.34321	78.532
9	.91477	.71864	78.560
10	.63256	.49661	78.508
11	.32728	.25701	78.529
12	.38042	.29899	78.595
13	.16580	.13027	78.571
14	1.01607	.79873	78.610
15	1.31635	1.03545	78.661
16	.91945	.72315	78.650
17	.53100	.41773	78.668
18	.82381	.64728	78.572
19	.81139	.63754	78.574
20	.76698	.60292	78.610
21	1.13693	.89412	78.643
22	2.00259	1.57495	78.646
23	1.04629	.82185	78.549
24	.48954	.38466	78.576
25	.69152	.54326	78.560

Mean, 78.613, ± .0099

Hence Co = 58.812.

Considered with reference to the purpose of the investigation, this mean and its probable error have no real significance. But it is very close to the means of other experimenters, and a study of the variations represented by the several fractions seems to indicate fortuity rather than system. Remmler regards his results as indicating lack of homogeneity in his material; but it seems more probable that such differences as exist are due to experimental errors and to impurities acquired in the long process of purification to which each fraction was submitted, rather than to any uncertainty regarding the nature of cobalt itself.

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[1] Zeit. anorg. Chem., 2, 221. Also more fully in an Inaugural Dissertation, Erlangen, 1891.

From the same point of view—that is, with reference to the supposed heterogeneity of nickel—Krüss and Schmidt[1] carried out a series of fractionations of the metal by distillation in a stream of carbon monoxide. Nickel oxide, free from obnoxious impurities, was first reduced to metal by heating in hydrogen, after which the current of carbon monoxide was allowed to flow. The latter, carrying its small charge of nickel tetracarbonyl was then passed through a Winkler's absorption apparatus containing pure aqua regia, from which, by evaporation, nickel chloride was obtained, and from that, by reduction in hydrogen, the nickel. Ten such fractions were successively prepared and studied; first, by preparation of NiO and its reduction in hydrogen; and, secondly, in some cases, by the reoxidation of the reduced metal, so as to give a synthetic value for the ratio Ni : O. The data obtained are as follows, the successive fractions being numbered:

Reduction of NiO.

	NiO.	*Ni.*	*Per cent. Ni.*
1	.3722	.2926	78.614
	.7471	.5870	78.571
2	.7659	.60085	78.450
	.7606	.5961	78.372
3	1.0175	.7984	78.467
	1.2631	.99065	78.430
	1.2582	.9868	78.429
4	.5193	.4076	78.490
	.9200	.7215	78.424
5	.4052	.3179	78.455
	.6518	.5111	78.414
6	.5623	.4399	78.232
	.5556	.4350	78.294
7	.9831	.7724	78.568
	.9765	.7646	78.300
	.9639	.7557	78.400
8	.5756	.4538	78.839
	.56765	.4451	78.411
	.5663	.4438	78.368
	.5449	.4272	78.400
9	.3174	.2491	78.481
	.3148	.2467	78.367
10	.4976	.3904	78.457
	.4961	.3891	78.432

Mean, 78.444, ± .0166

Hence Ni = 58.225.

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[1] Zeit. anorg. Chem., 2, 235. 1892.

Oxidation of Ni.

	Ni.	*NiO.*	*Per cent. Ni.*
1	.5870	.7471	78.571
2	.6011	.7659	78.372
	.5961	.7606	78.359
3	.7988	1.0175	78.506
	.9913	1.2631	78.482
	.9868	1.2582	78.429
4	.4093	.5193	78.818
	.7216	.9200	78.435
5	.3194	.4052	78.825
	.5111	.6518	78.414
6	.4415	.5623	78.517
	.4350	.5556	78.294
7	.7752	.9831	78.853
	.7667	.9765	78.515
	.7558	.9639	78.411
8	.4555	.5756	79.135
	.4456	.56765	78.499
	.44415	.5663	78.430
	.4423	.5642	78.394
9	.2508	.3174	79.015
	.2467	.3148	78.367
10	.3918	.4976	78.738
	.3891	.4961	78.432
			Mean, 78.557, ± .0319

Hence Ni=58.616.

To these data of Krüss and Schmidt the remarks already made concerning Remmler's work seem also to apply. The variations appear to be fortuitous, and not systematic, although the authors seem to think that they indicate a compositeness in that substance which has been hitherto regarded as elementary nickel. In view of all the evidence, however, I prefer to regard their varying estimations as affected by accidental errors, and to treat their means like others. On this basis, their work combines with previous work as follows, Schutzenberger's measurements of the ratio NiO : Ni being assigned equal weight with those of Mond, Langer and Quincke:

Russell	78.593, ± .0018
Zimmermann	78.582, ± .0006
Mond, Langer, and Quincke	78.545, ± .0061
Schutzenberger	78.534, ± .0061
Krüss and Schmidt, reduction series	78.444, ± .0166
Krüss and Schmidt, oxidation series	78.557, ± .0319
General mean	78.570, ± .0006

In 1889 Winkler[1] published a short paper concerning the gold method for determining the atomic weights in question, but gave in it no actual measurements. In 1893[2] he returned to the problem with a new line of attack, and at the same time he took occasion to criticise Krüss and Schmidt somewhat severely. He utterly rejects the notion that either nickel or cobalt contain any hitherto unknown element, and ascribes the peculiar results obtained by Krüss and Schmidt to impurities derived from the glass apparatus used in their experiments. For his own part he now works with pure nickel and cobalt precipitated electrolytically upon platinum, and avoids the use of glass or porcelain vessels so far as possible. With material thus obtained he operates by two distinct but closely related methods, both starting with the metal, nickel or cobalt, converting it next into neutral chloride, and then measuring the chloride gravimetrically in one process, volumetrically in the other.

After precipitation in a platinum dish, the nickel or cobalt is washed with water, rinsed with alcohol and ether, and then weighed. It is next dissolved in pure hydrochloric acid, properly diluted, and by evaporation to dryness and long heating to 150° converted into anhydrous chloride. The nickel chloride thus obtained dissolves perfectly in water, but the cobalt salt always gave a slight residue in which the metal was electrolytically determined and allowed for. In the redissolved chloride, by precipitation with silver nitrate, silver chloride is obtained, giving a direct ratio between that compound and the nickel or cobalt originally taken. The gravimetric data are as follows, with the metal equivalent to 100 parts of silver chloride given in a final column:

Nickel.

Ni.	*AgCl.*	*Ratio.*
.3011	1.4621	20.594
.2242	1.0881	20.605
.5166	2.5108	20.570
.4879	2.3679	20.605
.3827	1.8577	20.601
.3603	1.7517	20.568
		Mean, 20.590, ± .0049

Hence Ni = 59.03.

Cobalt.

Co.	*AgCl.*	*Ratio.*
.3458	1.6596	20.836
.3776	1.8105	20.856
.4493	2.1521	20.877

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[1] Ber. Deutsch. chem. Gesell., 22, 891. 1889.

[2] Zeit. anorg. Chem., 4, 10. 1893.

.4488	2.1520	20.855
.2856	1.3683	20.873
.2648	1.2768	20.886
		Mean, 20.864, ± .0050

Hence Co = 59.81.

In the volumetric determinations the neutral chloride, prepared as before, was decomposed by means of a slight excess of potassium carbonate, and in the potassium chloride solution, after removal of the nickel or cobalt, the chlorine was measured by titration by Volhard's method with a standard solution of silver. The amount of silver thus used was comparable with the metal taken.

Nickel.

Ni.	*Ag.*	*Ratio.*
.1812	.6621260	27.366
.1662	.6079206	27.339
.2129	.7775252	27.382
.2232	.8162108	27.346
.5082	1.8556645	27.386
.1453	.5315040	27.338
		Mean, 27.359, ± .0059

Hence Ni = 59.03.

Cobalt.

Co.	*Ag.*	*Ratio.*
.177804	.6418284	27.702
.263538	.9514642	27.699
.245124	.8855780	27.679
.190476	.6866321	27.741
.266706	.9629146	27.696
.263538	.9503558	27.731
		Mean, 27.708, ± .0064

In view of the possibility that the cobalt chloride of the foregoing experiments might contain traces of basic salt, Winkler, in a supplementary investigation,[1] checked them by another process. To the electrolytic cobalt, in a platinum dish, he added a quantity of neutral silver sulphate and then water. The cobalt gradually went into solution, and metallic silver was precipitated. The weights were as follows:

Co.	*Ag.*
.2549	.9187
.4069	1.4691

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[1] Zeit. anorg. Chem., 4, 462. 1893.

On examination of the silver it was found that traces of cobalt were retained—less than 0.5 mg. in the first determination and less than 0.2 mg. in the second. Taking these amounts as corrections, the two experiments give for the ratio 2Ag : Co :: 100 : x the subjoined values:

27.706
27.687

These figures confirm those previously found, and as they fall within the limits of the preceding series, they may fairly be included in it, when all eight values give a mean of 27.705, ±.0050. Hence Co = 59.78.

Still another method, radically different from all of the foregoing processes, was adopted by Winkler in 1894.[1] The metals were thrown down electrolytically upon platinum, and so weighed. Then they were treated with a known excess of a decinormal solution of iodine in potassium iodide, which redissolved them as iodides. The excess of free iodine was then determined by titration with sodium thiosulphate, and in that way the direct ratio between metal and haloid was ascertained. The results were as follows, with the metal proportional to 100 parts of iodine given in the third column:

Cobalt.

	Wt. Co.	*Wt. I.*	*Ratio.*
First series	.4999	2.128837	23.482
	.5084	2.166750	23.463
	.5290	2.254335	23.466
	.6822	2.908399	23.456
	.6715	2.861617	23.466
Second series	.5185	2.209694	23.465
	.5267	2.246037	23.450
	.5319	2.268736	23.445
			Mean, 23.462, ± .0027

Hence Co = 59.56.

Nickel.

	Wt. Ni.	*Wt. I.*	*Ratio.*
First series	.5144	2.217494	23.251
	.4983	2.148502	23.246
	.5265	2.268742	23.260
	.6889	2.970709	23.243
	.6876	2.965918	23.237
Second series	.5120	2.205627	23.267
	.5200	2.240107	23.267
	.5246	2.259925	23.267
			Mean, 23.255, ± .0091

Hence Ni = 59.03.

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[1] Zeitsch. anorg. Chem., 8, 1. 1894.

In these experiments, as well as in some previous series, a possible source of error is to be considered in the occlusion of hydrogen by the metals. Accordingly, in a supplementary paper, Winkler[1] gave the results of some check experiments made with iron, which, however, was not absolutely pure. The conclusion is that the error, if existent, must be very small.

In 1895 Hempel and Thiele's work on cobalt appeared.[2] First, cobalt oxide, prepared from carefully purified materials, was reduced in hydrogen. The weights of metal and oxygen are subjoined, with the percentage of cobalt in the oxide deduced from them:

Co.	*O.*	*Percentage.*
.90068	.24429	78.664
.79159	.21445	78.686
1.31558	.35716	78.648
		Mean, 78.666, ± .0074

Hence Co = 58.998.

In their next series of experiments, excluding a rejected series, Hempel and Thiele weighed cobalt, converted it into anhydrous chloride, and noted the gain in weight. In four of the experiments the chloride was afterwards dissolved, precipitated with silver nitrate, and then the silver chloride was weighed. The data are as follows:

Co.	*Cl taken up.*	*AgCl.*
.7010	.8453	.....
.3138	.3793	.....
.2949	.3562	1.4340
.4691	.5657	2.2812
.5818	.7026	2.8303
.5763	.6947	.....
.5096	.6142	2.4813

From these weights we get two ratios, thus:

$Cl_2:Co:100:x$.	$2AgCl:Co::100:x$.
82.929	20.565
82.731	20.564
82.791	20.556
82.924	20.538
82.807	
82.957	Mean, 20.556, ± .0043
82.970	
Mean, 82.873, ± .0241	
Hence Co = 58.77.	Hence Co = 58.93.

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[1] Zeitsch. anorg. Chem., 8, 291. 1895. See preceding section of this work, on iron, for the detailed determinations.

[2] Zeitsch. anorg. Chem., 11, 73.

The second of these ratios was also studied by Winkler, and the two series combine as follows:

Winkler	20.864, ± .0050
Hempel and Thiele...........	20.556, ± .0043
General mean	20.687, ± .0033

Hempel and Thiele apply to it a correction for silver chloride retained in solution, but its amount is small and not altogether certain. For present purposes the correction may be neglected.

The atomic weight of nickel was determined by Richards and Cushman[1] from analyses of nickel bromide. This salt, as first prepared, contained traces of oxide, which are to be deducted from the halide compound. In a preliminary series of experiments the following figures were obtained, representing vacuum weights:

$NiBr_2$.	*AgBr.*	*Insoluble, mg.*	*Ratio.*
2.26113	3.88769	3.22	58.161
2.80668	4.82431	7.08	58.178
1.41317	2.42880	3.05	58.184
1.71759	2.95307	.88	58.163
2.48565	4.27357	5.24	58.163
4.32997	7.44280	15.83	58.177
2.18072	3.74856	....	58.175
			Mean, 58.172, ± .0023

In the second set of analyses, both ratios were determined, namely, with silver and with silver bromide, by the standard methods. The data follow:

$NiBr_2$.	*AgBr.*	*Ag.*	*Ag ratio.*	*AgBr ratio.*
3.28039	5.63892	3.23910	101.275	58.174
2.70044	4.64208	2.66636	101.278	58.173
3.38230	5.81391	3.33990	101.270	58.176
1.33459	2.29435	1.31787	101.268	58.169
1.25054	2.14963	1.23482	101.273	58.175
1.32278	2.27384	1.30629	101.262	58.174
1.24452	2.85805	2.21652	101.263	58.177
		Mean,	101.270, ± .0015	58.174, ± .0007

This value for the AgBr ratio, combined with the preliminary series, gives a general mean of 55.1738, ± .0007.

From Ag ratio, Ni = 58.661.

From AgBr ratio, Ni = 58.661.

And Ag : Br :: 100 : 74.082.

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[1] Proc. Amer. Acad., 33, 97. 1897.

In a second memoir,[1] Richards and Cushman describe a series of determinations based upon the reduction of nickel bromide by heating in hydrogen. The corrected data appear in the next table:

$NiBr_2$.	*Ni.*	*Per cent. Ni.*
2.83325	.76081	26.853
3.21625	.86358	26.851
2.31241	.62094	26.853
2.87953	.77330	26.855
2.29650	.61679	26.858
2.98893	.80272	26.856
5.51291	1.48056	26.856
2.24969	.60415	26.855
		Mean, 26.855, ± .0005

Hence Ni=58.685.

In this series a correction was applied for traces of sodium bromide contained in the nickel salt. A similar correction, applied to the former series of determinations, would raise the atomic weight of nickel by 0.015.

The three memoirs upon cobalt, by Richards and Baxter,[2] contain data relative to the bromide, the chloride and the oxide. It is hardly necessary to state that all of the materials employed in the investigation were scrupulously purified, and that all weights were reduced to a vacuum basis. First, as in the case of nickel, the two silver ratios to the bromide were determined. A preliminary set of analyses gave results as follows:

$CoBr_2$.	*AgBr.*	*Ratio.*
2.25295	3.86818	58.243
2.88763	4.95732	58.250
1.88806	3.24056	58.263
		Mean, 58.252, ± .0040

The second and third series of analyses gave both ratios, and may be tabulated together:

$CoBr_2$.	*AgBr.*	*Ag.*	*Ag ratio.*	*AgBr ratio.*
1.33564	2.29296	1.31702	101.414	58.250
2.58129	4.43095	2.54585	101.392	58.256
2.84382	4.88135	2.80449	101.402	58.259
1.83722	3.15368	1.81170	101.409	58.256
2.68584	4.61046	2.64879	101.399	58.255
3.18990	5.47607	......	......	58.252

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[1] Proc. Amer. Acad., 34, 327. 1899. This memoir contains a very full criticism of all the earlier work on nickel.

[2] Proc. Amer. Acad., 33, 115. 1897. *Ibid.*, 34, 351. 1899. *Ibid.*, 35, 61. 1899. For a criticism of Richards, Cushman and Baxter, see Winkler, Zeitsch. anorg. Chem., 17, 236. 1898.

2.88914	4.95943	2.84891	101.412	58.255
2.32840	3.99706	2.29593	101.414	58.253
1.91703	3.29053	1.89033	101.413	58.259
			Mean, 101.407, ± .0018	58.255, ± .0007

The two series for the AgBr ratio, combined, give a weighted mean of 58.2549, ± .0007.

From Ag ratio, Co = 58.957.
From AgBr ratio, Co = 58.966.
And Ag : Br : : 100 : 74.075.

In their second memoir Richards and Baxter describe the reduction of cobalt bromide by heating in hydrogen. Three series of experiments were made, and in two of them a correction was necessary for small quantities of sodium bromide contained in the cobalt salt. In the following tables, the corrected weights of cobalt bromide and cobalt are given:

Series I.

$CoBr_2$.	*Co.*	*Per cent. Co.*
5.59023	1.50680	26.954
4.61518	1.24381	26.950
3.74498	1.00920	26.948
3.00135	.80899	26.954
		Mean, 26.951, ± .0010

Series II.

$CoBr_2$.	*Co.*	*Per cent. Co.*
5.32194	1.43428	26.950
7.50786	2.02321	26.948
2.32630	.62677	26.943
7.44694	2.00736	26.956
		Mean, 26.949, ± .0021

Series III.

$CoBr_2$.	*Co.*	*Per cent. Co.*
5.10891	1.37721	26.957
6.41339	1.72850	26.951
6.59805	1.77876	26.959
3.02854	.81606	26.953
		Mean, 26.953, ± .0021

The general mean of the three series is 26.9508, ± .0008. Hence Co = 58.971.

The third memoir of Richards and Baxter gives analyses of cobalt chloride and oxide. First, the chloride was reduced to metal by heating in hydrogen. Hempel and Thiele worked in the opposite direction, heating cobalt in chlorine and thereby effecting the synthesis of the compound. For uniformity of statement I give Richards and Baxter's series in the same form, as the ratio $Cl_2 : Co :: 100 : x$:

$CoCl_2$.	*Co.*	*Ratio.*
4.16483	1.89243	83.279
2.30512	1.04723	83.253
		Mean, 83.266, ± .0087

Hence Co = 59.050.

Hempel and Thiele's figures give for this ratio the figure 82.873, ± .0241. The general mean of both series is 83.220, ± .0082.

Five reductions of cobalt oxide in hydrogen are given, three in one series and two separate experiments with varied methods of manipulation. The results obtained are regarded by Richards and Baxter as unimportant, and they point out the difficulties of the process. Their data, arranged as one series, follow:

CoO.	*Co.*	*Per cent. Co.*
7.04053	5.53779	78.656
6.69104	5.26312	78.659
7.83211	6.15963	78.646
7.74240	6.09219	78.686
10.58678	8.32611	78.646
		Mean, 78.659, ± .0051

Hence Co = 58.973.

This mean combines with former means as follows:

Russell	78.592,	± .0023
Zimmermann	78.635,	± .0002
Remmler	78.613,	± .0099
Hempel and Thiele	78.666,	± .0074
Richards and Baxter	78.659,	± .0051
General mean	78.6324,	± .0002

Here Zimmermann's determinations practically appear alone.

The analyses of cobalt chloride by Baxter and Coffin[1] were made by

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[1] Journ. Amer. Chem. Soc., 28, 1580. 1906. Zeitsch. anorg. Chem., 51, 171.

the usual methods, as refined at Harvard University, and give the two silver ratios. The data, with vacuum weights, are as follows:

$CoCl_2$.	*Ag.*	*AgCl.*	*Ag ratio.*	*AgCl ratio.*
1.09959	1.82671	2.42676	60.195	45.311
1.47733	2.45398	3.26095	60.201	45.304
3.84133	6.38081	8.47735	60.201	45.313
3.64342	6.05232	......	60.199	.....
2.96315	4.92244	6.54019	60.197	45.307
3.48418	5.78815	7.69084	60.195	45.303
3.29523	5.47410	7.27284	60.197	45.309
1.57655	2.61905	3.48012	60.195	45.302
		Mean,	60.1975, ± .0006	45.3070, ± .0001

From Ag ratio, Co = 58.965.
From AgCl ratio, Co = 58.968.
And Ag : Cl : : 100 : 32.866.

For the silver ratio, $2Ag : CoCl_2 :: 100 : x$, there are two earlier sets of determinations. The three series combine as follows:

Marignac	60.118, ± .0192
Dumas	60.228, ± .0110
Baxter and Coffin............	60.1975, ± .0006
General mean	60.1975, ± .0006

In this combination the older series vanish. Their influence is apparent only in the fifth decimal place.

For the atomic weight of nickel we now have the following ratios:

(1). Per cent. of Ni in $NiC_2O_4.3H_2O$, 29.084, ± .006
(2). Per cent. of CO_2 from $NiC_2O_4.3H_2O$, 44.098, ± .027
(3). Per cent. of Ni in $NiC_2O_4.2H_2O$, 31.408, ± .0026
(4). Per cent. of CO_2 from $NiC_2O_4.2H_2O$, 47.605, ± .053
(5). Per cent. of Ni in brucia nickelocyanide, 5.7295, ± .0034
(6). Per cent. of Ni in strychnia nickelocyanide, 6.595, ± .005
(7). Per cent. of NiO in $NiSO_4$, 48.269, ± .018
(8). Per cent. of Ni in NiO, 78.570, ± .0006
(9). $2Ag:NiCl_2::100:60.194$, ± .0061
(10). $2AgCl:Ni::100:20.590$, ± .0049
(11). $2Ag:NiBr_2::100:101.270$, ± .0015
(12). $2AgBr:NiBr_2::100:58.1738$, ± .0007
(13). $NiBr_2:Ni::100:26.855$, ± .0005
(14). $2Ag:Ni::100:27.359$, ± .0059
(15). $2Au:3Ni::100:45.209$, ± .019
(16). $2BaSO_4:K_2Ni(SO_4)_2.6H_2O::100:93.6505$, ± .001
(17). $Ni:H_2::100:3.4211$, ± .001
(18). $I_2:Ni::100:23.255$, ± .0091

The values used in reducing these ratios are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
I = 126.9204, ± .00033
S = 32.0667, ± .00075

C = 12.0038, ± .0002
N = 14.0101, ± .0001
K = 39.0999, ± .0002
Ba = 137.363, ± .0025
Au = 197.269, ± .0030

H = 1.00779, ± .00001

In making the computations, the oxalate ratios of Schneider are combined, in order to avoid the uncertain hydration of the compounds. That is, in each set of ratios, instead of calculating from the percentage of nickel or cobalt found, the cross ratio is taken, $2CO_2 : Ni$ or Co, as the case may be. So much assumed we obtain the following values for Ni:

From ratio 5	Ni = 58.027, ± .0345
" " 1 and 2	58.044, ± .0319
" " 3 and 4	58.064, ± .0648
" " 6	58.085, ± .0441
" " 11	58.661, ± .0033
" " 12	58.661, ± .0028
" " 8	58.662, ± .0017
" " 13	58.685, ± .0009
" " 7	58.708, ± .0381
" " 16	58.789, ± .0068
" " 17	58.917, ± .0172
" " 9	58.958, ± .0131
" " 10	59.027, ± .0141
" " 14	59.030, ± .0127
" " 18	59.031, ± .0231
" " 15	59.456, ± .0250
General mean,	Ni = 58.682, ± .00074

This mean lies within the limits of variation of Richards and Cushman's determinations, and must be regarded as satisfactory. Their work and Zimmermann's dominates the entire combination.

For cobalt we have twenty ratios, as follows:

(1). Per cent. of Co in $CoC_2O_4.2H_2O$, 32.5555, ± .0149
(2). Per cent. of CO_2 from $CoC_2O_4.2H_2O$, 47.7475, ± .0213
(3). Per cent. of Co in CoO, 78.6324, ± .0002
(4). Per cent. of Co in purpureocobalt chloride, 23.6045, ± .0018
(5). Per cent. of Co in phenylammonium cobalticyanide, 11.8665, ± .0124
(6). Per cent. of Co in ammonium cobalticyanide, 21.943, ± .029
(7). Per cent. of Co in brucia cobalticyanide, 3.7437, ± .0036
(8). Per cent. of Co in strychnia cobalticyanide, 4.5705, ± .005
(9). Per cent. of CoO in $CoSO_4$, 48.287, ± .0135
(10). $2Ag:CoCl_2::100:60.1975$, ± .0006

(11). $2AgCl:CoCl_2::100:45.307$, ± .0011
(12). $2Ag:Co::100:27.705$, ± .0050
(13). $2AgCl:Co::100:20.687$, ± .0033
(14). $Cl_2:Co::100:83.220$, ± .0082
(15). $2Ag:CoBr_2::100:101.407$, ± .0018
(16). $2AgBr:CoBr_2::100:58.2549$, ± .0007
(17). $CoBr_2:Co::100:26.9508$, ± .0008
(18). $2Au:3Co::100:45.151$, ± .025
(19). $Co:H_2::100:3.4110$, ± .0009
(20). $I_2:Co::100:23.462$, ± .0027

Hence, for the atomic weight of cobalt,

From	ratio	9	Co = 58.706, ± .0286
“	“	3	58.880, ± .0006
“	“	15	58.957, ± .0040
“	“	10	58.965, ± .0014
“	“	16	58.966, ± .0027
“	“	11	58.968, ± .0032
“	“	17	58.971, ± .0019
“	“	14	59.017, ± .0059
“	“	5	59.042, ± .0630
“	“	19	59.091, ± .0156
“	“	6	59.093, ± .0810
“	“	8	59.100, ± .0647
“	“	4	59.183, ± .0048
“	“	7	59.203, ± .0570
“	“	13	59.305, ± .0095
“	“	18	59.378, ± .0330
“	“	20	59.556, ± .0069
“	“	12	59.776, ± .0108
“	“	1 and 2	60.006, ± .0384

General mean, Co = 58.915, ± .0005

It is evident that in this combination, ratio 3, representing principally the work of Zimmermann, receives excessive weight. For that reason, and also on chemical grounds, the final mean is probably too low. If, however, we arbitrarily assign to ratio 3 the "probable error" and weight of the next best ratio, No. 10, the general mean then becomes

Co = 58.961, ± .0008

This is probably not far from the truth; but the change thus effected serves to illustrate the fact that the rigorous mathematical combination is not always conclusive. Although the mathematical method is most useful, it cannot do away with the exercise of judgment as based upon other knowledge than that shown in the mere figures.

That the atomic weight of cobalt is higher than that of nickel clearly appears from the evidence. Nevertheless, attempts have been made, and that recently, to prove the opposite. For example, Parker and Sexton[1] assert that in fifteen electrolytic comparisons of silver and cobalt, they have obtained a mean value of Co = 57.7, which is lower than the atomic weight of nickel. Barkla and Sadler,[2] in studying the permeability of metals to the secondary Röntgen rays, have found that property to be a periodic function of the atomic weights. By interpolation in the periodic curve so obtained they find values for Ni ranging between 61.2 and 61.6, whereas the currently accepted atomic weight appears to be anomalous, at least as regards the physical property now under consideration. These conclusions, however, cannot weigh very heavily as against the clear chemical evidence. As for Parker and Sexton's work, the authors give no details which would furnish an adequate basis for discussion.

RUTHENIUM.

The atomic weight of this metal has been determined by Claus and by Joly. Although Claus[3] employed several methods, we need only consider his analyses of potassium rutheniochloride, K_2RuCl_5. The salt was dried by heating to 200° in chlorine gas, but even then retained a trace of water. The percentage results of the analyses are as follows:

	Ru.	*2KCl.*	*3Cl.*
	28.96	40.80	30.24
	28.48	41.39	30.22
	28.91	41.08	30.04
Mean,	28.78	41.09	30.17

Reckoning directly from the percentages, we get the following discordant values for Ru:

From percentage of metal........ Ru = 103.24
From percentage of 2KCl......... " = 107.41
From percentage of 3Cl.......... " = 97.09

These results are obviously of little importance, especially since the best of them is not in accord with the position of ruthenium in the periodic system. The work of Joly is more satisfactory.[4] Several com-

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[1] Nature, 76, 316. 1907.

[2] Phil. Mag. (6), 14, 408. 1907.

[3] Journ. prakt. Chem., 34, 435. 1845.

[4] Compt. Rend., 108, 946.

pounds of ruthenium were analyzed by reduction in a stream of hydrogen with the following results:

First, reduction of RuO_2:

RuO_2.	Ru.	Per cent. Ru.
2.1387	1.6267	76.060
2.5846	1.9658	76.058
2.3682	1.8016	76.075
2.8849	2.1939	76.046
		Mean, 76.060, ± .0040

Second, reduction of the salt $RuCl_3.NO.H_2O$:

Per cent. Ru.
39.78
39.66
Mean, 39.72, ± .0405

Third, reduction of $RuCl_3.NO.2NH_4Cl$:

Per cent. Ru.
29.44
29.47
Mean, 29.455, ± .0101

To reduce these ratios we have—

Cl = 35.4584, ± .0002
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From $RuCl_3.NO.2NH_4Cl$ ....Ru = 101.622, ± .0378
" RuO_2 ....101.668, ± .0178
" $RuCl_3.NO.H_2O$ ....101.739, ± .1242

General mean, Ru = 101.661, ± .0160

More data are needed in order to thoroughly establish the atomic weight of ruthenium.

RHODIUM.

Berzelius[1] determined the atomic weight of this metal by the analysis of sodium and potassium rhodiochlorides, Na_3RhCl_6 and K_2RhCl_5. The latter salt was dried by heating in chlorine. The compounds were analyzed by reduction in hydrogen, after the usual manner. Reduced to percentages, the analyses are as follows:

In Na_3RhCl_6.

Rh.	*3NaCl.*	*3Cl.*
26.959	45.853	27.189
27.229	45.301	27.470
.....	.....	27.616
Mean, 27.094	Mean, 45.577	Mean, 27.425

In K_2RhCl_5.

Rh.	*2KCl.*	*3Cl.*
28.989	41.450	29.561

From analyses of the sodium salt we get the following values for Rh:

From per cent. of metal.................. Rh = 104.72
From per cent. of NaCl.................. " = 103.08
From per cent. of 3Cl.................... " = 106.10
From ratio between 3Cl and Rh.......... " = 104.85
From ratio between 3NaCl and Rh...... " = 104.27

These are discordant figures; but the last one fits in fairly well with the values calculated from the potassium compound, which are as follows:

From per cent. of metal.................. Rh = 104.30
From per cent. of KCl.................... " = 104.26
From per cent. of Cl..................... " = 104.36
From Rh:5Cl ratio....................... " = 104.32
From Rh:2KCl ratio..................... " = 104.29

Mean .... Rh = 104.37

The determinations by Jörgensen[2] seem to have been preliminary, but are good so far as they go. Rhodium pentamine chloride, $Rh(NH_3)_5Cl_3$, was ignited in hydrogen, and the residual metal was cooled in an atmosphere of carbon dioxide. The data are as follows:

Chloride.	*Rhodium.*	*Per cent. Rh.*
3.5180	1.2310	34.991
2.1507	.7517	34.951
.9091	.3182	35.002
1.9889	.6960	34.994
		Mean, 34.984, ± .0076

Hence Rh = 103.06.

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[1] Poggend. Annalen, 13, 435. 1828.
[2] Journ. prakt. Chem. (2), 27, 486. 1883.

In a single analysis of the corresponding bromide, 1.2736 grammes gave 0.3065 of rhodium, or 24.065 per cent. Hence Rh = 102.97. In another experiment, 1.2675 grammes of bromide gave 1.6683 of AgBr. Hence Rh = 103.12.

Seubert and Kobbe[1] determined the atomic weight in the same way, that is, by igniting rhodium pentamine chloride in hydrogen and weighing the residual metal. Their results are given below:

$Rh(NH_3)_5Cl_3$.	*Rh.*	*Per cent. Rh.*
1.8585	.6496	34.953
1.5560	.5435	34.929
1.5202	.5310	34.930
2.0111	.7031	34.961
1.8674	.6528	34.958
2.4347	.8513	34.965
2.3849	.8338	34.962
2.5393	.8881	34.974
1.4080	.4920	34.943
1.4654	.5123	34.960
		Mean, 34.954. ± .0032

Hence Rh = 102.94.

In the sixth experiment the ammonium chloride formed was collected in a bulb tube, and estimated by weighing as silver chloride. 3.5531 grms. of AgCl were obtained. Hence Rh = 103.12.

The same process was followed by Hüttlinger,[2] who obtained almost exactly the same result. His figures are as follows:

Chloride.	*Rh.*	*Per cent. Rh.*
1.60574	.56124	34.951
1.67310	.58492	34.960
1.30182	.45507	34.956
		Mean, 34.956, ± .0020

Hence Rh = 102.93.

Another series, somewhat later, by H. Dittmar,[3] gave the subjoined figures, with vacuum weights:

Chloride.	*Rh.*	*Per cent. Rh.*
2.01526	.70465	34.967
1.83589	.64173	34.954
1.57210	.54934	34.943
2.17528	.76046	34.959
2.03911	.71271	34.952
2.20000	.76890	34.950
1.02840	.35941	34.948
		Mean, 34.953, ± .0020

Hence Rh = 102.93.

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[1] Liebig's Annalen, 260, 318. 1890.

[2] Sitzungsb. phys. med. Soz. Erlangen, 39, 1. 1907.

[3] Sitzungsb. phys. med. Soz. Erlangen, 40, 184. 1909.

The four series of analyses of the chloride combine as follows:

Jörgensen	34.984, ± .0076
Seubert and Kobbe	34.954, ± .0032
Hüttlinger	34.956, ± .0020
Dittmar	34.953, ± .0020
General mean	34.955, ± .0013

The work of Hüttlinger and Dittmar was done in the laboratory at Erlangen, under the direction of Gutbier. So, too, was that of Renz,[1] who made similar analyses of rhodium pentamine bromide, $Rh(NH_3)_5Br_3$. His data, with vacuum weights, are as follows:

Bromide.	*Rh.*	*Per cent. Rh.*
.87624	.21057	24.031
1.56500	.37638	24.049
2.04033	.49069	24.049
2.00120	.48135	24.053
1.89278	.45525	24.051
2.30210	.55416	24.071
1.02065	.24555	24.058
1.31485	.31622	24.049
1.86060	.44766	24.059
1.51040	.36339	24.059
		Mean, 24.053, ± .0022

Hence Rh = 102.91.

Ignoring the early work of Berzelius, and the single analysis by Jörgensen of rhodium pentamine bromide, we have two ratios from which to compute the atomic weight of rhodium:

(1). $Rh(NH_3)_5Cl_3 : Rh :: 100 : 34.955, \pm .0013$
(2). $Rh(NH_3)_5Br_3 : Rh :: 100 : 24.053, \pm .0022$

To reduce these we have—

Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 2	Rh = 102.906, ± .0099
" " 1	102.934, ± .0044
General mean,	Rh = 102.929, ± .0040

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[1] Inaug. Diss., Erlangen, 1909.

PALLADIUM.

The first work upon the atomic weight of palladium seems to have been done by Berzelius. In an early paper[1] he states that 100 parts of the metal united with 28.15 of sulphur. Hence Pd = 113.91, a result which is clearly of no present value.

In a later paper[2] Berzelius published two analyses of potassium palladiochloride, K_2PdCl_4. The salt was decomposed by ignition in hydrogen, as was the case with the double chlorides of potassium with platinum, osmium and iridium. Reducing his results to percentages, we get the following composition for the substance in question:

Pd.	*2KCl.*	*Cl_2.*
32.726	46.044	21.229
32.655	45.741	21.604
Mean, 32.690	Mean, 45.892	Mean, 21.416

From these percentages, calculating directly, very discordant results are obtained:

From percentage of metal................ Pd = 106.86
From percentage of KCl.................. " = 104.90
From percentage of Cl_2 (loss)............ " = 111.11

Obviously, the only way to get satisfactory figures is to calculate from the ratio between the Pd and 2KCl, eliminating thus the influence of water in the salt. The two experiments give, as proportional to 100 parts of KCl, the following of Pd:

71.075
71.391

Mean, 71.233, ± .1066

Hence Pd = 106.22.

In 1847 Quintus Icilius[3] published a determination, which need be given only for the sake of completeness. He ignited potassium palladiochloride in hydrogen, and found the following amounts of residue. His weights are here recalculated into percentages:

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[1] Poggend. Annalen, 8, 177. 1826.

[2] Poggend. Annalen, 13, 454. 1828.

[3] "Die Atomgewichte vom Pd, K, Cl, Ag, C, und H, nach der Methode der kleinsten Quadrate berechnet." Inaug. Diss. Göttingen, 1847. Contains no other original analyses.

64.708
64.965
64.781

Mean, 64.818

From this mean, Pd = 112.05. This result has no present value.

In 1889 Keiser's first determinations of this constant appeared.[1] Finding the potassium palladiochloride to contain "water of decrepitation," he abandoned its use and resorted to palladiammonium chloride, $Pd(NH_3Cl)_2$, as the most available compound for his purpose. This salt, heated in hydrogen, yields spongy palladium, which was allowed to cool in a current of dry air, in order to avoid gaseous occlusions. The salt itself was dried, previous to analysis, first over sulphuric acid, and then in an air bath at a temperature from 120° to 130°. Two series of experiments were made, the second series starting out from palladium produced by the first series. The data are as follows:

First Series.

$Pd(NH_3Cl)_2$.	*Pd.*	*Per cent. Pd.*
.83260	.41965	50.402
1.72635	.86992	50.391
1.40280	.70670	50.378
1.57940	.79562	50.375
1.89895	.95650	50.370
1.48065	.74570	50.363
1.56015	.78585	50.370
1.82658	.92003	50.369
2.40125	1.20970	50.378
1.10400	.55629	50.389
.93310	.47010	50.380

Mean, 50.379, ± .0008

Reduced to vacuum this becomes 50.360.

Hence Pd = 106.51.

Second Series.

$Pd(NH_3Cl)_2$.	*Pd.*	*Per cent. Pd.*
2.61841	1.31900	50.374
2.23420	1.12561	50.381
1.73553	.87445	50.385
1.69160	.85210	50.372
1.72403	.86825	50.362
1.12222	.56535	50.378
1.17457	.59200	50.401
2.42760	1.22280	50.371

Mean, 50.378, ± .0028

Reduced to vacuum, 50.359

Hence Pd = 106.50.

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[1] Am. Chem. Journ., 11, 398. 1889.

The reductions to vacuum are neglected by Keiser himself, but are here added in order to secure uniformity with later results by the same author.

Bailey and Lamb[1] made experiments upon several compounds of palladium, but finally settled upon palladiammonium chloride, like Keiser. Two preliminary experiments, however, with potassium palladiochloride are given, in which the salt was reduced in hydrogen, and both Pd and KCl were weighed. The data are as follows, with the ratio (calculated as with Berzelius' experiments) given in a third column:

$2KCl$.	*Pd.*	*Ratio.*
1.49767	1.05627	70.528
.90484	.63738	70.441

Mean, 70.485, ± .0290

Hence Pd = 105.11.

The palladiammonium chloride was studied by two methods. First, weighed quantities of the salt were reduced in hydrogen, the ammonium chloride so formed was collected in an absorption apparatus, and then precipitated with silver nitrate. The weights found were as follows, with the $Pd(NH_3Cl_2)$ proportional to 100 parts of silver chloride given in the third column:

$Pd(NH\ Cl)_2$.	*AgCl.*	*Ratio.*
1.24276	1.682249	73.879
1.08722	1.468448	74.040
1.47666	2.000164	73.828
1.34887	1.837957	73.390
1.74569	2.362320	73.898

Mean, 73.807, ± .0742

Hence Pd = 106.60. Bailey and Lamb regard this as too high, and suspect loss of NH_4Cl during the operation.

The second series of data resemble Keiser's. The salt was reduced in hydrogen, and the spongy palladium was weighed in a Sprengel vacuum. The data are as follows:

	$Pd(NH_3Cl)_2$.	*Pd.*	*Per cent. Pd.*
A	1.890597	.947995	50.143
	1.874175	.940271	50.170
B	1.307076	.654687	50.088
	1.340045	.633207	50.238
	1.905536	.955950	50.167
	1.685582	.846472	50.218
C	1.691028	.849120	50.213
	2.112530	1.059690	50.162
	2.110653	1.057910	50.122
	1.969100	.988155	50.184

Mean, 50.171, ± .0099

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[1] Journ. Chem. Soc., 61, 745. 1892.

Hence Pd = 105.71. Bailey and Lamb's weighings are all reduced to a vacuum.

Keller and Smith,[1] reviewing Keiser's work, find that palladiammonium chloride, prepared as Keiser prepared it, may retain traces of foreign metals, and especially of copper. Accordingly, they prepared a quantity of the salt, after a thorough and elaborate process of purification, dried it with extreme care, and then determined the palladium by electrolysis in silver-coated platinum dishes. The precipitated palladium was dried under varying conditions, concerning which the original memoir must be consulted, and was proved to be free from occluded hydrogen. By this method two sets of experiments were made to determine the atomic weight of palladium; but for present purposes the two may fairly be treated as one. The data obtained are as follows, but the weights do not appear to have been reduced to a vacuum:

	$Pd(NH_3Cl)_2$.	*Pd.*	*Per cent. Pd.*
A	1.29960	.65630	50.504
	1.05430	.53253	50.510
	1.92945	.97455	50.509
B	1.94722	.98343	50.504
	1.08649	.54870	50.502
	1.28423	.64858	50.503
	1.68275	.85010	50.519
	1.69113	.85431	50.517
	1.80805	.91310	50.502

Mean, 50.508, ± .0014

Hence Pd = 107.14, a result notably higher than Keiser's.

Keller and Smith account for the difference between their determinations and Keiser's partly by the assumption that the materials used by the latter were not pure, and partly by considerations based on the process. In order to clarify the latter part of the question they made three sets of experiments by Keiser's method, slightly varying the conditions. First, the chloride was not pulverized before ignition, and slight decrepitation took place, while dark stains of palladium appeared in the reduction tube, indicating loss by volatilization. Secondly, the chloride was prepared from crude palladium exactly as described by Keiser, but was pulverized before reduction. No decrepitation ensued, but traces of palladium were volatilized. The third series, also on finely pulverized material, was like the second; but the palladiammonium chloride was purified by Keller and Smith's process. The three series, here treated as one, are as follows:

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[1] Amer. Chem. Journ., 14, 423. 1892.

	$Pd(NH_3Cl)_2$.	*Pd.*	*Per cent. Pd.*
First series..	.62955	.31743	50.422
	.77270	.38942	50.397
	.83252	.41918	50.350
	.99055	.49895	50.371
Second series.	1.02175	.51468	50.372
	1.10325	.55590	50.388
	.66690	.33590	50.367
	.86840	.43733	50.360
	1.41430	.71255	50.382
	1.15234	.58050	50.376
Third series..	.96229	.48502	50.403
	.97804	.49294	50.401
	.94253	.47517	50.414
	.86090	.43405	50.430

Mean, 50.388, ± .0043

Hence Pd = 106.63.

The three series seem to be fairly in agreement between themselves, and with Keiser's work, but diverge seriously from the electrolytic data.

Keller and Smith also attempted to determine the atomic weight of palladium by heating the palladiammonium chloride in sulphuretted hydrogen, and so converting it into the sulphide, PdS. These data were obtained:

$Pd(NH_3Cl)_2$.	*PdS.*	*Per cent. PdS.*
.71699	.47066	65.644
1.31688	.86445	65.659

Mean, 65.651, ± .0051

Hence Pd = 107.30. This result, however, is affected by the work of Petrenko-Kritschenko,[1] who has shown the existence of the sulphide PdS to be uncertain.

Joly and Leidié,[2] in their determinations of this atomic weight, returned to the potassium palladiochloride, K_2PdCl_4. In their first series of experiments the salt was dried in vacuo at ordinary temperatures. It was then electrolyzed in a solution acidulated with hydrochloric acid, both the deposited palladium and the potassium chloride being weighed. The palladium was dried, ignited in a stream of hydrogen and cooled in an atmosphere of carbon dioxide. The results were as follows, with the column added by me giving the Pd equivalent to 100 parts of KCl:

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[1] Zeit. anorg. Chem., 4, 251. 1893.

[2] Compt. Rend., 116, 147. 1893.

K_2PdCl_4.	Pd.	2KCl.	Ratio.
1.0255	.3919	.5520	70.996
1.2178	.3937	.5551	70.924
1.2518	.4048	.5687	71.016
			Mean, 70.979, ± .0188

Hence Pd = 105.84.

This series was rejected by the authors, because the salt was found to contain water—in one case 0.23 per cent. This error, however, should not invalidate the Pd:KCl ratio. In a second series the palladiochloride was dried in vacuo at 100°, giving the following data:

K_2PdCl_4.	Pd.	2KCl.	Ratio.
1.3635	.4422	.6186	71.484
3.0628	.9944	1.3929	71.391
1.4845	.4816	.6782	71.011
1.7995	.5838	.8206	71.143
			Mean, 71.257, ± .0736

These experiments seem to be less concordant than the preceding set. It must be noted, however, that the authors reject the KCl determinations and compute directly from the ratio between the salt and the metal. But the ratio here chosen agrees best with the determinations made by other observers, giving for this series the mean value Pd = 106.26, and is, moreover, uniform with the data given by Berzelius and by Bailey and Lamb.

Joly and Leidié also give two experiments made by reducing the K_2PdCl_4 in hydrogen, with the subjoined results:

K_2PdCl_4.	Pd.	2KCl.	Ratio.
2.4481	.7949	1.1168	71.177
1.8250	.5930	.8360	70.933
			Mean, 71.055, ± .0823

Hence Pd = 105.96.

Combining these data with previous series, we have—

Berzelius	71.233, ± .1066
Bailey and Lamb	70.485, ± .0290
Joly and Leidié, first	70.979, ± .0188
Joly and Leidié, second	71.257, ± .0736
Joly and Leidié, third	71.055, ± .0823
General mean	70.865, ± .0150

In view of the discordance among the determinations hitherto cited and because of the criticisms made by Keller and Smith, Keiser, jointly with Miss Mary B. Breed,[1] repeated his former work, with some variations, and added precautions to ensure accuracy. His general method was the same as before, namely, the reduction of palladiammonium chloride by a stream of hydrogen. First, palladium was purified by distillation as $PdCl_2$ at low red heat in a current of chlorine. From this chloride the palladiammonium salt was then prepared. Upon heating the compound gently in a stream of hydrogen, decomposition ensued absolutely without decrepitation or loss of palladium by volatilization. Neither source of error existed. The results obtained were these:

$Pd(NH_3Cl)_2$.	Pd.	Per cent. Pd.
1.60842	.80997	50.358
2.08295	1.04920	50.371
2.02440	1.01975	50.373
2.54810	1.28360	50.375
1.75505	.88410	50.375

Mean, 50.370, ± .0023
Reduced to vacuum, 50.351

Hence Pd=106.46.

In a second series of experiments, palladium was purified as in the earlier investigation, but with special care to eliminate rhodium, iron, copper, gold, mercury, etc. The palladiammonium salt prepared from this material gave as follows:

$Pd(NH_3Cl)_2$.	Pd.	Per cent. Pd.
1.50275	.75685	50.364
1.23672	.62286	50.365
1.34470	.67739	50.375
1.49059	.75095	50.379

Mean, 50.371, ± .0026
Reduced to vacuum, 50.352

Hence Pd=106.47.

Here, again, no loss from decrepitation or volatilization occurred, although evidence of such loss was carefully sought for.

Hardin,[2] in 1899, made three series of determinations of the atomic weight of palladium, by reduction of three palladium salts in hydrogen. His results, with weights reduced to a vacuum, are as follows:

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[1] Am. Chem. Journ., 16, 20. 1894.

[2] Journ. Amer. Chem. Soc., 21, 947.

First: Reduction of diphenyl-pallad-diammonium chloride, $Pd(C_6H_5.NH_2Cl)_2$:

Salt.	*Pd.*	*Per cent. Pd.*
.98480	.28953	29.490
1.10000	.32310	29.376
1.02820	.30210	29.381
1.19230	.35040	29.389
1.40550	.41300	29.385
1.26000	.37040	29.397
1.25510	.66310	29.404
		Mean, 29.390, ± .0029

Hence Pd = 107.01.

Second: Reduction of diphenyl-pallad-diammonium bromide, $Pd(C_6H_5.NH_2Br)_2$:

Salt.	*Pd.*	*Per cent. Pd.*
.88567	.20917	23.617
1.31280	.31000	23.614
1.50465	.35540	23.620
2.01635	.47635	23.624
2.92300	.69080	23.633
		Mean, 23.622, ± .0023

Hence Pd = 107.01.

Third: Reduction of palladium ammonium bromide, $(NH_4)_2PdBr_4$:

Salt.	*Pd.*	*Per cent. Pd.*
.77886	.18006	23.118
1.53109	.35381	23.108
2.75168	.63614	23.118
1.88136	.43478	23.110
		Mean, 23.1135, ± .0018

Hence Pd = 106.95.

These determinations are notably higher than those made by other methods. After reduction, the palladium was heated to redness for two hours in a stream of dry air, to remove possible carbon. It was then heated again in hydrogen, and finally cooled in a current of air. Hydrogen could hardly have been occluded in the final product.

Amberg,[1] whose determinations appeared in 1905, resorted to palladiammonium chloride as his initial substance. Three series of analyses were made, with scrupulously purified material, and all weights were reduced to a vacuum. First, the salt was reduced electrolytically. The

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[1] Liebig's Annalen, 341, 255.

precipitated palladium was weighed, and in the rinsings from it the chlorine was determined as silver chloride. The data are subjoined, with the ratio $2AgCl : Pd(NH_3Cl)_2 :: 100 : x$ in the fifth column:

Salt.	*Pd.*	*AgCl.*	*Per cent. Pd.*	*AgCl ratio.*
1.06045	.53609	......	50.553	.....
1.00028	.50528	1.35867	50.493	73.622
1.66386	.84085	2.25437	50.541	73.807
.83195	.42092	1.12282	50.594	74.095
1.91591	.96886	2.59799	50.569	73.746
			Mean, 50.550,	Mean, 73.818,
			± .0110	± .0677

From the percentage of metal, Pd = 107.32.
From the AgCl ratio, Pd = 106.63.

Amberg's second series of analyses resembles the first, except that the palladium was precipitated by hydrazin sulphate. The percentage of metal is given by Amberg, but not the weights actually obtained:

Salt.	*AgCl.*	*Per cent. Pd.*	*AgCl ratio.*
1.32493	1.78656	50.12	74.171
1.02642	1.39247	50.29	73.712
1.30335	1.76875	50.36	73.518
1.59709	2.16641	....	73.721
1.88622	2.55028	50.49	73.961
2.59665	3.51783	50.68	73.812
		Mean, 50.388,	Mean, 73.814,
		± .2064	± .0614

From percentage of metal, Pd = 106.63.
From AgCl ratio, Pd = 106.62.

The silver chloride ratio combines with previous determinations as follows:

Bailey and Lamb.............	73.807, ± .0742
Amberg, first series............	73.818, ± .0677
Amberg, second series.........	73.814, ± .0614
General mean	73.813, ± .0388

In his third series of analyses Amberg determined only the palladium, which was precipitated electrolytically from a sulphuric acid solution of the palladiammonium chloride with a rapidly rotating anode. This series is excellent, but the preceding series of palladium determinations are negligible:

Salt.	*Pd.*	*Per cent. Pd.*
.62446	.31470	50.396
.83878	.42280	50.407
1.50282	.75725	50.389
1.06704	.53763	50.385
1.98342	.99971	50.403
1.53093	.77153	50.396
1.18995	.59971	50.398
.62635	.31572	50.406
1.76110	.88739	50.388
3.79639	1.91298	50.389
3.97553	2.00333	50.392
4.62100	2.32834	50.386
		Mean, 50.395, ± .0015

Hence Pd = 106.66.

The atomic weight determinations by Krell, Woernle and Haas were all made in the laboratory of Professor Gutbier at Erlangen. Krell[1] reduced palladiammonium (palladosamine) chloride in hydrogen, and afterwards heated the reduced metal in a stream of carbon dioxide. His figures, with vacuum weights, are subjoined:

Salt.	*Pd.*	*Per cent. Pd.*
1.83034	.92197	50.372
1.73474	.87433	50.401
1.92532	.96524	50.396
2.63544	1.32868	50.416
3.23840	1.63175	50.387
		Mean, 50.3945, ± .0050

Hence Pd = 106.65. The first, aberrant determination in the series, is rejected by Krell.

Woernle[2] also made analyses of palladiammonium chloride. The first two reductions were effected in hydrogen, the other determinations were electrolytic. His figures, with vacuum weights, are as follows:

Salt.	*Pd.*	*Per cent. Pd.*
2.94682	1.48493	50.391
1.83140	.92296	50.396
1.02683	.51479	50.397
1.22435	.61708	50.401
1.46735	.73944	50.393
.59796	.30139	50.403
2.64584	1.33329	50.392
		Mean, 50.396, ± .0012

Hence Pd = 106.66.

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[1] Inaugural Dissertation, Erlangen, 1906.

[2] Sitzungsb. phys. med. Soz., Erlangen, 38, 278. 1907.

Haas[1] analyzed palladiammonium bromide, $Pd(NH_3Br)_2$, by reduction in hydrogen. The reduced metal was subsequently heated in carbon dioxide. His data, with vacuum weights, are as follows:

Bromide.	*Pd.*	*Per cent. Pd.*
2.06470	.73274	35.488
1.73455	.61563	35.492
2.64773	.93978	35.493
1.29106	.45821	35.491
2.26758	.80490	35.495
1.90770	.67704	35.489
1.77729	.63082	35.493

Mean, 35.492, ± .0006

Hence Pd = 106.69.

Kemmerer[2] analyzed two palladiammonium compounds, the chloride and the cyanide, both by reduction in hydrogen, with subsequent cooling of the reduced metal in an atmosphere of nitrogen. Vacuum weights are given throughout. With the chloride, two sets of determinations were made, on two distinct preparations, but both series are here treated as one. The data are subjoined:

	Chloride.	*Pd.*	*Per cent. Pd.*
A	.89187	.44885	50.327
	.77931	.39218	50.324
	.66980	.33711	50.330
	1.08373	.54541	50.327
	.96048	.48338	50.327
B	.95615	.48129	50.336
	.94087	.47356	50.332
	.90106	.45353	50.333
	1.16994	.58908	50.351

Mean, 50.332, ± .0018

Hence Pd = 106.39.

With the cyanide, $Pd(NH_3CN)_2$, Kemmerer obtained the following results:

Cyanide.	*Pd.*	*Per cent. Pd.*
.85860	.47463	55.280
1.19378	.66002	55.288
1.41818	.78408	55.288
1.05254	.58206	55.301
1.39510	.77153	55.303
1.66196	.91881	55.285

Mean, 55.291, ± .0025

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[1] Inaug. Dissertation, Erlangen, 1908.

[2] Thesis, University of Pennsylvania, 1908.

Hence Pd = 106.47. Why these analyses should give low values is unexplained.

The various series of figures for the percentage of palladium in palladiammonium chloride now combine thus:

Keiser, first series	50.360,	± .0008
Keiser, second series	50.359,	± .0028
Bailey and Lamb	50.171,	± .0099
Keller and Smith, electrolytic	50.508,	± .0014
Keller and Smith, hydrogen series	50.388,	± .0043
Keiser and Breed, first series	50.351,	± .0023
Keiser and Breed, second series	50.352,	± .0026
Amberg, first series	50.550,	± .0110
Amberg, second series	50.388,	± .2064
Amberg, third series	50.395,	± .0015
Krell	50.3945,	± .0050
Woernle	50.396,	± .0012
Kemmerer	50.332,	± .0025
General mean	50.3882,	± .0005

Like Haas, Gebhardt[1] also made analyses of palladiammonium bromide, and by the same method. His figures, with vacuum weights, are as follows:

Bromide.	*Pd.*	*Per cent. Pd.*
3.32462	1.17984	35.488
2.68383	.95245	35.488
1.40117	.49731	35.492
2.61673	.92877	35.494
2.64229	.93787	35.495
2.54424	.90293	35.489
2.00456	.71143	35.491
		Mean, 35.491, ± .0007

Hence Pd = 106.68.

The work of Haas and Gebhardt was done under the direction of Gutbier, who has combined their material in a memoir bearing their names in joint authorship with his.[2] In this memoir ten additional analyses of the bromide are given, but six of them are rejected by Gutbier as unsatisfactory. I prefer, however, to include them in this discussion, but with low weight. The ten determinations I have divided into two sets, one containing the four preferred analyses, the other the

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[1] Sitz. phys.-med. Soz., Erlangen, 40, 65. 1909.

[2] Gutbier, Haas and Gebhardt, Journ. prakt. Chem. (2), 79, 457. 1909.

six questionable ones. Their unequal value appears in the probable errors:

Bromide.	*Pd.*	*Per cent. Pd.*
3.09278	1.09783	35.496
1.98039	.70288	35.491
1.50032	.53253	35.494
2.84500	1.00992	35.498
		Mean, 35.495, ± .0011

Hence $Pd = 106.70$.

Bromide.	*Pd.*	*Per cent. Pd.*
.54402	.19286	35.450
.80237	.28468	35.479
.91601	.32509	35.489
.42942	.15288	35.461
.76884	.27271	35.470
.62795	.22270	35.464
		Mean, 35.469, ± .0038

Hence $Pd = 106.58$.

The four series of analyses of the bromide now combine thus:

Haas	35.492, ± .0006
Gebhardt	35.491, ± .0007
Gutbier, Haas and Gebhardt	35.495, ± .0011
Gutbier, Haas and Gebhardt	35.469, ± .0038
General mean	35.491, ± .00042

The influence of the determinations rejected by Gutbier is insignificant.

Nine ratios are now available from which to compute the atomic weight of palladium, as follows:

(1). $2KCl : Pd :: 100 : 70.865$, ± .0150
(2). $2AgCl : Pd(NH_3Cl)_2 :: 100 : 73.813$, ± .0388
(3). $Pd(NH_3Cl)_2 : Pd :: 100 : 50.3882$, ± .0005
(4). $Pd(NH_3Br)_2 : Pd :: 100 : 35.491$, ± .00042
(5). $Pd(NH_3CN)_2 : Pd :: 100 : 55.291$, ± .0025
(6). $Pd(C_6H_5.NH_2Cl)_2 : Pd :: 100 : 29.390$, ± .0029
(7). $Pd(C_6H_5 : NH_2Br)_2 : Pd :: 100 : 23.622$, ± .0023
(8). $(NH_4)_2PdBr_4 : Pd :: 100 : 23.1135$, ± .0018
(9). $Pd(NH_3Cl)_2 : PdS :: 100 : 65.651$, ± .0051

To reduce these ratios we have—

$Ag = 107.880$, ± .00029
$Cl = 35.4584$, ± .0002
$Br = 79.9197$, ± .0003
$K = 39.0999$, ± .0002
$C = 12.0038$, ± .0002
$N = 14.0101$, ± .0001
$S = 32.0667$, ± .00075
$H = 1.00779$, ± .00001

Hence,

From ratio 1 .... Pd = 105.672, ± .0224
" " 5 .... 106.472, ± .0077
" " 2 .... 106.613, ± .1112
" " 3 .... 106.627, ± .0016
" " 4 .... 106.682, ± .0015
" " 8 .... 106.948, ± .0087
" " 6 .... 107.009, ± .0115
" " 7 .... 107.014, ± .0109
" " 9 .... 107.299, ± .0491

General mean, Pd = 106.662, ± .0011

The final mean is a little lower than the values found in Gutbier's laboratory. The latter, however, could not be unqualifiedly accepted without rejecting other determinations which seem to be good. The international value, Pd = 106.7, is not far from the truth.

OSMIUM.

The atomic weight of this metal has been determined by Berzelius, by Fremy, and by Seubert.

Berzelius[1] analyzed potassium osmichloride, igniting it in hydrogen like the corresponding platinum salt. 1.3165 grammes lost .3805 of chlorine, and the residue consisted of .401 grm. of potassium chloride, with .535 grm. of osmium. Calculating only from the ratio between the Os and the KCl, the data give Os = 198.94.

Fremy's determination[2] is based upon the composition of osmium tetroxide. No details as to weighings or methods are given; barely the final result is stated, namely, Os = 199.65.

When the periodic law came into general acceptance, it became clearly evident that both of the foregoing values for osmium must be several units too high. A redetermination was therefore undertaken by Seubert,[3] who adopted methods based upon that of Berzelius. First, ammonium osmichloride was reduced by heating in a stream of hydrogen. The residual osmium was weighed, and the ammonium chloride and hydrochloric acid given off were collected in a suitable apparatus, so that the

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[1] Poggend. Annalen, 13, 530. 1828.
[2] Compt. Rend., 19, 468. Journ. prakt. Chem., 31, 410. 1844.
[3] Ber. Deutsch. chem. Ges., 21, 1839. 1888.

total chlorine could be estimated as silver chloride. The weights were as follows:

Am_2OsCl_6.	Os.	$6AgCl$.
1.8403	.7996	3.5897
2.0764	.9029	4.0460
2.1501	.9344	4.1950
2.1345	.9275	4.1614

Hence we have for the percentage of osmium and for the osmichloride proportional to 100 parts of AgCl—

Per cent. Os.	*AgCl : Salt.*
43.446	51.266
43.484	51.320
43.458	51.254
43.453	51.293
	Mean, 51.283, ± .0099

In a later paper[1] two more reductions are given, in which only osmium was estimated:

Salt.	*Os.*	*Per cent. Os.*
2.6687	1.1597	43.456
2.6937	1.1706	43.457

These determinations, included with the previous four as one series, give a mean percentage of Os in Am_2OsCl_6 of 43.459, ± .0036.

Secondly, potassium osmichloride was treated in the same way, but the residue weighed consisted of Os + 2KCl. From this the potassium chloride was dissolved out, recovered by evaporating the solution, and weighed separately. The volatile portion, 4HCl, was also measured by precipitation as silver chloride. In Seubert's first paper these data are given:

K_2OsCl_6.	Os.	$2KCl$.	$4AgCl$.
2.5148	....	.7796	2.9837
2.1138	.8405	.6547	2.5076

Hence, with salt proportional to 100 parts of AgCl in the last column, we have—

Per cent. Os.	*Per cent. KCl.*	*AgCl : Salt.*
.....	31.000	84.091
39.762	30.973	84.102
		Mean, 84.097, ± .0030

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[1] Liebig's Annalen, 261, 258.

In his second paper Seubert gives fuller data relative to the potassium osmichloride, but treats it somewhat differently. The salt was reduced by a stream of hydrogen as before, but after that the boat containing the Os+2KCl was transferred to a platinum tube, in which, by prolonged heating in the gas, the potassium chloride was completely volatilized. The determinations of 4Cl as 4AgCl were omitted. Two series of data are given, as follows:

K_2OsCl_6.	*Os.*	*Per cent. Os.*
1.1863	.4691	39.543
.9279	.3667	39.519
1.0946	.4330	39.558
1.6055	.6351	39.558
.4495	.1778	39.555
.8646	.3417	39.521
.7024	.2781	39.593
1.2742	.5041	39.562
1.0466	.4141	39.566

Mean, 39.553, ± .0052

K_2OsCl_6.	*2KCl.*	*Per cent. KCl.*
2.2032	.6820	30.955
2.0394	.6312	30.950
2.7596	.8544	30.961
2.4934	.7710	30.922
2.8606	.8843	30.913
2.8668	.5768	30.898
1.2227	.3778	30.899

Mean, 30.931

Earlier set, {31.000
30.973

Mean of all nine determinations, 30.941, ± .0079

The single percentage of osmium in the earlier memoir is obviously to be rejected.

The ratios to examine are now as follows:

(1). Per cent. Os in Am_2OsCl_6, 43.459, ± .0036
(2). $6AgCl : Am_2OsCl_6 :: 100 : 51.283$, ± .0099
(3). $4AgCl : K_2OsCl_6 :: 100 : 84.097$, ± .0030
(4). Per cent. Os in K_2OsCl_6, 39.553, ± .0052
(5). Per cent. KCl in K_2OsCl_6, 30.951, ± .0079

To reduce these ratios we have—

Ag = 107.880, ± .00029
N = 14.0101, ± .0001
Cl = 35.4584, ± .0002
K = 39.0999, ± .0002
H = 1.00779, ± .00001

Hence,

From ratio 4 ....Os = 190.374, ± .0299
" " 5 ....190.832, ± .0417
" " 3 ....191.229, ± .0173
" " 1 ....191.260, ± .0200
" " 2 ....192.216, ± .0852

General mean, Os = 191.067, ± .0114

A modern determination of the atomic weight of osmium seems to be desirable.

IRIDIUM.

The only early determination of the atomic weight of iridium was made by Berzelius,[1] who analyzed potassium iridichloride by the same method employed with the platinum and the osmium salts. The result found from a single analysis was not far from Ir = 196.7. This is now known to be too high. I have not, therefore, thought it worth while to recalculate Berzelius' figures, but give his estimation as it is stated in Roscoe and Schorlemmer's "Treatise on Chemistry."

In 1878 the matter was taken up by Seubert,[2] who had at his disposal 150 grammes of pure iridium. From this he prepared the iridichlorides of ammonium and potassium $(NH_4)_2IrCl_6$ and K_2IrCl_6, which salts were made the basis of his determinations. The potassium salt was dried by gentle heating in a stream of dry chlorine.

Upon ignition of the ammonium salt in hydrogen, metallic iridium was left behind in white coherent laminæ. The results obtained were as follows:

Am_2IrCl_6.	*Ir.*	*Per cent. Ir.*
1.3164	.5755	43.725
1.7122	.7490	43.745
1.2657	.5536	43.739
1.3676	.5980	43.726
2.6496	1.1586	43.739
2.8576	1.2489	43.705
2.9088	1.2724	43.742
		Mean, 43.732, ± .0035

Hence Ir = 193.395.

The potassium salt was also analyzed by decomposition in hydrogen with special precautions. In the residue the iridium and the potassium

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[1] Poggend. Annalen, 13, 135. 1828.

[2] Ber. Deutsch. chem. Gesell., 11, 1767. 1878.

chloride were separated after the usual method, and both were estimated. Eight analyses gave the following weights:

K_2IrCl_6.	*4Cl, loss.*	*Ir.*	*KCl.*
1.6316	.4779	.6507	.5030
2.2544	.6600	.8993	.6953
2.1290	.6238	.8488	.6560
1.8632	.5457	.7430	.5745
2.6898	.7878	1.0726	.8291
2.3719	.6952	.9459	.7308
2.6092	.7641	1.0406	.8040
2.5249	.7395	1.0070	.7775

Hence we have the following percentages, reckoned on the original salt:

Ir.	*2KCl.*	*4Cl.*
39.881	30.829	29.290
39.890	30.842	29.277
39.868	30.813	29.300
39.876	30.835	29.289
39.877	30.825	29.287
39.879	30.811	29.310
39.882	30.814	29.285
39.883	30.792	29.288
Mean, 39.880, ± .0015	Mean, 30.820, ± .0037	Mean, 29.291, ± .0024
Ir = 192.999	192.881	193.274

Joly[1] studied derivatives of iridium trichloride. The salts were dried at 120°, and reduced in hydrogen. With $IrCl_3.3KCl.3H_2O$ he found as follows:

Salt.	*Ir.*	*KCl.*
1.5950	.5881	.6803
1.6386	.6037	.7000
2.6276	.9689	1.1231

These data, if the weight of the salt itself is considered, give discordant results, but the ratio Ir : 3KCl : : 100 : x is satisfactory. The values of x are as follows:

115.677
115.952
115.915

Mean, 115.848, ± .0583

Hence Ir = 193.277.

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[1] Compt. Rend., 110, 1131. 1890.

The ammonium salt, $IrCl_3.3NH_4Cl$, gave the subjoined data:

Wt. of Salt.	*Wt. of Ir.*	*Per cent. Ir.*
1.5772	.6627	42.017
1.6056	.6742	41.990
		Mean, 42.003, ± .0094

Hence Ir = 193.078.

To sum up, the ratios available for iridium are these:

(1). Per cent. Ir in Am_2IrCl_6, 43.732, ± .0035
(2). Per cent. Ir in K_2IrCl_6, 39.880, ± .0015
(3). Per cent. KCl in K_2IrCl_6, 30.820, ± .0037
(4). Per cent. 4Cl in K_2IrCl_6, 29.291, ± .0024
(5). Per cent. Ir in Am_3IrCl_6, 42.003, ± .0094
(6). Ir:3KCl::100:115.848, ± .0583

To reduce these ratios we have—

Cl = 35.4584, ± .0002
K = 39.0999, ± .0002
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 3 ....Ir = 192.881, ± .0189
" " 2 ....192.999, ± .0084
" " 6 ....193.078, ± .0971
" " 4 ....193.274, ± .0416
" " 5 ....193.277, ± .0534
" " 1 ....193.395, ± .0196

General mean, Ir = 193.047, ± .0070

In a preliminary note Archibald[1] states that from analyses of the salt K_2IrCl_6 he has obtained the value Ir = 192.90. His investigation is still in progress, and no details have yet been published.

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[1] Chem. News, 100, 150. 1909. Paper read before the British Association for the Advancement of Science.

PLATINUM.

The earliest work upon the atomic weight of this metal was done by Berzelius,[1] who reduced platinous chloride and found it to contain 73.3 per cent. of platinum. Hence Pt = 194.69. In a later investigation,[2] he studied potassium chloroplatinate, K_2PtCl_6. 6.981 parts of this salt, ignited in hydrogen, lost 2.024 of chlorine. The residue consisted of 2.822 platinum and 2.135 potassium chloride. From these data we may calculate the atomic weight of platinum in four ways:

1. From loss of Cl upon ignition.......... Pt = 198.25
2. From weight of Pt in residue......... " = 197.42
3. From weight of KCl in residue........ " = 196.63
4. From ratio between KCl and Pt....... " = 197.10

The last of these values is undoubtedly the best, for it is not affected by errors due to the possible presence of moisture in the salt analyzed.

The work done by Andrews[3] is even less satisfactory than the foregoing, partly for the reason that its full details seem never to have been published. Andrews dried potassium chloroplatinate at 105°, and then decomposed it by means of zinc and water. The excess of zinc having been dissolved by treatment with acetic and nitric acids, the platinum was collected upon a filter and weighed, while the chlorine in the filtrate was estimated by Pelouze's method. Three determinations gave as follows for the atomic weight of platinum:

197.86
197.68
198.12

Mean, 197.887

Unfortunately, Andrews does not state how his calculations were made.

In 1881 Seubert[4] published his determinations, basing them upon very pure chloroplatinates of potassium and ammonium. The ammonium salt, $(NH_4)_2PtCl_6$, was analyzed by heating in a stream of hydrogen, expelling that gas by a current of carbon dioxide, and weighing the residual metal. In three experiments the hydrochloric acid formed during such a reduction was collected in an absorption apparatus, and

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[1] Poggend. Annalen, 8, 177. 1826.
[2] Poggend. Annalen, 13, 468. 1828.
[3] British Assoc. Report, 1852.
[4] Ber. Deutsch. chem. Gesell., 14, 865.

estimated by precipitation as silver chloride. Three series of experiments are given, representing three distinct preparations, as follows:

Series I.

Am_2PtCl_6.	*Pt.*	*Per cent. Pt.*
2.1266	.9348	43.957
1.7880	.7858	43.948
1.8057	.7938	43.960
2.6876	1.1811	43.946
4.7674	2.0959	43.963
2.0325	.8935	43.961
		Mean, 43.956, ± .002

Series II.

Am_2PtCl_6.	*Pt.*	*Per cent. Pt.*
3.0460	1.3363	43.871
2.6584	1.1663	43.876
2.3334	1.0238	43.872
1.9031	.8351	43.881
3.1476	1.3810	43.875
2.7054	1.1871	43.889
		Mean, 43.876, ± .001

Another portion of this preparation, recrystallized from water, of 1.4358 grm. gave 0.6311 of platinum, or 43.955 per cent.

Series III.

Am_2PtCl_6.	*Pt.*	*Per cent. Pt.*
2.5274	1.1118	43.990
3.2758	1.4409	43.986
1.9279	.8483	44.001
2.0182	.8884	44.020
1.8873	.8303	43.994
2.2270	.9798	43.996
2.4852	1.0936	44.004
2.5362	1.1166	44.026
3.0822	1.3561	43.998
		Mean, 44.001, ± .003

If these series are treated as independent and combined, giving each a weight as indicated by its probable error, and regarding the single experiment with preparation II as equal to one in the first series, we get a mean percentage of 43.907, ± .0009. On the other hand, if we regard the twenty-two experiments as all of equal weight in one series, the mean percentage of platinum becomes 43.953, ±.0078. Hence Pt= 195.14. Upon comparing the work with that done later by Halberstadt, and by Archibald, the latter mean seems the fairer one to adopt.

For the chlorine estimations in the ammonium salt, Seubert gives the subjoined data. I add in the last column the weight of salt proportional to 100 parts of silver chloride:

Am_2PtCl_6.	*Pt.*	$6AgCl$.	*Ratio.*
2.7054	1.1871	5.2226	51.802
2.2748	.9958	4.3758	51.986
3.0822	1.3561	5.9496	51.805
			Mean, 51.864, ± .041

Hence Pt=197.22.

The potassium salt, K_2PtCl_6, was also analyzed by ignition in hydrogen, treatment with water, and weighing both the platinum and the potassium chloride. The weights given are as follows:

K_2PtCl_6.	*Pt.*	$2KCl$.
5.0283	2.0173	1.5440
7.0922	2.8454	2.1793
3.5475	1.4217	1.0890
3.2296	1.2941	.9904
3.5834	1.4372	1.1001
4.4232	1.7746	1.3547
4.0993	1.6444	1.2589
4.4139	1.7713	1.3516

Hence we have these percentages, reckoned on the original salt:

Pt.	*KCl.*
40.119	30.706
40.120	30.728
40.076	30.698
40.070	30.666
40.107	30.700
40.120	30.627
40.114	30.710
40.130	30.621
Mean, 40.107, ± .005	Mean, 30.682, ± .009

Hence Pt=194.83. Hence Pt=195.06.

As with the ammonium salt, three experiments were made upon the potassium compound to determine the amount of chlorine (four atoms in this case) lost upon ignition in hydrogen. In the fourth column I add the amount of K_2PtCl_6 corresponding to 100 parts of AgCl:

K_2PtCl_6.	*Pt.*	*4AgCl.*	*Ratio.*
6.7771	2.7158	7.9725	85.006
3.5834	1.4372	4.2270	84.774
4.4139	1.7713	5.2144	84.648
			Mean, 84.809, ± .071

Hence Pt = 195.31.

Halberstadt,[1] like Seubert, studied the chloroplatinates of potassium and ammonium, and also the corresponding double bromides and platinic bromide as well. The metal was estimated partly by reduction in hydrogen, as usual, and partly by electrolysis. Platinic bromide gave the following results:

I. By reduction in H.

$PtBr_4$.	*Pt.*	*Per cent. Pt.*
.6396	.2422	37.867
1.7596	.6659	37.844
.9178	.3476	37.873
1.1594	.4388	37.847
1.9608	.7420	37.842
2.0865	.7898	37.853
4.0796	1.5422	37.852
6.8673	2.5985	37.839

II. By electrolysis.

$PtBr_4$.	*Pt.*	*Per cent. Pt.*
1.2588	.4763	37.837
1.4937	.5649	37.819

Mean of all ten experiments, 37.847, ± .0033

Hence Pt = 191.66.

The ammonium platinbromide, $(NH_4)_2PtBr_6$, was prepared in two ways, and five distinct lots were studied. With this salt, as well as with those which follow, the data are given in distinct series, with from one to several experiments in each group, but for present purposes it seems best to consolidate the material and so put it in more manageable form. The percentages of platinum and weights found are as follows:

I. By reduction in H.

Am_2PtBr_6.	*Pt.*	*Per cent. Pt.*
.6272	.1719	27.408
1.0438	.2865	27.447
1.1724	.3215	27.422
1.4862	.4076	27.426
1.0811	.2966	27.435
1.3383	.3672	27.437

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[1] Ber. Deutsch. chem. Gesell., 17, 2962. 1884.

1.0096	.2769	27.426
1.1935	.3269	27.390
1.3182	.3611	27.393
2.2476	.6159	27.402
1.3358	.3668	27.451
1.7859	.4899	27.431
4.1641	1.1427	27.441
1.1835	.3250	27.460
2.4003	.6591	27.459
2.5293	.6940	27.438
1.7147	.4705	27.439
2.3014	.6316	27.444
3.0052	.8245	27.435
4.8592	1.3329	27.430
1.5337	.4210	27.449
2.0373	.5594	27.457
2.0939	.5751	27.465

II. By electrolysis.

Am_2PtBr_6.	*Pt.*	*Per cent. Pt.*
1.5586	.4272	27.409
1.6052	.4397	27.392
3.1229	.8569	27.439
1.1612	.3180	27.386
2.5817	.7081	27.427
1.0231	.2809	27.456
1.6744	.4591	27.418
1.6744	.4591	27.418
1.6052	.4397	27.392

Mean of all thirty-two experiments, 27.429, ± .0027

Hence Pt = 194.88.

With potassium platinbromide Halberstadt found as follows:

I. By reduction in H.

K_2PtBr_6.	*Pt.*	*2KBr.*	*Per cent. Pt.*	*Per cent. KBr.*
2.5549	.6630	.8071	25.940	31.590
2.6323	.6831	.8318	25.947	31.599
2.9315	.7598	.9259	25.910	31.584
3.4463	.8939	1.0895	25.938	31.613
4.0081	1.0404	1.2653	25.957	31.568
3.9554	1.0266	1.2495	25.954	31.589
2.0794	.5388	.6558	25.911	31.538
2.1735	.5635	.6849	25.926	31.511
2.3099	.5986	.7297	25.914	31.590
1.4085	.3645	.4446	25.880	31.565
2.6166	.6772	.8279	25.881	31.640
2.6729	.6923	.8469	25.900	31.684

II. By electrolysis.

K_2PtBr_6.	*Pt.*	*2KBr.*	*Per cent. Pt.*	*Per cent. KBr.*
2.2110	.5726	.6997	25.898	31.647
3.1642	.8188	.9983	25.877	31.550
1.9080	.4947	.6025	25.927	31.577
1.6754	.4341	.5286	25.915	31.550
1.3148	.3403	.4160	25.882	31.640
1.5543	.4025	.4911	25.895	31.596

Mean of eighteen experiments, 25.915, ±.0040 31.591, ±.0068

Hence Pt = 195.09 and 195.79.

For ammonium platinchloride Halberstadt gives the following data:

I. By reduction in H.

Am_2PtCl_6.	*Pt.*	*Per cent. Pt.*
1.0604	.4662	43.964
1.3846	.6087	43.962
1.5065	.6617	43.923
2.3266	1.0227	43.956
1.3808	.6059	43.880
1.7396	.7638	43.906
2.7420	1.2068	44.011
3.1882	1.4019	43.971
5.4644	2.4035	43.984
3.4859	1.5321	43.951

II. By electrolysis.

Am_2PtCl_6.	*Pt.*	*Per cent. Pt.*
.9474	.4161	43.920
1.1069	.4865	43.951
1.5101	.6634	43.930
.5345	.2347	43.910
1.6035	.7044	43.928
1.9271	.8459	43.894
1.1046	.4858	43.979
1.4179	.6233	43.959

Mean of eighteen experiments, 43.943, ± .0054

Hence Pt = 195.01.

For potassium platinchloride Halberstadt's data are—

I. By reduction in H.

K_2PtCl_6.	*Pt.*	*2KCl.*	*Per cent. Pt.*	*Per cent. KCl.*
1.6407	.6574	.5029	40.069	30.651
1.9352	.7757	.5921	40.084	30.600
1.5793	.6334	.4836	40.106	30.621
1.6446	.6595	.5049	40.101	30.700
1.0225	.4102	.3133	40.117	30.640
2.4046	.9641	.7388	40.094	30.724
5.8344	2.3412	1.7905	40.127	30.688
7.1732	2.8776	2.1998	40.116	30.666

II. By electrolysis.

K_2PtCl_6.	*Pt.*	*2KCl.*	*Per cent. Pt.*	*Per cent. KCl.*
1.2354	.4953	.3792	40.092	30.695
2.5754	1.0318	.7898	40.063	30.667
1.0933	.4387	.3355	40.126	30.668
1.3560	.5438	.4167	40.103	30.730
1.7345	.6956	.5298	40.104	30.545
2.0054	.8038	.6147	40.081	30.652
2.0666	.8291	.6356	40.117	30.755
1.2759	.5118	.3908	40.112	30.629
1.9376	.7763	.5927	40.065	30.589
2.3972	.9608	.7355	40.080	30.681
2.7249	1.0929	.8364	40.108	30.691
Mean of nineteen experiments,			40.098,±.0031	30.663,±.0080
Seubert found,				30.682,±.0090
General mean,				30.671,±.0060

Hence Pt=194.78 and 195.36, from Halberstadt's data alone.

The work of Dittmar and M'Arthur[1] on the atomic weight of platinum is difficult to discuss and essentially unsatisfactory. They investigated potassium platinchloride, and came to the conclusion that it contains traces of hydroxyl replacing chlorine and also hydrogen replacing potassium. It is also liable, they think, to carry small quantities of potassium chloride. In their determinations, which involve corrections indicated by the foregoing considerations, they are not sufficiently explicit, and give none of their actual weighings. They attempt, however, to fix the ratio 2KCl : Pt, and after a number of discordant, generally high results, they give the following data for the atomic weight of platinum based upon the assumption that 2KCl=149.182:

195.54
195.48
195.60
195.37

Mean, 195.50, ± .0330

This ratio can also be computed from Seubert's and Halberstadt's analyses, and also the ratio 2KBr : Pt. It has not seemed necessary to do so, in view of the overwhelming weight of Archibald's more recent work.

Dittmar and M'Arthur also discuss Seubert's determinations, seeking to show that the latter also, properly treated, lead to a value nearer to 195.5 than to 195. Seubert at once replied to them,[2] pointing out that

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[1] Trans. Roy. Soc. Edinburgh, 33, 561. 1887.

[2] Ber. Deutsch. chem. Gesell., 21, 2179. 1888.

the concordance between his determinations by very different methods (a concordance verified by Halberstadt's investigation) precluded the existence of errors due to impurities such as Dittmar and M'Arthur assumed.

The recent determinations by Archibald[1] of the atomic weight of platinum were based upon analyses of the platinchlorides and platinbromides of potassium and ammonium. In these analyses every precaution was taken which modern experience had shown to be necessary. The possible presence of moisture in the several salts was carefully considered, and the potassium compounds in particular were dried at 380° to 400°. For the elaborate details of manipulation the original memoir must be consulted.

First, as to the analyses of potassium platinchloride. The salt, after thorough drying and weighing, was reduced by heating in a stream of pure hydrogen. The hydrochloric acid so formed was absorbed in water, and afterwards converted into silver chloride and weighed. Known quantities of silver were used in this operation, so that two distinct ratios were determined. From the residual mixture of potassium chloride and platinum the chloride was washed out, and its chlorine content was estimated as in the previous determinations. The metallic platinum, converted into sponge by again heating in hydrogen, was also weighed. Vacuum weights are given in all of Archibald's determinations. The weights were as follows:

K_2PtCl_6.	Pt.	4AgCl.	2AgCl.	4Ag.	2Ag.
1.43605	.57667	1.69324	.84690	1.27475	.63722
1.69914	.68226	2.00402	1.00172	1.50834	.75401
2.11830	.85062	2.49836	1.24894	1.88046	.93993
2.49734	1.00287	2.94462	1.47249	2.21626	1.10841
......	.86012	......	1.26271	......	.95030
2.20619	.88588	2.60135	1.30106	1.95842	.97909
1.70600	.68486	2.01201	1.00580	......	......
1.74397	.70018	2.05691	1.02820	1.54816	.77402
2.06137	.82789	2.43096	1.21526	1.82982	.91481
2.34095	.93991	2.76105	1.38034	2.07759	1.03868
1.54787	.62150	1.82560	.91266	1.37391	.68702
1.95944	.78694	2.31070	1.15522	1.73902	.86967
2.28366	.91697	2.69304	1.34636	2.02640	1.01338
2.27441	.91320	2.68244	1.34093	2.01870	1.00924

From these weights Archibald computes nine ratios as follows. In the first ratio I have recalculated the figures into the percentage form used for previous investigations. The other ratios are as Archibald gives them: but with the probable errors computed by myself:

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[1] Proc. Roy. Soc. Edinburgh, 29, 721. 1909.

	Per cent. Pt.	*½AgCl : Pt.*	*2AgCl : Pt.*	*½AgCl : K_2PtCl_6.*	*2AgCl : K_2PtCl_6.*
	40.157	34.057	68.092	84.811	169.57
	40.153	34.045	68.109	84.787	169.62
	40.156	34.047	68.107	84.788	169.61
	40.158	34.058	68.107	84.810	169.60
	.....	.....	68.117	.....	169.61
	40.154	34.055	68.089	84.810	169.57
	40.144	34.039	68.091	84.791	169.62
	40.149	34.040	68.098	84.786	169.61
	40.162	34.056	68.125	84.797	169.62
	40.151	34.042	68.093	84.785	169.59
	40.152	34.044	68.098	84.787	169.60
	40.161	34.056	68.120	84.799	169.62
	40.154	34.050	68.107	84.799	169.62
	40.151	34.045	68.102	84.789	169.62
Mean,	40.154, ± .0009	34.049, ± .0013	68.104, ± .0020	84.795, ± .0019	169.606, ± .0034
Pt =	195.21	195.22	195.24	195.23	195.20

	½Ag : Pt.	*2Ag : Pt.*	*½Ag : K_2PtCl_6.*	*2Ag : K_2PtCl_6.*
	45.238	90.498	112.65	225.36
	45.233	90.484	112.65	225.35
	45.235	90.498	112.65	225.37
	45.251	90.478	112.68	225.31
	.....	90.510	.....	225.36
	45.234	90.480	112.65	225.33
	45.227	90.460	112.65	225.31
	45.244	90.499	112.65	225.33
	45.240	90.488	112.68	225.37
	45.236	90.463	112.66	225.30
	45.252	90.487	112.68	225.31
	45.251	90.486	112.70	225.35
	45.237	90.484	112.67	225.36
Mean,	45.240, ± .0016	90.486, ± .0026	112.66, ± .0030	225.34, ± .0047
Pt =	195.22	195.23	195.20	195.24

For the first of these ratios, the percentage of Pt in K_2PtCl_6, there are previous determinations. The three series combine thus:

Seubert	40.107,	± .0050
Halberstadt	40.098,	± .0031
Archibald	40.154,	± .0009
General mean	40.1484,	± .00085

Similarly, the ratio $4AgCl : K_2PtCl_6$, as determined by Seubert, may be combined with Archibald's series. Better still the two series may be reduced to uniform type with Archibald's ratio for 2AgCl, and given in the form $AgCl : K_2PtCl_6 :: 100 : x$. The three series then combine as follows:

Seubert, 4AgCl	339.236, ± .2840
Archibald, 4AgCl	339.180, ± .0076
Archibald, 2AgCl	339.212, ± .0068
General mean	339.204, ± .0051

Archibald's two series of measurements of the ratios between silver and the platinchloride can also be reduced to the form $Ag : K_2PtCl_6 :: 100 : x$, and combined:

4Ag series	450.64, ± .0120
2Ag series	450.66, ± .0094
General mean	450.654, ± .0074

Archibald's data for ammonium platinchloride are rather simpler than with the potassium salt, since the total chlorine was determined at once, instead of in two portions. His weights are subjoined:

Am_2PtCl_6.	*Pt.*	*6AgCl.*	*6Ag.*
1.75088	.76976	3.39181	2.55181
1.36500	.59997	2.64317	1.99014
1.15060	.50585	2.22810	1.67695
1.27475	.56049	2.46936	1.85794
2.54096	1.11688	4.92047	3.70420

The derived ratios are as follows:

	Per cent. Pt.	*6AgCl:Pt.*	*$6AgCl:Am_2PtCl_6$.*	*6Ag:Pt.*	*$6Ag:Am_2PtCl_6$.*
	43.964	22.695	51.621	30.165	68.613
	43.954	22.699	51.643	30.147	68.588
	43.964	22.703	51.640	30.165	68.613
	43.969	22.698	51.623	30.167	68.611
	43.955	22.699	51.641	30.152	68.597
Mean,	43.961, ± .0061	22.699, ± .0031	51.634, ± .0032	30.159, ± .0027	68.604, ± .0034
Pt =	195.20	195.22	195.24	195.21	195.23

Two of the ratios can be combined with earlier measurements, as follows:

Percentage Pt in Am_2PtCl_6.

Seubert	43.953, ± .0078
Halberstadt	43.943, ± .0054
Archibald	43.961, ± .0061
General mean	43.951, ± .0036

Ratio $6AgCl : Am_2PtCl_6 :: 100 : x$.

Seubert	51.864, ± .0110
Archibald	51.634, ± .0032
General mean	51.636, ± .0032

For ammonium platinbromide Archibald gives these data:

Am_2PtBr_6.	*Pt.*	$6AgBr$.	$6Ag$.
1.83860	.50497	2.91430	1.67448
2.31057	.63437	3.66269	2.10379
2.33965	.64272	3.70900	2.13049

Hence the following ratios:

	Per cent. Pt.	$6AgBr : Pt$.	$6AgBr : Am_2PtBr_6$.	$6Ag : Pt$.	$6Ag : Am_2PtBr_6$.
	27.465	17.327	63.089	30.157	109.801
	27.455	17.320	63.084	30.154	109.829
	27.471	17.329	63.080	30.168	109.827
Mean,	27.464, ± .0032	17.325, ± .0018	63.084, ± .0018	30.160, ± .0030	109.816, ± .0055
Pt =	195.22	195.22	195.23	195.22	195.22

The percentage of platinum in Am_2PtBr_6 combines with Halberstadt's figures thus:

Halberstadt	27.429, ± .0027
Archibald	27.464, ± .0032
General mean	27.443, ± .0021

The analyses of potassium platinbromide were like those of chloride, the bromine being estimated in two portions, 2Br and 4Br. The weights are these:

K_2PtBr_6.	*Pt.*	$4AgBr$.	$2AgBr$.	$4Ag$.	$2Ag$.
2.19076	.56779	2.18543	1.09273	1.25544	.62770
2.42094	.62766	2.41510	1.20758	1.38761	.69378
1.78705	.46344	1.78284	.89156	1.02416	.51214
1.81840	.47156	1.81430	.90703	1.04228	.52105
2.47056	.64063	2.46507	1.23246	1.41572	.70800
2.19017	.56787	2.18525	1.09260	1.25530	.62756

From these weights nine ratios are deducible, as in the case of the platinchloride, as follows:

	Per cent. Pt.	$4AgBr:Pt$.	$2AgBr:Pt$.	$4AgBr:K_2PtBr_6$.	$2AgBr:K_2PtBr_6$.
	25.918	25.981	51.961	100.244	200.485
	25.926	25.989	51.976	100.242	200.479
	25.933	25.995	51.981	100.236	200.441
	25.933	25.991	51.990	100.226	200.478
	25.931	25.988	51.980	100.223	200.458
	25.928	25.990	51.974	100.225	200.455
Mean,	25.928,	25.989,	51.977,	100.233,	200.466,
	± .0015	± .0013	± .0027	± .0025	± .0048
Pt =	195.22	195.23	195.23	195.23	195.23

	$4Ag:Pt$.	$2Ag:Pt$.	$4Ag:K_2PtBr_6$.	$2Ag:K_2PtBr_6$.
	45.226	90.456	174.50	349.01
	45.233	90.470	174.47	348.95
	45.251	90.491	174.49	348.94
	45.243	90.502	174.46	348.99
	45.251	90.485	174.51	348.95
	45.238	90.489	174.47	349.00
Mean,	45.240,	90.482,	174.48,	348.97,
	± .0028	± .0045	± .0051	± .0085
Pt =	195.22	195.23	195.20	195.22

The percentage of platinum in the platinbromide combines with Halberstadt's figures as follows:

Halberstadt	25.915, ± .0040
Archibald	25.928, ± .0015
General mean	25.927, ± .0014

Several other ratios, given in diverse forms by Archibald, are also capable of consolidation. The ratio between silver bromide and potassium platinbromide, reduced to uniform type, that is, to $AgBr : K_2PtBr_6 :: 100 : x$, becomes—

2AgBr series	400.932, ± .0096
4AgBr series	400.932, ± .0100
General mean	400.932, ± .0069

For the ratio $Ag : K_2PtBr_6 :: 100 : x$ we have—

2Ag series	697.940, ± .0170
4Ag series	697.920, ± .0204
General mean	697.936, ± .0131

For the ratio AgBr : Pt :: 100 : x—

2AgBr series	103.954, ± .0054
4AgBr series	103.956, ± .0052
General mean	103.955, ± .0037

For the ratio AgCl : Pt :: 100 : x—

2AgCl series with K_2PtCl_6	136.208, ± .0046
4AgCl series with K_2PtCl_6	136.196, ± .0052
6AgCl series with Am_2PtCl_6	136.194, ± .0186
General mean	136.203, ± .0031

For the ratio Ag : Pt :: 100 : x—

2Ag series with K_2PtCl_6	180.972, ± .0052
4Ag series with K_2PtCl_6	180.960, ± .0064
6Ag series with Am_2PtCl_6	180.954, ± .0162
6Ag series with Am_2PtBr_6	180.960, ± .0180
2Ag series with K_2PtBr_6	180.964, ± .0090
4Ag series with K_2PtBr_6	180.960, ± .0112
General mean	180.965, ± .0034

From the last two ratios the cross ratio Ag : Cl :: 100 : 32.864, ± .0039 is deducible, which agrees closely with the measurements by Richards and Wells. From the corresponding ratios Ag : Pt and AgBr : Pt, we have the ratio Ag : Br :: 100 : 74.080, ± .0070. These agreements with the best determinations of the silver-halogen ratios is good evidence in favor of Archibald's work.

Rejecting the work of Berzelius and Andrews, the following ratios are now available from which to compute the atomic weight of platinum:

(1). Am_2PtCl_6 : Pt :: 100 : 43.951, ± .0036
(2). 6Ag : Am_2PtCl_6 :: 100 : 68.604, ± .0034
(3). 6AgCl : Am_2PtCl_6 :: 100 : 51.636, ± .0032
(4). K_2PtCl_6 : Pt :: 100 : 40.1484, ± .00085
(5). Ag : K_2PtCl_6 :: 100 : 450.654, ± .0074
(6). AgCl : K_2PtCl_6 :: 100 : 339.204, ± .0051
(7). Am_2PtBr_6 : Pt :: 100 : 27.443, ± .0021
(8). 6Ag : Am_2PtBr_6 :: 100 : 109.816, ± .0055
(9). 6AgBr : Am_2PtBr_6 :: 100 : 63.084, ± .0018
(10). K_2PtBr_6 : Pt :: 100 : 25.927, ± .0014
(11). Ag : K_2PtBr_6 :: 100 : 697.936, ± .0131
(12). AgBr : K_2PtBr_6 :: 100 : 400.932, ± .0069
(13). $PtBr_4$: Pt :: 100 : 37.847, ± .0033
(14). K_2PtCl_6 : 2KCl :: 100 : 30.671, ± .0060
(15). K_2PtBr_6 : 2KBr :: 100 : 31.591, ± .0068
(16). 2KCl : Pt :: 149.182 : 195.50, ± .0330
(17). Ag : Pt :: 100 : 180.965, ± .0034
(18). AgCl : Pt :: 100 : 136.203, ± .0031
(19). AgBr : Pt :: 100 : 103.955, ± .0037

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
Br = 79.9197, ± .0003
K = 39.0999, ± .0002
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 13 ....Pt = 194.663, ± .0199
“ “ 7 ....195.014, ± .0160
“ “ 1 ....195.123, ± .0203
“ “ 4 ....195.169, ± .0050
“ “ 10 ....195.212, ± .0112
“ “ 11 ....195.215, ± .0142
“ “ 5 ....195.216, ± .0081
“ “ 8 ....195.216, ± .0356
“ “ 17 ....195.225, ± .0037
“ “ 2 ....195.227, ± .0221
“ “ 19 ....195.227, ± .0070
“ “ 9 ....195.229, ± .0203
“ “ 12 ....195.231, ± .0130
“ “ 18 ....195.231, ± .0045
“ “ 14 ....195.236, ± .0951
“ “ 6 ....195.248, ± .0074
“ “ 3 ....195.252, ± .0276
“ “ 16 ....195.416, ± .0330
“ “ 15 ....195.785, ± .1622

General mean, Pt = 195.210, ± .0020

SCANDIUM.

Cleve,[1] who was the first to make accurate experiments on the atomic weight of this metal, obtained the following data: 1.451 grm. of sulphate, ignited, gave .5293 grm. of Sc_2O_3. .4479 grm. of Sc_2O_3, converted into sulphate, yielded 1.2255 grm. of the latter, which, upon ignition, gave .4479 grm. of Sc_2O_3. Hence, for the percentage of Sc_2O_3 in $Sc_2(SO_4)_3$ we have:

36.478
36.556
36.556

Mean, 36.530, ± .0175

Hence Sc = 45.12.

Later results are those of Nilson,[2] who converted scandium oxide into the sulphate. I give in a third column the percentage of oxide in sulphate:

.3379 grm. Sc_2O_3 gave	.9343 grm. $Sc_2(SO_4)_3$.	36.166 per cent.
.3015 "	.8330 "	36.194 "
.2998 "	.8257 "	36.187 "
.3192 "	.8823 "	36.178 "
	Mean,	36.181, ± .004

Hence Sc = 44.09.

Combining the two series, we have—

Cleve	36.530, ± .0175
Nilson	36.181, ± .0040
General mean	36.190, ± .0039

Hence, if S = 32.0667, ± .00075, Sc = 44.115, ± .0085.

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[1] Compt. Rend., 89, 419.

[2] Compt. Rend., 91, 118.

YTTRIUM.

Nearly all the regular determinations of the atomic weight of yttrium depend upon analyses or syntheses of the sulphate. A series of analyses of the oxalate, however, by Berlin,[1] is sometimes cited, and the data are as follows. In three experiments upon the salt $Yt_2(C_2O_4)_3.3H_2O$ the subjoined percentages of oxide were found:

45.70
45.65
45.72

Mean, 45.69, ± .0141

Hence Yt=89.55.

The early work of Berzelius[2] may be ignored. The first determinations of the atomic weight of yttrium to be considered are those of Popp,[3] who evidently worked with material not wholly free from earths of higher molecular weight than yttria. The yttrium sulphate was dehydrated at 200°; the sulphuric acid was then estimated as barium sulphate, and after the excess of barium in the filtrate had been removed the yttrium was thrown down as oxalate and ignited to yield oxide. The following are the weights given by Popp:

Sulphate.	*$BaSO_4$.*	*Yt_2O_3.*	*H_2O.*
1.1805 grm.	1.3145 grm.	.4742 grm.	.255 grm.
1.4295 "	1.593 "	.5745 "	.308 "
.8455 "	.9407 "	.3392 "	.1825 "
1.045 "	1.1635 "	.4195 "	.2258 "

Eliminating water, these figures give us for the percentages of Yt_2O_3 in $Yt_2(SO_4)_3$ the values in column A. In column B I put the quantities of Yt_2O_3 proportional to 100 parts of $BaSO_4$:

A.	*B.*
51.237	36.075
51.226	36.064
51.161	36.058
51.209	36.055
Mean, 51.208, ± .011	Mean, 36.063, ± .003

Hence Yt=102.05 from A, 102.27 from B.

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[1] Forhandlingar ved de Skandinaviske Naturforskeres, 8, 452. 1860.

[2] Lehrbuch, 5 Aufl., 3, 1225.

[3] Ann. Chem. Pharm., 131, 179. 1864.

In 1865 Delafontaine[1] published some results obtained from yttrium sulphate, the yttrium being thrown down as oxalate and weighed as oxide. In the fourth column I give the percentages of Yt_2O_3 reckoned from the anhydrous sulphate:

Sulphate.	Yt_2O_3.	H_2O.	*Per cent.* Yt_2O_3.
.9545 grm.	.371 grm.	.216 grm.	50.237
2.485 “	.9585 “	.565 “	49.922
2.153 “	.827 “	.4935 “	49.834

Mean, 49.998, ± .081

Hence Yt=96.09.

In another paper[2] Delafontaine gives the following percentages of Yt_2O_3 in dry sulphate. The mode of estimation was the same as before:

48.23
48.09
48.37

Mean, 48.23, ± .055

Hence Yt=87.89.

Bahr and Bunsen,[3] and likewise Cleve, adopted the method of converting dry yttrium oxide into anhydrous sulphate, and noting the gain in weight. Bahr and Bunsen give us the two following results. I add the usual percentage column:

Yt_2O_3.	$Yt_2(SO_4)_3$.	*Per cent.* Yt_2O_3.
.7266 grm.	1.4737 grm.	49.304
.7856 “	1.5956 “	49.235

Mean, 49.2695, ± .0233

Hence Yt=92.64.

Cleve's first results are published in a joint memoir by Cleve and Hoeglund,[4] and are as follows:

Yt_2O_3.	$Yt_2(SO_4)_3$.	*Per cent.* Yt_2O_3.
1.4060 grm.	2.8925 grm.	48.608
1.0930 “	2.2515 “	48.545
1.4540 “	2.9895 “	48.637
1.3285 “	2.7320 “	48.627
2.3500 “	4.8330 “	48.624
2.5780 “	5.3055 “	48.591

Mean, 48.605, ± .0096

Hence Yt=89.58.

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[1] Ann. Chem. Pharm., 134, 108. 1865.

[2] Arch. Sci. Phys. Nat. (2), 25, 119. 1866.

[3] Ann. Chem. Pharm., 137, 21. 1866.

[4] K. Svenska Vet. Akad. Handlingar, Bd. 1, No. 8. 1873.

In a later paper Cleve[1] gives syntheses of yttrium sulphate made with yttria which was carefully freed from terbia. The weights and percentages are as follows:

Yt_2O_3.	$Yt_2(SO_4)_3$.	Per cent. Yt_2O_3.
.8786	1.8113	48.507
.8363	1.7234	48.526
.8906	1.8364	48.497
.7102	1.4645	48.494
.7372	1.5194	48.519
.9724	2.0047	48.506
.9308	1.9197	48.487
.8341	1.7204	48.483
1.0224	2.1073	48.517
.9384	1.9341	48.519
.9744	2.0093	48.494
1.5314	3.1586	48.484

Mean, 48.503, ± .0029

Hence Yt = 89.12.

The yttria studied by Jones[2] had been purified by Rowland's method—that is, by precipitation with potassium ferrocyanide—and certainly contained less than one-half of one per cent. of other rare earths as possible impurities. Two series of determinations were made—one by ignition of the sulphate, the other by its synthesis. The results were as follows, with the usual percentage column added:

First series. Syntheses.

Yt_2O_3.	$Yt_2(SO_4)_3$.	Per cent. Yt_2O_3.
.2415	.4984	48.455
.4112	.8485	48.462
.2238	.4617	48.473
.3334	.6879	48.466
.3408	.7033	48.457
.3418	.7049	48.489
.2810	.5798	48.465
.3781	.7803	48.456
.4379	.9032	48.483
.4798	.9901	48.460

Mean, 48.467, ± .0025

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[1] K. Svenska Vet. Akad. Handlingar, No. 9, 1882. See also Bull. Soc. Chim., 39, 120. 1883.

[2] Amer. Chem. Journ., 17, 154. 1895.

Second series. Analyses.

$Yt_2(SO_4)_3$.	Yt_2O_3.	Per cent. Yt_2O_3.
.5906	.2862	48.459
.4918	.2383	48.455
.5579	.2705	48.485
.6430	.3117	48.478
.6953	.3369	48.454
1.4192	.6880	48.478
.8307	.4027	48.477
.7980	.3869	48.484
.8538	.4139	48.477
1.1890	.5763	48.469
		Mean, 48.472, ± .0024

From syntheses .... Yt = 88.96
From analyses .... " = 88.98

These data of Jones were briefly criticised by Delafontaine,[1] who regards a lower value as more probable. In a brief rejoinder[2] Jones defended his own work: but neither the attack nor the reply needs farther consideration here. They are referred to merely as part of the record.

By Muthmann and Böhm[3] there is a single determination. 2.46505 grammes $Yt_2(SO_4)_3$ gave 1.19511 Yt_2O_3. Per cent. Yt_2O_3, 48.482, and Yt = 89.00.

In a preliminary note, G. and E. Urbain[4] state that Yt = 88.6, but they give no details. Three determinations by Bodman[5] are as follows:

Yt_2O_3.	$Yt_2(SO_4)_3$.	Per cent. Yt_2O_3.
.4381	.8928	49.070
.5929	1.2093	49.028
.4062	.8286	49.022
		Mean, 49.040, ± .0102

Hence Yt = 91.57.

There are also two determinations by Brill,[6] made with the microbalance. The percentages of Yt_2O_3 in the sulphate are

48.647
48.617

Mean, 48.632, ± .0100

Hence Yt = 89.70.

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[1] Chem. News, 71, 243.
[2] Chem. News, 71, 305.
[3] Ber. Deutsch. chem. Ges., 33, 42. 1900.
[4] Compt. Rend., 132, 136. 1901.
[5] Bihang Svensk. Vet. Akad. Handl., 26 (2), No. 3. 1901.
[6] Zeitsch. anorg. Chem., 47, 464. 1905.

For the percentage of yttria in the sulphate we now have the following data, to be combined in the usual way. The one determination by Muthmann and Böhm is arbitrarily given equal weight with the figure assigned to Brill:

Popp	51.208,	± .0110
Delafontaine, first	49.998,	± .0810
Delafontaine, second	48.230,	± .0550
Bahr and Bunsen	49.2695,	± .0233
Cleve and Hoeglund	48.605,	± .0096
Cleve, later	48.503,	± .0029
Jones, syntheses	48.467,	± .0025
Jones, analyses	48.472,	± .0024
Muthmann and Böhm	48.482,	± .0100
Bodman	49.040,	± .0102
Brill	48.632,	± .0100
General mean	48.543,	± .0014

If we reject the first four of the values in this combination, the mean becomes 48.495, ± .0014. Hence Yt = 89.040, ± .0047, as compared with Yt = 89.299, derived from the mean of all. The determinations, previous to those of Cleve and Hoeglund, are of no present value.

The determinations made by Feit and Przibylla,[1] by their volumetric method, are as follows:

Yt_2O_3.	O.	*Atomic weight.*
.3677	.07781	89.415
.4928	.10438	89.309
.3660	.07749	89.356
.3660	.07751	89.328
.3704	.07840	89.387
.3635	.07701	89.284
		Mean, 89.346, ± .0135

From the sulphate, when S = 32.0667, ± .00075, Yt = 89.040, ± .0047. Combined with Feit and Przibylla's value the general mean becomes

Yt = 89.094, ± .0044

This is probably too high, by at least 0.1. But it would be unwise to reject any of the values included in the final combination.

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[1] Zeitsch. anorg. Chem., 50, 262. 1906. For the process, see under lanthanum.

LANTHANUM.

If we leave out of account the work of Mosander, and some worthless experiments of Choubine, our discussion of the atomic weight of lanthanum must begin with a single analysis by Rammelsberg[1] published in 1842. From 0.700 gramme of lanthanum sulphate he obtained 0.883 of barium sulphate. Hence 100 parts of $BaSO_4$ are equivalent to 79.276 of $La_2(SO_4)_3$, and La = 133.48.

Marignac,[2] working also with the sulphate of lanthanum, employed two methods. First, the salt in solution was mixed with a slight excess of barium chloride. The resulting barium sulphate was filtered off and weighed; but, as it contained some occluded lanthanum compounds, its weight was too high. In the filtrate the excess of barium was estimated, also as sulphate. This last weight of sulphate, deducted from the total sulphate which the whole amount of barium chloride could form, gave the sulphate actually proportional to the lanthanum compound. The following weights are given:

$La_2(SO_4)_3$.	$BaCl_2$.	1st $BaSO_4$.	2d $BaSO_4$.
4.346 grm.	4.758 grm.	5.364 grm.	.115 grm.
4.733 "	5.178 "	5.848 "	.147 "

Hence we have the following quantities of $La_2(SO_4)_3$ proportional to 100 parts of $BaSO_4$. Column A is deduced from the first $BaSO_4$ and column B from the second, after the manner above described:

A.	B.
81.022	83.281
80.934	83.662
Mean, 80.978, ± .030	Mean, 83.471, ± .128

From A .... La = 139.44
From B .... " = 148.17

A agrees best with other determinations, although, theoretically, it is not so good as B.

Marignac's second method, described in the same paper with the foregoing experiments, consisted in mixing solutions of $La_2(SO_4)_3$ with solutions of $BaCl_2$, titrating one with the other until equilibrium was

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[1] Poggend. Annalen, 55, 65.

[2] Arch. Sci. Phys. Nat. (1), 11, 29. 1849. Oeuvres Complètes, 1, 230.

established. The method has already been described under cerium. The weighings give maxima and minima for $BaCl_2$. In another column I give $La_2(SO_4)_3$ proportional to 100 parts of $BaCl_2$, mean weights being taken for the latter:

$La_2(SO_4)_3$.	*$BaCl_2$.*	*Ratio.*
11.644	12.765 — 12.825	91.004
12.035	13.195 — 13.265	90.968
10.690	11.669 — 11.749	91.297
12.750	13.920 — 14.000	91.332
10.757	11.734 — 11.814	91.362
12.672	13.813 — 13.893	91.475
9.246	10.080 — 10.160	91.364
10.292	11.204 — 11.264	91.615
10.192	11.111 — 11.171	91.482
		Mean, 91.322, ± .048

Hence La = 141.21.

Although not next in chronological order, some still more recent work of Marignac's[1] may properly be considered here. The salt studied was the sulphate of lanthanum, purified by repeated crystallizations. In two experiments the salt was calcined, and the residual oxide weighed: in two others the lanthanum was precipitated as oxalate, and converted into oxide by ignition. The data follow:

$La_2(SO_4)_3$.	*La_2O_3.*	*Per cent. La_2O_3.*
2.0988	1.2082	57.566
2.3504	1.3532	57.573
2.8113	1.6165	57.500
3.3385	1.9215	57.556
		Mean, 57.549, ± .0112

Hence La = 138.81.

The atomic weight determinations of Holzmann[2] were made by analyses of the sulphate and iodate of lanthanum, and the double nitrate of magnesium and lanthanum. In the sulphate experiments the lanthanum was first thrown down as oxalate, which, on ignition, yielded oxide. The sulphuric acid was precipitated as $BaSO_4$ in the filtrate:

Sulphate.	*La_2O_3.*	*$BaSO_4$.*
.9663	.5157	1.1093
.6226	.3323	.7123
.8669	.4626	.9869

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[1] Ann. Chim. Phys. (4), 30, 68. 1873. Oeuvres Complètes, 2, 566.

[2] Journ. prakt. Chem., 75, 321. 1858.

These results are best used by taking the ratio between the $BaSO_4$, put at 100, and the La_2O_3. The figures are then as follows:

46.489
46.652
46.873

Mean, 46.671, ± .075

In the analyses of the iodate the lanthanum was thrown down as oxalate, as before. The iodic acid was also estimated volumetrically, but the figures are hardly available for present discussion. The following percentages of La_2O_3 were found:

23.454
23.419
23.468

Mean, 23.447, ± .0216

The formula of this salt is $La_2(IO_3)_6.3H_2O$.

The double nitrate, $La_2(NO_3)_6.3Mg(NO_3)_2.24H_2O$, gave the following analytical data:

Salt.	*H_2O.*	*MgO.*	*La_2O_3.*
.5327	.1569	.0417	.1131
.5931	.1734	.0467	.1262
.5662	.1647	.0442	.1197
.3757	....	.0297	.0813
.3263	....	.0256	.0693

These weighings give the subjoined perecentages of La_2O_3:

21.231
21.278
21.141
21.640
21.238

Mean, 21.3056, ± .058

These data of Holzmann give values for the atomic weight of La as follows:

From sulphate .... La = 139.42
From iodate .... " = 137.65
From magnesian nitrate......... " = 138.65

Czudnowicz[1] based his determination of the atomic weight of lantha-

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[1] Journ. prakt. Chem., 80. 33. 1860.

num upon one analysis of the air-dried sulphate. The salt contained 22.741 per cent. of water.

.598 grm. gave .272 grm. La_2O_3 and .586 grm. $BaSO_4$

The La_2O_3 was found by precipitation as oxalate and ignition. The $BaSO_4$ was thrown down from the filtrate. Reduced to the standards already adopted, these data give for the percentage of La_2O_3 in the anhydrous sulphate the figure 58.668. 79.117 parts of the salt are proportional to 100 parts of $BaSO_4$. Hence La = 146.13 and 132.93.

Hermann[1] studied both the sulphate and the carbonate of lanthanum. From the anhydrous sulphate, by precipitation as oxalate and ignition, the following percentages of La_2O_3 were obtained:

57.690
57.663
57.610

Mean, 57.654, ± .016

Hence La = 139.51.

The carbonate, dried at 100°, gave the following percentages:

68.47 La_2O_3
27.67 CO_2
3.86 H_2O

Reckoning from the ratio between CO_2 and La_2O_3, the molecular weight of the latter becomes 326.66.

Zschiesche's[2] experiments consist of six analyses of lanthanum sulphate, which salt was dehydrated at 230°, and afterwards calcined. I subjoin his percentages, and in a fourth column deduce from them the percentage of La_2O_3 in the *anhydrous* salt:

H_2O.	SO_3.	La_2O_3.	La_2O_3 *in anhydrous salt.*
22.629	33.470	43.909	56.745
22.562	33.306	44.132	56.964
22.730	33.200	44.070	57.034
22.570	33.333	44.090	56.947
22.610	33.160	44.240	57.150
22.630	33.051	44.310	57.277

Mean, 57.021, ± .051

Hence La = 135.34.

Erk[3] found that .474 grm. of $La_2(SO_4)_3$, by precipitation as oxalate and

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[1] Journ. prakt. Chem., 82, 396. 1861.

[2] Journ. prakt. Chem., 104, 174.

[3] Jenaisches Zeitschrift, 6, 306. 1871.

ignition, gave .2705 grm. of La_2O_3, or 57.068 per cent. Hence La = 135.64. .7045 grm. of the sulphate also gave .8815 grm. of $BaSO_4$. Hence 100 parts of $BaSO_4$ are equivalent to 79.921 of $La_2(SO_4)_3$, and La = 135.74.

From Cleve we have two separate investigations relative to the atomic weight of lanthanum. In his first series[1] strongly calcined La_2O_3, spectroscopically pure, was dissolved in nitric acid, and then, by evaporation with sulphuric acid, converted into sulphate:

1.9215	grm. La_2O_3 gave	3.3365	grm. sulphate.	57.590	per cent.
2.0570	"	3.5705	"	57.611	"
1.6980	"	2.9445	"	57.667	"
2.0840	"	3.6170	"	57.617	"
1.9565	"	3.3960	"	57.612	"
				Mean, 57.619, ± .0085	

Hence La = 139.28.

In his second paper,[2] published nine years later, Cleve gives results similarly obtained, but with lanthanum oxide much more completely freed from other earths. The data are as follows, lettered to correspond to different fractions of the material studied:

B	.8390	grm. La_2O_3 gave	1.4600	sulphate.	57.466	per cent.
C	1.1861	"	2.0643	"	57.458	"
	.8993	"	1.5645	"	57.482	"
	.8685	"	1.5108	"	57.486	"
	.8515	"	1.4817	"	57.468	"
D	.6486	"	1.1282	"	57.490	"
	.7329	"	1.2746	"	57.500	"
E	1.2477	"	2.1703	"	57.490	"
F	1.1621	"	2.0217	"	57.481	"
	1.5749	"	2.7407	"	57.463	"
G	1.3367	"	2.3248	"	57.497	"
	1.4455	"	2.5146	"	57.484	"
					Mean, 57.480, ± .0040	

Hence La = 138.35.

Brauner, in 1882, published two sets of determinations, both based upon the conversion of pure La_2O_3 into $La_2(SO_4)_3$.

In his first paper, Brauner[3] gives only two syntheses, as follows:

1.75933	grm. La_2O_3 gave	3.05707	$La_2(SO_4)_3$.	57.566	per cent.
.92417	"	1.60589	"	57.549	"
				Mean, 57.5575, ± .0057	

Hence La = 138.87.

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[1] K. Svensk. Vet. Akad. Handlingar, Bd. 2, No. 7. 1874.

[2] K. Svensk. Vet. Akad. Handlingar, No. 2, 1883.

[3] Monats. Chem., 3, 1.

In Brauner's second[1] paper six determinations are given, one being affected by a misprint, which is corrected by a citation in Abegg's Handbuch:[2]

.7850 grm. La_2O_3	gave 1.3658 $La_2(SO_4)_3$.	57.476 per cent.
2.3500 "	4.0917 "	57.433 "
2.1052 "	3.6633 "	57.467 "
1.0010 "	1.7411 "	57.525 "
1.3807 "	2.4021 "	57.479 "
1.5275 "	2.6588 "	57.451 "
		Mean, 57.472, ± .0086

Hence La = 138.30.

Brauner's weighings are all reduced to a vacuum.

Both Bauer and Bettendorff made their determinations of the atomic weight of lanthanum by the same general method. Bauer's data[3] are as follows:

.6431 grm. La_2O_3	gave 1.1171 sulphate.	57.569 per cent.
.7825 "	1.3613 "	57.482 "
1.0112 "	1.7571 "	57.549 "
.7325 "	1.2725 "	57.564 "
		Mean, 57.541, ± .0136

Hence La = 138.76.

Bettendorff found[4]—

.9146 grm. La_2O_3	gave 1.5900 sulphate.	57.522 per cent.
.9395 "	1.6332 "	57.525 "
.9133 "	1.5877 "	57.523 "
1.0651 "	1.8515 "	57.526 "
		Mean, 57.524, ± .0006

Hence La = 138.65.

The few determinations by Wolcott Gibbs[5] were made by the oxalate method, which is described in the chapter on cerium. Their purpose, however, was rather to test the method than to definitely fix an atomic weight. The data given are as follows, with the ratio $3C_2O_3 : La_2O_3$ added:

La_2O_3.	*C_2O_3.*	*Ratio.*
45.61	30.15	151.327
45.64	30.07	151.729
	30.08	151.679
Mean, 45.625	30.11	151.528
		Mean, 151.566, ± .0607

Hence La = 139.71.

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[1] Monats. Chem., 3, 486.

[2] Band 3, Abth. 1, p. 240. Brauner's discussion of the atomic weight.

[3] Inaugural Dissertation, Freiburg, 1884.

[4] Liebig's Annalen, 256, 168. 1890.

[5] Proc. Amer. Acad., 28, 260. 1893.

Gibbs cites three determinations, by the same method, made by Shapleigh, who found La = 139.75, 139.72 and 139.67. The weighings, however, are not given, and the data are unavailable for present purposes.

In 1901 Bodman[1] published three determinations, based on syntheses of lanthanum sulphate. The figures are:

La_2O_3.	*$La_2(SO_4)_3$.*	*Per cent. La_2O_3.*
.4038	.7013	57.579
.4408	.7660	57.546
.4467	.7758	57.579
		Mean, 57.568, ± .0070

Hence La = 138.94.

In 1902 Jones[2] published his elaborate series of determinations, based upon scrupulously purified materials. He effected twelve syntheses of lanthanum sulphate from the oxide, and examined his product carefully for acid sulphate, whose presence would tend to lower the apparent atomic weight of the metal. This source of error Jones regards as excluded from his determinations. His results are as follows:

La_2O_3.	*$La_2(SO_4)_3$.*	*Per cent. La_2O_3.*
1.0122	1.7592	57.538
1.1268	1.9581	57.546
.94585	1.6437	57.543
1.0675	1.8553	57.538
.9030	1.5692	57.545
1.1273	1.9589	57.548
.9407	1.6347	57.546
1.0455	1.8168	57.546
1.1271	1.9586	57.546
1.3074	2.2720	57.544
1.3389	2.3267	57.545
1.2012	2.0874	57.545
		Mean, 57.544, ± .0006

Hence La = 138.78.

The atomic weight determinations by Brauner and Pavliček[3] included a study of both the sulphate and the oxalate methods. In lanthanum sulphate the acid salt was always found to be present, and its amount was determined by titration, with sodium hydroxide, using ethyl-orange as an indicator. The excess of acid thus measured tends to lower the apparent atomic weight of lanthanum, and Brauner argues very forcibly that all previous determinations of atomic weights among the rare earths

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[1] Bihang Svensk. Vet. Akad. Handl., 26 (2), No. 3. 1901.

[2] Amer. Chem. Journ., 28, 23. 1902.

[3] Journ. Chem. Soc., 81, 1243. 1902. Preliminary notice in Proc. Chem. Soc., 17, 63. 1901.

are vitiated by this error. The authors give three series of syntheses of the sulphate, in which corrections for the acid salt are applied. First, there is a preliminary series, with weights in air. The data, with the acid correction, are as follows, representing eight different fractions of the oxide:

La_2O_3.	$La_2(SO_4)_3$.	*Per cent.* La_2O_3.
.93205	1.6198	57.541
.8416	1.46234	57.552
.85993	1.49440	57.543
.7847	1.3635	57.550
.80645	1.40145	57.544
1.0760	1.86913	57.567
*1.0683	*1.51479	57.572
.8721	1.51449	57.584
.9755	1.69381	57.592
.9188	1.5955	57.587
.9507	1.65040	57.604
.9677	1.68062	57.580
.8570	1.48736	57.619

Mean, 57.572, ± .0047

Corrected to a vacuum this mean becomes 57.564, ± .0047. Hence La = 138.91. Its significance, however, is diminished by the fact that the fractions show a progressive change in composition, which appears in the percentage column. The later fractions are higher in La_2O_3 than the earlier ones.

The next set of syntheses, representing a different series of lanthanum preparations, gave the subjoined results, corrected for acid sulphate, but with weights in air:

La_2O_3.	$La_2(SO_4)_3$.	*Per cent.* La_2O_3.
.8262	1.4353	57.563
.95652	1.66133	57.576
.45780	.79574	57.538
1.34754	2.34074	57.569
1.17280	2.03804	57.545

Mean, 57.558, ± .0048

Corrected for weighing in air this becomes 57.550. La = 138.82.

This series, like the preceding one, is given by Brauner and Pavliček as preliminary to more exact work, which seems to have been continued by Brauner alone. His final series of figures, determined with extreme

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* These weights are erroneous, either through misprinting in the original or because of copying. The percentage given is that calculated by Brauner and Pavliček.

care, and with all corrections applied, including the reduction to a vacuum, follows:

La_2O_3.	$La_2(SO_4)_3$.	Per cent. La_2O_3.
1.06562	1.85054	57.5843
1.00694	1.74856	57.5868
1.12553	1.95457	57.5845
1.70276	2.95707	57.5827
1.02460	1.77943	57.5802
1.28650	2.23419	57.5824
1.06488	1.84910	57.5891
		Mean, 57.5843, ± .00075

Hence La = 139.05.

The oxalate series of determinations by Brauner and Pavlíček is less satisfactory than this sulphate series, although it leads to sensibly the same value for the atomic weight of lanthanum. Rejecting one experiment in their series, which is thrown out as abnormal by the authors, their percentages are as given in the next table, together with the usual ratios:

Per cent. C_2O_3.	Per cent. La_2O_3.	Ratio.
31.041	46.884	151.039
	46.876	151.013
29.673	44.722	150.716
	44.711	150.679
	44.694	150.622
	44.719	150.706
	44.746	150.797
29.755	44.879	150.828
	44.845	150.714
31.920	48.197	150.993
29.689	44.751	150.733
30.883	46.719	151.277
	46.678	151.145
31.762	48.129	151.530
	48.118	151.495
	48.133	151.543
		Mean, 150.989, ± .0543

Hence La = 139.08.

The Gibbs value for the same ratio is 151.566, ± .0607. The general mean of both series is 151.246, ± .0400.

In a criticism of Jones' determinations by the sulphate method Brauner[1] reiterates his statements concerning the acid salt, and also suggests other sources of error, such as contamination of the lanthanum

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[1] Zeitsch. anorg. Chem., 33, 317. 1903.

preparations by cerium, and losses by spattering. To this criticism Jones[1] promptly replied, giving a new series of determinations as follows:

La_2O_3.	$La_2(SO_4)_3$.	Per cent. La_2O_3.
1.2161	2.1132	57.548
1.6311	2.8342	57.551
1.7804	3.0938	57.547
1.4168	2.4619	57.549
1.9702	3.4235	57.549
		Mean, 57.549, ± .0010

Hence La=138.81.

The material was spectroscopically pure, and the sulphate was neutral and soluble. The operations were performed in porcelain crucibles, and the oxide was perfectly white. Brauner used platinum crucibles, and Jones found that lanthanum oxide, heated in platinum, became perceptibly discolored. Two determinations made in platinum gave the following results:

La_2O_3.	$La_2(SO_4)_3$.	Per cent. La_2O_3.
1.2820	2.2264	57.582
1.3885	2.4110	57.590
		Mean, 57.586, ± .0027

Hence La=139.06, a value in accord with Brauner's. According to Jones the discoloration and variation in atomic weight suggest the presence of some other oxide than the normal compound in Brauner's preparations. The controversy, however, remains unsettled, and additional investigations are needed to determine the truth.

The two determinations by Brill[2] are of slight value, and hardly worth considering. Small quantities of lanthanum sulphate were calcined to oxide, and the weighings were made with the Nernst microbalance, in order to test its applicability to work of this kind. The percentages of oxide in sulphate are given below, more for the sake of completeness than for any real significance in them:

57.664
57.726

Mean, 57.695, ± .0207

Hence La=139.79., a very high value.

In 1906 Feit and Przibylla[3] determined the atomic weights of several rare earth metals by a special volumetric process, which, however, seems

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[1] Zeitsch. anorg. Chem., 36, 92. 1903. Chem. News, 88, 13.

[2] Zeitsch. anorg. Chem., 47, 464. 1905.

[3] Zeitsch. anorg. Chem., 50, 248. 1906. See also an earlier paper in Vol. 43, p. 213. 1905.

to be approximate rather than exact. In each case the weighed oxide was dissolved in an excess of half-normal sulphuric acid, the excess being afterwards measured by titration with tenth-normal sodium hydroxide solution. From the data thus obtained, by a process which is not clearly explained, the authors compute the proportion of oxygen in the oxides, and thence deduce the atomic weights of the several methods. It would have been better to have given the H_2SO_4 equivalent to the oxide, and then to have made a more direct calculation. However, I cite the determinations for what they may be worth, their value being essentially corroborative. For lanthanum the authors give the following determinations:

La_2O_3.	*O.*	*Atomic weight.*
.5125	.07544	139.05
.5256	.07731	139.11
.4835	.07116	139.08
.5235	.07706	139.04
.4815	.07088	139.03
.5156	.07585	139.15
.5348	.07867	139.15
		Mean, 139.09, ± .0430

We may now combine the similar means into general means, and deduce a value for the atomic weight of lanthanum. For the percentage of oxide in sulphate we have estimates as follows. The single experiments of Czudnowicz and of Erk are assigned the probable error and weight of a single experiment in Hermann's series:

Czudnowicz	58.668,	± .027
Erk	57.068,	± .027
Hermann	57.654,	± .016
Zschiesche	57.021,	± .051
Marignac	57.549,	± .0112
Cleve, earlier series	57.619,	± .0085
Cleve, later series	57.480,	± .0040
Brauner, 1882, first series	57.5575,	± .0057
Brauner, 1882, second series	57.472,	± .0086
Bauer	57.541,	± .0136
Bettendorff	57.524,	± .0006
Bodman	57.568,	± .0070
Jones, 1902	57.544,	± .0006
Brauner and Pavliček, first	57.564,	± .0047
Brauner and Pavliček, second	57.550,	± .0048
Brauner and Pavliček, third	57.5843,	± .00075
Jones, 1903, porcelain series	57.549,	± .0010
Jones, 1903, platinum series	57.586,	± .0027
Brill	57.695,	± .0207
General mean	57.5469,	± .00034

This mean agrees very closely with the figures given by Jones. The early determinations, previous to Marignac, might be properly rejected altogether, as their influence upon the combination is imperceptible.

For the quantity of $La_2(SO_4)_3$ proportional to 100 parts of $BaSO_4$, we have five experiments, which may be given equal weight and averaged together:

Marignac	81.022
Marignac	80.934
Rammelsberg	79.276
Czudnowicz	79.117
Erk	79.921

Mean, 80.054, ± .270

In all there are eight ratios from which to calculate the atomic weight of lanthanum:

(1). Percentage of La_2O_3 in $La_2(SO_4)_3$, 57.5469, ± .00034
(2). $3BaCl_2:La_2(SO_4)_3::100:91.322$, ± .048—Marignac
(3). $3BaSO_4:La_2(SO_4)_3::100:80.054$, ± .270
(4). $3BaSO_4:La_2O_3::100:46.671$, ± .075—Holzmann
(5). Percentage of La_2O_3 in iodate, 23.447, ± .0216—Holzmann
(6). Percentage of La_2O_3 in magnesian nitrate, 21.3056, ± .058—Holzmann
(7). $3C_2O_3:La_2O_3::100:151.246$, ± .0400
(8). $O:La::16:139.09$, ± .0430—Feit and Przibylla

Hermann's single experiment on the carbonate is omitted from this scheme as being of no value.

The antecedent atomic weights are—

Ba = 137.363, ± .0025
Mg = 24.304, ± .0006
S = 32.0667, ± .00075
C = 12.0038, ± .0002
Cl = 35.4584, ± .0002
I = 126.9204, ± .00033
N = 14.0101, ± .0001
H = 1.00779, ± .00001

Hence,

From ratio 3	La = 136.205, ± .6875
" " 5	137.652, ± .1557
" " 6	138.637, ± .4587
" " 1	138.801, ± .0020
" " 8	139.090, ± .0430
" " 7	139.363, ± .0303
" " 4	139.416, ± .2626
" " 2	141.208, ± .1500

It is evidently unnecessary to combine these values into a general mean, for only one of them, that from ratio 1, carries any appreciable weight. The other values could not modify it to any noteworthy extent.

The value La = 138.8 is essentially that found by Jones, whose work is entitled to high credit. Brauner, however, by two distinct methods, found La = 139, with much to be said in favor of his determinations. The question as to the true atomic weight of lanthanum is therefore not closed; and it should be taken up anew by means of other methods than those heretofore employed.

CERIUM.

Although cerium was discovered almost at the beginning of the nineteenth century, its atomic weight was not properly determined until after the discovery of lanthanum and didymium by Mosander. In 1842 the investigation was undertaken by Beringer,[1] who employed several methods. His cerium salts, however, were all rose-colored, and therefore were not wholly free from didymium; and his results are further affected by a negligence on his part to fully describe his analytical processes.

First, a neutral solution of cerium chloride was prepared by dissolving the carbonate in hydrochloric acid. This gave weights of ceric oxide and silver chloride as follows. The third column shows the amount of CeO_2 proportional to 100 parts of AgCl:

CeO_2.	*AgCl.*	*Ratio.*
.5755 grm.	1.119 grm.	40.557
.6715 "	1.6595 "	40.464
1.1300 "	2.786 "	40.560
.5366 "	1.3316 "	40.297

Mean, 40.469, ± .0415

Hence Ce = 142.02.

The analysis of the dry cerium sulphate gave results as follows. In a fourth column I show the amount of CeO_2 proportional to 100 parts of $BaSO_4$:

Sulphate.	CeO_2.	$BaSO_4$.	*Ratio.*
1.379	.8495	1.711	49.649
1.276	.7875	1.580	49.836
1.246	.7690	1.543	49.838
1.553	.9595	1.921	49.948

Mean, 49.819, ± .042

Hence Ce = 142.44.

Beringer also gives a single analysis of the formate and the results of

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[1] Ann. Chem. Pharm., 42, 134. 1842.

one conversion of the sulphide into oxide. The figures are, however, not valuable enough to cite.

The foregoing data involve one variation from Beringer's paper. Where I put CeO_2 as found he puts Ce_2O_3. The latter is plainly inadmissible, although the atomic weights calculated from it agree curiously well with some other determinations. Obviously, the presence of didymium in the salts analyzed tends to raise the apparent atomic weight of cerium.

Shortly after Beringer, Hermann[1] published the results of one experiment. 23.532 grm. of anhydrous cerium sulphate gave 29.160 grm. of $BaSO_4$. Hence 100 parts of the sulphate correspond to 123.926 of $BaSO_4$, and Ce = 138.14.

In 1848 similar figures were published by Marignac,[2] who found the following amounts of $BaSO_4$ proportional to 100 of dry cerium sulphate:

122.68
122.00
122.51

Mean, 122.40, ± .138

Hence Ce = 141.97.

If we give Hermann's single result the weight of one experiment in this series, and combine, we get a mean value of 122.856, ± .130.

Still another method was employed by Marignac. A definite mixture was made of solutions of cerium sulphate and barium chloride. To this were added, volumetrically, solutions of each salt successively, until equilibrium was attained. The figures published give maxima and minima for the $BaCl_2$ proportional to each lot of $Ce_2(SO_4)_3$. In another column, using the mean value for $BaCl_2$ in each case, I put the ratio between 100 parts of this salt and the equivalent quantity of sulphate. The latter compound was several times recrystallized:

$Ce_2(SO_4)_3$.		$BaCl_2$.	Ratio.
First crystallization...	11.011	11.990 — 12.050	91.606
First crystallization...	13.194	14.365 — 14.425	91.657
Second crystallization.	13.961	15.225 — 15.285	91.518
Second crystallization.	12.627	13.761 — 13.821	91.559
Second crystallization.	11.915	12.970 — 13.030	91.654
Third crystallization...	14.888	16.223 — 16.283	91.602
Third crystallization...	14.113	15.383 — 15.423	91.755
Fourth crystallization.	13.111	14.270 — 14.330	91.685
Fourth crystallization.	13.970	15.223 — 15.283	91.588

Mean, 91.625, ± .016

Hence Ce = 141.33.

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[1] Journ. prakt. Chem., 30, 185. 1843.

[2] Arch. Sci. Phys. Nat. (1), 8, 273. 1848. Oeuvres Complètes, 1, 215.

Omitting the valueless experiments of Kjerulf,[1] we come next to the figures published by Bunsen and Jegel[2] in 1858. From the air-dried sulphate of cerium the metal was precipitated as oxalate, which, ignited, gave CeO_2. In the filtrate from the oxalate the sulphuric acid was estimated as $BaSO_4$:

1.5726 grm.	sulphate gave	.7899 grm.	CeO_2 and	1.6185 grm.	$BaSO_4$.
1.6967	"	.8504	"	1.7500	"

Hence, for 100 parts $BaSO_4$, the CeO_2 is as follows:

48.804
48.575

Mean, 48.689, ± .077

Hence Ce = 138.48.

One experiment was also made upon the oxalate:

.3530 grm. oxalate gave .1913 CeO_2 and .0506 H_2O

Hence, in the dry salt, we have 63.261 per cent. of CeO_2.

In each sample of CeO_2 the excess of oxygen over Ce_2O_3 was estimated by an iodometric titration; but the data thus obtained need not be further considered.

In two papers by Rammelsberg[3] data are given for the atomic weight of cerium, as follows. In the earlier paper cerium sulphate was analyzed, the cerium being thrown down by caustic potash, and the acid precipitated from the filtrate as barium sulphate:

.413 grm. $Ce_2(SO_4)_3$ gave .244 grm. CeO_2 and .513 grm. $BaSO_4$

Hence $100BaSO_4 = 47.563\ CeO_2$, a value which may be combined with others, thus; this figure being assigned a weight equal to one experiment in Bunsen's series:

Beringer	49.819, ± .042
Bunsen and Jegel	48.689, ± .077
Rammelsberg	47.563, ± .108
General mean	49.360, ± .035

It should be noted here that this mean is somewhat arbitrary, since Bunsen's and Rammelsberg's cerium salts were undoubtedly freer from didymium than the material studied by Beringer.

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[1] Ann. Chem. Pharm., 87, 12.

[2] Ann. Chem. Pharm., 105, 45. 1858.

[3] Poggend. Annalen, 55, 65; 108, 44.

In his later paper Rammelsberg gives these figures concerning cerium oxalate. One hundred parts gave 10.43 of carbon and 21.73 of water. Hence the dry salt should yield 48.862 per cent. of CO_2, whence Ce = 138.18.

In all of the foregoing experiments the ceric oxide was somewhat colored, the tint ranging from one shade to another of light brown, according to the amount of didymium present. Still, at the best, a color remained, which was supposed to be characteristic of the oxide itself. In 1868, however, some experiments of Dr. C. Wolf[1] were posthumously made public, which went to show that pure ceroso-ceric oxide is white, and that all samples previously studied were contaminated with some other earth, not necessarily didymium but possibly a new substance, the removal of which tended to lower the apparent atomic weight of cerium very perceptibly.

Cerium sulphate was recrystallized at least ten times. Even after twenty recrystallizations it still showed spectroscopic traces of didymium. The water contained in each sample of the salt was cautiously estimated, and the cerium was thrown down by boiling concentrated solutions of oxalic acid. The resulting oxalate was ignited with great care. I deduce from the weighings the percentage of CeO_2 given by the *anhydrous* sulphate:

Sulphate.	*Water.*	*CeO_2.*	*Per cent. CeO_2.*
1.4542	.19419	.76305	60.559
1.4104	.1898	.7377	60.437
1.35027	.1820	.70665	60.487
			Mean, 60.494

After the foregoing experiments the sulphate was further purified by solution in nitric acid and pouring into a large quantity of boiling water. The precipitate was converted into sulphate and analyzed as before:

Sulphate.	*Water.*	*CeO_2.*	*Per cent. CeO_2.*
1.4327	.2733	.69925	60.311
1.5056	.2775	.7405	60.296
1.44045	.2710	.7052	60.300
			Mean, 60.302

From another purification the following weights were obtained:

1.4684 grm.	.1880 grm.	.7717 grm.	60.270 per cent.

A last purification gave a still lower percentage:

1.3756 grm.	.1832 grm.	.7186 grm.	60.265 per cent.

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[1] Amer. Journ. Science (2), 46, 53.

The last oxide was perfectly white, and was spectroscopically free from didymium. In each case the CeO_2 was titrated iodometrically for its excess of oxygen. It will be noticed that in the successive series of determinations the percentage of CeO_2 steadily and strikingly diminishes to an extent for which no ordinary impurity of didymium can account. The death of Dr. Wolf interrupted the investigation, the results of which were edited and published by Professor F. A. Genth.

In the light of more recent evidence, little weight can be given to these observations. All the experiments, taken equally, give a mean percentage of CeO_2 from $Ce_2(SO_4)_3$ of 60.366, ± .0308. This mean has obviously little or no real significance. It gives Ce = 138.74.

The experiments of Wolf attracted little attention, except from Wing,[1] who partially verified certain aspects of them. This chemist, incidentally to other researches, purified some cerium sulphate after the method of Wolf, and made two similar analyses of it, as follows:

Sulphate.	*Water.*	*CeO_2.*	*Per cent. CeO_2.*
1.2885	.1707	.6732	60.225
1.4090	1857	.7372	60.263
			Mean, 60.244

Hence Ce = 137.88.

The ceric oxide in this case was perfectly white. The cerium oxalate which yielded it was precipitated boiling by a boiling concentrated solution of oxalic acid. The precipitate stood twenty-four hours before filtering.

In 1875 Buehrig's[2] paper upon the atomic weight of cerium was issued. He first studied the sulphate, which, after eight crystallizations, still retained traces of free sulphuric acid. He found, furthermore, that the salt obstinately retained traces of water, which could not be wholly expelled by heat without partial decomposition of the material. These sources of error probably affect all the previously cited series of experiments, although, in the case of Wolf's work, it is doubtful whether they could have influenced the atomic weight of cerium by more than one or two-tenths of a unit. Buehrig also found, as Marignac had earlier shown, that upon precipitation of cerium sulphate with barium chloride the barium sulphate invariably carried down traces of cerium. Furthermore, the ceric oxide from the filtrate always contained barium. For these reasons the sulphate was abandoned, and the atomic weight determinations of Buehrig were made with air-dried oxalate. This salt was placed in a series of platinum boats in a combustion tube behind copper oxide. It was then burned in a stream of pure, dry oxygen, and the carbonic

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[1] Am. Journ. Sci. (2), 49, 358. 1870.

[2] Journ. prakt. Chem., 120, 222. 1875.

acid and water were collected after the usual method. Ten determinations were made; in all of them the above-named products were estimated, and in five analyses the resulting ceric oxide was also weighed. By deducting the water found from the weight of the air-dried oxalate, the weight of the anhydrous oxalate is obtained, and the percentages of its constituents are easily determined. In weighing, the articles weighed were always counterpoised with similar materials. The following weights were found:

Oxalate.	*Water.*	CO_2.	CeO_2.
9.8541	2.1897	3.6942	.....
9.5368	2.1269	3.5752	.....
9.2956	2.0735	3.4845	.....
10.0495	2.2364	3.7704	.....
10.8249	2.4145	4.0586	.....
9.3679	2.0907	3.5118	4.6150
9.7646	2.1769	3.6616	4.8133
9.9026	2.2073	3.7139	4.8824
9.9376	2.2170	3.7251	4.8971
9.5324	2.1267	3.5735	4.6974

These figures give us the following percentages for CO_2 and CeO_2 in the anhydrous oxalate:

CO_2.	CeO_2.
48.256	.....
48.249	.....
48.248	.....
48.257	.....
48.257	.....
48.258	63.417
48.257	63.436
48.262	63.446
48.249	63.429
48.253	63.430
Mean, 48.2546, ± .001	Mean, 63.4316, ± .0032

Hence Ce = 141.56. Hence Ce = 141.48.

These results could not be appreciably affected by combination with the single oxalate experiments of Jegel and of Rammelsberg, and the latter may therefore be ignored.

Robinson's work, published in 1884,[1] was based upon pure cerium chloride, prepared by heating dry cerium oxalate in a stream of dry, gaseous hydrochloric acid. This compound was titrated with standard solutions of pure silver, prepared according to Stas, and these were

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[1] Chemical News, 50, 251. 1884. Proc. Roy. Soc., 37, 150.

weighed, not measured. In the third column I give the ratio between $CeCl_3$ and 100 parts of silver:

$CeCl_3$.	Ag.	Ratio.
5.5361	7.26630	76.189
6.0791	7.98077	76.172
6.4761	8.50626	76.133
6.98825	9.18029	76.122
6.6873	8.78015	76.164
7.0077	9.20156	76.158
6.9600	9.13930	76.150
		Mean, 76.155, ± .0065

Reduced to a vacuum this becomes 76.167. Hence Ce = 140.13.

In a later paper,[1] Robinson discusses the color of ceric oxide, and criticises the work of Wolf. He shows that the pure oxide is not white, and makes it appear probable that Wolf's materials were contaminated with compounds of lanthanum. He also urges that Wolf's cerium sulphate could not have been absolutely definite, because of defects in the method by which it was dehydrated.

Brauner,[2] in 1885, investigated cerium sulphate with extreme care, and appears to have obtained material free from all other earths and absolutely homogeneous. The anhydrous salt was calcined with all necessary precautions, and the data obtained, reduced to a vacuum, were as follows:

$Ce_2(SO_4)_3$.	CeO_2.	Per cent. CeO_2.
2.16769	1.31296	60.5693
2.43030	1.47205	60.5707
2.07820	1.25860	60.5620
2.21206	1.33989	60.5721
1.28448	.77845	60.6043
1.95540	1.18436	60.5687
2.46486	1.49290	60.5673
2.04181	1.23733	60.5997
2.17714	1.31878	60.5739
2.09138	1.26654	60.5605
2.21401	1.34139	60.5863
2.44947	1.48367	60.5711
2.22977	1.35073	60.5771
2.73662	1.65699	60.5486
2.62614	1.59050	60.5642
1.67544	1.01470	60.5632
1.57655	.95540	60.6007
2.72882	1.65256	60.5600

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[1] Chemical News, 54, 229. 1886.

[2] Sitzungsb. Wien. Akad., Bd. 92. July, 1885.

2.10455	1.27476	60.5716
2.10735	1.27698	60.5965
2.43557	1.47517	60.5692
3.01369	1.82524	60.5649
4.97694	3.01372	60.5537
		Mean, 60.5729, ± .0021

Hence Ce = 140.22.

In 1895 several papers upon the cerite earths were published by Schutzenberger.[1] In the first of these a single determination of atomic weight is given. Pure CeO_2, of a yellowish-white color, was converted into sulphate, which was dried in a current of dry air at 440°. This salt, dissolved in water, was poured into a hot solution of caustic soda, made from sodium, and, after filtration and washing, the filtrate, acidulated with hydrochloric acid, was precipitated with barium chloride. The trace of sulphuric acid retained by the cerium hydroxide was recovered by re-solution and a second precipitation, and added to the main amount. 100 parts of $Ce_2(SO_4)_3$ gave 123.30 of $BaSO_4$. This may be assigned equal weight with one experiment in Marignac's series, giving the following combination:

Hermann	123.926, ± .238
Marignac	122.40, ± .138
Schutzenberger	123.30, ± .238
General mean	122.958, ± .1139

Schutzenberger, criticising Brauner's work, claims that the latter was affected by a loss of oxygen during the calcination of the cerium dioxide.

In his second and third papers Schutzenberger describes the results obtained upon the fractional crystallization of cerium sulphate. Preparations were thus made yielding oxides of various colors—canary-yellow, rose, yellowish-rose, reddish and brownish-red. These oxides, by synthesis of sulphates, the barium-sulphate method, etc., gave varying values for the atomic weight of cerium, ranging from 135.7 to 143.3. Schutzenberger therefore infers that cerium oxide from cerite contains small quantities of another earth of lower molecular weight; but the results as given are not conclusive. The third paper is essentially a continuation of the second, with reference to the didymiums.[2]

Schutzenberger's papers were promptly followed by one from Brauner,[3] who claimed priority in the matter of fractionation, and gave some new

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[1] Compt. Rend., 120, pp. 663, 962, and 1143. 1895.

[2] Similar results were also obtained by Boudouard (Compt. Rend., 125, 772. 1897). The correctness of these conclusions is questionable.

[3] Chem. News, 71, 283.

data, the latter tending to show that cerium oxide is a mixture of at least two earths. One of these, of a dark salmon color, he ascribed to a new element, "meta-cerium." The other he called cerium, and gave for it a preliminary atomic weight determination. The pure oxalate, by Gibbs' method, gave 46.934 per cent. of CeO_2, and, on titration with potassium permanganate, 29.503 and 29.506 per cent. of C_2O_3. Hence Ce = 139.62. In mean, this ratio may be written—

$$3C_2O_3 : 2CeO_2 :: 100 : 159.074$$

which will be combined with other corresponding expressions later.

Wyrouboff and Vernenil[1] determined the atomic weight of cerium by analyses of the sulphate, $Ce_2(SO_4)_3.8H_2O$. The salt was prepared from three different sources, two samples from monazite, and one from cerite. It was dehydrated at 250°, and reduced to CeO_2 at 1500°. The latter was perfectly white. The weights were as follows:

Hydrous sulphate.	*Anhydrous sulphate.*	*CeO_2.*
1.2385	.9875	.5977
1.2730	1.0148	.6138
1.2030	.9590	.5794
1.5420	1.2295	.7430
.9642	.7685	.4642
1.3260	1.0571	.6389
1.1429	.9112	.5512
.9072	.7232	.4372
1.2114	.9658	.5840
1.2411	.9894	.5984

Hence the following percentages:

	H_2O.	*CeO_2 in hydrate.*	*CeO_2 in $Ce_2(SO_4)_3$.*
	20.267	48.259	60.526
	20.282	48.216	60.484
	20.282	48.162	60.417
	20.265	48.184	60.431
	20.296	48.143	60.403
	20.279	48.182	60.438
	20.273	48.280	60.557
	20.282	48.192	60.453
	20.274	48.208	60.468
	20.280	48.215	60.481
Mean,	20.278, ± .0019	48.204, ± .0095	60.466, ± .0103
Hence Ce =	139.21	139.39	139.45

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[1] Bull. Soc. Chim. (3), 17, 679. 1897. Also Ann. Chim. Phys. (8), 9, 349. Moissan, Compt. Rend., 124, 1233, also describes white ceric oxide.

Kölle[1] studied anhydrous cerium sulphate, which he reduced by calcination to CeO_2. His figures are as follows:

$Ce_2(SO_4)_3$.	CeO_2.	Per cent. CeO_2.
1.84760	1.11648	60.429
1.16074	.70078	60.331
1.53599	.92722	60.366
.97196	.58661	60.353
1.40374	.84760	60.384
1.75492	1.05956	60.377
1.53784	.92853	60.379
1.64233	.99150	60.372
		Mean, 60.374, ± .0067

Hence Ce = 138.80, an unusually low and improbable value.

The very careful investigation by Brauner and Batěk[2] involved the study of two cerium salts, the sulphate and the oxalate. The sulphate was dehydrated at 440°, and then calcined to oxide. The figures given in the next table represent a number of different samples of the salt, but I have here combined the data into one series:

$Ce_2(SO_4)_3$.	CeO_2.	Per cent. CeO_2.
1.5074	.9130	60.568
1.7979	1.08945	60.596
1.5937	.9665	60.645
2.6240	1.5895	60.575
1.2161	.7370	60.604
1.5074	.9130	60.568
1.2192	.7386	60.581
		Mean, 60.591, ± .0070

Reduced to a vacuum basis this becomes 60.584.

Hence Ce = 140.30.

Combining this series with others, we have

Wolf	60.366, ± .0308
Wing	60.244, ± .0308
Brauner	60.5729, ± .0021
Wyrouboff and Verneuil	60.466, ± .0103
Kölle	60.374, ± .0067
Brauner and Batěk	60.584, ± .0070
General mean	60.5528, ± .0019

Wing's mean is here arbitrarily given equal weight with that of Wolf, but both series practically vanish.

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[1] Beiträge zur Kenntnis des Cers. Inaugural Dissertation, Zürich, 1898.

[2] Zeitsch. anorg. Chem., 34, 103. 1903.

Cerium oxalate contains water, in proportions which are not absolutely constant; at least not constant enough for good atomic weight determinations. In Buehrig's analyses the water was estimated, but it is doubtful whether the estimations can be made with adequate sharpness. Cerium oxalate, therefore, is best handled by the method of Stolba and Gibbs; which consists in determining the amount of ceric oxide left after calcination; and in another portion of the same sample, estimating the radicle C_2O_3 by titration with potassium permanganate. From the ratio $3C_2O_3 : 2CeO_2$ the atomic weight of cerium can be calculated.

This method was followed by Brauner, in a single determination which has already been cited. It was also adopted by Brauner and Batěk, who give five sets of determinations, with vacuum weights, as follows. I cite now only the percentages of CeO_2 and C_2O_3, as computed from the weighings, together with the required ratio:

Sample I.

Per cent. CeO_2.	*Per cent. C_2O_3.*	*Ratio.*
46.949	29.423	150.503
46.939	29.391	159.685
46.913	29.393	159.664
46.920	29.442	159.398
	29.422	159.507
Mean, 46.930	29.397	159.642
	29.406	159.593
	29.459	159.306
	29.414	159.540
	29.459	159.306
	29.459	159.306
	29.436	159.431

The ratio here is computed from the individual figures for C_2O_3 and the mean for CeO_2.

Sample II.

Per cent. CeO_2.	*Per cent. C_2O_3.*	*Ratio.*
47.197	29.601	159.353
47.089	29.564	159.552
47.225	29.559	159.579
Mean, 47.170		

Samples III, IV, V.

	Per cent. CeO_2.	*Per cent. C_2O_3.*	*Ratio.*
III	47.161 47.160	29.512	159.718
IV	46.926 46.922	29.391	159.654
V	47.039	29.531	159.287

The mean of the 18 values for the ratio is 159.501, ± .0285. Hence Ce = 140.28.

In a later memoir, Brauner[1] gives additional analyses of cerium oxalate and sulphate. The oxalate figures are as follows:

Per cent. CeO_2.	*Per cent.* C_2O_3.	*Ratio.*
47.070	29.548	159.304
47.067	29.544	159.325
47.077	29.478	159.682
47.070	29.486	159.638
47.074		
		Mean, 159.487, ± .0677
Mean, 47.071		

Hence Ce = 140.26.

If we give to Brauner's earliest, single determination, the weight of one experiment in the Brauner and Batěk series, the values for this ratio combine thus:

Brauner, early	159.074, ± .0990
Brauner and Batěk	159.501, ± .0285
Brauner, latest	159.487, ± .0677
General mean	159.471, ± .0254

In the memoir last cited Brauner also gives a series of determinations based on the calcination of the octohydrated cerium sulphate. In the subjoined table I include two separate determinations given near the beginning of the paper. All weights were reduced to a vacuum standard:

Sulphate.	CeO_2.	*Per cent.* CeO_2.
1.98989	.96175	48.332
1.99154	.96251	48.330
2.33919	1.13027	48.319
1.95882	.94679	48.335
1.20961	.58453	48.324
1.54162	.74504	48.329
1.67748	.81074	48.331
2.02736	.97985	48.331
		Mean, 48.329, ± .0011

Brauner rejects the third determination, a procedure which changes the mean to 48.330, ± .0009. We may adopt the latter and combine it with another series, thus:

Wyrouboff and Verneuil	48.204, ± .0095
Brauner	48.330, ± .0009
General mean	48.329, ± .0009

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[1] Zeitsch. anorg. Chem., 34, 207. 1903.

From Brauner's series Ce = 140.26.

Brauner discusses at some length the color of ceric oxide, and describes it as nearly white, but with a faint tinge of brownish-yellow.

The ratios, good and bad, for cerium now are—

(1). $Ce_2(SO_4)_3 : 3BaSO_4 :: 100 : 122.958$, ± .1139
(2). $3BaSO_4 : 2CeO_2 :: 100 : 49.360$, ± .035
(3). $3BaCl_2 : Ce_2(SO_4)_3 :: 100 : 91.625$, ± .016
(4). $3AgCl : CeO_2 :: 100 : 40.469$, ± .0415
(5). Percentage CeO_2 from $Ce_2(SO_4)_3$, 60.5528, ± .0019
(6). Percentage CeO_2 in $Ce_2(SO_4)_3.8H_2O$, 48.329, ± .0009
(7). Percentage H_2O in $Ce_2(SO_4)_3.8H_2O$, 20.278, ± .0019
(8). Percentage CeO_2 from $Ce_2(C_2O_4)_3$, 63.4316, ± .0032
(9). Percentage CO_2 from $Ce_2(C_2O_4)$, 48.2546, ± .001
(10). $3Ag : CeCl_3 :: 100 : 76.167$, ± .0065
(11). $3C_2O_3 : 2CeO_2 :: 100 : 159.471$, ± .0254

The antecedent atomic weights are—

Ag = 107.880, ± .00029
Cl = 35.4584, ± .0002
S = 32.0667, ± .00075
C = 12.0038, ± .0002
Ba = 137.363, ± .0025
H = 1.00779, ± .00001

Hence,

From ratio 7 .... Ce = 139.210, ± .0276
" " 5 .... 140.079, ± .0100
" " 10 .... 140.132, ± .0105
" " 11 .... 140.247, ± .0275
" " 6 .... 140.251, ± .0045
" " 1 .... 140.663, ± .2638
" " 2 .... 140.832, ± .1225
" " 3 .... 141.330, ± .0500
" " 8 .... 141.483, ± .0176
" " 9 .... 141.559, ± .0064
" " 4 .... 142.023, ± .1785

General mean, Ce = 140.583, ± .0032

This mean appears to be, on chemical grounds, too high, because of the evident overweighting of ratio 9. If we reject all values in excess of 141, the general mean of the remaining seven values is

Ce = 140.197, ± .0038

This represents mainly the work of Brauner and Robinson.

PRASEODYMIUM.

In 1885 Auer von Welsbach[1] succeeded in proving that the old "didymia" was a mixture of two earths, one yielding green, and the other rose-colored salts. To the corresponding metals, praseodymium and neodymium, he assigned the atomic weights Pr = 143.6 and Nd = 140.8, respectively, values which were curiously reversed, either in printing or by the error of a copyist. The true values are now known to be nearly Pr = 141 and Nd = 144, in round numbers. For "didymium," many discordant atomic weight determinations had been made, which now have only historical interest, and need, therefore, no consideration now. They are thoroughly summed up in the first edition of this work, which was published about three years before Welsbach's brilliant discovery.

In 1898 Brauner[2] published a preliminary notice upon praseodymium. Thirteen determinations of the atomic weight, by both the sulphate and the oxalate methods, gave values from 140.84 to 141.19, in mean 140.95, but the details of the work were not given. These early data, therefore, are not now available for discussion. The first fully described series of determinations was made by Jones,[3] who published his results a little later than Brauner.

Jones effected the synthesis of praseodymium sulphate from the oxide, the latter having been first reduced from Pr_4O_7 to Pr_2O_3 by heating in hydrogen. The material, after purification, still contained minute traces of lanthanum and neodymium, but these were too small to seriously affect the atomic weight determination. The weights and percentages appear in the following table:

Pr_2O_3.	$Pr_2(SO_4)_3$.	Per cent. Pr_2O_3.
.5250	.9085	57.789
.6436	1.1135	57.800
.7967	1.3788	57.782
.7522	1.3018	57.782
.7788	1.3473	57.805
.6458	1.1172	57.805
.6972	1.2062	57.801
.7204	1.2464	57.798
.8665	1.4990	57.805
.6717	1.1624	57.796
.7439	1.2873	57.788
.6487	1.1224	57.796
		Mean, 57.796, ± .0016

Hence Pr = 140.17.

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[1] Monatsh. Chem., 6, 477. 1885.

[2] Proc. Chem. Soc., 14. 70. 1898.

[3] Amer. Chem. Journ., 20, 345. 1898.

C. von Schéele,[1] also in 1898, gave three series of determinations. First, by the synthesis of praseodymium sulphate, which, however, contained a little lanthanum sulphate, as follows:

Pr_2O_3.	*$Pr_2(SO_4)_3$.*	*Per cent. Pr_2O_3.*
1.6738	2.8926	57.865
1.4327	2.4788	57.798
1.1105	1.9221	57.775
1.0072	1.7431	57.782
		Mean, 57.805, ± .0139

Hence Pr = 140.53.

The second series by von Schéele is somewhat obscure, being a combination of the oxalate and sulphate methods. A part of the oxalate was converted into sulphate, and in another part the C_2O_3 radicle was determined by titration with permanganate solution. The proportion of oxide in sulphate was also found. I give below the several percentages, and also the ratio $3C_2O_3 : Pr_2(SO_4)_3$:

Sulphate from oxalate.	*Per cent. C_2O_3.*	*Pr_2O_3 in sulphate.*	*Ratio.*
81.682	31.07	57.73	
81.638	31.06	57.71	
	31.11		
Mean, 81.665			
	Mean, 31.08		262.515
77.828	29.60	57.77	262.845
	29.58		
	29.64		
	Mean, 29.61	57.737	262.680
		± .0117	± .1113
		Pr = 140.07	139.62

These two series have small claims to consideration, and may be regarded as preliminary. The third series, by the sulphate method, is far better:

Pr_2O_3.	*$Pr_2(SO_4)_3$.*	*Per cent. Pr_2O_3.*
.6872	1.1890	57.796
.7834	1.3550	57.815
.6510	1.1260	57.815
.7640	1.3216	57.809
.5183	.8967	57.801
		Mean, 57.807, ± .0026

Hence Pr = 140.54.

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[1] Zeitsch. anorg. Chem., 17, 310. 1898.

In 1901 Brauner[1] gave a preliminary notice of an investigation upon the atomic weight of praseodymium, but without details. He has since published his data in Abegg's Handbuch der anorganischen Chemie,[2] as follows:

First, the octohydrated sulphate was dehydrated at 500°, then calcined to oxide, and the latter finally analyzed iodometrically to determine the true proportions of Pr_2O_3. Percentage A in the next table is that of Pr_2O_3 in the hydrous sulphate, and B refers to the anhydrous salt:

$Pr_2(SO_4)_3.8H_2O$.	$Pr_2(SO_4)_3$.	Pr_2O_3.	*Per cent. A.*	*Per cent. B.*
1.29269	1.03242	.59747	46.219	57.871
1.27990	1.02193	.59137	46.204	57.868
			Mean, 46.211,	57.8695
			± .0050	
			Pr = 141.09	

Secondly, four samples of praseodymium oxalate were analyzed by the method already described under cerium and lanthanum. I give the ratio computed from the percentages, in the form $3C_2O_3 : Pr_2O_3 :: 100 : x$:

Sample.	*Per cent.* Pr_2O_3.	*Per cent.* C_2O_3.	*Ratio.*	*Weight.*
A	45.183	29.581	152.743	2
		29.581		
B	45.142	29.499	152.671	4
	45.098	29.593		
	45.095	29.532		
	45.102	29.511		
	45.063	.....		
C	45.032	29.503	152.840	2
	45.123	29.483		
D	45 136	29.565	152.677	2
		29.562		

Each value for the ratio is here weighted according to the number of the C_2O_3 determinations. The mean of all, thus weighted, is 152.720, ±.0148.

Hence Pr = 140.95.

Finally, Brauner affected the synthesis of the anhydrous sulphate. Praseodymium oxalate was calcined, and the composition of the oxide produced was ascertained by iodometric titration. It was then converted into sulphate, with the correction, described under lanthanum, for excess of sulphuric acid. The corrected data are these:

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[1] Proc. Chem. Soc., 17, 65.

[2] Bd. 3, Abth. 1, pp. 263-265.

Pr_2O_3.	$Pr_2(SO_4)_3$.	Per cent. Pr_2O_3.
.73359	1.26782	57.863
.64871	1.12059	57.890
.74103	1.28051	57.870
.72894	1.25972	57.865
.36559	.63350	57.867
.82769	1.43024	57.871

In his first series Brauner found, for the same percentage, 57.871 and 57.868. Including these, the eight determinations, taken as one series, give a mean of 57.871, ± .0021.

Hence Pr = 140.97.

Combining this mean with other means for the same ratio we have—

Jones	57.796,	± .0016
Schéele, first	57.805,	± .0139
" second	57.737,	± .0117
" third	57.807,	± .0026
Brauner	57.871,	± .0021
General mean	57.8194,	± .0010

Three other determinations by Welsbach[1] were published in 1903. He used the sulphate method and found Pr = 140.64, 140.50 and 140.56 when O = 16. Unfortunately, he gave no weighings, nor did he state what value he used for the atomic weight of sulphur. His figures, therefore, are unavailable for discussion now.

There are also three determinations by Feit and Przibylla,[2] who used the peculiar volumetric method already described under lanthanum. Their results are as follows:

Pr_2O_3.	O.	Atomic weight.
.54010	.07879	140.518
.53420	.07789	140.601
.50054	.07302	140.516
		Mean, 140.545, ± .0189

The ratios for praseodymium now are—

(1). $Pr_2(SO_4)_3.8H_2O : Pr_2O_3 : : 100 : 46.211$, ± .0050

(2). $Pr_2(SO_4)_3 : Pr_2O_3 : : 100 : 57.8194$, ± .0010

(3). $3C_2O_3 : Pr_2(SO_4)_3 : : 100 : 262.680$, ± .1113

(4). $3C_2O_3 : Pr_2O_3 : : 100 : 152.720$, ± .0148

(5). $O : Pr : : 16 : 140.545$, ± .0189

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[1] Sitzungsb. Wien. Akad., 112, 1037. 1903.

[2] Zeitsch. anorg. Chem., 50, 258. 1906.

To reduce these ratios we have S=32.0667, ±.00075; C=12.0038, ± .0002; and H=1.00779, ±.00001.

Hence,

From ratio 3	Pr = 139.623, ± .1200
" " 5	140.545, ± .0189
" " 2	140.628, ± .0035
" " 4	140.954, ± .0160
" " 1	141.090, ± .0236
General mean,	Pr = 140.619, ± .0033

Brauner's determinations make Pr=141, very nearly, and must be taken into consideration in criticizing the foregoing combination. His value may be nearer the truth, but the work of Jones and of Scheele cannot yet be rejected. There is still an uncertainty of half a unit in the atomic weight of praseodymium. The later determinations by Welsbach are in harmony with the general mean of all the other estimations.

NEODYMIUM.

Our knowledge of the atomic weight of neodymium is almost entirely based upon a study of the sulphate. Welsbach's first determination was cited under praseodymium, and needs no farther consideration. So also Brauner's[1] first, preliminary figure, Nd=143.63, given without analytical details, may be dismissed here. The first important series of determinations is that by Jones,[2] published in 1898. The synthesis of the sulphate was effected in the usual way, with the following results:

Nd_2O_3	$Nd_2(SO_4)_3$.	Per cent. Nd_2O_3.
.8910	1.5296	58.251
.7880	1.3530	58.241
.9034	1.5509	58.250
.7668	1.3166	58.241
.8908	1.5296	58.237
.8848	1.5194	58.234
.8681	1.4903	58.250
.8216	1.4103	58.257
.8531	1.4646	58.248
.8711	1.4957	58.240
.8932	1.5332	58.257
.8893	1.5268	58.246
		Mean, 58.246, ± .0015

Hence Nd=143.54.

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[1] Proc. Chem. Soc., 14, 72. 1898.

[2] Amer. Chem. Journ., 20, 345. 1898. See additional note in Zeitsch. anorg. Chem., 19, 339. 1899.

One determination, by Boudouard,[1] was made by calcination of the sulphate. 2.758 grammes of $Nd_2(SO_4)_3$ gave 1.605 of Nd_2O_3, or 58.194 per cent. Hence Nd = 143.18.

Brill,[2] in 1905, made two analyses of neodymium sulphate, with the aid of the microbalance. His percentages of Nd_2O_3 are

58.000
58.180

Mean 58.090, ± .0600

Hence Nd = 142.46.

In Abegg's Handbuch, Brauner[3] gives the details of a synthesis of neodymium sulphate, with corrections for excess of acid. 0.93788 gramme Nd_2O_3 gave 1.60873 $Nd_2(SO_4)_3$. Per cent. Nd_2O_3, 58.299, whence Nd = 143.90.

Holmberg,[4] who employed the usual synthetic method, found no serious difficulty in obtaining a neutral sulphate. In his series of determinations, therefore, a correction for excess of sulphuric acid was not needed. His six syntheses are as follows:

Nd_2O_3	$Nd_2(SO_4)_3$.	*Per cent.* Nd_2O_3.
.9692	1.6618	58.322
.6584	1.1287	58.333
1.0292	1.7643	58.335
1.0118	1.7346	58.330
.5518	.9462	58.317
.5345	.9164	58.326
		Mean, 58.327, ± .0019

Hence Nd = 144.10.

In combining these various determinations of the oxide-sulphate ratio, the single experiments by Boudouard and Brauner are each given the probable error of one experiment in Jones' series:

Jones	58.246,	± .0015
Boudouard	58.194,	± .0067
Brill	58.090,	± .0600
Brauner	58.299,	± .0067
Holmberg	58.327,	± .0019
General mean	58.2831,	± .0011

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[1] Compt. Rend., 126, 900. 1898.
[2] Zeitsch. anorg. Chem., 47, 464. 1905.
[3] Abegg's Handbuch, Bd. 3, Abth. 1, p. 276. Preliminary note in Proc. Chem. Soc., 17, 66. 1901.
[4] Zeitsch. anorg. Chem., 53, 124. 1907.

In this combination only the series by Jones and Holmberg are important. The other figures count for little or nothing. Welsbach's[1] determinations, also by the sulphate method, cannot be safely utilized, for lack of details. He found Nd = 144.55, 144.52 and 144.57; in mean, 144.547, ± .0103.

Feit and Przibylla[2] give the following data for neodymium, obtained by their volumetric method:

Nd_2O_3.	*O.*	*Atomic weight.*
.5380	.07661	144.542
.5388	.07675	144.485
.5358	.07632	144.491
.5265	.07497	144.547
		Mean, 144.516, ± .0111

Calculating with S = 32.0667, ± .00075, we now have two distinct values for neodymium, as follows:

From the sulphate......	Nd = 143.752, ± .0057
From the oxide................	144.516, ± .0111
General mean,	Nd = 143.910, ± .0051

If we assume that Welsbach's latest determinations were based upon essentially the same value for sulphur as that given above, his mean, 144.547, ± .0103, may be combined with the other values. In that case the general mean becomes Nd = 144.037. The round number, Nd = 144, is as near the truth as the present evidence will permit us to approach. It is possibly some tenths of a unit too low.

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[1] Sitzungsb. Wien. Akad., 112, 1037. 1903.

[2] Zeitsch. anorg. Chem., 50, 259. 1906. For the process, see under lanthanum.

SAMARIUM.

According to Marignac,[1] the atomic weight of samarium is 149.4. He gives, however, one analysis of the octohydrated sulphate, as follows: 1.8515 grammes gave 0.365 of water, and on calcination lost 0.607 SO_3. Hence the percentage of Sa_2O_3 in the hydrous salt is 47.502, and in the anhydrous sulphate 59.166. From these data Sa = 149.87 and 150.02. Brauner,[2] with purer material, made Sa = 150.7, but gave no details. The first regular series of atomic weight determinations was by Cleve,[3] who effected the synthesis of the sulphate from the oxide. Data as follows:

Sa_2O_3.	$Sa_2(SO_4)_3$.	Per cent. Sa_2O_3.
1.6735	2.8278	59.180
1.9706	3.3301	59.175
1.1122	1.8787	59.201
1.0634	1.7966	59.190
.8547	1.4440	59.190
.7447	1.2583	59.183
		Mean, 59.1865, ± .0025

Hence Sa = 150.17.

Another set of determinations by Bettendorff,[4] after the same general method, gave as follows:

Sa_2O_3.	$Sa_2(SO_4)_3$.	Per cent. Sa_2O_3.
1.0467	1.7675	59.219
1.0555	1.7818	59.238
1.0195	1.7210	59.225
		Mean, 59.227, ± .0038

Hence Sa = 150.46.

In a single analysis of the hydrous sulphate, Brauner[5] obtained the following figures: 1.36567 grammes $Sa_2(SO_4)_3.8H_2O$ gave 1.09770 $Sa_2(SO_4)_3$ and 0.65046 Sa_2O_3. Per cent. Sa_2O_3 in hydrate, 47.629; in anhydrous salt, 59.257. Hence Sa = 150.76 and 150.67.

Käppel,[6] cited by Muthmann and Weiss, from 4.12673 grammes $Sa_2(SO_4)_3$ obtained 2.45028 Sa_2O_3, or 59.376 per cent. Hence Sa = 151.59.

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[1] Arch. Sci. Phys. Nat. (3), 3, 435. 1880. Oeuvres Complètes, 2, 709.

[2] Journ. Chem. Soc., 43, 287. 1883.

[3] Journ. Chem. Soc., 43, 362. 1883.

[4] Ann. Chem. Pharm., 263, 164. 1891.

[5] Abegg's Handbuch, Bd. 3, Abth. 1, p. 284

[6] Liebig's Annalen, 331, 16. 1904.

Brill's[1] analyses of samarium sulphate, with the aid of the microbalance, need not be considered, for his two experiments, as recorded, are widely discordant. The most thorough investigation is that by Urbain and Lacombe,[2] whose material was scrupulously freed from other rare earths, an impurity to be discussed more fully a little later. The samarium preparations of Urbain and Lacombe were derived from different sources, gadolinite, monazite sand, etc., and the octohydrated sulphate was analyzed by dehydration and calcination in the ordinary way. In the next table I give their weights, and also three percentage columns, as follows: A, percentage of $Sa_2(SO_4)_3$, in $Sa_2(SO_4)_3.8H_2O$. B, Sa_2O_3 in the hydrous sulphate. C, Sa_2O_3 in the anhydrous sulphate. The different samples of material are indicated by brackets:

$Sa_2(SO_4)_3.8H_2O$.	$Sa_2(SO_4)_3$.	Sa_2O_3.	*A.*	*B.*	*C.*
1.0499	.8435	.4996	80.341	47.585	59.229
1.2898	1.0362	.6137	80.338	47.580	59.225
1.3650	1.0969	.6497	80.359	47.597	59.230
1.7992	1.4453	.8557	80.330	47.560	59.206
1.8636	1.4977	.8873	80.366	47.605	59.244
.8407	.6749	.4001	80.277	47.591	59.283
2.5107	2.0172	1.1948	80.344	47.588	59.228
3.1171	2.5045	1.4840	80.347	47.608	59.253
2.9425	2.3635	1.4004	80.323	47.592	59.251
3.2200	2.5872	1.5324	80.348	47.590	59.230
2.8382	2.2804	1.3508	80.347	47.594	59.237
		Mean,	80.338,	47.590,	59.238,
			± .0048	± .0026	± .0040

From A, Sa = 150.34.
From B, Sa = 150.49.
From C, Sa = 150.54.

Two of the ratios given by Urbain and Lacombe's experiments may now be combined with former series of determinations. First, for the percentage of Sa_2O_3 in the anhydrous sulphate, giving the single determinations of Marignac, Brauner and Käppel the weight of one experiment in Urbain and Lacombe's series:

Marignac	59.166,	± .0199
Cleve	59.1865,	± .0025
Bettendorff	59.227,	± .0038
Brauner	59.257,	± .0199
Käppel	59.376,	± .0199
Urbain and Lacombe	59.238,	± .0040
General mean	59.2074,	± .0018

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[1] Zeitsch. anorg. Chem., 47, 461. 1905.

[2] Compt. Rend., 138, 1166. 1904.

Secondly, for the percentage of Sa_2O_3 in the hydrous sulphate, giving, arbitrarily, the single determinations of Marignac and Brauner the same weight as in the anhydrous series:

Marignac	47.502, ± .0199
Brauner	47.629, ± .0199
Urbain and Lacombe	47.590, ± .0026
General mean	47.589, ± .0025

The value of these combinations is perhaps questionable. The earlier work on the atomic weight of samarium is affected by the discovery of europium, which was made by Demarçay.[1] According to this chemist, the original samaria contained admixtures of europia, which tended to raise its apparent molecular weight. For samarium itself, by the sulphate method, he found Sa = 147.2 to 148, and for europium, Eu = 151. The material studied by Urbain and Lacombe, however, was free from europium, and still gave a higher percentage of oxide in sulphate than the substances examined by the earlier investigators. Their material, therefore, was either free from europium, or else contained compensating impurities. At all events, the general means are close to Urbain and Lacombe's figures, and may be allowed to stand unchanged.

Still another method for measuring the atomic weight of samarium has been proposed by Matignon,[2] who found that the normal sulphate, heated to between 500° and 1000°, yielded a stable basic salt, Sa_2SO_6. In one determination, 0.7325 gramme $Sa_2(SO_4)_3$ gave 0.5335 of Sa_2SO_6. Hence Sa = 150.67.

There are also the determinations by Feit and Przibylla,[3] with their special volumetric method, as follows:

Sa_2O_3.	O.	*Atomic weight.*
.5576	.07668	150.522
.5576	.07670	150.477
.5583	.07684	150.378
.5633	.07747	150.514
		Mean, 150.473, ± .0221

In all, there are five ratios relative to the atomic weight of samarium:

(1). $Sa_2(SO_4)_3.8H_2O : Sa_2(SO_4)_3 :: 100 : 80.338$, ± .0048
(2). $Sa_2(SO_4)_3.8H_2O : Sa_2O_3 :: 100 : 47.589$, ± .0025
(3). $Sa_2(SO_4)_3 : Sa_2O_3 :: 100 : 59.2074$, ± .0018
(4). $Sa_2(SO_4)_3 : Sa_2SO_6 :: 100 : 72.883$
(5). $O : Sa :: 16 : 150.473$, ± .0221

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[1] Compt. Rend., 122, 728. 1900. *Ibid.*, 130, 1185, 1469. 1900. *Ibid.*, 132, 1484.
[2] Compt. Rend., 141, 1230.
[3] Zeitsch. anorg. Chem., 50, 259. 1906.

To reduce these we have S = 32.0667, ± .00075, and H = 1.00779, ± .0001. Hence, giving to the value from ratio 4 the arbitrary weight represented by ± .075—

From ratio 3	Sa = 150.316, ± .0095
" " 1	150.344, ± .0740
" " 5	150.473, ± .0221
" " 2	150.483, ± .0124
" " 4	150.666, ± .0750
General mean,	Sa = 150.390, ± .0071

The average from the determinations by Urbain and Lacombe is Sa = 150.46. The rounded-off figure 150.4 is probably near the truth, with an actual uncertainty as large as 0.1.

EUROPIUM.

Demarçay,[1] the discoverer of europium, found for its atomic weight the approximate number 151. The first detailed determinations, however, were those of Urbain and Lacombe,[2] who analyzed the octohydrated sulphate. I give their weights, and three percentage columns, as follows: A, $Eu_2(SO_4)_3$ in $Eu_2(SO_4)_3.8H_2O$. B, Eu_2O_3 in $Eu_2(SO_4)_3.8H_2O$. C, Eu_2O_3 in $Eu_2(SO_4)_3$.

$Eu_2(SO_4)_3.8H_2O$.	$Eu_2(SO_4)_3$.	Eu_2O_3.	*A.*	*B.*	*C.*
1.7787	1.4303	.8500	80.413	47.788	59.428
2.4785	1.9935	1.1848	80.432	47.803	59.433
2.4177	1.9449	1.1554	80.444	47.789	59.407
2.4831	1.9968	1.1870	80.416	47.803	59.445
2.2988	1.8488	1.0990	80.425	47.807	59.444
		Mean,	80.426, ± .0038	47.798, ± .0027	59.431, ± .0047
		Hence Eu =	151.99	151.95	152.01

Jantsch[3] determined the atomic weight of europium by calcination of the hydrous sulphate:

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[1] Compt. Rend., 132, 1484. 1900.

[2] Compt. Rend., 138, 627. 1904.

[3] Compt. Rend., 146, 473. 1908.

Sulphate.	Eu_2O_3.	*Per cent.* Eu_2O_3.
1.3501	.6455	47.811
1.5054	.7197	47.808
1.5213	.7274	47.814
1.2881	.6159	47.815
		Mean, 47.812, ± .0010

Hence Eu = 152.05.

Urbain and Lacombe, for the same ratio, found 47.798, ± .0027. The two series combined give 47.810, ± .00094.

Feit and Przibylla[1] have also applied their volumetric method to the determination of this atomic weight, with the following results:

Eu_2O_3.	*O.*	*Atomic weight.*
.3961	.05385	152.535
.4096	.05566	152.615
.4115	.05594	152.546
		Mean, 152.565, ± .0170

The four ratios for Eu now are—

(1). $Eu_2(SO_4)_3.8H_2O : Eu_2(SO_4)_3 :: 100 : 80.426$, ± .0038
(2). $Eu_2(SO_4)_3.8H_2O : Eu_2O_3 :: 100 : 47.810$, ± .00094
(3). $Eu_2(SO_4)_3 : Eu_2O_3 :: 100 : 59.431$, ± .0047
(4). $O : Eu :: 16 : 152.565$, ± .0170

Reducing these ratios with S = 32.0667, ± .00075 and H = 1.00779, ± .00001 we have —

From ratio 1 ....Eu = 151.991, ± .0542
" " 3 ....152.012, ± .0247
" " 2 ....152.035, ± .0048
" " 4 ....152.265, ± .0170

General mean, Eu = 152.072, ± .0045

In round numbers the atomic weight of europium is 152.

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[1] Zeitsch. anorg. Chem., 50, 260. 1906.

GADOLINIUM.

Marignac,[1] the discoverer of gadolinium, assigned to its oxide the "equivalent" 120.5, whence Gd = 156.75. Boisbaudran[2] found Gd = 155.33, 156.06, 155.76 and 156.12, with preference for the last figure. Cleve, quoted by Boisbaudran, found Gd = 154.15, 155.28, 155.1 and 154.77. For these determinations there are no details, and all, probably, are referred to $SO_3 = 80$.

The first chemist to publish his determinations with individual data was Bettendorff.[3] He effected the synthesis of the sulphate from the oxide, and his weights were as follows. The percentage of Gd_2O_3 in $Gd_2(SO_4)_3$ is given in the third column:

Gd_2O_3.	$Gd_2(SO_4)_3$.	Per cent. Gd_2O_3.
1.0682	1.7779	60.082
1.0580	1.7611	60.076
1.0796	1.7969	60.081
		Mean, 60.080, ± .0013

Hence Gd = 156.75.

Benedicks'[4] series of determinations were also by the synthetic process, as follows:

Gd_2O_3.	$Gd_2(SO_4)_3$.	Per cent. Gd_2O_3.
.4308	.7171	60.075
.5675	.9451	60.047
.5726	.9534	60.059
.6785	1.1301	60.039
.7399	1.2329	60.013
1.3253	2.2063	60.069
		Mean, 60.050, ± .0020

Hence Gd = 156.52.

The two determinations by Marc[5] are unimportant, but cannot be overlooked. The data are—

Gd_2O_3.	$Gd_2(SO_4)_3$.	Per cent. Gd_2O_3.
.2201	.3666	60.014
.2444	.4070	60.049
		Mean, 60.032, ± .0120

Hence Gd = 156.39.

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[1] Oeuvres Complètes, 2, 704

[2] Compt. Rend., 111, 409. 1890.

[3] Ann. Chem. Pharm., 270, 376. 1892.

[4] Zeitsch. anorg. Chem., 22, 393. 1899

[5] Zeitsch. anorg. Chem., 38, 121. 1904.

Brauner,[1] in a single experiment, found 0.88884 Gd_2O_3 = 1.48257 $Gd_2(SO_4)_3$ = 1.83903 $Gd_2(SO_4)_3.8H_2O$. Per cent. Gd_2O_3 in hydrous sulphate, 48.332; in anhydrous salt, 59.951. Hence Gd = 155.725 and 155.78.

The material studied by Brauner was received from Cleve, and was not perfectly pure. The atomic weight found is too low. In combining the figures for the percentage of Gd_2O_3 in $Gd_2(SO_4)_3$, Brauner's determination may be given equal weight with that of Marc. We have then—

Bettendorff	60.080, ± .0013
Benedicks	60.050, ± .0020
Marc	60.032, ± .0120
Brauner	59.951, ± .0120
General mean	60.070, ± .0011

The purest gadolinium preparations were probably those studied by Urbain,[2] who calcined the octohydrated sulphate to oxide. Two series of determinations are given, representing different groups of fractions obtained in the purification of his material. The data are as follows:

Series I.

$Gd_2(SO_4)_3.8H_2O$.	Gd_2O_3.	Per cent. Gd_2O_3
1.9256	.9350	48.557
1.9749	.9589	48.555
1.9975	.9698	48.551
2.1083	1.0231	48.528
1.8993	.9214	48.514
2.2065	1.0707	48.525
1.9535	.9479	48.524
2.2008	1.0685	48.551
2.2482	1.0914	48.546
2.1932	1.0646	48.541
		Mean, 48.539, ± .0033

Series II.

$Gd_2(SO_4)_3.8H_2O$.	Gd_2O_3.	Per cent. Gd_2O_3.
2.0551	.9974	48.534
2.1555	1.0469	48.570
2.2277	1.0867	48.512
2.2559	1.0946	48.529
2.2523	1.0939	48.569
		Mean, 48.543, ± .0077

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[1] Abegg's Handbuch, Bd. 3, Abth. 1, p. 304.

[2] Compt. Rend., 140, 583. 1905.

The weighted mean of both series is nearly 48.540, ± .0030. Hence Gd = 157.24. Brauner's single determination may be neglected.

There are also two determinations made by Feit and Przibylla[1] with their volumetric method:

Gd_2O_3.	*O.*	*Atomic weight.*
.3852	.05097	157.377
.3956	.05234	157.398
		Mean, 157.388, ± .0067

The three ratios for gadolinium are—

(1). $Gd_2(SO_4)_3.8H_2O:Gd_2O_3::100:48.540$, ± .0030
(2). $Gd_2(SO_4)_3:Gd_2O_3::100:60.070$, ± .0011
(3). O:Gd::16:157.388, ± .0067

Reducing these ratios with S = 32.0667, ± .00075, and H = 1.00774, ± .0001, we have—

From ratio 2 ....Gd = 156.677, ± .0117
" " 1 ....157.258, ± .0154
" " 3 ....157.388, ± .0067

General mean, Gd = 157.218, ± .0055

This final value is near Urbain's determination, which, upon chemical grounds, is probably the best.

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[1] Zeitsch. anorg. Chem., 50, 260. 1906.

TERBIUM.

The older determinations of atomic weight, made upon terbium preparations of doubtful character, may well be ignored. Boisbaudran[1] has published two estimates of this constant. First, for two preparations, one with a lighter and one with a darker earth, he gives Tb=161.4 and 163.1. In his second paper he makes Tb=159.01 to 159.95; probably with $SO_3=80$. According to Feit[2] Tb=158.6. Emma Potratz,[3] by various methods, found Tb=154, approximately. For all of these determinations the essential details are lacking.

The series of determinations by Urbain[4] is more satisfactory. The octohydrated sulphate was converted into the anhydrous salt by careful heating, with the following results:

$Tb_2(SO_4)_3.8H_2O$.	$Tb_2(SO_4)_3$.	Per cent. H_2O.
2.0407	1.6489	19.199
1.9626	1.5859	19.194
2.2580	1.8245	19.198
2.2385	1.8087	19.201
2.0037	1.6190	19.200
		Mean, 19.198, ± .0008

Hence Tb=159.201, ±.0130.

DYSPROSIUM.

The atomic weight of dysprosium has been well determined by Urbain and Demenitroux.[5] They reduced the octohydrated sulphate to oxide, by calcination, with the following results, taking all their data as one series:

$Dy_2(SO_4)_3.8H_2O$.	Dy_2O_3.	Per cent. Dy_2O_3.
1.6966	.8359	49.269
2.0926	1.0301	49.226
1.8415	.9069	49.248
1.5519	.7649	49.288
2.4955	1.2296	49.273
1.8130	.8927	49.238

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[1] Compt. Rend., 102, 396, and 111, 474. 1886-1890.

[2] Zeitsch. anorg. Chem., 43, 280. 1905.

[3] Chem. News, 92, 3. 1905.

[4] Compt. Rend., 142, 957. Preliminary papers by Urbain are in C. R. 141, 521; *ibid.*, 142, 727. Bull. Soc. Chim. (3), 33, 403. See also Journ. Chim. Phys., 4, 321.

[5] Compt. Rend., 143, 598. 1906.

1.8817	.9271	49.269
1.1164	.5500	49.266
1.7308	.8528	49.272
2.6038	1.2820	49.236
1.6942	.8346	49.262
2.1776	1.0726	49.256
		Mean, 49.259, ± .0036

Hence Dy = 162.550, ± .0190.

ERBIUM, HOLMIUM, THULIUM.

Since the earth which was formerly regarded as the oxide of erbium is now known to be a mixture of two or three different oxides, the older determinations of its molecular weight have little more than historical interest. Nevertheless the work done by several investigators may properly be cited, if only for the sake of completeness.

First, Delafontaine's[1] early investigations may be considered. A sulphate, regarded as erbium sulphate, gave the following data. An oxalate was thrown down from it, which, upon ignition, gave oxide. The percentages in the fourth column refer to the anhydrous sulphate. In the last experiment water was not estimated, and I assume for its water the mean percentage of the four preceding experiments:

Sulphate.	*Er_2O_3.*	*H_2O.*	*Per cent. Er_2O_3.*
.827	.353	.177	54.308
1.0485	.4475	.226	54.407
.803	.3415	.171	54.035
1.232	.523	.264	54.028
1.1505	.495	. . .	54.760
			Mean, 54.308, ± .0915

Hence Er = 118.75.

Bahr and Bunsen[2] give a series of results, representing successive purifications of the earth which was studied. The final result, obtained by the conversion of oxide into sulphate, was as follows:

.7870 grm. oxide gave 1.2765 grm. sulphate. 61.653 per cent. oxide.

Hence Er = 169.59.

Hoeglund,[3] following the method of Bahr and Bunsen, gives these figures:

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[1] Ann. Chem. Pharm., 134, 108. 1865.

[2] Ann. Chem. Pharm., 137, 21. 1866.

[3] K. Svensk. Vet. Akad. Handlingar, Bd. 1, No. 6

Er_2O_3.	$Er_2(SO_4)_3$.	Per cent. Er_2O_3.
1.8760	3.0360	61.792
1.7990	2.9100	61.821
2.8410	4.5935	61.848
1.2850	2.0775	61.853
1.1300	1.827	61.850
.8475	1.370	61.861

Mean, 61.8375, ± .0063

Hence Er = 170.61.

According to Thalén,[1] spectroscopic evidence shows that the "erbia" studied by Hoeglund was largely ytterbia.

Humpidge and Burney[2] give data as follows:

1.9596 grm. $Er_2(SO_4)_3$ gave 1.2147 grm. Er_2O_3. 61.987 per cent.
1.9011 " 1.1781 " 61.965 "

Mean, 61.976, ± .0074

Hence Er = 171.75.

The foregoing data were all published before the composite nature of the supposed erbia was fully recognized. It will be seen, however, that three sets of results were fairly comparable, while Delafontaine evidently studied an earth widely different from that investigated by the others. Since the discovery of ytterbium, some light has been thrown on the matter. The old erbia is a mixture of several earths, to one of which, a rose-colored body, the name erbia is now restricted. For the atomic weight of the supposedly true erbium Cleve[3] gives three determinations, based on syntheses of the sulphate after the usual method. His weights were as follows, with the percentage ratio added:

Er_2O_3.	$Er_2(SO_4)_3$.	Per cent. Er_2O_3.
1.0692	1.7436	61.321
1.2153	1.9820	61.317
.7850	1.2808	61.290

Mean, 61.309, ± .0068

Hence Er = 166.31.

The discussion over the complexity of erbia, however, did not stop with the work of Cleve. Krüss,[4] assisted by K. Hofmann, made a long series of fractionations of erbium material, and gave crude atomic weight determinations of them, which varied widely. The figures need not be

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[1] Wiedemann's Beiblätter, 5, 122. 1881.
[2] Journ. Chem. Soc., 35, 116. 1879.
[3] K. Svensk. Vet. Akad. Handlingar, No. 7. 1880.
[4] Zeitsch. anorg. Chem., 3, 353. 1893.

reproduced here. L. Hermann[1] also has studied the subject, and states that the old erbia is separable into two earths, one giving red and the other yellow salts.

More recent determinations of the atomic weight of erbium are as follows. First, two unimportant analyses made by Brill[2] with the aid of the microbalance:

$Er_2(SO_4)_3$.	Er_2O_3.	Per cent. Er_2O_3.
92.35	56.55	61.234
36.75	22.68	61.496
		Mean, 61.365, ± .0873

Hence Er=166.76.

Under the name "neo-erbium" Hofmann and Burger[3] describe carefully purified material, which gave a sharp and distinct spectrum. Four syntheses of the sulphate gave the subjoined figures:

Er_2O_3.	$Er_2(SO_4)_3$.	Per cent. Er_2O_3.
.9048	1.4724	61.451
.4666	.7594	61.443
1.4181	2.3077	61.451
1.0789	1.7563	61.436
		Mean, 61.445, ± .0031

Hence Er=167.40.

It is not necessary to combine these data. The latest, by Hofmann and Burger, is the most probable, and should be accepted.

The atomic weight of thulium has not yet been carefully determined. Cleve[4] assigned to it the atomic weight 170.7, but without details as to weighings. According to Urbain,[5] the atomic weight is below 168.5. Urbain[6] also states that the value for holmium is near 140.

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[1] Dissertation, Technische Hochschule, München. 1906.

[2] Zeitsch. anorg. Chem., 17, 464. 1905.

[3] Ber. Deutsch. chem. Ges., 41, 308. 1908.

[4] Compt. Rend., 91, 329. 1880.

[5] Compt. Rend., 145, 759. 1907. According to Auer von Welsbach (Anzeiger Wien. Akad., 45, 529), thulium is really complex. The atomic weight assigned to it has little significance.

[6] Bull. Soc. Chim. (3), 33, 403. 1905.

YTTERBIUM AND LUTECIUM.

Although ytterbium was long supposed to be a definite element, it has recently been shown to be complex, and its oxide is a mixture of at least two distinct earths. Nevertheless, the data relative to the atomic weight of the old ytterbium are worth assembling, if only for historical reference.

The first good series of determinations was by Nilson,[1] who effected the synthesis of the sulphate from the oxide in the usual manner. His figures are as follows:

Yb_2O_3.	$Yb_2(SO_4)_3$.	Per cent. Yb_2O_3.
1.0063	1.6186	62.171
1.0139	1.6314	62.149
.8509	1.3690	62.155
.7371	1.1861	62.145
1.0005	1.6099	62.147
.8090	1.3022	62.126
1.0059	1.6189	62.134
		Mean, 62.147, ± .0036

Hence Yb=173.18.

Astrid Cleve,[2] by the same method, obtained the subjoined results:

Yb_2O_3.	$Yb_2(SO_4)_3$.	Per cent. Yb_2O_3.
.7791	1.2535	62.154
.5190	.8353	62.133
.4905	.7894	62.136
		Mean, 62.141, ± .0044

Hence Yb=173.13.

Brill,[3] who used the microbalance, gives the following figures:

$Yb_2(SO_4)_3$.	Yb_2O_3.	Per cent. Yb_2O_3.
106.00	65.90	62.170
92.35	57.30	62.047
		Mean, 62.108, ± .0407

Hence Yb=172.85.

Brauner,[4] from 1.67279 grammes Yb_2O_3, obtained 2.69209 of $Yb_2(SO_4)_3$. Percentage Yb_2O_3, 62.137, and Yb=173.10. There is also a preliminary note by G. and E. Urbain,[5] who found Yb=172.6, but who give no details of the determination.

---

[1] Compt. Rend., 91, 56. 1880. Ber. Deutsch. chem. Ges., 13, 1430.

[2] Zeitsch. anorg. Chem., 32, 129. 1902.

[3] Zeitsch. anorg. Chem., 47, 464. 1905.

[4] Abegg's Handbuch, 3 (1), 335.

[5] Compt. Rend., 132, 136.

Feit and Przibylla,[1] by their volumetric method, obtained the following figures:

Yb_2O_3.	O.	*Atomic weight Yb.*
.6424	.07808	173.459
.6408	.07783	173.600
.6403	.07779	173.547
.6466	.07858	173.485
		Mean, 173.523, ± .0214

The complexity of the old ytterbium was proved almost simultaneously by Auer von Welsbach and Urbain. Urbain,[2] by a long series of fractionations, obtained from it two end products, one, *neo-ytterbium,* with an atomic weight near 170; the other, *lutecium,* approximately 174. Welsbach,[3] whose work was published a little later, proposed for his earths the names *aldebaranium* and *cassiopeium,* and gave more explicit figures as to their atomic weights. His data are as follows:

Ad_2O_3.	$Ad_2(SO_4)_3$.	*Per cent.* Ad_2O_3.
.4181	.6730	62.125
.5984	.9634	62.113
.6173	.9939	62.109
		Mean, 62.116, ± .0011

Hence Ad=172.92.

Cp_2O_3.	$Cp_2(SO_4)_3$.	*Per cent.* Cp_2O_3.
.3716	.5967	62.276
.3086	.4956	62.268
.4026	.6465	62.274
		Mean, 62.273, ± .0017

Hence Cp=174.24.

It is not necessary to enter here into the general controversy between Welsbach and Urbain relative to priority. In the matter of nomenclature alone, the priority of Urbain is clear.[4]

His name lutecium is therefore accepted, with Lu=174.24. For the other component of the mixed earths the original name ytterbium would seem to be preferable to neoytterbium, and Yb=172.92. The round numbers 174 and 173 are perhaps equally probable, for the determinations by Welsbach are certainly not final.

---

[1] Zeitsch. anorg. Chem., 50, 261. 1906.

[2] Compt. Rend., 145, 759, 1907; and 146, 406. 1908.

[3] Monatsh. Chem., 29, 192. 1908.

[4] For a reclamation of priority, see Welsbach, Monatsh. Chem., 33, 695.

THE HELIUM-ARGON GROUP.

The five inert gases, helium, neon, argon, krypton and xenon are apparently incapable of forming compounds. Their atomic weights, therefore, can only be inferred from their densities, for which the following data are available.

For helium the earliest determinations by Ramsay[1] are too high. He obtained values ranging from 3.89 to 4.84, and later figures above 2.13, when the density of oxygen, as the standard is put at 16. Langlet,[2] a little later, assigned to helium the density 2.00. Ramsay and Travers,[3] after the discovery of neon, krypton and xenon, with purer helium, found its density to be 1.98, which is the best value now assignable to it.

The density of argon has been more carefully determined, both by Ramsay[4] and by Rayleigh.[5]

Compared with an equal volume of oxygen weighing 2.62760 grammes, Rayleigh found for argon the following weights:

3.2710
3.2617
3.2727
3.2652
3.2750
3.2748
3.2741

Rayleigh accepts the last three determinations, which give 3.27463 in mean. With O = 16, the density of argon becomes 19.9399, ±.0012.

Ramsay's figures, also referred to O = 16, are as follows:

19.904
19.823
19.816
19.959
19.969
19.932

Rejecting the second and third of these determinations the four remaining values give in mean a density of 19.941, ±.0099. Correcting the figures for argon by the method of limiting densities, D. Berthelot[6] assigns to argon the density 19.911.

---

[1] Proc. Roy. Soc., 58, 81. 1895. Journ. Chem. Soc., 57, 684. 1895

[2] Zeitsch. anorg. Chem., 10, 289. 1895.

[3] Proc. Roy. Soc., 67, 329. 1900. Phil. Trans., 197, A, 17. 1900.

[4] Phil. Trans., 186, 238. 1905.

[5] Proc. Roy. Soc., 59, 201. 1896.

[6] Compt. Rend., 126, 1501.

The densities of the three other inert gases were first determined by Ramsay and Travers.[1] Referred to O = 16, the figures become

Ne	9.99	9.94
Kr	40.88	40.78
Xe	64.00	63.64

They also assign to argon the densities 19.93 and 19.96.

Ladenburg and Krügel,[2] in their determinations of the density of krypton, obtained erroneous values, namely, 29.335 and 29.405. Ramsay,[3] in still later experiments, found for Kr the densities 40.81, 40.82 and 40.73, in agreement with the figures given by Ramsay and Travers.

More conclusive data for krypton and xenon are given by Moore,[4] who worked with residues separated by fractionation from 120 tons of liquid air. For krypton the densities found were 41.504 and 41.509, or 41.506 in mean. For xenon Moore found the densities 65.380 and 65.328, in mean, 65.354.

Since these gases are all monatomic, their atomic weights are double their densities as given in the foregoing paragraphs. They are then to be taken as follows:

He = 3.96
Ne = 19.93
A = 39.882
Kr = 83.013
Xe = 130.704

Moore's figures are preferred for krypton and xenon, and Berthelot's for argon.

---

[1] Proc. Roy. Soc., 67, 329. Phil. Trans., 197A, 47. 1900.

[2] Chem. News, 81, 205. From Sitzungsb. Berlin. Akad., 1900, 212.

[3] Proc. Roy. Soc., 71, 421. 1903.

[4] Journ. Chem. Soc., 93, 2181. 1908.

TABLE OF ATOMIC WEIGHTS.

In the following table the results of the foregoing calculations are brought together, each atomic weight being rounded off to five significant figures, or sometimes fewer. For convenience, the values are given in two columns, referring to the two ultimate standards, O = 16 and H = 1. Many chemists prefer the latter, and their wishes are, in a work like this, entitled to respectful consideration:

	O = 16.	*H = 1.*
Aluminum	27.040	26.831
Antimony	120.05	119.11
Argon	39.882	39.574
Arsenic	74.957	74.378
Barium	137.36	136.30
Bismuth	208.06	206.45
Boron	10.980	10.896
Bromine	79.920	79.302
Cadmium	112.40	111.53
Cæsium	132.81	131.78
Calcium	40.132	40.006
Carbon	12.004	11.911
Cerium	140.20	139.11
Chlorine	35.458	35.184
Chromium	52.019	51.617
Cobalt	58.961	58.505
Columbium	93.528	92.805
Copper	63.555	63.064
Dysprosium	162.55	161.30
Erbium	167.40	166.10
Europium	152.07	150.90
Fluorine	19.041	18.894
Gadolinium	157.22	156.00
Gallium	69.91	69.385
Germanium	72.50	71.95
Glucinum	9.0945	9.0242
Gold	197.27	195.74
Helium	3.96	3.93
Hydrogen	1.0078	1.0000
Indium	114.86	113.97
Iodine	126.92	125.94
Iridium	193.05	191.55
Iron	55.880	55.448
Krypton	83.013	82.371
Lanthanum	138.80	137.73
Lead	206.97	205.37
Lithium	6.9379	6.8843
Lutecium	174.24	172.90
Magnesium	24.304	24.116
Manganese	54.947	54.522
Mercury	200.05	198.51
Molybdenum	96.029	95.287
Neodymium	143.91	142.80
Neon	19.93	19.776
Nickel	58.682	58.228
Nitrogen	14.010	13.908
Osmium	191.07	189.59
Oxygen	16.000	15.876
Palladium	106.66	105.84
Phosphorus	31.041	30.872
Platinum	195.21	193.70
Potassium	39.100	38.798
Praseodymium	140.62	139.53
Radium	226.37	224.62
Rhodium	102.93	102.13
Rubidium	85.436	84.776
Ruthenium	101.66	100.87
Samarium	150.39	149.23
Scandium	44.115	43.774
Selenium	79.176	78.564
Silicon	28.246	28.028
Silver	107.88	107.05
Sodium	23.011	22.833
Strontium	87.616	86.938
Sulphur	32.067	31.819
Tantalum	181.02	179.62
Tellurium	127.52	126.53
Terbium	159.20	157.97
Thallium	204.04	202.74
Thorium	232.60	230.80
Thulium	168.5	167.2
Tin	119.06	118.14
Titanium	48.099	47.727
Tungsten	184.09	182.67
Uranium	238.98	237.13
Vanadium	51.037	50.642
Xenon	130.71	129.70
Ytterbium	172.92	171.58
Yttrium	89.094	88.405
Zinc	65.418	64.912
Zirconium	90.483	89.784

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SMITHSONIAN MISCELLANEOUS COLLECTIONS
PART OF VOLUME 54

FIVE NEW RODENTS FROM BRITISH EAST AFRICA

WITH TWO PLATES

BY

EDMUND HELLER
Field Naturalist, Smithsonian African Expedition

No. 1924

CITY OF WASHINGTON
PUBLISHED BY THE SMITHSONIAN INSTITUTION
February 28, 1910

WASHINGTON, D. C.
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1910

FIVE NEW RODENTS FROM BRITISH EAST AFRICA

By EDMUND HELLER

Field Naturalist, Smithsonian African Expedition

(With Two Plates)

Five Murine rodents collected by members of the Smithsonian African Expedition in British East Africa during October and November, 1909, appear to be new to science. This paper, in which they are described, is the fourth dealing with the results of the expedition.

PELOMYS ROOSEVELTI, new species

Plate 1, *a*, Skull, Natural Size

Type from the Nzoia River, Guas Ngishu plateau, British East Africa; adult male, number 162881, U. S. Nat. Mus.; collected by Edmund Heller, November 16, 1909; original number, 1231.

Coloration.—Above bright tawny golden, heavily lined with black; hairs annulated black and tawny, with a mixture of much longer black hairs which possess a strong olive iridescence similar to the color of a golden mole. Sides more tawny, with a slight mixture of grayish, but the color of the sides well defined against the white of the belly. Underparts white, faintly tinged with cream-buff, the hairs white nearly to the base, where they become grayish for a short distance. Limbs tawny above, whitish below. Tail sharply bicolor, dusky black above, below tawny yellow. Ears thinly haired with tawny yellow.

Measurements.—Head and body, 170; tail, 153; hind foot, 36; ear, 19. Skull: condylobasal length, 34.2; basilar length, 30; zygomatic breadth, 18.5; nasal, 14; interorbital constriction, 5; diastema, 9; mandible, 21.4; maxillary toothrow, 8.5.

Compared to *P. fallax* this is a longer-tailed species. In coloration it is more yellowish, the back with a marked olive iridescence, the sides with sharp line of demarcation. Annulation of tail less distinct, the tail clothed with short, stiff hair. Grooving of upper incisors wide, the grooves wide and close to the outer edge. Palatal foramina large and extending well behind the anterior border of the first upper molar; interorbital beads low and flat.

DASYMYS HELUKUS, new species

PLATE 1, *b*, SKULL, NATURAL SIZE

Type from Sirgoit, Guas Ngishu plateau, British East Africa; adult male number 162889, U. S. Nat. Mus.; collected by Edmund Heller, November 19, 1909; original number, 1239.

Diagnosis.—Like *Dasymys incomptus* of Natal, but skull with less heavy rostrum, audital bullæ distinctly smaller, and palate wider in proportion to toothrow; teeth less robust than those of *D. incomptus*, the reëntrant angles on anterior border of first and second laminæ of m^1 and m^2 better developed.

Coloration.—Dorsal coloration a mixture of black and tawny, the black predominating greatly, giving a general blackish effect, the individual hairs annulated with tawny and black with a mixture of longer black hairs. Color of sides more grayish and merging gradually into the buffy wash of the lower parts. Feet and ears thinly clothed with dusky hairs. Tail blackish with prominent annulations and a sparse covering of short black hairs.

Measurements.—Head and body, 185; tail, 143; hind foot, 34; ear, 22. Skull: condylobasal length, 37.8; basilar length, 33; zygomatic breadth, 20.2; diastema, 11.8; mandible, 24; maxillary toothrow, 7.

This species has somewhat the appearance of *M. alexandrinus*, but the hair is much softer and longer and the tail is conspicuously shorter.

LEGGADA NAIVASHÆ, new species

PLATE 2, *a*, SKULL, NATURAL SIZE

Type from eastern edge of Naivasha plains, at the base of the Aberdare Mountains, British East Africa; adult male, number 162885, U. S. Nat. Mus.; collected by Edmund Heller, October 18, 1909; original number, 1186.

Coloration.—Upperparts blackish with a slight mixture of tawny hairs, the pelage consisting of long black hairs and shorter tawny and black annulated ones; sides somewhat more tawny than the median dorsal region. Underparts pearl gray, whitening about the throat and chin, sharply defined from the dark sides, the hair everywhere plumbeous basally. Limbs dusky brown above, below gray like the belly. Tail black above, dark brown below, thinly haired throughout. Ears externally thinly haired with black, internally clothed with tawny hairs.

Measurements.—Head and body, 76; tail, 54; hind foot, 16.5; ear, 12. Skull: condylobasal length, 19.6; basilar length, 16.4; zygomatic breadth, 11; nasal, 8; interorbital constriction, 3.8; diastema, 5.5; mandible, 12.8; maxillary toothrow, 4.

Appears to be most nearly related to *L. triton* Thomas.

SACCOSTOMUS MEARNSI, new species

PLATE 2, *b*, SKULL, NATURAL SIZE

Type from Changamwe, British East Africa; adult male, number 162882, U. S. Nat. Mus.; collected by E. A. Mearns, November 28, 1909; original number, 7292.

Coloration.—Above drab gray washed with hair brown, the individual hairs plumbeous basally, medially light grayish with the tips blackish. Sides more grayish and merging gradually into the light grayish of the underparts. Ears and limbs grayish like the body coloration. Feet whitish. Underparts light gray with a faint buffy wash, the hairs plumbeous basally. Tail dusky brownish above, whitish below.

Measurements.—Head and body, 156; tail, 65; hind foot, 23; ear, 16. Skull: condylobasal length, 32; basilar length, 28.5; zygomatic breadth, 17; nasal, 14.2; interorbital constriction, 4; diastema, 10; mandible, 20; maxillary toothrow, 6.4.

Compared to *S. mashonæ* of South Africa, to which this species is most nearly related, it differs in its much larger size, and relatively larger hind feet, tail, and skull. It is lighter in coloration without the iron gray effect to the upperparts and with the underparts considerably lighter gray.

STEATOMYS ATHI, new species

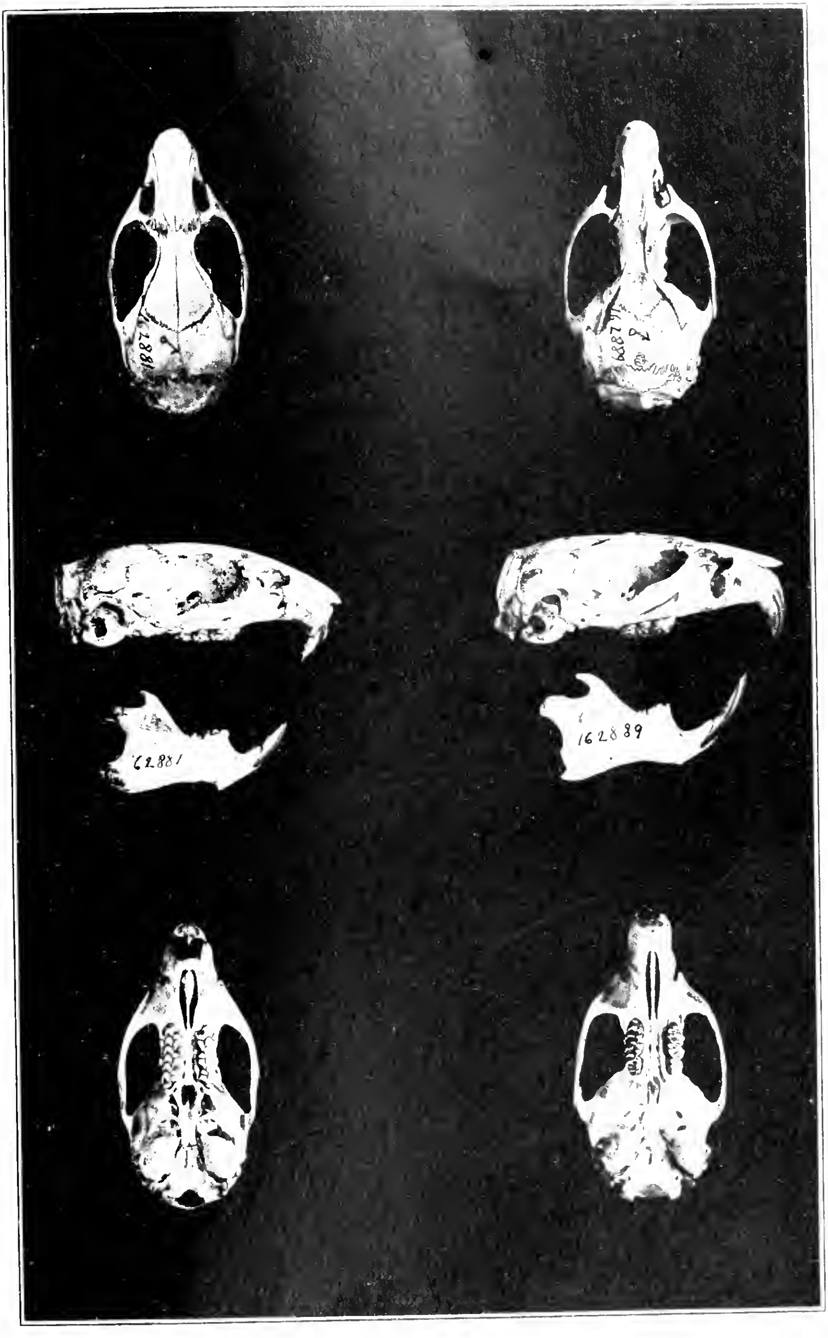
PLATE 2, *c*, SKULL, NATURAL SIZE

Type from the Lukenia Hills, Athi Plains, British East Africa; adult male, number 162883, U. S. Nat. Mus.; collected by J. A. Loring, November 22, 1909; original number, 8259.

Coloration.—Above hair-brown with an intermixture of buffy; the pelage composed chiefly of buffy hairs with dark brown tips mixed profusely with longer black hairs throughout the median dorsal region. Sides of body a mixture of buffy and grayish without any blackish hairs, and sharply defined against the pure white of the underparts. Ears dusky, very thinly haired: a conspicuous white tuft of hair immediately below the opening. Underparts and feet pure silky white, the hairs white to the roots. Tail dark brownish above, buffy below; covered with short hairs.

Measurements.—Head and body, 83; tail, 46.5; hind foot, 16.5; ear, 11.5. Skull: condylobasal length, 22.2; basilar length, 19; zygomatic breadth, 12.5; nasal, 9.7; interorbital constriction, 4.5; diastema, 6.3; mandible, 13.2; maxillary toothrow, 4.4.

This species differs from *S. pratensis* of South Africa and Mozambique in its much lighter dorsal coloration, which shows no tendency toward rufous, and in its larger size.

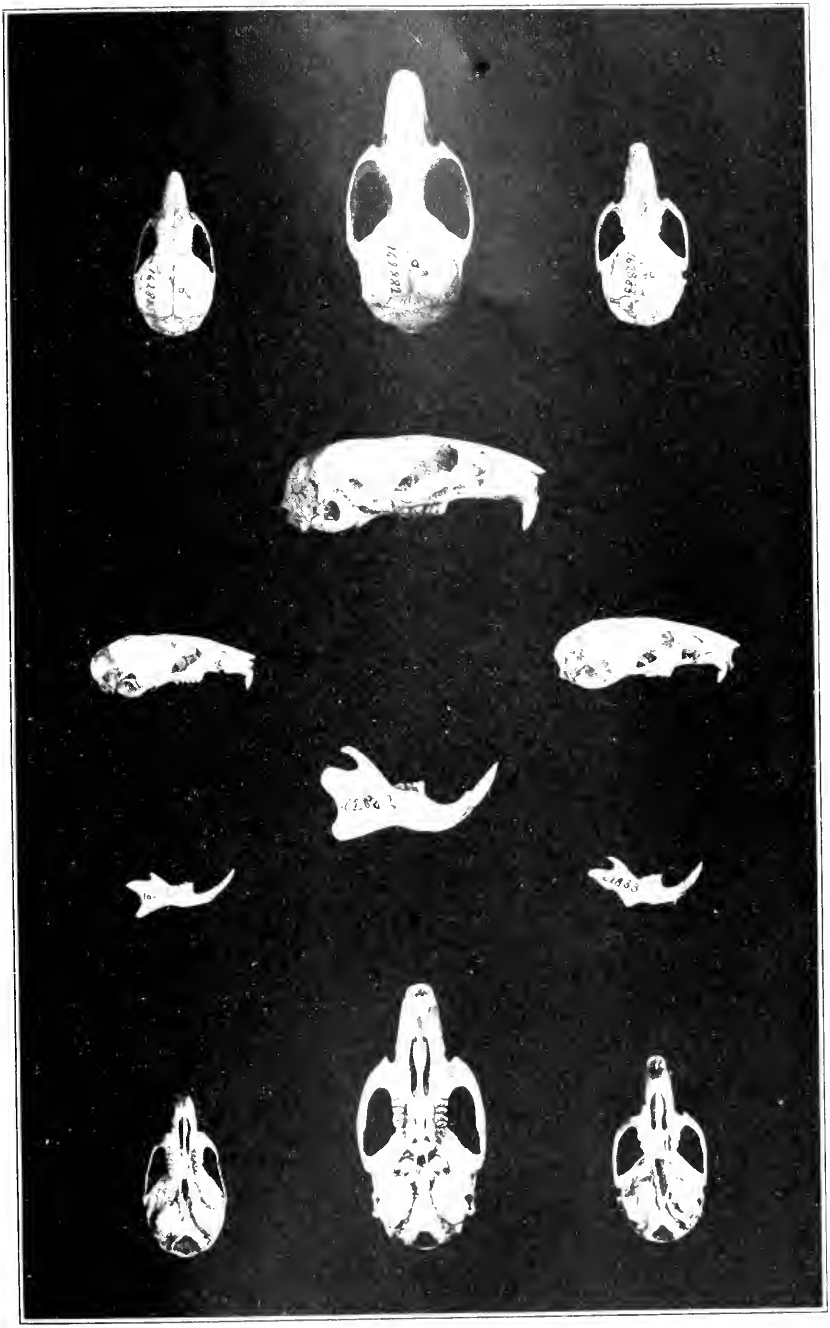


a

PELOMYS ROOSEVELTI HELLER
Type. Natural size

b

DASYMYS HELUKUS HELLER
Type. Natural size



a

LEGGADA NAIVASHÆ HELLER

Type. Natural size

b

SACCOSTOMUS MEARNSI HELLER

Type. Natural size

c

STEATOMYS ATHI HELLER

Type. Natural size

SMITHSONIAN MISCELLANEOUS COLLECTIONS

PART OF VOLUME 54

A NEW RODENT OF THE GENUS SACCOSTOMUS FROM BRITISH EAST AFRICA

WITH ONE PLATE

BY

GERRIT S. MILLER, Jr.

Curator, Division of Mammals, U. S. National Museum

No. 1925

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A NEW RODENT OF THE GENUS SACCOSTOMUS FROM BRITISH EAST AFRICA

By GERRIT S. MILLER, Jr.

Curator, Division of Mammals, U. S. National Museum

(With One Plate)

A species of *Saccostomus* equally distinct from *S. campestris* Peters and *S. mearnsi* Heller[1] is represented by three specimens collected by J. Alden Loring, Field Naturalist of the Smithsonian African Expedition, at Njori Osolali, British East Africa. It is described in this paper, the fifth dealing with the results of the expedition.

SACCOSTOMUS UMBRIVENTER, sp. nov.

Plate 1, *a*, Skull, Natural Size

Type.—Adult female (skin and skull), No. 162612. Collected at Njori Osolali (Sotik), British East Africa, June 26, 1909, by J. Alden Loring. Original number, 6447.

Diagnosis.—Size and general external appearance as in *Saccostomus campestris* and *S. mearnsi*, but differing from the former in the conspicuous plumbeous bases of hairs of underparts and from the latter in the more buffy suffusion of back and sides. Skull with pits near posterior margin of palate minute as in *S. campestris*, but with incisive foramina extending fully half way from maxillo-premaxillary suture to incisors, as in *S. mearnsi*.

Color.—Upperparts an indefinite light brown approaching the wood-brown of Ridgway, lighter and more buffy on sides, darker on back, along middle of which there is a faint clouding of much darker brown; a few longer black hairs on both back and sides, but these not producing any evident effect of "lining"; underparts sharply defined white with a faint buffy tinge, the gray (about gray No. 6 of Ridgway) bases of the hairs everywhere appearing conspicuously at surface; ear dusky with a narrow silvery rim; feet white; tail too thinly haired to have any definite color, the hairs of under surface white, those of upper surface mixed black and white.

---

[1] Smithsonian Miscellaneous Collections, vol. 54 No. 1924, p. 3. February 28, 1910.

Skull.—The skull agrees with that of *S. campestris* and *S. mearnsi* in general size and aspect. From that of the former as figured by Peters[1] it is readily distinguishable by the much greater forward extension of the incisive foramina, and the narrower interorbital region. From that of the latter it differs in the broader rostrum, more nearly parallel supraorbital ridges, longer, less rapidly divergent pterygoids, smaller ectopterygoid plates, and by the absence of well-defined pits and median ridge at posterior margin of palate, the posterior palatal foramina opening practically on general palatal level.

Teeth.—The teeth agree with those of *Saccostomus mearnsi* and differ from those of the South African species as figured by Peters, in their large size, the greatest transverse diameter of m^1 decidedly more than half greatest width of palate between toothrows. Pattern of enamel folding as in *S. mearnsi*, but reëntrant angles on anterior border of second and third laminæ of m^1 and of anterior (second) lamina of m^2 better defined.

Measurements.—Type: head and body, 136; tail, 55; hind foot (dry), 19.6. A second adult female: head and body, 133; tail, 60; hind foot (dry), 19.6. Skull of type: condylobasal length, 33.0;[2] zygomatic breadth, 17.2; interorbital constriction, 3.8; occipital breadth, 13.8; depth of braincase at middle, 9.0; nasal, 16.4; diastema, 10.4; mandible, 22.0; maxillary toothrow (alveoli), 6.2; mandibular toothrow (alveoli), 5.8.

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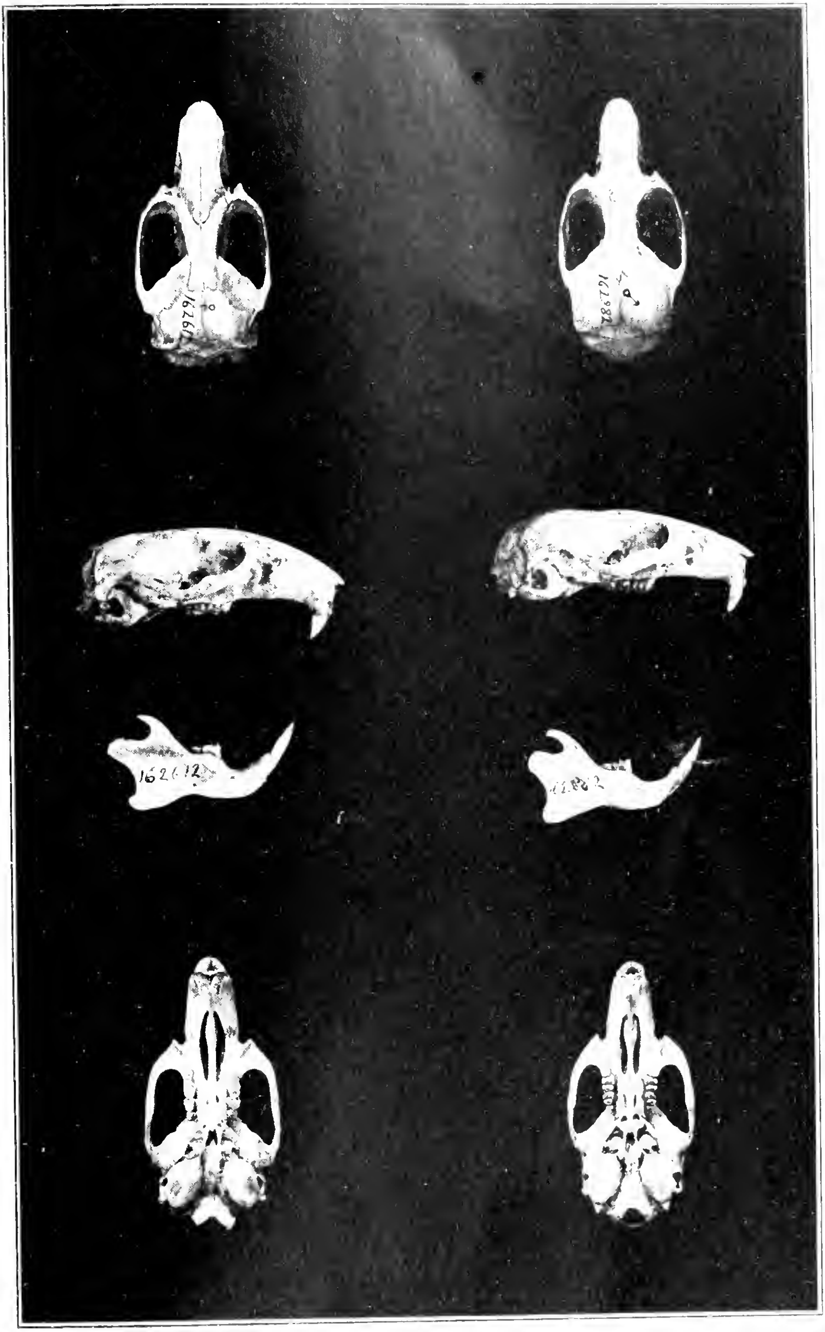
[1] Reise nach Mossambique, pl. xxxv, fig. 12.

[2] Possibly a minute fraction of a millimeter too short.



a

SACCOSTOMUS UMBRIVENTER MILLER
Type. Natural size

b

SACCOSTOMUS MEARNSI HELLER
Type. Natural size

SMITHSONIAN MISCELLANEOUS COLLECTIONS

VOLUME 54, PART 6

A NEW SABLE ANTELOPE FROM BRITISH EAST AFRICA

BY

EDMUND HELLER

Field Naturalist, Smithsonian African Expedition

No. 1926

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A NEW SABLE ANTELOPE FROM BRITISH EAST AFRICA

By EDMUND HELLER,

Field Naturalist, Smithsonian African Expedition

A Sable Antelope collected by Kermit Roosevelt in the Shimba Hills, British East Africa, represents a form easily distinguishable from *Ozanna niger*. It is here described, this paper being the sixth dealing with the results of the Smithsonian African Expedition.

OZANNA ROOSEVELTI, new species

Type from the Shimba Hills, British East Africa, adult female, number 550, E. Heller, collected by Kermit Roosevelt, December 14, 1909.

Coloration.—Snout black medially from the tip to the base of the horns, the black separated from the rhinarium and horn bases by a narrow band of reddish brown; a black stripe enclosing eye and extending from base of horns to the upper lip midway between angle and tip of snout; hairs covering anteorbital gland creamy white, set off from rest of face by their whiteness and greater length, and merging rather abruptly into the buffy stripe which extends forward to the tip of the snout; cheeks and throat below the dark ocular stripe whitish; sides of head and neck posterior to eye, including base and outside of ear, light chestnut like the general body color. Inside and margin of ear conch white, the tip with short, dark brown tufts. Whole dorsal region, including head, posteriorly light chestnut, darkest dorsally becoming slightly lighter on the sides, where it extends well down and is sharply defined against the white underparts. Neck clothed with a well-developed mane, which extends from the ears posteriorly to the middle of the back, the individual hairs black at base and tip, with the median third chestnut brown like the back. Chest and lower throat black, the black extending down front of fore legs as a broad stripe to the hoofs, where it spreads and embraces the pasterns and false hoofs; posterior part of fore legs colored like the upperparts. Hind legs colored like the upperparts, with the exception of bands above the hoofs embracing pasterns and false hoofs. Tail black, well haired terminally; the underside basally becoming brownish. Underparts white from between fore legs posteriorly to base of tail and lower rump, the white extending down on inside of thigh as far as knee.

Measurements.—Skull: Condylobasal length, 415; basilar length, 305; zygomatic width, 150; interorbital constriction, 108; nasal, 144; maxillary toothrow, 109 mm.

Compared to *O. niger*, of South Africa, this species is very much lighter in dorsal coloration, only the dark head stripes, throat, and fore legs being sable, the general body coloration being light chestnut in marked contrast to these dark areas. There is also less contrast in the head markings, the light stripes being buffy yellow rather than white.

Named for Kermit Roosevelt, to whose indefatigable energy many of the rarer species of big game in the collection are due.

SMITHSONIAN MISCELLANEOUS COLLECTIONS
VOLUME 54, NUMBER 7

DESCRIPTION OF A NEW SPECIES OF HIPPOPOTAMUS

WITH FOUR PLATES

BY

GERRIT S. MILLER, JR.
Curator, Division of Mammals, U. S. National Museum

(PUBLICATION 1927)

CITY OF WASHINGTON
PUBLISHED BY THE SMITHSONIAN INSTITUTION
March 28, 1910

DESCRIPTION OF A NEW SPECIES OF HIPPOPOTAMUS

By GERRIT S. MILLER, Jr.,
Curator, Division of Mammals, U. S. National Museum

(With Four Plates)

Two skulls of *Hippopotamus* in the U. S. National Museum, one from the Zambezi River, the other from Angola, differ noticeably from each other in important details of form. Hitherto the exact value of these characters could not be determined, owing to lack of material to illustrate the normal variation in individuals from one locality. The series of eight skulls from British East Africa collected by the Smithsonian African Expedition shows conclusively that individual variation in this genus is not unusually great, and that the Zambezian and Angolan specimens must be regarded as representing distinct species. In all essential characters the Zambezian skull agrees with those from British East Africa; these are for the present assumed to represent true *Hippopotamus amphibius* of the upper Nile.[1] The Angolan animal may be known as:

HIPPOPOTAMUS CONSTRICTUS, sp. nov.

Type.—Skull of immature male (m^3 not fully in place), number 34787, U. S. National Museum; collected in Angola, Africa, by H. Chatelain.

Diagnosis.—Skull in general like that of *Hippopotamus amphibius*, but rostral constriction deeper, dorsal surface of cranium more flattened, difference in level between anterior border of maxillary and of premaxillary more pronounced, and mandibular symphysis much shorter, its median length less than one-third length of mandible; teeth smaller than in *Hippopotamus amphibius*, the transverse diameter of m^2 scarcely more than one-half that of palate.

Skull.—The skull is throughout less robust than that of *Hippopotamus amphibius*, a peculiarity equally noticeable in dorsal, ventral, or lateral aspect; depth at level of anterior border of orbit contained about 4 times in condylobasal length, instead of 3½ times, as in *H. amphibius;* rostral constriction very pronounced, its least

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[1] This paper is the seventh dealing with the results of the Smithsonian African Expedition.

breadth contained nearly 7 times, instead of about 5 times in condylobasal length; anterior portion of rostrum less expanded than in *H. amphibius,* the ratio of breadth across canines to zygomatic breadth about 77, instead of about 88; form of rostral expansion differing from that of the related species in the less forward extension of maxillaries, so that level of canines is noticeably further behind that of anterior incisors, and the bases of the three teeth of each side lie nearly in a straight oblique line; occipital region and dorsal surface of orbits much less elevated above level of median portion of frontals than in *H. amphibius,* the vertical diameter of orbit not conspicuously greater than transverse diameter; mandible with posterior portion less developed, both longitudinally and vertically, than in the related species, the ratio of height through coronoid process to length (from articular process to front of alvedus of canine) about 60, instead of about 65; posterior border somewhat flattened immediately below articular process, the region of its greatest convexity at level of middle of ramus, rather than above middle; symphysis both shorter and narrower than in *H. amphibius,* the longitudinal trough on its upper surface less well defined, the hinder border opposite front of middle premolar instead of front of posterior premolar, the ratio of median length of symphysis to length of mandible (from articular process to front of alveolus of canine) about 27, instead of about 35.

Teeth.—The teeth are essentially similar to those of *Hippopotamus amphibius,* except that they are relatively smaller (transverse diameter of m^1 scarcely more than one-half that of palate); the crowns of the molars are narrower, and the outward curve of maxillary series behind premolars is more pronounced.

Measurements.—Cranial measurements of type (millimeters): Condylobasal length, 690 (730);[2] zygomatic breadth, 435± (435); rostral constriction, 110 (144); rostral expansion, 332 (370); occipital breadth, 278 (332); occipital depth (to basion), 196 (214); median depth between orbits, 181 (208); nasal, 390 (400); height of orbit, 75 (90); width of orbit, 70 (65); elevation of orbit above level of forehead, 24 (40); mandible (condyle to front of alveolus of canine), 587 (620); coronoid height, 355 (410); canine width 385 (403); length of symphysis (without median spine), 168 (224); maxillary toothrow (alveoli), 265 (258); mandibular toothrow, 293 (286); first upper molar, 44.7 x 41.3 (45.5 x 45.5); second upper

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[2] Measurements in parentheses are those of the adult male Zambezian skull (No. 123387).

molar, 53.4 x 45.4 (51.0 x 50.8); first lower molar, 46.2 x 31.0 (51.4 x 33.8); second lower molar, 59.0 x 35.4 (57.0 x 38.6); width of palate at anterior lobe of m^1, 68.2 (76.2).

Remarks.—At present there appear to be three well-marked species of *Hippopotamus* recognizable among the living members of the genus: (*a*) True *Hippopotamus amphibius*[3] of eastern and northeastern Africa, (*b*) *H. constrictus* of Angola, and (*c*) *H. australis*[4] Duvernoy of the Cape region. The last I have not seen, but, so far as can be judged from the descriptions of Desmoulins and Duvernoy, it is a form in which the flattening of the cranium is carried even further than in *H. constrictus*, the orifice of the orbit is wider than high, and there are certain important peculiarities in the interrelations of the upper and lower canines, and in the form of the anterior cheek-teeth.

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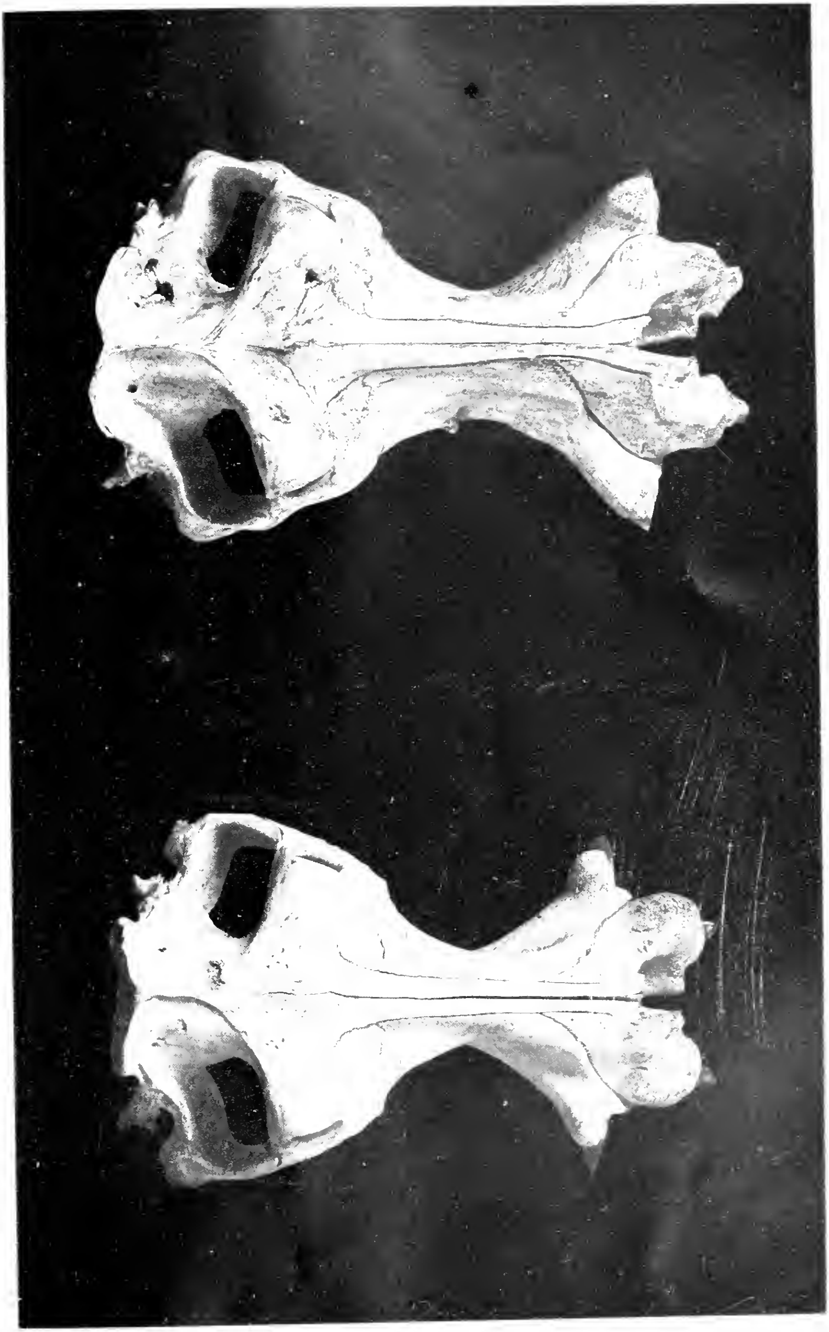
[3] Synonymy:

1758. *Hippopotamus amphibius* Linnaeus, Syst. Nat., I, 10th ed., p. 74 (Nile).

1846. *Hippopotamus typus* Duvernoy, l'Institut, XIV, p. 333. October 7, 1846 ("Senegal or Abyssinia").

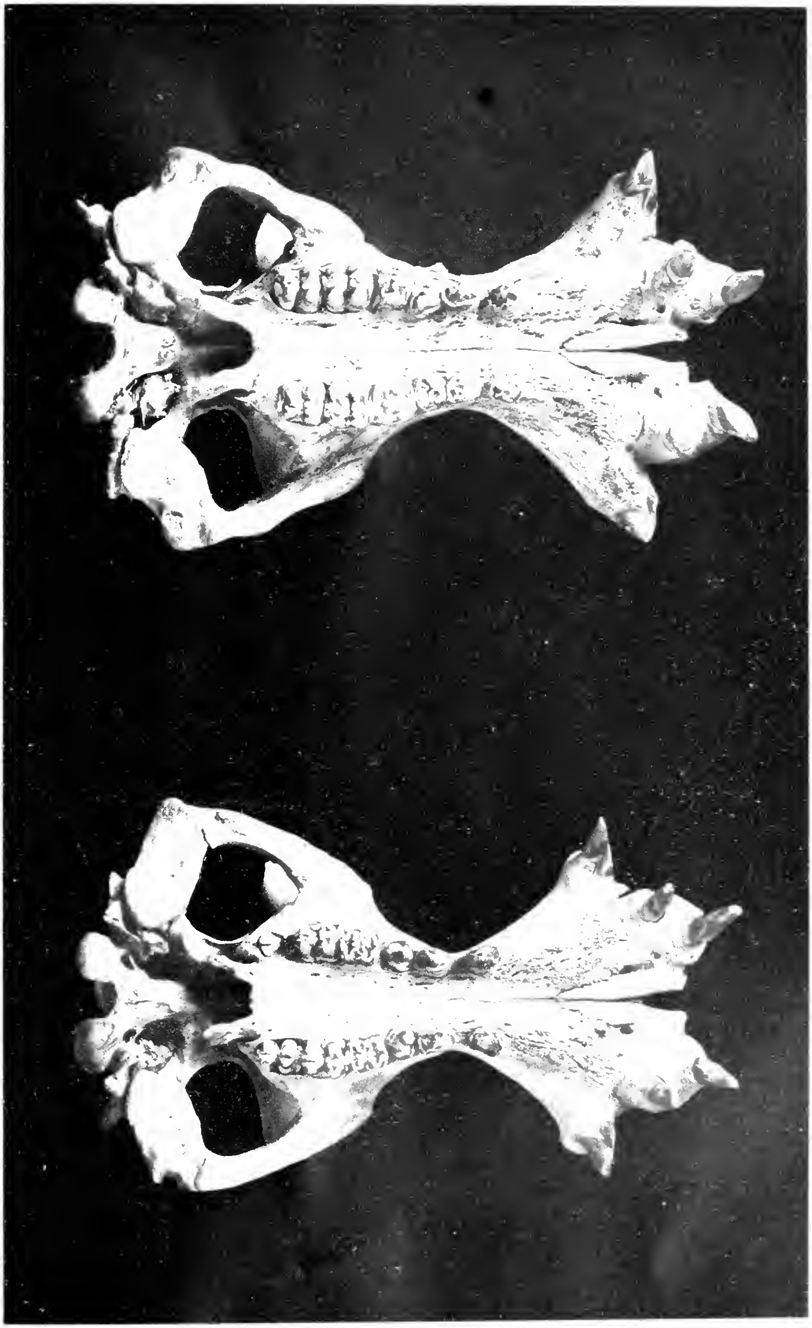
[4] Synonymy:

1846. *Hippopotamus australis* Duvernoy, l'Institut, XIV, p. 333. October 7, 1846. (Previously described but not named by Desmoulins, Journ. Physiol. Exp. et Path., V, p. 354, October, 1825.)



HIPPOPOTAMUS AMPHIBIUS. ZAMBEZI RIVER

HIPPOPOTAMUS CONSTRICTUS. TYPE



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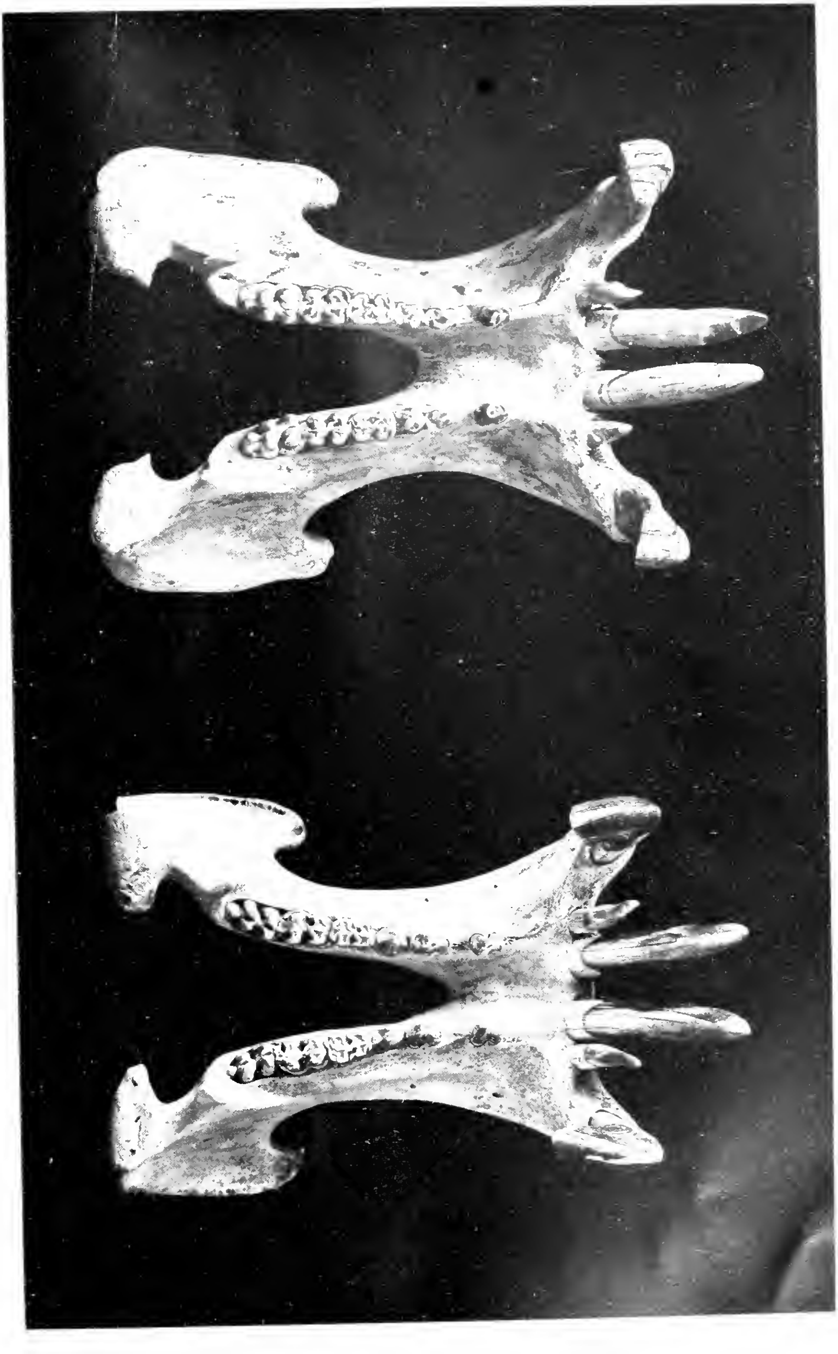

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