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(From a recent Photograph.)

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Original Communications.

ART. I.—*Inaugural Address, including a Paper on Infant Asylums.*[1] By A. JACOBI, M. D., President of the Medical Society of the County of New York, Professor of Diseases of Children in the College of Physicians and Surgeons, New York.

I HOLD in my hands the record of the members of this Society in the last year. There are names among them of men who no longer adorn these seats. There is not one of them who has not been, when he had passed away from us, honorably mentioned by us; there are a few, whose loss all of us shall long mourn, and whose memories we shall forever cherish. The two members to whom I consider it my sacred duty to allude were Dr. Bibbins and Dr. Geo. T. Elliot.

Dr. Bibbins was for many years a member of your comitia minora, and your Treasurer. To say that he was a diligent and trustworthy officer, although that is saying a great deal, is not much when we recollect his untiring industry in behalf of the honor and standing of this Society, and of the profession in general. There are but few who, with equal erudition, and

[1] Read at the Stated Meeting, December 4, 1871.

tact, and zeal, and *modesty*, have untiringly worked for common interests as he did, to the hour of his last sickness; which may have found its work of destruction easy in one whose nervous system was overstrained and exhausted.

Dr. Elliot was one of those to whose exertions this Society owes a great part of its flourishing condition. It was his ambition and pride to gather large numbers of members, secure good papers, and worthy discussions, and to hold extra meetings. We owe his memory thanks for what he has accomplished in that line. It is true that his predecessors had facilitated his work, for there was no society in the city that had shown a better record for many years, when he commenced his useful career as our President in 1868. But it is also true that no public man in a public place was ever more diligent, zealous, and at the same time gracious. To his other private and public virtues I need not allude; they have been recalled to your mind from this place before to-night. Whenever his name is mentioned, we shall remember a gentleman, a man of taste, a refined scholar, and an amiable colleague. I have to thank him, personally, for my finding the Medical Society of the County of New York, when I had the honor of being the first time elected to preside over your meetings, a year ago, large in numbers, regular in attendance, earnest in purpose, and progressive in spirit.

Fortunately, death has not been the only power that has worked its changes among us. The records of last year exhibit a rapid increase of our members. Seventy medical men have joined our ranks, against eight whom death has taken from our midst, and a few who have left the city and county. I hail this fact with joyful expectations of further increase. It shows that individualism is not the para mount tendency of medical men as a class, and that the universal law of centripetal attraction exerts its power. It proves that medical men are aware of the existence of common interests which can better be served by large bodies than by the few or the one. The liberty of the individual is not thwarted by a certain amount of centralization. Hundreds of private men feel the necessity of closing up into a regiment, in order and discipline. Small communities, and states, with the same language, customs, and interests, have for centuries, up to this very year, sought and found means to abolish artificial boundaries and melt into one. Decentralizing State rights have been wiped out with fire and blood to develop the growth and power of a great country, free at last, which is destined to be the harbor and fosterer of the civilization of the republican world of the future, as it has become the protector and home of fugitive republicans of Europe. Thus the tendency to follow the laws of attraction and gravitation yields its fruits in the political, the social, the literary fields. The readiness with which new names are added to our lists, proves to what extent, consciously or unconsciously, medical men feel the necessity of uniting their strength. It will be our duty to prove that, even without our legal standing in relation to the political administration, we have attractive power enough to hold them and to grow; but at the same time it will also be our bounden duty to improve the advantages of our legal standing in the interest of our members, our Society, the medical profession, and the public at large. I hope to be capable within a short time of pointing out the possibilities of availing ourselves of our connection with the State and the other county medical societies for the benefit of both the profession and the community.

Among the additions to our number during the last year I notice dozens of foreign names; the schools and universities of Great Britain, Germany, France, Switzerland, Poland, and Russia, have sent their pupils to swell our ranks, while our own schools have offered their full share as heretofore. Thus our Society but repeats the historical and ethnological fact of our great Commonwealth, which has been the result of the mixture of all the nations of the universe. While, however, the question of the beneficial influences of such mixture has, in a political point of view, sometimes not been considered as settled to universal satisfaction; while even a dozen years ago there could be, as a political issue, a strong party of nativists, there was never, to my knowledge, in the liberal profession to which we belong, any such party feeling. For, those of us who are old enough will remember that, just in those years which engendered and saw the political efforts of Knownothingism, a small number of foreigners, who are still among us, were bidden a hearty welcome in your midst, and have even been promoted since to the highest honors the profession can confer upon any member.

This difference in the reception and appreciation of the foreign-born member of the profession from what we notice in political life, now and then, is very suggestive. The United States, for instance, claims the necessity of a five-years' residence before a new-comer can be admitted to citizenship. Five years are considered necessary to acquaint the average immigrant with our Constitution, and the spirit and the habits of our republican people; they are considered an apprenticeship for those whose lives have been spent under monarchical rule. But, as soon as they are considered to be imbued with the principles of republicanism and its practical working, they are admitted as equal. There is no doubt but that, if the necessary knowledge and habit should be thought accessible in less time, the term of five years would be reduced. We have the proof of this assertion in the fact that a number of States have reduced the term of preparatory residence, as far as their own simpler, more uncomplicated affairs are concerned. What, then, would be the condition of things, if the newly-imported immigrant came, as a rule, not from monarchical communities, but from republican states, with institutions, rules, habits, like our own? Can any one think of a legislator who would, for an average immigration of *that* kind, consider an apprenticeship of five years necessary? On the contrary, we can safely say that when, in future, the monarchies of Europe shall have been swept away before republicanism conquering the world; when France shall have the fact and not only the name of a republic, when Germany shall have adopted a self-government worthy of the character of its people and the high standard of its thinkers; when old England, whose very sap and juice are drying up over the old outworn roots of its existence—meaningless and powerless as it is nowadays in the councils and destinies of nations—shall have been rejuvenated to its original Anglo-Saxon freshness and vigor by republicanism; when in fact all Europe shall have stepped over the boundaries between monarchism and republicanism, bondage and freedom, dynastic sway and enlightened self-government—there will be no apprenticeship any more than there is between the States of this one and indivisible republic of the United States. Such is the influence of equal education, habits, aims, and interests. And such is the cause and source of the universal citizenship among the members of our liberal profession from whatever geographical country they may have arrived. The first minute of your acquaintance with a medical man who is born and raised your antipode, reveals in him a relative, a brother. The same ideas, even the same terminology, render your intercourse with him like that of an old friend. For we are fortunate enough to live in a time when medical science has the same base and foundation, that of Nature and its study.

It has not always been so, nor could it be so, as long as or wherever the standard of science and society was generally low. It is a peculiarly instructive fact that these two will closely correspond with each other. The science and art of old Egypt have no greater accomplishments to boast of than hieroglyphs and pyramids, and her physicians were the priests of Isis. The Greeks, however, whose art, resulting from their enjoyment and close observation of Nature, was in many instances equal or even superior to our own, produced a Hippocrates, one of the greatest and truest observers of all times. Among the Jews, whose one science was the combination and union of politics, religion, dietetics, and medicine, the practice of the healing art was exclusively in the hands of priests and legislators. The Romans sometimes borrowed the culture of their neighbors, and some of their medical men were Greek; usually, however, they destroyed it. Their path was marked by the sword, and the blessing they carried along was slavery. Accordingly, most of their physicians were slaves; and Justinian fixed the price at which their persons could be bought. The Christian middle ages, which knew no higher scientific authorities than Aristotle and Galen, or his Arabian translators or transcribers, and have not succeeded in producing a single original mind in medicine before the times of Paracelsus, relied on the healing powers of their priests and—by way of contrast—of despised Jews. The Indian's medical man is his sorcerer; for the red man believes in miracles, in supernatural or rather unnatural powers, just as much as the illiterate and credulous white man who follows the footsteps of the clairvoyant, the medium, the quack.

In this manner every age and every country had its own medical science. It required the results of centuries, struggling for light, to yield a common base; that base being the increased knowledge of Nature. But more than some descriptive knowledge of natural bodies or phenomena was required. A universal medicine has been, at last, the result of the genetic idea and researches, the idea that there is nothing unalterable and unchangeable in Nature; that every thing is incipient, growing, and disintegrating; that the same process which had been noticed in nations, in the course of centuries, is taking place in every being from minute to minute. The researches in chemistry, showing rapid or slow transformations, in comparative anatomy pointing out small and trifling differences, have been the first to sharpen the senses, and to prepare the way for modern medical ideas. To France belong the first great names of Lavoisier, Laplace, Bichat, Dupuytren, Laennec. To Germany the glory of establishing medicine on its solid modern foundation. To the world belongs the credit of fully appreciating and recognizing the working of the mind, and the progress of development, never minding the language or nationality of the man who has a claim on priority. Thus, the men whose names I have mentioned, and the names of Oken, Schwann, Johannes Müller, Rokitansky, Schönlein, Virchow, Hunter, Davy, Faraday, Darwin—are no longer national names, any more than in other fields the names of Galileo, Copernicus, Newton, or Raphaele Santi and Correggio, or Beethoven and Mozart. Those men speak the language of the human mind, they are the leading citizens of the universal world-republic of science to which we all, equal, free, and fraternal, have sworn allegiance. There is no blockade, no fire, no Franco-German War, that will ever disprove our belonging to the same community. The progress of one man, of one country, is at the present day the common property of all men, all countries, and an isolated civilization or science belongs to the past. Let us hope, and every one at his own wheelwork, that the unity of science may be but the precursor of the unity of mankind.

With this view, American physicians have always received their foreign brethren friendly and hospitably, no matter whether they had come to settle or to visit. With this view I welcome to our Society those who have lately joined us. Besides, the fact of their coming to us proves their agreeing with the sentiments I have briefly expressed. There is many a common battle we shall have to join in.

Among our new acquisitions I notice also a large number of younger members of the profession, some of whom have already drawn the attention of the medical public to their labors, and earned golden opinions. I cannot but express my satisfaction at their accession to our numbers. If there is a physiological difference between them and the older members, it consists in the relation of the new-formed young cells entering the organism, the framework of which is established before. The latter gives the necessary firmness, is less changeable, but, it is true, less necessary for the performance of the physiological functions. The former are the active and enlivening portion of the organism. It is the young blood-cells, the young gland-cells, the organism is supported by; their healthy action is the condition of healthy and active life; without them the organs and the organism would soon die of atrophy. I hope our young cells will not carry this comparison any further. They might feel like saying that the old blood-corpuscles cannot do better than to get disintegrated as speedily as possible, and disappear from the field of action, or that long organized connective tissue is but a scar and inert, or that the old pavement epithelia of the epidermis, with their shrunken nuclei, ought to be rubbed off at once, and buried in the bath-tub. They may be right sometimes. But the old-established framework might have a line of defence; it might retort that young cells might overdo their physiological function; that it might happen that over-copious proliferation will break down the whole organ, young and old, or that the too copious and irregular invasion might prove a malignant growth. We shall easily get along, though, and all of us, old and young, perform our share of the work, by contributing our best to the general stock of knowledge. At all events, our younger brethren, or part of them, are conversant with those methods of study and investigation which have elevated modern medicine to its present standard. Therefore, the Society hopes for and requests such contributions to the papers and discussions of our meetings as will prove their efficiency, and fill our wants. These gentlemen will surely not forget that every one has to do his best where our aims are the same; and that, by so doing, they work for the future, which ought to be, and necessarily is, theirs.

While pointing to these wants, and stating my earnest request for a diligent and faithful coöperation of such of our members as are accustomed to the exact methods of investigation with the microscope, or chemistry, I still think that the papers read and discussed before this Society during the last twelvemonth are of a high order, and creditable to both their authors and our Society. Allow me to recall the following items:

November Meeting, 1870.—Paper by the PRESIDENT, on "Craniotabes," and general introductory remarks. Cases of, and remarks on, "Blepharoplasty," by Dr. KNAPP.

December Meeting.—Paper by Dr. FORDYCE BARKER, on "Blood-letting as a Therapeutic Resource in Obstetric Medicine."

January Meeting, 1871.—Paper by Dr. H. KNAPP, on "Formation of Bone in the Eye."

February Meeting.—Paper by the Vice-President, Dr. AUSTIN FLINT, on "The Pathological Relations of the Gastric and Intestinal Tubules."

March Meeting.—Paper by Dr. J. LEWIS SMITH, on "Scrofula."

April Meeting.—Paper by Dr. LEONARD WEBER, on "Abscess of the Appendix Vermiformis."

May Meeting.—Paper by Dr. H. B. SANDS, "On the Use of the Plaster-of-Paris Bandage, in the Treatment of Simple Fracture, especially Fracture of the Femur." "Case of Abscess of the Appendix Vermiformis," by Dr. ERNST KRACKOWIZER.

June Meeting.—Paper by Dr. F. N. OTIS, on "Syphilitic Infection, with Special Reference to the Channels through which the System becomes contaminated, and to the so-called Incubation Period of the Disease."

September Meeting.—Nomination of Officers.

Concerning our recent admissions I have another remark to offer.

It is not a small satisfaction to me that, in this year of my presidency, one of the most urgent questions of the day should have been quietly and noiselessly answered. The admission of females into the ranks of the medical profession—or, rather, as their obtaining the degree of M. D. is a matter belonging to chartering Legislatures, and their obtaining a practice depends on the choice or prejudice of the public—into the existing medical societies, has been decided by you by a simple vote, not attended by either the hisses and clamors of excited young men in medical schools, or the confusion and derogations of the meetings of a medical association. I think we can say that our action has finally settled a question the importance of which was recognized by everybody. The vote of the largest society of the kind in the Empire State, and I believe in the Union, will have the effect of soothing the passions and levelling prejudices in the circles of the army of medical men, forty thousand strong, in the United States, and of raising us in this respect to the standard of European countries. Even the conservative seat of learning, Edinburgh, has admitted women to medical studies. Paris has turned out a woman doctor of medicine, who will prove, I hope, none of the least ornaments of this Society, the profession of this city, and our common country. Russia can boast already of her Kaschewarowa, and will within two years permit any well-educated and sufficiently-prepared woman to enter the halls of medical learning; and Switzerland, little but republican Switzerland, enjoys in its University of Zurich the presence of dozens of female medical students. I say "enjoys," for it has been a matter of public congratulation on the part of the professors of that institution that, since the admission of the females, not only has the university gained a number of hard-working and successful students, but that, besides, the general bearing of the students of the stronger sex has been more quiet, sedate, moral, and studious.

The question whether females shall be admitted to the study of medicine, in the existing medical schools in our country, will be solved in time. It appears improbable, for several reasons, for the present. The standard of many of the young men entering upon the study of medicine, as far as preparatory studies are concerned, is so little elevated, that the schools will not lack sufficient numbers of students; for it is true, that ours is one of the liberal professions in the Union which does not consider the previous acquisition of a classical or literary education a *conditio sine qua non.* And further, as long as new institutions, worthy and unworthy ones, male and female, are daily chartered, upon the recommendation of lay members of State Legislatures, the increase in number of special colleges for females can be continued *ad infinitum.* Thus it may happen that, for some time to come, this question of admitting females to our medical schools for male students will not be very eagerly ventilated, as its practical necessity may, to many of us, not be very obvious. Still, let it not be forgotten in the history of this Medical Society of the County of New York, that we have opened our doors to worthy members of the medical profession, male or female, white or colored, and thus granted reality to the gospel of American citizenship, the Declaration of Independence, according to which we are all free and equal. Let it not be forgotten, either, that we, in our circle, have generalized and idealized the peculiarly American proverb, "Help yourself." Emancipation, both of color and sex, means nothing else but to universalize the postulation of helping one's self. The future constitution of an ideal human society will be such that every member will take such a place, fill such a position, as is both adapted to his or her taste, and adequate to his faculties and services. The choice of a calling will depend on the first, the recognition by society, position, on the latter. That is the meaning of "help yourself," which never excludes that everybody else should also help himself, nor renders the helping each other impossible. On the contrary, the very existence of human society in general, and this republic in particular, is based on the liberty and independence of one and all. Monarchs and oligarchs only claim liberty and self-destination for their sole persons and systems. I wish we, in our political and social system, and in the institutions of our private and scientific circles, might forever bear in mind that we have always been the banner-bearers of universal liberty; and that, if the public opinion of Europe, greatly influenced by a monarchical and antirepublican press and office-holders, has frequently thrown the "help yourself" into our faces as a reproof, we mean and meant to help ourselves and each other, and them also! We need not sacrifice truth and modesty to spread-eagleism, when we point to the facts of our Sanitary Commission, our Chicago, or to the hungry of their Ireland, or the wounded and starving of their Germany and France. And, from this general mode of viewing all questions of great importance concerning the requirements of progressive development, we have, all of us, coöperated in solving the woman question in our department, no matter whether we have all been equally enthusiastic in deciding it, or whether we have simply followed the dictates of our longing for justice or equity, or resolved upon giving every member of human society a chance to develop his or her faculties, on their own responsibility as to failure or success.

After all I have said, I think I am justified in asserting that we have progressed in the right direction, in the acknowledgment of equal rights and universal solidarity, in a truly republican spirit. Besides, there is one progress we have made which is too evident to be overlooked. We have done our share, we believe, in teaching each other by papers and discussions. We have commenced, besides, to stimulate scientific researches. Hitherto, we must confess, the sun of science has risen for us, and mankind, in the far East, in Europe. She has the advantage of longer centuries, stored-up knowledge, hundreds of seats of learning, which are not schools in which a special branch of science and art is taught, but universities of science. She has her large museums, libraries, and collections. She has, which is more important than any thing else, a general base of thorough elementary and either literary or classical education previous to a special course of professional training. She has the advantage of the habit of study and thinking. In Europe, the universities, as they have the office of finishing the sixteen or twenty years' school education, have also that of advancing science *per se*. The great works of literature, general and special, the classical results of combined observation, learning, and thinking, have made their appearance from the laboratories, the cliniques, the libraries of European universities. The solutions of many grave questions we owe to the prizes established, judged, and crowned by them. If we compare the four hundred pages of the "Report on Education, by John W. Hoyt, U. S. Commissioner," as contained in the sixth volume (1870) of the "Reports of the U. S. Commissioners to the Paris Universal Exposition, 1867, published under direction of the Secretary of State, by authority of the Senate of the United States, edited by William P. Blake, Commissioner of the State of California," we shall admit the fact that we have good schools, but no European universities. Especially the task of advancing medical science, of stimulating strictly scientific researches, which our medical schools cannot fulfil, must, with us, necessarily fall upon the medical societies. Now, from this point of view, our Society, in my opinion, has, by approving of and authorizing a prize on a strictly scientific subject, which will require researches of a laborious and partially novel kind, begun a new era in the efficiency of medical societies, and proved her earnest appreciation of her position in relation to medical science.

Thus we have commenced to work according to the duties of every medical society, and the profession in general, in a number of important directions. There are, in fact, but three views which can be taken of the work, the actions, the duties of the medical profession—but three different relations.

The first two I have cursorily spoken of. They are the relations to the members of the profession itself, that is, to themselves, or each other, and to science. A thorough review was not expected this evening, nor was it in my plan to dilate on them. The third highly-important question, that of our relation to the public and to the political community, I expected to discuss this evening. I meant to discuss our responsibility to the public, and in connection with that the raising of the standing of the average practitioner, in the interest both of the profession and the public.

The relation of medical science to almost every branch of civilized life is self-evident. I do not speak of the care of individual disease; its importance as a duty of the medical man is understood by the lowest degree of intellect. I do speak of the whole province of hygiene and social science. Protection against epidemics, supervision of the sale of medicines, medical and in part also physical care of the poor (sick or well), coroners' department, supervision of dead-houses, public and private institutions for the sick, orphan asylums and foundling-houses, the condition of cemeteries, measures against syphilis, sanitary inspection of schools with regulation of hours, subjects of teaching, condition of school-benches, supervision of factories, of prisons, are duties which form the natural province of a well-informed medical profession. I go further. Part of the humane jurisdiction of the future will form a portion of the domain of the philosophical physician of the future, which will not leave the plea and the proof of insanity, or total or partial responsibility of the accused, in the hands of a shrewd or blustering solicitor, or to the discriminating minds of twelve men whose only claim to sit as jurors sometimes consists in that they could find no excuse for staying away. I also meant to discuss the difference between a trade and a liberal profession, and the relation of both to the public and the political community. Also, how it happens that, when a man is out of coats, he goes to a tailor; out of shoes, to a shoemaker; when his watch is broken, to the man who knows something about watches; when he is out of health, to a seller of nostrums, to a clairvoyant, a medium, a grandmother, a neighbor; how it happens that, when his horse is sick, he will send for the horse-doctor; when his child is sick, for the priest, the school-madam, the auntie, or Mrs. Soothing Syrup. How it comes that, when a portion of his money is at stake, he goes to a lawyer whom he knows to be informed about the laws of the land; when his health is failing, to somebody who knows any thing but the laws of his body. How it is that there are laws against coining false moneys, coining checks, coining false pretences to obtain money, laws protecting your pockets, but no laws protecting the health and life of the community, of the very people who make the laws of the land; how it is that this criminal carelessness and ignorance on the part of the public and the lawgivers have contributed to demoralize even the ranks of a liberal profession, and to impede their progress; how, that this profession, usually upbraided, made light of, neglected, has always taken the initiatory steps to protect the health and lives of the public, almost against their wishes and remonstrances, enforced the laws of hygiene, diminished mortality, lengthened the average duration of life, and improved the means of protecting the community, when their services were thought by them as superfluous as they were life-saving? I also meant to speak of the mode in which, in my opinion, the constant cry among our own ranks, for elevation of our profession, in the interest of the public, could be satisfied. I am sorry I have to simply announce this subject of medical education and practical reform for some other occasion, as I have felt compelled to lay a subject before you, which appeared to me so urgent that I could not but present it at once. It is of such particular urgency because of immediate practical importance; and at the same time has claims to the attention, and study, and coöperation of every physician.

A few mornings ago, I learned, through my newspaper, of the opening of a new lying-in asylum and foundling hospital. The number of such institutions begins to swell; the interest of the public is aroused, money is freely forwarded, and the lay and professional members of the public are thoroughly aware of the necessity of saving infant lives. I shall not here discuss the questions whether an effort should be made to save abandoned children, or whether the effort to save abandoned children will encourage crime. I shall simply try this evening to contribute my share to answering a third question of an "appeal in behalf of the New York Infant Asylum," which met my eye but a few days ago—the question, Can these children be saved? I shall also partially answer the question, What has been done for them in New York City? How many have been saved? If many, why change the old plans? If few, why not make a radical change?

I cannot answer the question to my entire satisfaction; for, to do so, the most exact and positive statistics on all institutions would be required. Now, part of the necessary information is found in the general literature on the subject. Part of the statistics concerning New York, however, I published some time ago, but have not taken particular pains to give them a wide notoriety. Still, some medical journals have republished part of my statements. The subject, however, is too important to be dealt with in a superficial or supercilious manner. Therefore, I shall give to-night what I have, and for what it is worth.

About a year and a half ago I read to the medical board of the Infant Hospital, Randall's Island, a report on "the raising and education of abandoned children in Europe, with statistics and general remarks on the subject." It was printed in the minutes of the Commissioners of Public Charities and Correction. A few hundred copies, which they liberally placed at my disposal, I distributed among the medical journals of the country, such persons as I knew to take an interest in, or have a connection with, foundling or infant institutions, and a number of medical gentlemen. I now believe it was a false professional pride that induced me to withhold my essay from the secular papers and the public on the ground that it was but a report to my colleagues of the medical board of the Infant Hospital, and because I was, perhaps, too deeply impressed with, or wrongly influenced by, the rules laid down in the best intended, but in some respects rather *naïve* and unpractical law-book, the "Code of Ethics." I now believe that the public and the papers had a claim to possess that report, and that, if its contents had been thoroughly ventilated in the press, public opinion might have been, before this, corrected to a certain degree in regard to the best means of raising abandoned infants. For I had enjoyed unusual facilities. Not only had I several months to spend on collecting the necessary material, during the summer of 1869, but the authorities, both public and professional, of England, France, Germany, and Austria, aided me in my endeavors, and a great many otherwise inaccessible, never-printed statistics have been copied by or for me; not to speak of a manuscript volume on the foundlings of Bohemia, handed over to me by one of the best authorities on that subject, Prof. Ritter von Rittershain, of Prague. My mistake in not giving the report the publicity it ought to have had was, however, in part corrected by some medical journals, in part by a paper read before the Social Science Association in Philadelphia, by Dr. Parry, who has deservedly earned the thanks of the public for his dealing with a number of questions, both social and physical, of the foundling question, in accordance with the statistics and results of my little book, to the extracts of which he adds valuable information on the city of Philadelphia. Thus I cannot complain of my work having been in vain; still, the most important question appears not to be as yet answered sufficiently to the satisfaction of the public at large. This question is, Ought children, or rather ought infants, to be raised in public institutions or in private families? In the city or in the country? By wet-nursing or artificial food? Of this latter I shall not speak, because in theory everybody agrees, if it were only on philosophical or religious principles, that babies ought to be raised on breast-milk.

Thus, the question is practically reduced to this: Is it desirable to collect infants in an institution, combined or not with a lying-in establishment, there to raise them. Is it preferable to farm them out to private parties? Is it preferable to take the intermediate road, and divide their numbers up in a number of country cottages?

The first plan, to gather and raise infants in a public institution, in a large city, commends itself at first sight. There is a large, commodious building, facility of getting the necessary help, kind-hearted superintendence, the proximity of city comforts and medical attendance; there is a large number of people whom you expect to call upon for generous contributions. The idea suggests itself, also, that a lying-in establishment ought to be combined with every such institution destined to receive foundlings; for the saving must be immense when the baby is taken care of, before his birth, by the same kind hands that are to fondle him afterward. Let us, however, do away with this plan at once. Every medical man knows it to be a fact that a lying-in asylum will generate diseases more than any other hospital, and infect the babies. Every man with hospital experience has observed it, and every general practitioner has met with puerperal diseases in women, and severe affections of the new-born, at the same time or in the same houses. It is not long since Lorain published a volume on the "Puerperal Fever of Women and Infants." I have myself noticed the fact that when in a public institution, of which I shall have to speak, erysipelas appeared among the children, one of the houses, which was by far inferior to the other from a sanitary point of view, was entirely free of the disease, while the better house was infected with erysipelas among the children, only because part of it was used as a lying-in establishment. I wish those gentlemen and ladies who think of new institutions of the kind, would consult the physicians with whom they have the good luck to be connected, and with whose recommendations they are always eager to go before the public; I also wish the medical gentlemen would forward their knowledge, their opinion, their judgment, although not asked for, for the benefit of their friends who work, to the best of their knowledge, on the impulses of their hearts. These friends of theirs cannot, however, ask for advice about special questions, as they know nothing about them. It is impossible to ask questions without a certain knowledge. This knowledge about the absolute danger of combining large lying-in hospitals with foundling or child's institutions they have not. Therefore, I urge upon the medical gentlemen to warn their friends and the public against a step attended with positive danger to the infants whom they mean to benefit. The question and its answer is such an old one, and has been answered so often and so uniformly, that it ought no longer to be necessary either to put or answer it. Even where there are large foundling institutions and lying-in establishments in the same city, in Europe, they are no longer found under the same roof. In this particular department of raising babies it appears as if the arts of printing and steam-shipping had never been invented. The experience of all Europe, so dearly bought, so bitterly complained of, goes for naught here. We mean to make our own sacrifices, have our own victims, mourn our own losses, do as badly as any by-gone century, because we do not take the trouble of profiting by the experience of the Old World. A lady at the head of a large new institution of the kind in this city has told me herself that she knew nothing about the results of the different modes of raising babies in Europe. Let, then, the medical men in good standing, large practice, and social connections, who may be asked for advice, or whose information will be thankfully received by the generous planners of new institutions, protest against the combination of lying-in and foundling hospitals. By so doing, they will counteract the empiricism which in so many instances has been the curse of our political and social institutions.

Next in order is the question whether babies ought to be raised in the country or city.

One reason why infants should be raised in the country, even under equal circumstances, is the statistical fact that they will thrive better. Of 100 children born alive, there died before the fifth year:

		In the cities.	In the country.	Difference.
France	1853–'54	35.69	28.56	7.13
Holland	1850–'54	36.25	28.90	7.35
Sweden	1851–'55	38.86	24.50	14.36
Denmark	1850–'54	29.66	22.68	6.98
Sleswic	1845–'54	27.42	23.42	4.00
Holstein	1845–'54	29.92	25.29	4.63
Saxony	1847–'49	39.88	36.22	3.66
Hanover	1854–'55	28.70	26.47	2.23
Prussia	1849	36.02	29.47	6.55
Average		33.60	27.28	6.32

Of 100 deaths, of all ages, in England, there were—

	Up to the end of the second year.	Up to the end of the tenth year.
In all England	31.58	44.91
Cities with 100,000 inhabitants or more.	35.12	51.39
" " less than 20,000	31.49	46.79
Manufacturing country districts	35.36	45.90
Agricultural districts	24.33	35.40

Mr. Husson urges even the shortening of the preliminary stay of the foundlings at the central depot, although a number of wet-nurses are kept there, and wants them transferred to the country instantly.

Last in order, not least, is the question where foundlings ought to be raised—in institutions, or private families.

Places inhabited by many can never yield an atmosphere as fit for breathing as well-kept private residences. Moreover, young infants, in consequence of their delicate constitution, and their not producing vital warmth by physical exercise, are confined to the house and room during the greater part of the year and day. Besides, offensive admixtures to the atmosphere of rooms in which many children are living cannot be avoided. Even the institutions in which adults are kept, suffer from the same influences, to such an extent that not unfrequently the very entrance into such a place is a guarantee of imminent disease, and portions of hospitals have sometimes to be closed. Alvine discharges and urine contaminate the air of infants' wards to a considerable degree. From this source originate the numerous cases of poor sanguification, and of constitutional diseases, such as rickets, scrofula, etc., even typhoid fever and scurvy; from this source comes part of the really immense mortality of foundling hospitals. Whenever the attempt is made to correct this cause of disease and death, you will find that this attempt is punished at once. Ventilation is never complete except by opening windows. To relieve the wards of their unbearable stench—I advise you to visit a large, fine-looking, whitewashed, clean ward in a foundling hospital, in a Nursery and Child's Hospital, at 6 A. M.—you open the window, and in come the enemies of mucous membranes — intestinal catarrh, entero-colitis, bronchial catarrh, pneumonia. Of 88 deaths in the Nursery and Child's Hospital which I shall specify to you, more than 40 are due exclusively, or partially, to pneumonia. These facts have been the causes of the universal changes in the rearing of the infants left on the hands of society in all Europe. At present, the former foundling institutions are nothing but depots for temporary admission, and speedy distribution about the country.

There may be drawbacks also, as far as private boarding is concerned. But, where, in such an individual case, or a number of individual cases, changes are required, they are easier to make than in institutions, which as a rule are more than comfortably filled.

Even if the feeding is the same in private boarding and public institutions, the results are more favorable in the former category. That a baby should live and thrive on artificial food, in a private family, is by no means a rare occurrence. Every attentive person, every medical man, has ample opportunities for such observations. That, however, bottle-fed babies in a public institution should survive, is a rare exception. In the wards of infants' hospitals everywhere the receiving of a baby in the purely bottle-fed department is acknowledged by all as amounting to a sentence of slow death. Moreover, the only article of food without which a baby could not be kept alive, viz., milk, can be more readily and more regularly procured by the poorest countrywoman than by the richest and most circumspect institution.

Besides, the nurses of institutions having charge of a number of infants at once, by day and by night, are very apt to, and surely will, lose the self-sacrificing patience and the everlasting attention which are absolute requisites for the sustenance of a young human being.

A task that requires all the holy instincts, the self-immolating, restless care of maternal love, is left sometimes in the hands of corrupt, lazy, whimsical, or malicious women, who make it their business to neglect their business, and are womanly and motherly only as far as they are so anatomically. It is much more probable that the poorest countrywoman who takes charge of a society's child, under the superintendence of the proper authority, under the eyes of her neighbors, and with motherly feelings developed in the poorest one bound in marriage and family ties, will succeed in saving a nursling from certain death.

I have spoken of superintendence. It is necessary and must be close. Of the infants sent out by the "general office of nurse children" under M. Husson, at Paris, and closely watched, the mortality under a year is 17 per cent., viz., but one per cent. more than the average mortality of the same age in all France.

Those placed out, on the same conditions, by private offices, and not watched, yield a mortality of 42.

Human nature is the same everywhere. The general results of not watching the parties to whom children are confided, must be feared, if not expected. We could learn from the ladies of Berlin, Germany, how the united efforts of the public, especially of the ladies, can be made useful under the directing control of the official authorities.

The latter I prefer as a directing power. Society itself, the State, must be considered responsible for the life of every human being that can be saved. It is a duty, not good-will; it is good policy, as I have proved in my report, to practise charity. Human society has committed both a blunder and a crime, when a member that could be saved, physically, suffers death; when a member whose soul and heart might have been kept pure, will sin.[1] How is it with us in many instances? A party of ladies or gentlemen favor the idea of founding some institution. They ask for contributions. Sometimes they will contribute themselves, although they may not expect to read their names in the newspapers with the amounts attached. They erect large buildings, which they cannot pay for, or receive patients whom they have no

[1] Of the whole population of the countries of Europe, according to Wappaeus, 33.66 per cent. are below fifteen years of age. Thus one-third of the living are consumers only, while they produce nothing at all. Between fifteen and twenty years, when most individuals are still unproductive, very many still preparing for their vocation or trade, are 9.72 per cent. But 48.88 per cent. are between twenty and sixty years, the period of activity and work. Between sixty and seventy years, a period of life which is almost unproductive, are 4.92 per cent., and beyond that age, where unproductiveness is the rule, there are 2.81 per cent. of the whole population. At all events, nearly one-half of the population are consumers only, before they are able to repay society for the sacrifices the community has to bring in order to raise them and render them productive. Thus a sound political economy requires the continuation of life until and beyond the period of full and unbiassed productivity. Whatever life is thrown away before, is just as much capital thrown away. Therefore both social, moral, and political economy insist upon the protection of the lives of the newly-born and young infant. Humanity requires it, and common prudence commands the saving of a product after it has been called into existence, and has given rise to a waste of working power. Political economy need not be told that a mother who carries a child does less work than in normal circumstances. To waste the product after it has given rise to expense, which is equal to non-production, is a direct injury to national wealth and power. Every new invention in medicine and surgery, the forceps, vaccination, chloroform, have been so many means of increasing the national wealth by saving life.

But this is not the only consideration of importance. The lost life is a dead loss, but the raising of unhealthy children, or vicious ones, amounts to a constant injury to society, a perpetual malignant disease eating the marrow of the land. If, therefore, any means are resorted to to save the lives of, and providing an education for, the abandoned or orphan children, they ought to be sufficient, and amply so. If this duty is neglected, the punishment falling upon a community in particular, society in general, is but just. Neglect of either physical welfare or moral and mental education is equally dangerous.

Insufficient physical development, depending upon incompetent nursing, scanty or injudicious feeding, results in the raising of a class of persons sufficient means to support. Then, instead of paying from their own pockets, as they have followed their own hearts and imaginations, they ask for further contributions, they make people embroider, knit, and sew, and buy their own work, they make the public buy musical entertainments, for which nobody pays, ay, they make them dance. If all that is in-

whose presence in society is a dead weight and an eating cancer. Feeble men, crippled women, raised by insufficient measures for the bringing up of children, will require renewed efforts for their support on the part of society as long as they live. Thus capital is wasted on their being born, nursed, and supported. If they had never been conceived and born, it would have been better for society. As they exist, they have a claim on humanity. When they have facilities to work, society has a claim on them and will thrive through them; not otherwise. Thus raising the poor into healthy and robust persons is a direct gain.

If the moral and mental education of the same class of individuals is neglected, there is more than a mere probability of demoralization being the result. Public order is destroyed by such a population, and public means squandered. Means that were saved in the raising and educating of the babies, will be required tenfold to sustain houses of correction and state-prisons. In 1853 there were, in the bagnios of France, 5,758 persons. Of these 391 had been illegitimate children, and 146 foundlings. In the state-prisons, of 18,205 inmates, 880 illegitimate and 361 foundlings. And the same proportion holds good for all houses of correction. Of 1,300 Frenchmen, one was the subject of legal punishment, and among former foundlings one of 158. Thus, of the foundlings of France, eight times as many get punished by law as the average population.

Thus it appears that the most economical policy consists in raising and educating infants and children into physically and mentally healthy men and women. Money spent on them is easily saved in hospitals and prisons. There is but one excuse for a community for neglectiug the obvious duties toward the children and itself, viz., extreme poverty. Therefore where a special community has but deficient means, the whole people, society in general, ought to hold themselves responsible. Society in general is benefited either by or suffering from their constituent parts, and therefore the care of the individual is a matter of common concern. If there is any meaning in the principle of general solidarity, it includes the right of every individual to a healthy body and a sound edneation. The equality preached by early Christianity and the doctrines of modern social science agree perfectly on that point, and the shrewdness of political economists has arrived at the same conclusion.—(*The Raising and Education of Abandoned Children in Europe, with Statistics and General Remarks on that Subject, by Abraham Jacobi, M. D., Member of the Medical Boards, of the Infants' Hospital, Randall's Island; and of the Nursery and Child's Hospital, New York*, 1870.)

sufficient, the common enemy is attacked; the common enemy is the treasury, the people's money, given away less by ignorant or injudicious legislators than by unscrupulous lobbyists. Thus you will find, in the financial report of the "Seventeenth Annual Report of the Nursery and Child's Hospital, in the city of New York, Fifty-first Street, corner of Lexington Avenue, March 1, 1871," the statement that of $45,000 dollars spent in one year (rent not included), the house inmates paid about $12,000, the treasuries of the State and other authorities $24,000, and that private subscriptions and donations amounted to little more than $1,700 dollars. The balance was made up by the receipts of the great charity ball.

If, on further consideration, you discover that, *besides* subscriptions, donations, payments of inmates, and proceeds of charity ball, the treasuries of the people of the State of New York pay alone 30 per cent. more than the rate of sustaining the infants under the charge of the Commissioners of Charities and Correction, you will, I hope, agree with me in my conclusion that the State, that society, can work at a cheaper rate, and on a more uniform plan, than the dozens of self-constituted authorities. Altogether, you will find that the total cost of sustaining the infants of the Nursery and Child's Hospital amounts to more than double the expense of the Commissioners for the same purpose. I wish I could say that their successes were double as to general care, good food, clean wards, and mortality. Unfortunately, the high standards of food, wards, and mortality, are undeniable.

Now, in my opinion, if the expenses are to be borne by the State, the State ought to have the credit; for it has the moral responsibility toward the indigent and helpless, and the tax-paying public, who seldom learn what is being done with their money, or to what extent it will be given away. I remind you of the fact that the Legislature of last year decreed away nearly a million as their contribution to private or even sectarian institutions. I know even of instances where large sums of money, people's money, were spent for purposes altogether different from what they were asked and given for. Let me, however, return to another special subject of this paper.

The subject, when brought before an intelligent and knowing public, is so plain that it commands interest at once as one of the most urgent questions of the day. Therefore, the Medical Society of the State of New York passed, in its second meeting in February, 1871, the following resolutions:

Whereas, Humanity acknowledges the claims of every human being to life and to some degree of prosperity, and recognizes in every civilized country the right of every new-born to be protected and supported; and,

Whereas, Political economy requires the saving of a being which has given rise to outlay until and after it can become useful and repay the expenses incurred in its full development; and

Whereas, The moral constitution of society requires that every member of society should obtain a sufficient training of its intellectual and moral powers; and

Whereas, The mortality of infants, being large from natural causes, is three times larger in public institutions destined for the maintaining of infants than in the general infant population; and

Whereas, The Board of Commissioners of Charities and Correction, always willing to be guided by competent advice, and desirous of doing their best, have already had a report prepared for them suggesting changes and improvements in the raising of their infants:

Therefore, be it resolved by the State Medical Society to appoint a committee to investigate and report, in the meeting of 1872, upon the following subjects:

1. The causes of the fearful mortality of abandoned infants in general, and those in large public institutions in particular.

2. The reasons for the giving up of large institutions, and the success of the dispersing system for abandoned infants, in every country of Europe, where the preservation of lives was an object.

3. The causes of the unusually large infant mortality in the institutions in charge of either public or self-constituted authorities in New York City and State.

4. The plans and means for improving the condition of foundlings and abandoned children in New York City and State—

a. During their infancy, when they are most subject to disease and death.

b. During childhood and adolescence, when they require an education sufficient to make them useful members, and prevent them from becoming enemies of and dangerous to society.

As I am a member of the chosen committee, I have herewith offered a small contribution to the elucidation of the subject, and offer another one in studying the statistics of one of our great institutions which has been founded and supported for the alleged purpose of saving life. I select the Nursery and Child's Hospital, for the very simple reason that I know as much about it as about any other; in fact, my knowledge of the minutiæ of that institution you will find tolerably complete. I consider the statistics I lay to-night before you as but preliminary to, and part of, my future report to the State Medical Society. By them I mean to prove the absolute impossibility of raising infants in a large institution, a fact that has been ever so many times proved in Europe. The first communication I have to make, I beg permission to recapitulate from my report on "The Raising and Education of Abandoned Children in Europe," etc.

The *Nursery and Child's Hospital*, New York, under the management of thirty-five estimable ladies of the city, in which the infants are fed half on breast-milk, half on well-selected artificial food, a mixture so frequently and advantageously used in private families, exhibits in the records of 1870 the following facts: I take the liberty of adding at once, that I make use of limited statistics only, because up to March, 1870, the records have not been well kept. Since that period they have been kept regularly, as I, being one of the medical staff of the institution, know from personal experience. There have been, from March 2d to May 31st, 97 admissions, 20 discharges, 10 deaths.

The admitted nurslings were by no means new-born; in fact, very few belong to that category. Eighty of these admitted children had a total age of 367 months, averaging 4.5 months for each child at the date of admission. Seventeen of the admitted children were 2 years and over, up to 10; altogether there is a total number of 84 years for 17 children over 2 years, that is, an average age of 5 years. Of these 17, being of an age where the rates of mortality are always low, none died. Thus we have 10 deaths in 80 infants with an average age of 4.5 months at the date of admission, within a single quarter of a year. Further, of these 80 infants (from 2 days to 2 years old), admitted during these 90 days, 20 were discharged. The shortest stay was 1 day, the longest 68 days. The total days of these infants in the institution was 324 days for 20 inmates, that is, discharges took place, or were taken, in 20 cases out of 80, after an average stay of 16.2 days in the Nursery. Thus there are 10 deaths in 60 children of an average age of 4.5 months at the date of admission, within the 3 months following their admission. The average age is a little higher, because most of the infants who were discharged were very young, and have been counted in the grand total of ages. Now, if we grant that March and one-half of April are unfavorable months, we have to admit that May is favorable to health, that the winter months from December to February are just as untoward as March, and that the heated term of the summer is surely still more dangerous. Thus we may safely assume that the rate of the general yearly mortality in the Nursery is certainly about the same as in the mentioned quarter of March, April, and May; therefore the mortality through the year would amount to 40 out of the number of 60, or, if we mean to count the infants that got their discharges after 16 days' stay in the institution, out of 80 children who were admitted at an average age of 4.5 months. I prefer this latter figure for the following reasons of both justice and charity: The 50 children remaining, having grown a quarter of a year older, meanwhile, would, in the second, third, and fourth quarters exhibit a smaller rate of mortality, while those newly admitted would yield the very same mortality we figured above. Thus we can afford to count those 20 discharged ones with the rest. If in future the records are kept as fairly as in the last few months, we shall have facts instead of estimates.

Now, then, there are 10 deaths quarterly in 80 children, each one 4 to 5 months old at the date of admission. Grand total of 50 per cent. deaths yearly of children of 4 to 5 months and upward to 2 years.

Statistics prove that the mortality of the infants born alive, from the date of birth to the fifth month, is larger than that of infants between that age and 2 years. Of 3 infants who die before the termination of their first year, 2 are less than 5 months old, and one is between 5 and 12; and, of 31 who die before the end of their second year, 26 have not reached the end of the first, and but 5 die between their first and second year. Thus, of the above 50 per cent., 8 would belong to the second year, 42 to the first. They were admitted at a time of life when mortality is but half of what it is in the first months. Thus it appears that the mortality of the Nursery, if all of the admitted infants were new-born instead of being 4 to 5 months old, would be so appalling that I am glad I am not required to state its exact figures. The worst figures of the European foundling-hells of former centuries are not more fearful than ours, and, although being an officer of that institution myself, and believing that I and all the rest of us have conscientiously tried to do our duty, I cannot but testify and bow down to the truth, that, in spite of all the efforts of the medical staff, and the painstakings of kind-hearted ladies, the probability of the lives of children intrusted to a public institution is very slim indeed. The younger the children, and the larger the institution, the surer is death. Every story added to an edifice which is meant to be a temple of love is an additional hecatomb of the innocents. Modern civilization, planning for the best, but mistaken about the means, has succeeded in out-Heroding Herod.

These facts are sufficient to justify the abrogation of large institutions designed for the raising of young infants. The facts appear to show, besides, that older children (not a single death occurring in 17 of an average age of 5 years) bear up easily under the same circumstances that are a source of death to the infants.

In the same institution, viz., the Nursery and Child's Hospital, there were 41 births from the 1st day of January to the last of May. Of the infants, 4 were stillborn, 6 died, 23 were discharged, 8 remained in the institution to 1st of July. Those remaining in the institution on the 1st of July were all born in April and May; with a single exception, every one born previous to March 31st having left the institution, or died. The 23 discharged infants were in the institution 609 days, each averaging 26.5 days. Those who were born and died in the institution, lived altogether 274 days, an average life of 45.6 in the institution. Those 8 who remained in the institution on July 1st had lived, *in toto*, 340 days, an average of 40.25 for each of the 8. Thus, their average ages was not yet the average age at which those 6 died, nor were the ages of the discharged 23 much more than one-half of the average ages of those who died. The naked fact is, that of 14 infants (23 having been discharged) 6 died.

Now, if I add the fact that the women are well kept, the food is good and plentiful, medical attendance as efficient as the sometimes faulty method of appointing medical attendants in our public institutions can make it, and the whole institution under the assiduous management of thirty-five ladies belonging to the best society of New York City, I believe I am justified in concluding that a large institution is the very place that nurslings and infants ought to be kept out of. For, the poor tenements of our working-classes yield better results in their raising of infants than the large institutions the city might be proud of.

From the 1st of June to the 1st of October, 1870, 101 children were admitted to the Nursery and Child's Hospital. Their average ages were more than 1 year 8 months. There were 29 from 2 to 13 years old, and therefore beyond the principal ravages of fatal disease; 27 were removed, after they had been in the institution an average time of 20.4 days. Thus there remained 55 *bona-fide* inmates of less than 2 years. In that same quarter of a year there are 33 deaths recorded in the book of the Nursery and Child's Hospital.

It is important to know that, according to a list before me containing names, ages, etc., 18 more children, who were admitted before October 1, 1870, died after that day.

This frightful mortality of the inmates of the Nursery and Child's Hospital becomes more apparent by comparing it with the following statistics (of Report, p. 38): According to Farr, of 392,224 children born in England in 1867, there died before the end of their first year 65,464, viz., 16.69 per cent. According to Prof. Ritter, of Prague, the mortality during the first year of legitimate and illegitimate children born alive in 1855–1861 was 25.36 per cent. in the Austrian Empire. In Hungary, considered separately, in 1862–1865, 24.95 per cent. In Berlin, according to Chamisso, the mortality of all the infants born alive from 1816 to 1841 was 22.7 per cent. up to the end of their first year (33.5 of the third, 36.9 of the fifth). The rate was lower in 1842–1860; but in 1861-1866 it was 28.4 per cent.

These reports were written and printed, although—I am at this day ashamed to acknowledge it—withheld from the public at large in consequence of false professional pride—when the following newspaper article—one of very many—appeared on November 1, 1870:

The Infants' Home—a Worthy Institution—what it is doing, and how it is done.—Among the legion institutions supported by New York charity, few appeal with greater force to the promptings of humanity than the organization the name of which stands at the head of this article. Doubtless, one must watch intelligently its daily workings fully to appreciate the end it is designed to and does accomplish; but yet the most casual observer can indorse what the managers tell.

The histories which could make our annual reports intensely interesting must be as silent as they are sad. They say: "We often feel that it is not alone our prayers which have brought God's favor on our work, but the sighs and prayers of penitent sufferers have been answered by the constant stream of heavenly blessing."

There have been two large additions to the building. The Asylum, corner of Fifty-first Street and Lexington Avenue, since its occupation for present purposes sixteen years ago, and the last annual report, shows 371 children in its care, 100 of whom were born there, and the success of the lying-in wards has exceeded the most sanguine hopes of friends. To these come women—never without reference—some from poverty, some from shame and the world's cold scorn—for bodily and spiritual comfort and sustenance.

The interest of the State Legislature has been so awakened by representations of the board of officers that power has been accorded to take in many who from poverty and despair would have been driven to suicide or infanticide.

The histories and the mysteries of the place may never be written, as we have said before; howbeit, as an example and epitome of all, the reporter might speculate as best might be on the wan, joyless face of one young mother lying there recently, of whom nothing was known save inviolably to those in charge, if, indeed, there was a history, but the framed Scripture text at the head of her bed seemed a chapter of revelation to the visitor, reading, "Whom the Lord loveth he chasteneth, and scourgeth every son whom he receiveth."

We went to the children's school, for there is a school for such of the children as are large enough to attend, and neat copy-books were shown us with great pride by sturdy-looking boys, and little girls with roguish, happy faces. Their childish voices united in singing for our entertainment, too, and we were shown the first composition that had ever been written in the school—the *chef-d'œuvre* of little black-eyed Bertha.

Who could think unmoved of these tender ones, rescued, perhaps, from the tyranny of a drunken father, from the breast, earlier, of an intemperate mother, giving thanks that here they were safe and happy? The annual charity ball, in behalf of the institution, has always met with brilliant success, but its necessities increase with its sphere of usefulness, and the provision of a quarantine establishment for contagious diseases has wellnigh or quite absorbed the proceeds of the last entertainment, so that, if subscriptions and donations should not be forwarded to the rescue, the work of charity must be much curtailed. The officers the present year are Mrs. Cornelius Dubois, First Directress; Mrs. T. C. Doremus, Second Directress; Mrs. Henry Anthon, Third Directress; Mrs. E. W. Stoughton, Treasurer; Mrs. Algernon S. Sullivan, Secretary; Miss R. B. Hunter, Assistant Secretary. Among the active managers and honorary members are found the names of many that are "household words" where moral and physical want abound.

This article is but a specimen of many, and none of the most brilliant or affecting of its kind. How they are made up, on the spur of, and for the moment, we all know who read our daily papers, the result of many different brains, hearts, and pens. Maybe even those are right who assert that now and then lips of honey, cheeks of milk and blood, eyelashes of silk, hands of velvet, voices of silver, dollars of gold, and other organic and inorganic contrivances have a great influence on men, nations, and newspaper articles. At all events, when you compare a lengthy article in the same paper (the *New York Times* of November 12, 1871), you will find that in consequence of statistical data contained in my report on "The Raising and Education of Abandoned Children in Europe," and other facts credited to Dr. Parry, of Philadelphia, the placing-out system is pronounced the far superior one.

Let me, however, return to my statistics, or rather go on. First, let me direct your attention again to a memorandum copied from the records of the Nursery and Child's Hospital, which I here present. It contains the names and ages of 18 infants and children who had been admitted before, but died *after*, the 1st of October, 1870, that is, after my last report was made up. They range from 21 days to one year and 10 months. It also contains the ages of these 18 children at the dates of their deaths; they range from 2 months, to 3 years 3 months and 13 days. It further contains the causes of their deaths, which one by one read as follows: Diarrhœa, tuberculosis, diarrhœa and pneumonia, pneumonia and cholera infantum, diarrhœa and pneumonia, chronic pneumonia, pneumonia, pneumonia, marasmus, pneumonia, measles and cancrum oris, measles, diarrhœa and pneumonia, chronic diarrhœa, capillary bronchitis, pneumonia, entero-colitis, chronic diarrhœa, diarrhœa and bronchitis.

I have read this list to you, because, after hearing the preceding reports of mine, you might have been under the impression that there were not a sufficient number of children left in the institution from which it could be recruited.

Finis coronat opus. Let me continue:

According to the records of the Nursery and Child's Hospital, which appear to have been as well kept as those from March 1st to October, 1870, 117 babies were born in the lying-in department of that institution from October 1, 1870, to October 1, 1871. Of this number 69 were discharged within a short time after their births; most babies go out with their mothers within a few weeks, some remain a little while longer. The aggregate stay of the 69 little ones amounted to 108 months and 5 days. A month is always taken in my accounts as averaging 30 days; thus the average stay of each of the 69 amounts to 1 month and 17 days.

My information on one of the rest is not positive. I do not know whether James McAlister has been discharged or died. I have not counted him among the dead. Of the other 47 babies, who were not so fortunate as to get discharged, 27 died. Their aggregate ages at the time of their deaths were 69 months, or 2 months and 17 days per head. We have no means of knowing how many of the discharged 69 would have succumbed, if they had averaged a stay at the institution of 2 months and 17 days, instead of 1 month and 17 days.

Of the 20 who remain alive within the Nursery, 9 have been born in the last quarter of the year; 5, viz., 20 per cent., in the very last month. They have not yet averaged 2 months and 17 days. The future will show whether they are to be counted among the living, the dead, or the discharged. The list of the 18 diagnoses (more or less), confirmed by *post-mortem* examinations which I have read to you before, renders it doubtful whether all of them will live.

I wish every practitioner of medicine present in this hall to compare his own experience and statistics, among the rich and the poor, with these results obtained in the Nursery and Child's Hospital, where every one of the 47 has had its mother's, or, in some cases, nurse's milk. Of 47 new-born babies, 27 have died at the average age of 2 months 17 days, and half of the rest are not old enough to have reached this average.

You will now be prepared for some more figures:

Admissions of Infants and Children to the Nursery and Child's Hospital, October 1, 1870, to October 1, 1871, between the ages of one day and nine years one month:

October, 1870....	25,	with aggregate ages,	22 years,	5 months,	23 days.
November, "	22	"	35 "	11 "	27 "
December, "	21	"	27 "	7 "	27 "
January, 1871	22	"	14 "	4 "	4 "
February, "	18	"	23 "	4 "	26 "
March, "	17	"	38 "	11 "	9 "
April, "	21	"	30 "	10 "	8 "
May, "	17	"	24 "	8 "	3 "
June, "	19	"	37 "	0 "	3 "
July, "	27	"	37 "	5 "	8 "
August, "	26	"	43 "	1 "	25 "
September, "	18	"	25 "	7 "	13 "
Total........	253		361	9	26

Thus the average of 253 infants or children admitted in good health from October 1, 1870, to October 1, 1871, amounted to 1 year 5 months 16 days. I naturally lay stress on the fact of their health being good when they were admitted; for it is the rule of the institution that it should be so. This much is sure, that no child has died this year of a disease contracted before it entered the nursery. Still, so great is the liability of the inmates to fall sick in the institution, that the secretary of the medical board publishes, in the annual report gotten up in 1870, the unnatural fact that 2,000 serious cases of sickness occurred in one year, among 377 admissions; and, in that of 1871, over 1,400 cases of sickness, in 358 admissions.

Of the 253 admitted from October 1, 1870, to October 1, 1871, 128 were discharged within a short time after their admission. I will presume they were all in good health when they left the institution.

		Discharges took place.	Aggregate stay, in days, at the Institution.
October,	1870, ...	12	761
November,	"	10	755
December,	"	16	1,049
January,	1871, ...	12	393
February,	"	11	800
March,	"	10	666
April,	"	12	316
May,	"	8	274
June,	"	8	283
July,	"	17	848
August,	"	7	304
September,	"	5	132
Total.......................		128	6,581

Thus, the average residence of each of the 128, inside the Nursery, amounts to 1 month 21.4 days.

If you compare the enumerated discharges and admissions in the several months with the deaths, or if you will take the trouble to look over the record I have before me, you will reach the number ot babies remaining alive in the institution:

		Admissions.	Discharged since.	Died since.	Remain alive.
In September,	1871, ...	18	5	4	9
August,	"	27	7	3	17
July,	"	27	17	4	6
June,	"	19	8	7	4
May,	"	17	8	4	5
April,	"	21	12	9	0
March,	"	17	10	3	4
February,	"	18	11	3	4
January,	"	22	12	8	2
December,	1870, ...	19	16	2	1
November,	"	22	10	8	4
October,	"	25	12	12	1

After all, there were 125 *bona-fide* inmates, who stayed more than the average of 1 month 21.4 days. Of these died:

			Years.	months.	days.
October, 1870, ...	12	at aggregate ages	7	1	8
November, "	8	"	6	6	20
December, "	4	"	0	11	28
January, 1871;	8	"	3	10	0
Carried forward.....	32		16	28	56

			Years.	months.	days.
Brought forward	32		16	28	56
February, 1871, ...	3	at aggregate ages	1	8	1
March, "	3	"	3	0	25
April, "	9	"	11	10	2
May, "	4	"	2	6	4
June, "	7	"	8	11	12
July, "	4	"	3	1	19
August, "	3	"	2	6	27
September, "	4	"	5	9	21
Total.................	69		58	00	17

If you remember the meaning of the record of the 18, you will expect some more to die. Up to a fortnight ago, one, who was admitted at the age of 1 year 3 months 21 days, died at the age of 1 year 7 months, on November 19th, of pneumonia. Thus, up to this date of November 19th, there were 70 deaths among 125 healthy children, admitted to the Nursery. The average age at the time of death was 10 months 7 days.

The causes of death are attributed, in one case each, to croup, pleuro-pneumonia, entero-colitis and peritonitis, measles, pneumonia and croup, scarlatina and croup, diarrhœa and broncho-pneumonia, pleuritis, intussusception, broncho-pneumonia, atelectasis (child of 11 months 4 days), pulmonary tubercle and pneumonia, measles and pneumonia, diphtheria, pulmonary tubercle—in 2 cases each, to marasmus, hypostatic pneumonia, measles and croup, cholera infantum, whooping-cough—in 3 cases each, to tuberculosis, atrophy, measles—in 4 each, to chronic diarrhœa and pneumonia, diarrhœa and pneumonia—in 6, diarrhœa, 10 chronic diarrhœa, 13 pneumonia.

Of these 70 deaths, 18 occurred in children over a year, 52 in such as were less than a year old. But 3 of the former children had been admitted before they were a year old: viz., 2 were admitted at 10 months; 1 at 11 months 18 days. They died when they were 1 year 2 days; 1 year 17 days; 1 year 2 months 7 days old. Thus we arrive at a sum of 55 deaths among babies, who were admitted before they were 12 months old. A large number of them had reached nearly that age at the time of their admission.

But how many babies were admitted under a year, of whom 55 could die within the short space of time reviewed in this retrospect?

Of the total of 253 admitted, 42 were over 3 years, 30 between 2 and 3, 44 from 1 to 2 years, together 116 over 1 year. Of these 116, 76 were discharged in a short time. Of the remaining 40, 15 (18 less 3) have died within this limited time—a percentage, for the time being, of 37.5 among children over a year, very many of them over 2 and 3 years, and all of them intrusted to the Nursery in perfect health.

Of the 135 admitted at less than a year, 52 were discharged after a short period; 83 were left in the Nursery as *bona-fide* inmates. Of these 83, the number of 55 died within the limited period which is the subject of this compilation. The aggregate ages of these 83 at their admission was 377 months; the average, 2 months 23.8 days.

Thus it results that the *mortality of babies intrusted in good health to the Nursery, at the age of nearly* 3 *months*, within this limited period, is 66.26 per cent.

The aggregate ages of the 55, at the time of their deaths, including those 3 who passed their first birthday while in the institution, count up to 26 years 11 months 1 day; the average age of each to 5 months 26 days. As their average admission took place at 2 months 23.8 days, they lasted 3 months and 2 days each in the institution.

Some questions submit themselves very readily:

1. What will happen to those who have reached, like the dead, the end of their sixth month by this time, and will stay in the institution to the full end of their first year? For the average ages of those 18 above mentioned, who were admitted before October 1, 1870, and died after my former report was made, amount to 10 months 6 days.

2. Was it fortunate, or not, for the 128 discharged children to stay but 51.4 days in the institution, as the time averaged between admission and death is 3 months 2 days?

3. What is likely to become of the 20 living babies born in the place, and remaining at the present time in the institution, provided their stay is extended to the end of their first year? On the 20th of November their average life was a

trifle more than 6 months, and up to that period 27 out of 47 (57.45 per cent.) had died.

4. If 66¼ per cent. perish among healthy infants admitted, as those of the Nursery, at an average age of 2 months 23.8 days, what would be the percentage if the babies were admitted at birth, under the same circumstances?

To facilitate the answer to this latter question, I beg you to compare the statements laid down in a table contained in my "Report on the Raising and Education of Abandoned Children in Europe," New York, 1870, p. 29.

Of 100 newly-born infants died in

	Belgium, 1840–'50.	Holland, 1848–'53.	Austria, 1851.	Sardinia, 1823–'37.	France, 1853.
0– 1 month	5.18	4.70	10.96	11.14	6.60
1– 2 months	1.76	2.29	2.55	1.87	2.85
2– 3 "	1.27	2.09	1.96	1.43	
3– 4 "	1.08	1.91			
4– 5 "	0.86	1.48	3.42	2.51	2.39
5– 6 "	0.76	1.19			
6– 7 "	0.72	1.77			
7– 8 "	0.66		2.40		
8– 9 "	0.66	1.42		4.89	3.15
9–10 "	0.65				
10–11 "	0.63	1.29	2.78		
11–12 "	0.80				
0–1 year	15.03	18.14	24.07	21.84	14.99

From this table, which has been taken from official documents, it is evident that the mortality of babies who have reached the end of their first quarter is but one-third or one-sixth, for each following quarter, of what it would be from the first hour to the end of the third month of life.—The four above questions are herewith submitted to your consideration.

I think I might go on *ad infinitum* with the practical conclusions. I want to draw but one conclusion, viz., *that the attempt to raise babies in great institutions, even with large means to aid you, cannot be justified; that these institutions must be given up, and reserved for other purposes, and that the only system worthy of being sustained is, to place the children out with private parties.*

And now let us, for a moment, examine into the expenses of large institutions like the Nursery and Child's Hospital.

On p. 12 of the Seventeenth Annual Report, under the heading of "Financial Report," you will find the expenses between March 1, 1870, and March 1, 1871, laid down at a little more than $75,000. Of these I deduct at once $30,000 for "temporary investment," "part purchase of country hospital," and "furnishing and support of country hospital." Balance, $45,000. As repairs and insurance are counted up with more than $4,000, I estimate the rent of the immense buildings at $20,000 only. Thus I take $65,000 as a fair, or rather low, average estimate of the whole sum spent for the benefit and support of 253 admitted children, and 117 lying-in women with their infants. *They are the only beneficiaries*, for the mothers taken in with, or in behalf of, their nurslings, and the wet-nurses, cannot be counted in this class, any more than the matron, the ward nurses, or the domestics.

Those beneficiaries did not stay in the institution through the whole year, but a very small part of it only. The aggregate stay of the new-born, who were soon discharged, amounts to 8 years; those 27 who died at the average ages of 2 months 17 days, to 6 years; those 20 who remain after the close of the year (October, 1870, to October, 1871), to 8 years. The aggregate stay of the 128 children who were admitted and soon discharged, to 18 years; of the 125 who are dead or still alive, to 60 years. Total, 100 years. The aggregate stay of the pregnant women who were confined in the institution may be set down at 20 years. Thus $45,000, without rent, or $65,000, rent included, are spent on *a year's board* of 100 children (the new-born included) and 20 adults. Said board averaging the sum of about $400, rent not included.

How nearly correct this estimate is, you find corroborated by the fact that the sum of about $12,000 is credited as "house income" in this year's financial report. Our summing up would average a yearly board paid by the inmates of $100, or a monthly one of about $8.00, which is almost the very figure (a little less) of the average board paid to the institution.

While I remind you of the fact that my figures cover the time from October, 1870, to 1871, and the report alluded to the time from March, 1870, to March, 1871, and that, therefore, trifling differences may be found, you will still find a few of the items in the expenses highly interesting.

The 120 annual boards required in round numbers: $25,000 for provisions; wages amounted to $4,000; stationery, printing, and collecting (of $1,195 "subscriptions," I suppose), to $625; wine, brandy, drugs, and surgical instruments, $1,800.

Let, however, these figures suffice. He whom they have not yet convinced of the truth of my statement, that large institutions, no matter what their means are, will destroy their infant inmates, may, perhaps, change his mind on still further investigation. At all events, it will prove a difficult task to trace the fearful mortality, of the institution I have spoken of, to radical faults in the manner in which it is conducted. I do not think there are many shortcomings in the administration of that institution, which will not be found in all carried on upon the faulty principle of accumulating large numbers of infants under one roof. Still, it must be said that institutions under dozens of managers labor under unusual difficulties—never thrive well. There is always something meddlesome, fidgety, inconsistent, incongruous, in large numbers; nor is the transaction of business by a ring, if we are well informed, cheap or expedient; nor can we presume that, where less special knowledge than ambition and theoretical love is brought to bear upon a serious task, like that of conducting an infant asylum, the results are surprisingly favorable. I say "theoretical love;" for, where a board of several dozen of managers in New York City cannot command more than seventeen hundred dollars' worth of "subscriptions and donations," I dare say that love requires more practical illustration.

Old Homer says that a government of many heads does no good. He wants one master. Perhaps he thought of infant asylums. The improvements effected in the management and mortality of the Infant Hospital (Randall's Island) by the intelligent administration of a single medical officer with his subordinates, under the control and in the pay of the Commissioners of Public Charities and Correction, speak for the advantages of special knowledge and a uniform plan.

Let me, then, again urge the fact that large infant asylums will destroy children.

When this fact became known, many experiments were made of distributing infants over a number of places, the so-called cottage system. Six, ten, twelve, were kept in a small separate institution. The disadvantages are plain. The increased number of households raises the expenses, the difficulty of obtaining wet-nurses increases, control and medical attendance become more and more difficult. The cottage is, in fact, not much, if at all, better than a ward in a public institution.

What, then, is left but to board out the infants in the country? For, although the experiments of the Catholic foundling institution in Waverley Place are by no means so bad as the experience of twenty years ago rendered probable, common-sense, hygienic principles, and statistics, point to the country as the residence of the children of the Commonwealth. When this conclusion shall be the conviction of all, the necessary steps will be taken, no matter how great the difficulties may be. With us, they are not small.

Our population adjoining the great cities, especially New York, is not so large as in Europe, and is not so poor. It is is not of such vital importance for a country family to avail themselves of the trifling subsidy paid for the infant boarder. But there are some considerations which are to be taken into account. The first is, that the infants we have to care for do not count by six or ten thousands every year; and the second, that the sum which is at present spent for every infant under the charge of the Commissioners of Charities is by no means a trifle, and, under the Managers of the Nursery and Child's Hospital, enormous. It would be found, on trying, that the apparent difficulties in procuring proper country homes for our infants would by no means be so great as they may appear at first sight. Even if there were some in the beginning, we should always gain.

The question whether it would be desirable to leave, if possible, the young illegitimate child in charge of its mother, cannot be answered in a manner uniformly adapted to every case. The facts exhibited by the Munich records, according to which the children reared by their own mothers have a fearfully larger mortality than those intrusted to strangers, do not look encouraging. In our city, I am afraid that many of our unmarried mothers would not prove excellent nurses. Still, the fact of their being sufficiently supported might change the circumstances.

There is another consideration not to be lost sight of. Unfavorable though all circumstances be within the walls of an institution, mortality can be reduced by procuring paid wet-nurses for the same. We know that our nursed infants thrive much better than the bottle-fed. But no breast-milk is obtained except from those who have no home, the poorest, and most miserable. No married woman, as a rule, at least none who has the slightest means of escaping the discipline of, and submission to institution rules, will ever consent to become a wet-nurse to any of our children. Thus we have to take either the sickly, the profligate, the very poor, or consider ourselves very fortunate when we succeed in securing the own mother's breast for the support of the infant. Many mothers, however, who have a home in the country, have lost a young baby, or have milk enough left, after weaning, to nurse, or enough to nurse two, but who would never consent to leave their husbands and children, could be induced to take charge of an infant. A careful comparison of the direct expenses of the two modes of rearing infants, out of, and in asylums, in Europe, has proved that even there no pecuniary loss is incurred by the more advantageous and humane proceeding.

Besides, the nurses necessary for the infants in institutions are just so many nurses kept out of the service of the general public. In New York City, wet-nurses are scarce, since the humane efforts of the Commissioners of Charity, and the Catholic foundling institution, have been directed to the task of supplying our foundlings with human milk. Thus it is very probable that what society gains on one side, in the saving of the destitute and poor, is lost among the public in general. At all events, such element of proper food as is accessible at its own home only, that is, breast-milk of the countrywomen, is left unavailable and unused.

If not absolutely necessary, no attempts at obtaining breast-milk ought to be made within the limits of the city. Besides the other damaging influences of city life and city atmosphere, which alone destroy so many infants' lives, the experience of former times, of boarding the city's infants within the boundaries of the city, has been very unfavorable.

It is not my intention to go into the particulars of administration, at this moment. Still I beg the privilege of pointing out a mode of action which in some parts may prove faulty, but which under our circumstances will, in my opinion, prove sufficiently correct to enlist sympathy or bring out a discussion. Before so doing I again refer to my opinion on the responsibilities and duties, and the rights, of the State. *The whole administration of the foundlings ought to be controlled by the Commonwealth. Private or sectarian establishments ought to be under governmental supervision; ought not to be supported or aided by the State, but not interfered with so long as their successes and general management appear satisfactory; the department of the foundlings to be centred in one office; the necessary appointments of the head, or heads, to be made by the Governor of the State.*

The expense of boarding the foundlings to be borne by the people of the State of New York.

By concentrating the administration, the running expenses would be but small in proportion. New York City would have a single depot for the abandoned children, from which speedy distributions would take place. The large buildings at present dedicated to the purpose of raising infants would soon be required for those children who would be returned from the country, after reaching the age of 3 or 5 years. Some might become hospitals—we have no child's hospital in New York City—some schools and asylums for the older children of the community, where they would be taught to become useful citizens of the republic.

I assume a mortality in the first year, say, of 25 or 30 per cent. of infants abandoned in their first year. After that time, the mortality will become small. Of 1,000 abandoned infants, 750 or 700 must reach in future their twelfth month. I assume $150, the amount spent by the Commissioners of Charities and Correction, to be a fair average for yearly board. Thus 1,000 abandoned infants would cost the State, per annum, say $120,000. The 3,000 lives endangered or thrown away every year might cost us $350,000 yearly; but then we should certainly succeed in saving most of them, at a proportionately small expense, and educating those many who have been saved.

The first steps in this direction would be to awaken the interest of the public, particularly in the country. Sympathy and interest must be stimulated contemporaneously. The printed minutes of the Commissioners of Public Charities and Correction, of last year, contain a proposition to make preparations for boarding out babies, submitted by the Medical Board of the Infant Hospital, Randall's Island. The Commissioners have, I believe, deferred further action only in consequence of the necessity of keeping up all their numerous charities, and from their fear of not being capable of meeting a momentary increase of expenses. But lately a paper was prepared, which was intended to be circulated over the signatures of the Commissioners, containing similar propositions. It is but justice to say that I have been told by them, this very day, that they considered the publication impracticable at that moment, but approved of and indorsed all its contents. I have requested and obtained the permission to read it, and abstain from any remarks or additions. It is written in the form of a letter, which was to be distributed among such persons as are mentioned in it, and, with its remarks and suggestions, will explain itself. It reads as follows:

Dear Sir: Your special attention is herewith directed to the claims of a class of destitutes who, as they are helpless, are the more deserving of the sympathy of the just and benevolent. In their behalf the Commissioners of Charities and Correction have tried to improve the methods of supporting, raising, and educating, have built costly edifices, and gladly availed themselves of any advice their medical boards could afford them. Still, the results of their efforts are far from being satisfactory, and, after careful consideration of the difficulties to be overcome, and the aims to be reached, the undersigned request you to give your attention to the following remarks, and to lend your valuable aid in furthering their endeavors.

The class of destitutes in question are the foundlings and abandoned infants, amounting to the number of about three thousand a year in the city of New York. Their claims have been so well acknowledged of late, and the public at large have become so conversant with the humane and political aspects of their case, that a number of associations have been formed for the purpose of either raising them or educating those who survive.

From a report laid before them by the medical board of their Infant Hospital, which admits yearly about 1,200 or 1,400 of these destitutes, we gather the fearful and embarrassing fact that infants collected in large institutions, of the best hygienic designs, with the most careful dietetic and medical care, will die in large numbers. This immense mortality is particularly great in earliest infancy. Of 47 deaths in New York City under five years, 39 occur under two years, and as many as 30 under one year; the mortality of abandoned children under the charge of public or private authorities is still larger. The very accumulation of infants under one roof, the scarcity of breast-milk obtained, the difficulty of securing competent nursing for a large number of infants, the ravages of contagious diseases, the poisoning by deleterious exhalations and excretions, etc., are just as many obstacles to the health and life of the young inmates of our public institutions. The difficulties of raising infants in our institutions, and of gathering a sufficient amount of breast-milk in them, induce the undersigned to try a change with a part of their inmates. A number of them are to be given in charge of responsible parties in the country surrounding New York. The not unfavorable results of farming out even in cities, when compared with the mortality of institutions, encourage us to hope that infants farmed out in the country have a much greater certainty of life, and a healthy future. And, with regard to this plan, we have herewith taken the liberty of sending you this communication.

We propose to farm a number of babies out until they have reached the end of the third year. In particular cases, special arrangements may be made beyond that age.

Babies who have no teeth are expected to be fed on breast-milk exclusively. Such as have from two to four teeth, on mixed food. Afterward they are to be weaned according to such rules, concerning the feeding of the children, as shall be laid down by the undersigned or their medical board.

A single party is to be intrusted with but one nursling. A medical examination only can decide whether, in exceptional cases, a woman is fit to nurse two infants. She may, however, obtain an older child in addition to the nursling.

She must either be married, or a widow, or very well recommended. She must have plenty of breast-milk for the nursling in charge, no matter whether she has lost her own baby, or has sufficient nourishment for two (her own and the stranger). She must be healthy, not destitute, not intemperate, and known to be industrious, and not entirely dependent on the board paid for the nursling. She has to present a certificate from responsible parties—physicians, clergymen, postmasters, town authorities, or well-known citizens, concerning the above requirements, stating also how many children she has, and how many she has lost.

The applications of women who offer to take charge of infants are made at the office of the Commissioners of Charities and Correction. The depot of the babies is at Randall's Island. The house-physician notifies an applicant to call for her boarder. She has to call personally. Travelling expenses are refunded. The board-money is ten dollars a month, to be paid semi-monthly, monthly, or bi-monthly.

Besides, we offer to pay twenty dollars to a party, with whom a boarder has been living for sixteen consecutive months, at the end of his second year.

These are the outlines of the principal rules which, in all probability, will govern the farming out of infants in the country. We now apply to you, sir, and your friends, for your opinion and coöperation. You can advise us if, in your circle and neighborhood, the men in standing and authority, as mentioned above, would be found willing to help the cause of humanity and an enlightened political economy, by giving such certificates as parties would require, by even encouraging a party to serve herself and the public by taking charge of an infant, and also by paying a certain amount of attention to the little one who has no mother but the community.

The general superintendence will have to rest with the medical board of the Infant Hospital. Their house-physician shall be entitled to provide for special inspection. Still, it will be of the utmost importance to interest the public at large in the welfare of the foundlings, particularly the ladies, who, according to localities, might form committees for the purpose of watching and superintending the foundlings and their nurses.

You are respectfully requested to give the foregoing your attention, and to communicate to us your opinion as to the feasibility of our plans; whether, in your opinion, a certain number of women would be fit and willing to charge themselves with bringing up an abandoned infant in your neighborhood, and whether yourself or your friends, or their ladies, would be found willing, by occasional inspection, etc., to aid our attempts in raising infants, whose life is as valuable to society as our duties toward them are clear.

While offering the suggestions of this letter, and my previous remarks, to your consideration, I am fully aware of not having exhausted the subject. I have already, I know, to beg your pardon for keeping you so long. You will, I am sure, excuse me, on account of the importance of the subject on which I have spoken.

Quod felix faustum fortunatumque sit.

Art. II.—*The Nature and Treatment of Yellow Fever.* By James S. Bailey, M. D., Albany, N. Y.

Yellow fever is insidious in its attack. In many cases it is so masked, especially in the commencement of an epidemic, as to render the diagnosis difficult. In its mildest form it is by no means an easy task to distinguish the earliest cases. It is necessary to witness the symptoms which attend the fatal termination, nor is it always easy to settle the question satisfactorily with the first few deaths; sometimes it requires a number of cases before it assumes an unquestionable character.

During the prevalence of an epidemic, every person, whether acclimated or not, is sure to experience to some extent the dull headache and general neuralgic symptoms.

The attack is usually at night, commencing with a slight rigor, and often with a chill almost amounting to congestion. Persons having experienced an ordinary chill, recognize at once the peculiarity of the chill ushering in an attack of yellow fever, by the cold and creeping sensation along the spine, radiating from the nerve-centres toward the extremities. Some are attacked suddenly without premonition; the person may be walking the street or pursuing his avocation, and fall in a stupor. Such cases are denominated walking cases, and they usually die from congestion and coma.

There are three distinct stages in yellow fever: The first stage is that of primary febrile action. The second, that of repose or subsidence of fever. The third, that of secondary fever. After the chill, febrile action commences, and lasts usually three days, though sometimes as long as six. An unusual duration is always considered unfavorable. During the remission, the patient considers himself quite well, except debilitated, and if allowed would sit up.

During the apyrexia, the great prostration is remarkable, and not in proportion to the preceding fever; with this exception, the inexperienced observer can see nothing which does not indicate speedy convalescence. This is a deceptive period, and the morrow may develop black-vomit, and perhaps death. This period lasts from twelve to twenty-four hours. In mild cases convalescence dates from this time. The secondary fever may run an indefinite period.

The assemblage of characteristic symptoms is well marked, generally in all stages; but the history, the exposure, etc., must have their due consideration. The peculiarity of the invasive chill, the vascular excitement, the brilliant and injected eye, or, as it has sometimes been termed, the besotted expression, with severe neuralgia, the duration of the primary fever, etc., must have their weight.

The conjunctiva assumes a reddish appearance from the commencement of the fever, then is followed by a dull-yellow and leaden hue.

About the third stage the color of the skin commences to change, beginning at the white of the eye, and extending over the forehead, chest, abdomen, and extremities, presenting a dusky orange-color, more marked upon the chest and abdomen than elsewhere, and as the disease progresses approaches a mahogany hue.

The urine is highly colored, and stains linen as if deeply impregnated with bile. The pulse usually ranges from 100° to 120°, and in inflammatory cases runs as high as 140° per minute. There is usually great diversity in its volume; it may be weak and small or full and bounding, but is usually very compressible.

The color of the skin is not always yellow even after death, though in many cases it is well marked, presenting the various tints of a fully-ripe pumpkin to that of a purplish-mottled mahogany cast. The coloring-matter is derived from the blood, and is extravasated hematosin. The color is more intensified after death.

In some instances the perspiration stains any white substance with which it comes in contact. The tongue is usually moist and covered with a white, pasty fur, sometimes assuming the color of blue clay, the middle portion changing to brown in the latter stage, and in many cases it is red and smooth.

There is much difference in the manner of attack in this disease. The most favorable cases are those that have marked symptoms, ushered in at once with a chill, reaction, and fever. Its ravages are very destructive to the blood, and the chances for recovery are not so good in those cases that come on more slowly, which enable the patient to keep up and about his business.

A very serious source of fatality is the peculiar mental condition of most patients. Having experienced the aching sensation incident to the locality of its prevalence, they constantly imagine they are taking the fever; but when really attacked they cannot realize it, and fail to go to bed in time, and endeavor by exercise and an extra amount of clothing to sweat it off, while rest and absolute quiet are necessary for recovery.

It requires great watchfulness and discrimination in its management. If cases are detected soon and kept absolutely quiet, the chances for recovery are much enhanced, the enervating process of blood-poisoning not being carried to the same extent. For this reason, strangers, in trying to become acclimated, are so liable to sacrifice their lives: they are not willing to be covered, and endeavor to brave an attack.

If the case proves fatal during the secondary fever, death takes place most frequently upon the fourth, fifth, or sixth day, though in some rare cases as early as the third, and is sometimes deferred until the ninth day. Such cases usually assume a typhoid type, and recovery may be deferred weeks. Convalescence must necessarily be slow, considering the great repair necessary for the blood to undergo. The bowels are ordinarily constipated, and movements when obtained are very offensive.

The neuralgic symptoms are often very intense, particularly of the head, back, and limbs, frequently exciting screams, and usually lasting through the febrile stage. The patient complains of a burning sensation in the stomach, with nausea, with or without vomiting. The sense of weight and uneasiness about the epigastrium is almost universal, with tenderness upon pressure. There is a great desire for cold drinks, but any considerable amount of fluid taken only increases the irritability of the stomach.

Black-vomit may not be present in every case, though after death, if the stomach is inspected, it is usually found to contain it. The manner of ejection is peculiar: the patient first hiccoughs, which is followed by a spurt or vomit, without effort. The patient often notices it, but seldom remarks upon its fatal tendency.

The matter first vomited is such as had been swallowed; afterward, mucus streaked with brownish matter; until, finally, we have the true black-vomit. It usually has an acid reaction. When ejected upon linen it has a glistening appearance, imparting a stain very difficult to remove, though, when vomited in a vessel, it separates into two distinct parts, one resembling coffee-grounds, and settling, with a viscid mucus rising to the surface. The dark, flaky portion is doubtless an altered condition of the blood produced by congestion of the stomach.

One of the most difficult problems in the whole range of medical science is to determine the generative cause in a given disease. Reflecting physicians, having witnessed an epidemic, are apt to form their own theory of its cause, yet but few agree. It is known to be a disease of warm climates, and that it cannot exist in low temperatures, although the exact laws that govern it we do not understand, nor the exact temperature necessary for its production. Each epidemic has its own peculiar phases, and, when one opinion appears to be well grounded, another epidemic occurs, overthrowing it in many important particulars. The cause is entirely distinct, and does not produce any other known disease. Dengue more closely resembles it in its character than any other disease, and still they are quite dissimilar, although the premonitory symptoms sufficiently resemble to lead the physician astray in the commencement of an epidemic. Dengue does not destroy life, unless complicated with some organic affection.

Its occurrence is usually considered a receipt in full for yellow fever during that season. It is possible, though rarely the case, that both dengue and yellow fever prevail in the same locality during the same season.

Yellow fever is evidently a disease of populous cities, and usually attacks persons congregated together. Miasm, filth, foul air, or any cause sufficient to produce a low grade of fever, will not produce it, though such causes may act in a general way by enfeebling the vital forces and powers of the system, rendering it more liable to its influence.

Yellow fever is a disease of local origin. It is my impression that it may be generated in an inland locality, provided a combination of circumstances exists necessary for its production. The writer has seen a number of cases occurring upon a plantation remote from public thoroughfares and water-courses, resembling in every particular the peculiar phase of an established epidemic, with black-vomit occurring without premonition, spurting from the mouth without effort, together with the hæmorrhagic condition often manifested in this complaint.

This condition existed prior to the prevalence of an epidemic in Galveston, Texas, in 1864, upon a plantation in Austin County, which was situated four miles from the Central Railroad, and one hundred miles from the coast of Galveston. A negro died with black-vomit, and a similar matter oozed from the mucous surface, staining profusely the linen. It did not prevail epidemically in this immediate vicinity, but did in Galveston and Houston during this same season in a malignant form, causing the death of many of the citizens and Confedèrate troops.

According to my observations, epidemics prevail more malignantly in country places than in cities, and are more destructive to life. This may in part be attributed to inexperienced nurses, and to the buildings usually being more open, and not so well calculated to protect the sick from every sudden change of the atmosphere.

It is considered that persons once acclimated by having had yellow fever are not liable again, but well-established cases do occur in persons having lost their acclimation by a change of residence. Generally, I believe this is not the case. I must here state I have never witnessed a second attack in the same person, though the testimony of experienced observers is decidedly in favor of its recurrence.

Prior to 1853, it was considered impossible for this disease to exist and prevail epidemically outside of the populous towns and cities; but since that time it has done so, first following water-courses, and afterward penetrating the interior towns remote from the coast or any considerable thoroughfare, confining itself to small towns and plantations: 1867 affords innumerable instances of this kind.

The general opinion is, that it is not contagious, and cannot be taken by contact. Having myself passed through several malignant epidemics, acting the part of physician and nurse, and assisting in burying the dead, I never have contracted the fever. This opinion is expressed by Blair and Dr. Warren Stone. Both gentlemen having been long residents of climates where yellow fever prevails epidemically, their opportunities for observation have been great, and consequently their opinions are entitled to respect.

A peculiar atmosphere seems to favor the prevalence of yellow fever: it is a murky atmosphere, alternated with hot sunshine, and frequent showers, enough to keep the face of the country saturated. Books grow mouldy upon the shelves, and the boots you have worn during the day become mouldy during the night. The emanations from the earth in the hot sunshine during such seasons are sickening in the extreme.

Negroes are not so liable to yellow fever as white people, and their susceptibility seems to be in direct ratio to the proportion of Caucasian blood they possess.

Negroes imported direct from Africa, and introduced immediately into a yellow-fever atmosphere, seldom contract the disease, while they are very susceptible to the other diseases of the locality. This fever is very fatal to young children. I recall an instance where a mother and infant eleven days old both took yellow fever; the infant died with black-vomit, and the mother recovered upon the fourth day. Non-residents are more susceptible to its influence than residents otherwise acclimated. Those accustomed to excesses are marked victims, such as the intemperate and licentious. The plethoric, with apoplectic habit, seldom recover; the same holds true with the sanguine temperament.

The ordinary mortality should not exceed ten or twelve per cent., all things being favorable; but, if there is a large influx of strangers, the mortality will be much greater. The class of persons attacked also makes much difference. Dr. Fenner (*New Orleans Medical and Surgical Journal*, July, 1848) mentions the fact that the poorer and laboring classes were most subject to the fever, and says:

"People in easy circumstances, who are not exposed or imprudent, suffer but little from yellow fever in the great Southern emporium, and the mortality is comparatively small."

The matter of nausea is of great importance. Often, by a nice prescription, the physician can anticipate, and by suitable remedies prevent, what would inevitably follow.

Black-vomit does not necessarily imply death. Persons do sometimes recover after having it. According to my observation, about two per cent. recover.

The black-vomit of the congestive form of bilious remittents has its characteristics, and should not be mistaken for true black-vomit of yellow fever. The first is in reality altered bile, and imparts a bilious hue to water, while the black-vomit of yellow fever is altered blood, and does not stain or color water, but remains distinct and separate. There is a similar difference upon its being vomited upon linen, the one staining a white surface a bilious tinge, and the other staining a glistening bronze.

In bilious remittent fever the spleen is always enlarged and congested, but this is not the case in yellow fever: there are congestion and inflammation in the stomach when black-vomit occurs in either case.

Violent mania is exhibited in a small proportion of cases, with persistent screaming and struggling to relieve themselves from the restraint of the attendants to the last breath, when death relieves them, as if by an electric shock. The relation of a circumstance will explain: In visiting a German woman upon the fourth day of her attack, her symptoms having been previously very favorable, I was surprised to find her husband upon the bed astride of her, holding the bedclothes down tightly around her neck, and she screaming and biting at him, alternately cursing and imploring him to loosen his grasp, when in an instant she ceased to speak and struggle, and was really dead.

The individuality of this disease is unmistakably strong. The phenomena of each epidemic may vary, but all possess the specific features and essential characteristics of the malady. No matter what organ suffers, the stomach must bear the main burden. It is acknowledged to be a specific disease, and unlike, in its main features, any other. Its mode of attack and its duration are substantially the same. During its whole course only one symptom may be considered pathognomonic exclusively, and that is true black-vomit.

The peculiarity of the invasive chill already referred to, the history, and the exposure, must have their due consideration.

When the patient has an irrepressible hunger, and will eat greedily any thing within his reach, speaks intelligently, but really is not sane, it should be regarded as a threatening symptom, particularly if occurring before convalescence in the third stage.

The colliquative perspiration, during the apyrexia, when associated with cadaveric odor, with shrivelled features, lividity of the skin, thready pulse, and extreme sensibility of the epigastrium, is also alarming.

Persons addicted to dissipation are almost sure to die; and such persons, when exposed to an epidemic, are apt to indulge freely, partly from habit, and partly from fear, thinking that, if they keep stimulated to a certain point, they will escape; but this is fallacy, observation having proved the contrary. They are more liable to be attacked, and, when taken, the disease proves more fatal.

The urine of yellow-fever patients has been most attentively examined, with the hope that it would furnish means of diagnosis and prognosis.

The experiments of Bartlett, in three hundred cases, satisfied him that, during the first stage, the physical characteristics of the urine are the same as in other fevers. But, in the second stage, the urine, as the disease becomes developed, assumes more or less of a deep-brown color and viscid consistence. Subjected to nitric acid or heat, an albuminous precipitate is obtained, the abundance of the albumen varying according to the progress of the disease. If the termination is to be fatal, it increases until death; if favorable, the amount of albumen gradually decreases. It disappears before the yellowness of the eyes and skin is gone, which usually takes place between the eleventh and twentieth days.

There is no symptom so fatal as the suppression of urine, not excepting black-vomit. It is occasioned by the closing of the uriniferous tubes by accumulated epithelium-casts of the urinary tubuli, or portions of the mucous membrane, which produces mechanical lesions of the secreting structure of the kidneys. The approaching signs of congestion should, therefore, be watched carefully, to guard against this fatal symptom.

In females, the catamenia are sure to appear, whether due or not.

Any disease occurring during the prevalence of an epidemic is liable to assume the characteristic type. An illustrative case came under my observation in 1859. A jeweller, living in an inland town, fearing to expose his family to the epidemic influence, more than two miles distant in the country, remained in town during the day to conduct his business, and after closing his store walked to his countryresidence. Not being accustomed to the unusual fatigue, perspiration was freely excited, and, by taking cold, he contracted pneumonia, which yielded in nine days, only to give place to yellow fever of an aggravated form, presenting the mania so frequently an accompaniment of this disease—afterward passing into a typhoid condition, with the secondary symptoms of suppuration of the parotid glands. Here were three distinct diseases following each other in rapid succession, the patient finally recovering after the lapse of weeks.

There is an impression that a severe frost effectually checks the progress of an epidemic. This is not always the case, and will not stay permanently the ravages of yellow fever.

In 1859, in Texas, frost was deferred until the early part of December. During this season a malignant epidemic was prevailing, and many were anxiously looking for frost as a relief to the exposed and suffering. At length it came, and with it ice formed in a single night more than an inch thick in shallow water.

The joy was unbounded among the inhabitants surrounding the infected district, but, to their disappointment, in the course of ten days the epidemic doubled its malignancy, and spread with much greater rapidity. Those cases that were just convalescing at the time of this sudden change in the atmosphere, nearly all proved fatal, especially those that were unfortunately sick in open tenements. This is usually the case in the decline of epidemics; with warm days and frosty nights the mortality is very great. Dr. Bennett Dowler (*New Orleans Medical and Surgical Journal*, January, 1848) remarks that "there is by no means the connection between frost and the subsidence of yellow fever that has generally been supposed to exist. The epidemic of 1847 appeared in New Orleans early in July, and disappeared by the middle of October, and yet frost was delayed until several weeks afterward. Summer heat may continue far into autumn, the city be crowded with unacclimated persons, and still the health continue to improve. Strangers hasten into houses, all the inmates of which had perished but a few weeks before, and escape the infection."

Treatment.—The conservative mode of treatment is the most rational and successful, according to my experience; I mean by this, the employment of remedies calculated to mitigate symptoms and guide the malady to a favorable termination. Early attention to first symptoms is of great value in lessening mortality.

It is often remarked by individuals accustomed to see yellow fever, that they would rather risk their chances for recovery with the attendance of an experienced nurse than with the treatment of an inexperienced physician, and in explanation say that the nurse would rely only upon the powers of Nature to eliminate the effect of the poison, while the physician, anxious to do something, and not knowing exactly what to do, would do harm. This may be true, but there is much to be done for a yellow-fever patient, and the careful and discriminating physician is the one best calculated to pilot the vessel through this fearful storm to a safe termination of its decisive voyage in the harbor of health.

Upon seeing a case in the beginning, immediately after the invasive chill, if it was suspected that the stomach was charged with ingesta, an emetic of mustard-and-water was preceded by the administration of a laxative. If perspiration was not readily established, a mustard foot-bath given in bed was of service. Immediately after this, a full dose of quinine should be given. Its effects are remarkable. It generally quiets the neuralgia completely, and promotes perspiration.

When diaphoresis is once established, it is not difficult to maintain it by absolute quiet, and the case ordinarily does well.

Only give purgatives in the commencement of an attack. I have frequently seen patients go with impunity six days without an evacuation. It is important to rid the stomach and bowels of all irritating matter, and to get the skin to act freely.

After this, the less you interfere the better. Keep the patient covered and quiet, and perspiring, but not to excess. Enough covering is required to answer this purpose, but not enough to burden the sufferer. Nothing more is necessary but warm teas to allay thirst. Orange-leaf tea is very proper, or weak sage-tea, but if these are repulsive they should not be used.

If cool drinks are demanded, give them in moderation; the stomach is its own monitor. Yellow-fever stomachs are peculiar: they will retain any thing they like, or reject any thing unpleasant. It must be remembered that the first fever will terminate generally better without medicine than with it.

Some are in the habit of using stimulants throughout the disease; my personal experience is decidedly in favor of spirituous liquor in the exacerbation of fever, if the patient craves it, and there is much tendency to sinking. I do not wish to be understood as advocating stimulus, except during the stage of convalescence.

There are cases when the external soreness is excruciating; such patients must not be handled nor bruised, but kept in a horizontal position. The wastes from the body should be accomplished through urinal and bed-pan.

During the stage of repair the patient should be nourished —if not, he will sink—and by nourishment the deterioration of the blood seems in a great measure to be prevented.

The diet should be selected with reference to its ease of digestion and assimilation. Patients cannot get up, after even a slight attack, with safety, without being abundantly nourished.

There are cases which demand the use of quinine throughout their whole course; such are denominated congestive cases. Those occurring during the decline of an epidemic, when the weather is becoming variable, with warm days and cool nights (this very changeable atmosphere is very trying to yellow-fever patients), if not sufficiently protected and supported by appropriate care, are apt to succumb and perish.

One of the most alarming symptoms in yellow fever is irritability of the stomach. It is so apt to be present, and so often uncontrollable, that it is of importance to find a remedy to check it.

When muriatic acid is about being formed in the stomach, there is a burning sensation, which is generally relieved by the use of alkalies; chlorate of potash, or the super-carbonate of soda, and sulphate of morphia, have generally succeeded. For the excessive nausea, an emulsion of creosote acts quite promptly.

The suppression of urine is an exceedingly fatal symptom. Very few persons recover after this takes place ; remedies seem to be of little avail.

The internal use of soda and potash has sometimes been beneficial ; also friction over the lumbar region, with stimulating liniments.

Any acute disease occurring in a southern latitude, if not controlled in nine or ten days, is apt to assume a typhoid type. The treatment of these symptoms should be expectant, and the same as in typhoid fever. The supporting plan is always necessary, guarding scrupulously against enteric symptoms.

The occurrence of one of the secondary symptoms of a typhoid condition is often present. Of this I desire particularly to speak—that is, the treatment of the inflammation of the salivary glands. It frequently springs up very suddenly, augmenting the already troublesome symptoms, and, if allowed to go on to suppuration, may continue to annoy the patient for months by a profuse fetid discharge, and protract much the period of recovery. Having tried ineffectually the ordinary modes of dispersing it, such as the application of iodine, turpentine-frictions, blisters, etc., I resorted, with entire success, to the use of the lancet in the following manner : As soon as the patient complained of stiffness of the articulation of the jaws, and tumefaction was observed, I immediately thrust the point of a thumb-lancet to the bone, at the angle of the jaw. Sometimes a teaspoonful of blood would follow, and frequently only a few drops ; if it bled too much, it was easily checked by styptic applications.

This was usually quite sufficient ; but, if not, I repeated this procedure the second, or, if necessary, the third time upon each daily visit, when this troublesome symptom would disappear. I have never known suppuration to follow when treated in this manner.

Mustard-plasters applied to the spine, when the neuralgia is excessive, often afford relief, and may be applied frequently.

It is of the greatest importance to nourish your sick after the subsidence of fever; it does not require much, but, if they are not nourished, they will sink. If the patient will not take nourishment, it will be absorbed by the rectum, and must be then administered in this way.

The effect of quinine as a prophylactic is well established in yellow fever; but, if continued too long, the system will become accustomed to its use, and its effect will be similar to any species of dissipation, and not accomplish the desired effect. It is uncertain whether the system can be constantly kept under the influence of this drug, to insure exemption from the epidemic during seasons of its protracted prevalence. Many persons use quinine for this purpose and escape, while others escape without having taken any prophylactic. I now recall several cases during the epidemic of 1859, of persons who used quinine for this purpose, but succumbed and perished toward the close of the epidemic. There is no doubt that quinine is the best prophylactic within our knowledge against yellow fever, but it is a much more certain protective against miasm.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Adjourned Anniversary Meeting, November 27, 1871.

Dr. Abraham Jacobi, President, in the chair.

The meeting convened at an early hour, the attendance being exceptionally large.

The minutes of the Anniversary Meeting of October 2, 1871, were read and adopted.

The President stated that it would be impossible to complete the reading of the Minutes of the Comitia Minora for the past year, restrained at the last meeting by injunction of the Supreme Court; inasmuch as the Court, at the hearing on the injunction-order, had decided that it "continue and remain in full force and effect until the final termination of this action;" i. e., of Dr. Ruppaner against the Comitia Minora and Dr. Sayre.

DR. RUPPANER EXPELLED.

Dr. Roosa moved that the by-laws be suspended, and the motion prevailed, *nemine contradicente.*

Dr. Henry arose to offer a resolution concerning the offence which had been committed against the Society by Dr. Ruppaner.

The President said that, if that subject was to be discussed, he, as one of the enjoined parties, must leave the chair.

Dr. Roosa moved that, if the chair were vacated by the President, it be occupied by Dr. Peaslee.

Dr. Carroll thought there was no necessity for referring to any matters covered by the injunction. Entirely apart from the old charges against him, the member in question had been guilty of a grave offence against the Society, in refusing to abide by its rules, and in obtaining a legal injunction against its regular procedure—an offence which certainly called for a vote of censure; and this action might be taken without the President's leaving the chair.

After some further discussion of Dr. Roosa's motion, it was put by the President, and carried, in the form that the meeting resolve itself into a Committee of the Whole, with Dr. Peaslee in the chair.

In Committee of the Whole—Dr. Peaslee, Chairman; Dr. Bradley, Secretary:

Dr. Henry, taking the floor, said that he had a resolution of censure to offer, and referred to a pamphlet, issued a few days before by Dr. Ruppaner, entitled "Statement of the 'Sayre-Ruppaner Case' and Opinion of the Supreme Court." It consists of copies of the legal documents pertaining to the injunction-order, and of a prefatory letter addressed "To the Members of the Medical Society of the County of New York." From this letter the speaker read several paragraphs, among them the following, which, together with statements in Dr. Ruppaner's affidavits, he characterized as deliberate and malicious falsehoods, in the highest degree insulting to the Society as well as to the officers it had elected:

"It is perhaps proper for me to say, that I have never avoided a fair and impartial investigation of the so-called charges of Dr. Sayre; neither have I set at 'defiance' the rules and regulations of the Medical Society; nor have I in any manner violated the code of medical ethics.

. . . .

"I have resorted to the courts to prevent what I deemed preliminary injustice; to protect my own rights, as well as those of every member of the Society; and to have a fair and impartial hearing upon testimony and facts.

"In what I have done I have intended no discourtesy to the Society or any of its members."

After an allusion to some of Dr. Ruppaner's antecedents, the speaker proceeded to comment at length upon his statements concerning the Society, and his course in defying its authority. He was about to read further from the pamphlet, when

Dr. J. L. Brown said that this pamphlet had doubtless been read by every one present, and that it seemed a waste of time to go on with the citations. After several calls for the resolution,

Dr. Henry moved that the committee recommend the adoption of the following:

Whereas, Dr. A. Ruppaner has refused, on frivolous pretexts, to meet charges preferred against him, to the Comitia Minora of this Society, by a member, in accordance with the rules and by-laws of the Society; and

Whereas, The said Dr. Ruppaner has, by false representations to a court of law, obtained an injunction against said Comitia Minora, thus impeding their regular action, and that of the Society, thereby reflecting injuriously upon the honor and impartiality of said Comitia and of the Society, and subjecting the Society to tedious, expensive, and unnecessary litigation: Therefore, be it

Resolved, That, in the opinion of this Society, the said Dr. Ruppaner merits expulsion, and that he is hereby censured.

Dr. Finnell thought that the passage of any such resolution would only help the member to the notoriety he was seeking.

Dr. Jacobi said it was not for the Society to consider what the member desired or deprecated; it was for the Society to do its duty; and the fewer minutes it took for this, the better.

Dr. Roosa said the question was simply this: Shall we allow a member accused of an offence—the accusation being properly made to the Comitia Minora—to scout its authority to investigate the case and report upon it to the Society, and to deny competence of the Society to decide upon it? Shall we allow him to go before the courts, and procure an injunction upon our regular mode of procedure in matters purely professional, and having no legal color whatever? It was exactly as if a church-member, accused of heretical doctrines or unchristian practices, should deny the church's authority to question him, and appeal to the law to restrain its investigations, and vindicate his orthodoxy or his godliness. If such a flagrant violation of its rules were to be allowed to pass without censure, all discipline in the Society was at an end, and it was time for it to dissolve.

The motion was passed; and, after some discussion on parliamentary points, the Committee of the Whole rose and reported the above preamble and resolution for adoption by the Society. The report of the committee was received; when

Dr. Henry moved to amend by substituting a preamble and two resolutions, which he read, setting forth what was supposed to be the substance of the original charges against Dr. Ruppaner; stating that in the opinion of the Society they were sustained; and ending with expelling him.

The President ruled that he could entertain no such motion, covering the subject-matter concerning which the Comitia Minora had been enjoined.

Some discussion ensued on the question whether the report of the Committee of the Whole had been simply received, or had been adopted as the expression of the Society.

Dr. Raphael moved that the subject lie on the table. Tellers were demanded, and the motion was lost.

On a motion to adopt the preamble and resolutions reported by the Committee,

Dr. Folsom remarked that he thought the dignity of the Society would not allow it to adopt that report without amendment. If the member "deserved expulsion," as there stated, was there any reason for leniency in his case? Should not his expulsion be at once accomplished, as it might be under the existing suspension of the by-laws? He, therefore, moved to amend, by substituting, for the resolution of censure, the resolution, "That Dr. Ruppaner be now expelled from the Society."

The amendment was adopted, with a few voices in the negative; and the report, as amended, was adopted.

On motion, the suspension of the by-laws was now terminated.

CHICAGO—AN ACKNOWLEDGMENT AND AN APPEAL.

The following letter to Dr. Hamilton, chairman of the Executive Committee for the relief of Chicago physicians, was read by the Secretary, Dr. Purdy:

No. 384 Michigan Avenue, Chicago,
November 8, 1871.

My dear Doctor: Your generous letter of the 4th instant reached me late last night. I have not yet had the opportunity to lay it before our committee, but will take the liberty on my own behalf, for them, to accept your noble offer of continued aid to our suffering brethren. In the name of our common humanity, I dare not throw any obstacle between your charity and their necessity.

You ask of me, in turn, a service which I am most happy to render, if thereby I could coin my words into gold to feed the hungry mouths and shelter the chilled limbs from our fearful winter's cold.

Let me give you a little sketch of our operations thus far.

In the first place, I must premise by saying that we considered your charity to us as a matter purely professional, with which the general public and the *secular* press had no concern; and, hence, we have kept our operations strictly within the cognizance of the profession. Are we not right? What think you?

Immediately upon the appointment and organization of our committee, we published a card, requesting all *regular physicians* in *good standing in the profession*, who had suffered loss by the fire, to present to the Secretary a written statement of their losses. These, having been subjected to the scrutiny of the committee, were classified as follows:

1. All irregular, and *disreputable* regular, practitioners were at once rejected, and referred to the General Relief and Aid Society.

2. Those unknown to the committee were required to furnish certificates of regularity and standing from reputable professional sources; and

3. Those well known were placed at once on the relief-list, and the money in hand was divided among them as follows: to the first class, i. e., those having families, and who had lost both residences and offices (which here are generally separate establishments), was awarded the largest pos-

sible sum, $50; to the second, who had lost their offices only, was awarded $25 each; and to the third (single men without dependants), $10 each.

We thus were enabled, out of about $400, all the money at our disposal, to supply the immediate necessities of three, five, and nine persons in each of the three respective classes. One of these, a gentleman of thirty years' practice, for twenty-eight years a professor, and for some time president of his college, with his fifty dollars rented and furnished a little room about ten feet square, in a wooden shanty, with a pine table, a small cooking-stove, and two Windsor chairs, and has gone to work. His wife, one of the most thoroughly refined and elegant women in the West, a scion of one of your oldest New York families, sweeps and scrubs the office, and acts as her husband's chemical assistant from morning until night. Another recipient of fifty dollars was a man of sixty years of age, whose collection of five hundred oil-paintings, gems of art collected during forty years, and library of four thousand rare old volumes, had for years been one of the hidden treasures of our city, known only to the privileged few. Could you have seen the tears start from the eyes of these Nestors of the profession, not at their losses—of these they speak with a smile and a shrug—but at the sympathy and the noble charity of their professional brothers, you would, I am sure, have been touched to the very heart. Another, whose reputation has extended even across the ocean, said to me when I handed him his check, "This is more than I can stand. My *losses* cannot shake me, but this breaks me down."

These men have solicited, and some have secured, appointments from the city to attend the poor quartered in the public barracks, at $50 per month salary, whose daily incomes were formerly as great.

On the 27th ult. our hearts were gladdened by the arrival of Dr. Hubbard's check for $2,000, which enabled us, upon the same basis of apportionment, to extend our list of beneficiaries to thirty-five. And again, by means of Dr. Hubbard's second check for a like amount, together with $900 received from St. Louis, and $1,000 from the King's County Medical Society of Brooklyn, through its President, Dr. Burge, we have increased the number of beneficiaries to seventy-five, of whom seven are medical students.

The largest amount appropriated to any one individual, thus far, has been $135, and this sum only to men having families and without resources.

One gentleman, whose household furniture cost $6,000, is now, with his wife and three children, sleeping upon the floor of two small rooms, of which the rent was unpaid until your relief came to his aid. Another, with an insane wife and three children, was occupying one room without bedding, getting rations from the Relief Society, and clothed in second-hand garments. The grown-up daughters of one venerable gentleman escaped with their night-dresses and water-proof cloaks alone. The comprehensiveness of the calamity can scarcely be appreciated by one not upon the spot. None of our physicians were rich. Some were in very comfortable circumstances, while the majority were poor, and many foreigners especially, men of high culture, very, very poor indeed.

Some, who had been sufficiently fortunate to secure homes of their own, were encumbered by mortgages, by means of which they will lose eventually their real estate. Even those who had been so prudent as to insure their property find their insurance worthless, by reason of the failure of insurance companies. The strongest of the local companies express the hope to pay 25 per cent., when they can assess their stockholders—a vain hope, when the stockholders themselves are ruined. Under ordinary circumstances, when misfortune overtakes a medical man he can fall back on his book-accounts for relief; but in this case many, very many of the sufferers lost even these; and, even had they been saved, they would have been worthless, as the patients are all ruined; so that many of us, more fortunate, who were not "burned out," are dependent entirely upon the little that we can pick up in cash from day to day, to enable us to live.

Our list of applicants for relief, up to to-day, numbers one hundred and fifteen, of whom, as already stated, seventy-five have received aid in amount varying from thirty to one hundred and thirty-five dollars. The remaining forty have thus far been dependent upon public charity. This list is being swelled rapidly by the addition thereto of medical students of the Rush Medical College, which was in the "burned district." We have thus far placed the students upon the same basis as physicians, as most of them are poor, many utterly destitute by reason of their losses, and, without the aid received from you, would be compelled to abandon their studies. Some of them, too, are practitioners of some years' standing, with families, who have hitherto been too poor to attend lectures.

We would be glad to learn your opinion of our mode of administration of our Trust Fund.

In compliance with Dr. Hubbard's request in his last letter, I wrote to the Governors of Michigan and Wisconsin, for information regarding their suffering physicians. I have to-day a letter from Governor Fairchild, of Wisconsin, informing me that he has placed my letter in the hands of physicians, who will at once furnish the desired information.

I fear my catalogue of sorrows will seem like a dreary reiteration. Unfortunately, our sorrows are all that we can call our own.

Very truly,

William Hay, M. D.

Dr. Frank H. Hamilton.

Dr. Purdy, as Secretary of the Relief Committee, stated that, at its last meeting, the members had agreed individually to contribute each a certain amount monthly, for the next four or six months; and it was desired that this plan might be adopted by the profession generally, and that the names of those willing to become monthly contributors, with the amounts of their subscriptions, be sent as early as practicable to Dr. S. T. Hubbard, Treasurer of the committee, 27 West Ninth Street, New York.

The President, in a brief but earnest appeal, trusted there was no member who would not spare at least the value of two or three visits each month to aid the suffering physicians of the West.

The reading of the minutes of this Adjourned Anniversary Meeting was, on motion of Dr. Ellsworth Eliot, postponed until the next meeting.

The Adjourned Anniversary Meeting now adjourned *sine die;* and the meeting became the regular

STATED MEETING.

The President announced that the Comitia Minora had, under the lately amended by-law, recommended the following persons for membership, subject to approval by the Society: Drs. William B. Wallace, Herman Althof, Ramon Amabile, Beverhout Thompson, John N. Merrill, Seligman Teller, Mary C. Putnam, J. E. M. Lordly, Adolph Kessler, P. E. Donlin, Edward S. Smith, and Charles M. Allin. The Society adopted the report, and admitted them to membership.

Dr. Russell asked leave to introduce a matter of new business, which was ruled to be not in order at this stage of proceedings.

Voted that when the meeting adjourn it be to Monday, 4th December.

Adjourned.

After the adjournment, Dr. Russell stated, to the considerable number of physicians who remained, that, as he expected to have the honor of appearing, within a few days, before the committee of gentlemen who had under consideration the proposed changes in our municipal government, and of offering some suggestions for improving the operations of the Bureau of Vital Statistics, he had desired to be fortified in one respect by the indorsement of the County Society, which so fully represented the profession of New York. During the present year he had made great endeavors to obtain from physicians a compliance with the existing law requiring returns of all births which they attended. He was happy to state that, owing to the general coöperation of the profession, some twenty-one thousand births would have been registered for this year, against fourteen thousand for last. Even this figure, however, did not represent two-thirds of the actual number; and, although he would respectfully urge upon his professional brethren a strict adherence to the law, until some change for the better was effected, it was with the object of entirely relieving the profession from what was by many considered a most onerous and thankless responsibility that he proposed to ask for a change. The plan which he had in view looked to the division of the city into a number of registration districts, with a resident sub-registrar in each, to whom parents should be obliged to report their children's births, receiving therefor a written acknowledgment. The sub-registrar would also visit, once a year, every family in his district for the same purpose, and would make his returns monthly to the Central Bureau. The expense of the whole system need not exceed $10,000 per annum.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Adjourned Stated Meeting, December 4, 1871.

Dr. A. Jacobi, President, in the chair.

The minutes of the Adjourned Anniversary Meeting, and of the Stated Meeting, of November 27th, were read, and, after two unimportant amendments, adopted.

STANDING COMMITTEES.

The President announced his appointment of the following standing committees for the year, remarking that, as the reports on meteorology, to have any medical value, must be considered in connection with the prevalent diseases, he had, in order to facilitate this, made the members of the Meteorological Committee also members of the Committee on Diseases.

Committee on Library: Drs. Jerome C. Smith (chairman), Charles A. Leale, and George W. Robinson.

Committee on Intelligence: Drs. Fred. A. Castle (chairman), Lucius D. Bulkley, Charles S. Bull, Benjamin Howard, Samuel Sexton, Herman Althof, Andrew H. Smith, William R. Whitehead, E. Darwin Hudson, and P. Brynberg Porter.

Committee on Meteorology: Drs. D. H. Goodwillie (chairman), Salvatore Caro, Charles S. Wood, C. B. McQuesten, and Simeon N. Leo.

Committee on Diseases: Drs. Charles P. Russell (chairman), M. H. Henry, Daniel A. Leavitt, Leonard Weber, D. H. Goodwillie, Gustavus Langmann, Salvatore Caro, Charles S. Wood, C. B. McQuesten, and Simeon N. Leo.

Committee on Registration: Drs. D. B. St. John Roosa (chairman), C. R. Agnew, and Joseph Kammerer.

Committee on Finance: Drs. Joseph E. Janvrin (chairman), Edward Bradley, and Horace P. Farnham.

The President then delivered the Inaugural Address, which we publish elsewhere, recounting the history of the Society for the past year; touching on the position it had already taken upon some important questions, and was destined to take upon others; and concluding with an extended discussion of the subject of foundling asylums. In response to a question from Dr. Peters, he said that the statistics of mortality in these institutions would tell very different stories according to the way they were made up. There was but one plan of taking them which furnished any sound basis of calculation—that of following up every child, of the number admitted in a given period, to its death, removal from the institution, or continued presence therein at a given subsequent date. He had taken the trouble to do this in the statistics of the Nursery and Child's Hospital which he had presented. As statistics were often prepared, the children long resident in an asylum were counted over and over again at each monthly or quarterly census, and as they were meanwhile growing older and less subject to disease, this tended to give figures more favorable to the asylum than its real influence on the new admissions would warrant.

As the next Stated Meeting would regularly fall on Christmas, the by-law fixing its date was suspended, and the meeting adjourned, subject to the call of the President and Secretary.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, December 7, 1871.

Dr. E. R. Peaslee, President, in the chair.

After hearing the Annual Reports, making nominations for officers, and other business, the Academy resumed the discussion of Dr. Nott's paper, read at the last meeting, on

THE NATURAL HISTORY OF YELLOW FEVER.

Dr. J. C. Peters exhibited a map of the course of the fever in Texas, Louisiana, Alabama, and other States, in 1867, and remarked as follows:

The Report of the War Department on Yellow Fever in the U. S. Army, for 1867, has been appealed to in support of some of Dr. Nott's views; but it seems to be in direct contradiction to them. On page xviii. we read: "The more thoroughly the facts connected with the spread of yellow fever in the U. S. Army are known, the more strongly they appear to favor the *exotic* origin of this disease in the (whole of the) United States."

The reports clearly indicate two foreign sources from which the disease was imported, viz.: Vera Cruz, Mexico, and Havana, Cuba.

From Mexico it was brought to Indianola, and from there sent up into many parts of Texas. The fever was first introduced at New Orleans from Havana, and then forwarded up the Mississippi and Red Rivers, to Alabama, Tennessee, Mobile, Natchez, Memphis, and other places.

During the month of May twelve vessels arrived at New Orleans from the infected port of Havana, and fifty-two from Vera Cruz. So certain, doubtless, were the New Orleans physicians and officials that yellow fever was of indigenous origin, that no quarantine was enforced. The barks Jessie and Florence Peters probably introduced the first cases into New Orleans. This thoughtlessness or ignorance was followed by 1,349 cases and 428 deaths among the U. S. white troops; 171 cases and 25 deaths among the colored troops; and by 31 cases and 10 deaths among the U. S. medical officers.

But we are more particularly interested in the history of yellow fever in the city of New York. In 1798 it commenced, according to Hardie, in Front Street, on the East River, near Coenties Slip, and by July 20th there were cases in different parts of the city, and in streets very remote from each other. The total number of deaths was 2,086. In 1799 there were 260 cases at the Marine Hospital at Staten Island, taken from vessels arriving from the sickly ports in the West Indies. It broke out in New York, near Old Slip, and was at first confined to the streets in that neighborhood. Hardie attributed the outbreak to the ship Fame, from the West Indies; for, of seven persons who discharged her damaged and putrid cargo, and pumped out her bilge-water, which was so offensive that the neighbors were obliged to shut their windows, not one escaped sickness, and two died. In the nearest house five persons sickened, and two died; in the next house three sickened at one time, and one died. Many of the boatmen whose vessels lay in the slip fared no better, a number of them falling victims to the stench or contagion. This form of fever was undoubtedly communicable, and spread to distant parts of the town by means of the sick.

In 1803 there were 1,639 cases, and 605 deaths. The first cases were noticed in Coffee-house Slip, but the disease was soon discovered in many other places; in fact, fewer parts of the city were left exempt than on any former occasion. The disease did not break out on the North River until infected shipping was removed to it from the East River.

Dr. John B. Rogers, health-officer at Staten Island, in 1805, says there were always from 40 to 60 vessels in quarantine, from June to October, which had lost one or more of their crews from yellow fever; and 24 of these came up to the public wharf. The usual duration of quarantine was four days, and six days was regarded as a long time. Dr. Rogers says none were attacked on Staten Island, but some who went up into the poisoned air of the city contracted yellow fever. He was certain that it did not arise from any neglect of duty at quarantine, but Dr. Hosack says: "If I had before entertained any doubt of the origin of this disease, I now have none. It is not the product of our own soil and climate. The unlimited intercourse which has existed between the quarantine-ground and this city sufficiently accounts for the pestilence."

We now come to the celebrated epidemic of 1822. I was then three years old, living in Broadway, just above Pearl Street, and nearly opposite the old New York Hospital.

I have often heard my maternal grandfather and other old citizens speak of this epidemic. My grandfather had vessels sailing to and from Mobile, Charleston, and Savannah, in the cotton-trade, and took an active interest in the sanitary measures enforced against the epidemic. He was a stanch Episcopalian, a non-contagionist, and not inclined to believe in the importation of the disease; he was equally disinclined to believe that Trinity church-yard had any thing to do with its origin or increase.

I have always had intimate and extensive family connections in Mobile, Augusta, and, for the last twenty years, in Texas. The facts in which I place most confidence are as follows: The fever commenced in Rector Street, on the south side of Trinity Church. The infected vessels Nilo, Shamrock, Florida, and Eliza Jane, were lying at the Rector-Street wharf. Trinity church-yard was offensive to persons in its vicinity, especially in the evening. It was distant only 80 feet from the residences of persons sick of yellow fever, some of whom were afterward buried in it. Pascalis says Trinity church-yard, encompassed by its massive walls, was like a great reservoir of contaminated fluids suspended above Lumber, Greenwich, Washington, and other lower streets. The springs of water in Thames and other streets became so offensive that the inhabitants moved away. For more than a century this church-yard had been assigned as a public burial-place, and it was the privileged receptacle of strangers, and those who did not belong to any religious denomination. So crowded was it, that a charnel-house was erected for the storage of the bones and remains of those who were dug up to make place for new occupants. The continual opening of graves and disinterring of remains created an intolerable stench, often complained of and testified to before the Board of Health.

The U. S. ship Enterprise also came in for its share of blame. It had carried over 100 Spanish soldiers from Florida to Havana in 1821, when the former Territory was ceded to the United States. Some of her crew were attacked with yellow fever and died of it. She carried the disease back to Pensacola, and produced a carnage there which, it is said, was unexampled in the previous history of the disease. She then came to New York, with Lieutenant Cox and ten of her crew sick with yellow fever on her arrival. In order to cleanse the vessel properly, her crew were landed on July 11th; she was well whitewashed; unslaked lime was put in her timbers after they were cleared out; the ballast was washed and whitewashed; and several wind-sails were constantly kept in her hatchways; yet the disease broke out soon after the crew went on board again.

There was also an upper district of yellow fever in Lombardy, Catharine, Grand, and other streets, which is said to have originated from the ship Superior, Captain Jocelyn, from New Orleans. A Mr. Cary, who had been sick with yellow fever in New Orleans, came a passenger in the above vessel, and went with his clothes to his mother's house in Lombardy (now Monroe) Street; his mother and two other ladies died of yellow fever in the same house.

I took great interest in the epidemic of 1856, for I had a mother, sister, several nieces, and other relatives, and many friends, living on Staten Island; also some friends and patients on Long Island, on Bay Ridge, near Fort Hamilton. I attended Mr. Chandler White, whose case has some points of interest. He had sprained his ankle, and had not been out for several weeks. About 70 yellow-fever ships lay in Gravesend Bay, within a short distance of his house and grounds, which were situated on a high bluff, and were in the most perfect sanitary condition. He was the first victim, and died of the most marked and severe form of hæmorrhagic and purpuric yellow fever. The disease arose either from breathing the infected air which blew for a continuous period over the quarantine vessels, or from the infected matter cast overboard from these vessels and washed along the shore.

The same is true of the garrison at Governor's Island. Mattresses and clothing in considerable quantities floated ashore from the infected ships; as many as eight or ten of these mattresses were daily washed up by the tide, and burned by the police.

Dr. Nott, understanding Dr. Peters to advocate the contagiousness of yellow fever, reiterated some of his former arguments against this view, and in favor of its portability, as well as those leading to the supposition that certain epidemics showed a non-portable type of the fever.

Dr. A. N. Bell, referring to Dr. Nott's prediction that the epidemic of twenty years ago would, by pursuing its regular rate of travel, reach Norfolk in 1855, as in fact it did, deemed it unnecessary to resort to the notion of "epidemic force" to explain its arrival in that city. The evidence seemed to him conclusive that it was introduced there by the steamer Ben Franklin. This ship arrived at Norfolk from St. Thomas early in July, and after a quarantine of two weeks was taken to Gosport for repairs. Here her limbers were laid open, and her disease-germs fairly sown. How they grew and spread, and what a harvest of death they furnished, was known to all.

The speaker had never seen any type of yellow fever not portable, and strongly doubted the existence of any such. He would sooner believe that intermittent and remittent might themselves sometimes be transported, at least by ships; and he had certainly seen these diseases occur on board ship under circumstances which led irresistibly to the conclusion that their causes were domiciled in the vessel, exactly as were those of yellow fever. There were several vessels in the navy which had suffered from recurrent outbreaks of all three diseases on sailing into a hot, moist climate, without any fresh exposure either from entering infected ports, or from the reception of infected persons or fomites.

The non-contagiousness of yellow fever would seem to have been established, beyond all dispute, by the observations of the last thirty years, and by the most satisfactory tests. When the French frigate Gomér came into Pensacola (he thought about the year 1838), with several hundred cases of the fever on board, the popular dread of contagion caused the surgeons much difficulty in procuring proper attendance for the sick. One of them, to quiet these fears, slept all night in the bed on which a patient had just died, the bedding being still foul with black vomit and other discharges; and no harm resulted.

With regard to the conditions necessary to the development and spread of the disease, the speaker adhered to his opinion, expressed at the last meeting, that yellow fever rarely or never prevails at any place as an endemic, arises in any ship, or is introduced into any port as an epidemic, except where there has been a continuance, day and night, of a temperature of about 80° Fahr., for a longer or shorter period, accompanied by a moist atmosphere and probably by decomposition of organic matter. Certainly these conditions were found along our own southern Atlantic coast, as well as on the coasts of Mexico, Central America, Africa, and the islands where the fever had prevailed most frequently and extensively. And the same was true of Norfolk in 1855, Bay Ridge in 1866, and Governor's Island last year.

While not underrating the important contributions to the etiology of yellow fever made by those long resident in its endemic haunts, as Mobile and New Orleans, Dr. Bell yet claimed that the army and navy surgeons, English and French as well as American, had, in their widely-extended range of observation, done more to settle the character of the disease, and especially its portability and its non-contagiousness, than any other investigators.

Dr. G. M. Smith referred to the indefatigable exertions and the bold experiments of Dr. Chervin, forty years ago, as having furnished probably the most conclusive evidence upon the point last named. Leaving his home in Paris, expressly to study the subject by personal observation, he had travelled extensively; had demonstrated the identity of the yellow fever in this country with that in the British colonies and in Spain; established its non-contagiousness by the severest tests, such as swallowing black vomit; and returned to his own land to secure a modification of its quarantine laws.

Dr. Austin Flint, having had comparatively little practical acquaintance with the disease, would only congratulate the Academy that it had been able to enrich its Transactions by the views of one whose experience had been so great, and whose conscientious care as an observer and ability as a reasoner were so well known. He recollected that when the term "portability" was first introduced into medical literature, by the late Dr. Mitchell, of Philadelphia, it excited much ridicule. But it had since become accepted as well expressing the idea that the germs of a disease may be transported, although not produced in the bodies of those affected with it. That yellow fever was thus portable, and was not contagious, could hardly be regarded as longer open to question; and he had no doubt full investigation would prove that precisely the same considerations which had determined this with reference to yellow fever would apply equally well to epidemic cholera.

Dr. Elisha Harris, from a pretty thorough familiarity with the literature of the subject, and a careful observation of such epidemics as had come under his notice, adopted fully the doctrine of the non-contagiousness and the general portability of the fever; and he was inclined to agree with Dr. Nott that it was sometimes not transportable, though he considered this point still unsettled. In the epidemic of 1855, when Norfolk and Portsmouth were smitten almost like the hosts of Sennacherib, refugees thence flocked North, with their baggage, and came under his supervision at Staten Island. Some arrived sick with the fever—indeed, had been sick when they started. It was observed that there was no communication of the fever coming from this source, although, that same year, the fever coming from other sources did communicate itself. Again, Baltimore, which fairly invited the fever by its daily lines of steamers and packets from Norfolk and Portsmouth, protecting itself by no sanitary cordon, yet escaped altogether. The speaker could not close his eyes to such facts as these, although entertaining the highest respect for the experience and judgment of Dr. Bell. While we were still ignorant of the nature of the yellow-fever poison, we could not assign a reason why the disease should at some times spread freely, at others not. But further investigation might elucidate this point, and enable us to diagnosticate between the dangerous type and that against which we need take no precautions. Meanwhile, knowing that the disease would sometimes appear at unexpected places, and even under comparatively low temperatures, as at Swansea, Wales, in latitude 51° 37′ north, where it was carried by the bark Hecla, in the month of September, 1865; knowing that it would establish and propagate itself in places under the best hygienic conditions, as at Staten and Governor's Islands, with the purest of air and such policing as no city could hope for—it was necessary to regard it always as a dangerous enemy, and to consider every vessel arriving from an infected port between the first of June and the first of October as presumably a source of peril, to be rigidly quarantined. No matter how loudly commerce, or popular feeling, or even the general voice of the profession, might protest against such a measure as a needless embarrassment of trade and travel, it was the duty of quarantine-officers to act in accordance with these well-established facts.

Dr. Walser, of Staten Island, said that thirteen years' quarantine observation of yellow fever, as it came from the coast of Africa, from South America, and from the West Indies, had taught him to recognize three prominently-distinct types—the purpuric, the remittent, and the typhoid. Each year's added experience had confirmed the correctness of this classification.

As illustrating the means of communication of the fever, and its period of incubation, the speaker referred to the case of the United States steamer Alabama, which came into this port, with yellow fever on board, in 1865 (?). After a cruise of eight months in the West Indies, she entered the harbor of Havana, June 27th, and left the next day for Key West, having had no communication with the shore or the shipping in port, except putting an officer on board the Roanoke, and mailing some letters. There were a few cases of yellow fever in the harbor at the time. Arrived at Key West, June 28th; and took in coal from a Northern vessel, and provisions from the Government storehouses. The latter were chiefly stowed in the after-orlop; where also was put the baggage of an engineer, who took passage at Key West for Cape Haytien, to join the Rhode Island. Sailed from Key West, July 9th, and arrived at Cape Haytien the 14th, where a seaman, who had been confined in the after-orlop, one night after leaving Key West, and had slept on the baggage, was taken ill with yellow fever, and died in three days. The engineer, who had had frequent occasion to get at his baggage during the voyage, sickened on the 15th, and died on the 19th, he having been transferred to the Rhode Island on his arrival. The next victims were the ward-room and steerage officers, whose quarters were adjacent to the after-orlop; and they were followed by the marines who had guarded the prisoner confined there. It was not until twelve days subsequent to the first development of the disease, that any of the crew on the berth-deck were taken ill. The vessel left Cape Haytien, July 27th, and arrived in quarantine at New York, August 2d. Up to the date of her arrival, twenty-eight cases of yellow fever had occurred on board, four of which had died at Cape Haytien, and five on the passage thence. The eleven cases remaining sick were transferred to the hospital-ship on the day of arrival. All the rest of the officers and crew, to the number of one hundred and sixteen, were, the day following, received on board the vessel Magnolia, sent down for that purpose from the navy-yard. They were transferred in entirely new clothes, furnished by the Department, and nothing whatever was permitted to be taken from the Alabama to the Magnolia, except a suit of white linen clothing, previously well aired. Fifteen of the crew came down with the fever, after their transfer to the Magnolia, as follows: one, August 3d, the day of transfer; three, August 5th; two, August 6th; seven, August 7th; one, August 8th; and one, August 9th. After the 9th, that is, the sixth day from transfer, no new cases occurred among the original crew of the Alabama. Eight ship-keepers were put on board the Alabama, with strict orders not to go below-decks. Of these, five were taken ill, but not one of them until after the sixth day aboard.

These facts pointed pretty clearly to a period of incubation of five or six days. It was almost equally evident that the focus of the disease on the Alabama was the after-orlop; but whether the infected fomites were the baggage of the engineer who had been a month at Key West, or the provisions which had been in store at the same place, was left doubtful. It was, in all probability, one of these, for the after-orlop contained nothing else; and the coal recently stored in the hold under the orlop was received from a Boston vessel, that before in store having been taken at St. Thomas, in March.

Dr. B. S. Thompson alluded to the epidemic through which he had passed at Key West, in 1864, and which he had described in the *Medical Record*, August 1, 1868. The disease had broken out that year as early as the 23d of April, though it rarely began there before June or July. In this instance, it was supposed to have been introduced by a lot of infected clothing, some of which was sold at auction. A chaplain, who bought a saddle from the lot, was the first victim, and his case was accounted the severest of the season.

The Key contained many stagnant and offensive ponds of fresh water, constantly covered with a green slime. The temperature averaged, from April to October, 86° Fahr., and from October to April, 64°, frost being unknown. Sporadic cases occurred there every year, and epidemics every two or three years. It was observed that the upper stories of buildings were the safest, as remarked in the paper of Dr. Nott. Few of the nurses and attendants took the disease; and, though officers and men were frequently passing between this port and Fort Jefferson, Tortugas, ninety miles distant, yet no case occurred that season at the latter place. This was deemed remarkable, as the fort contained about nine hundred prisoners of war, mostly from Northern States, besides its garrison of one thousand men.

Referring to some clinical features of the disease, the speaker dwelt particularly upon a characteristic odor, which he had himself most distinctly observed after his own convalescence from a severe attack. The same thing had been noted by Dr. Harvey E. Brown, Assistant Surgeon, U. S. A., in his report from New Orleans to the Surgeon-General (S. G. O., Circular No. 1).

It was voted to discuss the prophylaxis and treatment of yellow fever at the next meeting.

Adjourned.

NEW YORK MEDICO-LEGAL SOCIETY.

REPORT ON CRIMINAL ABORTION.

REPORT of the committee appointed by the New York Medico-Legal Society, "to take into consideration the subject of the practical value of the existing statutes in this State relating to the prevention of criminal abortion, to report thereon to the Society, and to present such suggestions for further legislation on the subject as the committee may deem expedient."

The committee, appointed in pursuance of the above resolution, beg leave to submit the following report:

Two important influences, law and public opinion, deserve to be considered in relation to the practice of criminal abortion:

I. The history of public opinion on the subjecte onveys a forcible illustration of the evil results of erroneous teaching. Had popular views regarding the *fœtus* been the opposite of what they were and even still are, it is not too much to say that millions of lives, some, maybe, of priceless worth to society, would have been spared.

In ancient days, they who claimed to instruct the multitude in the theory and practice of morals not only looked leniently on fœticide, but, under certain circumstances, openly recommended it. Plato advocates it in a contingency mentioned in the "Republic" (lib. v.). Aristotle ("Polit.," lib. vii., c. 17) declared that no child should be allowed to be born alive whose mother was more than forty, or father over fifty years old. Lysias ("Pleadings," quoted by Harpocration) maintained that forced abortion was not homicide, because a child *in utero* was not indued with a separate existence.

Such being the teachings of some of the great leaders of public opinion in ancient times, first, with respect to the question of the separate life of the *fœtus in utero*, secondly, on the inference that abortion, far from being a crime, was allowable and in some cases commendable, it is not in the least surprising that it should have become a practice so prevalent in after-days as to merit the denunciations of Ovid ("Amor.," lib. ii.), of Seneca ("Consol. ad Helv.," 16), and of Juvenal (*Satire* vi., 594).

The ancient laws indeed, more circumspect than a misguided public opinion, are said to have punished artificial abortion with great rigor (*Cicero pro Cluentio. Strobœus Serm.* 73). But among the ancient peoples it was largely practised notwithstanding, under the unfortunate belief that the *fœtus*, not being alive *in utero* with a life of its own, had no special claims to humanity and no rights which they were, morally at least, bound to respect. The Romans carried this view to so outrageous an extreme as to hold that the *fœtus* was a mere excrescence of the mother, a simple appendage from which she could free herself as innocently as she might be rid of a troublesome disorder. Consequently, induced abortion became so common in Rome that the greedy quacks who flocked to her from all quarters could barely supply the demand for their services and nostrums. More particularly did the evil prevail, as in our own day and city, among the well-to-do—the so-called respectable classes. To this fact Juvenal bears ample testimony (*Satire* vi., 591–596). After paying a compliment to the exemplary patience with which the inferior class of matrons bore the pains of labor and the fatigues of nursing, he pours out the phials of his irony on the high-born and pampered dames of his day. "You'll scarce hear tell," he writes, "of a lying-in among ladies of quality; such is the power of art, such the force of medicines prepared by the midwife to cause barrenness and abortion."

> "Sed jacet aurato vix ulla, puerpera lecto.
> Tantum artes [illegible], tantum medicamina possunt,
> Quæ steriles facit, atque homines in ventre necandos
> Conducit."

At length Christianity came, to measure swords with the growing evil. For a time the contest was warm. A society corrupted by ill-gotten wealth and sensual gratification would not surrender such convenient doctrine without a determined resistance. The battle waxed fierce, but the already-assured triumph of the purifying faith was postponed by a compromise (how originated or by whom proposed does not appear) no less disastrous than the pagan theory it supplanted. By this compromise it was agreed to consider the *fœtus* as endued with life only from the date of the maternal sensation called "*quickening*." Abortions forced after "*quickening*" were branded as serious crimes, but all so caused before this period were suffered to pass unnoticed. Henceforth "*quick*" became a word of evil omen. It is true the canon law subsequently disregarded this compromise, declared the *fœtus* alive from conception, and condemned its destruction at any period of *utero-gestation* as a great and wicked crime. The Christian Church, to its eternal honor be it said, has ever advocated and enforced the principle of the inviolability of fœtal life.[1] But the mischief could not be undone. A doctrine, only a degree less heartless than its pagan predecessor, took a firm hold on society. How effectually it influences the opinion and practice of our own time, how completely it has permeated all, but more particularly the higher ranks of contemporary society, needs not to be insisted upon here.

Among those who are competent to pronounce on this question of "*quickening*," there is, however, but one opinion, and to it your committee ask the undivided attention of the community: *The fœtus is alive from conception, and all intentional killing of it is murder.* The world is free to discuss the transcendental problem concerning the stage of development at which the *fœtus* becomes endowed with a soul. Some may believe, with Plato, that this event is deferred till birth. Others may hold, with Aristotle, that it occurs at the fortieth day for boys and the eightieth for girls! Only, let such opinions have their due place and weight. Whatever may be

[1] "Omnes, qui abortûs seu fœtus immaturi, tam animati quam inanimati, formati vel informis, ejectionem procuraverint, pœnas propositus et inflictas tam divino quam humano jure, ac tam per canonicas sanctiones et apostolicas constitutiones quam civilia jura adversus veros homicidas incurrere, hâc nostrâ perpetuo valiturâ constitutione statuimus et ordinamus."—(*Reiffenstuell, Jus Canonicum Universum*, tome iii. Paris, 1854. Quoted from Storer's "Criminal Abortion," p. 38, note.)

their value as evidences of intellectual activity, they have no bearing whatever on the great practical question of child-murder. If there were never such an existence as a soul, if men perished utterly when they died, laws against murder would still hold good, because laws against murder were enacted not for the soul's sake, but to preserve the peace and even the existence of society. Opinions such as these now indicated are harmless enough if jealously confined to the field of abstract speculation. It is only when suffered to influence conduct toward the *fœtus* that they become delusive and pernicious errors. Too great, unfortunately, has been their power in this respect; hence the necessity of combating them and of exhibiting them to the public in their true light, as evils which have long waged war against the dearest interests of society. All such speculations cannot be too strictly excluded from the sphere of practical morals; and furthermore, the public should be taught that the significance attached to "*quickening*" is unfounded, that the current deductions therefrom, already indicated, are utterly erroneous and immoral, and warned that the community, whose regard for fœtal life is influenced by either, is courting a terrible retribution. Herein is ample work for the two great educators of a nation, its pulpit and its press.

Equally groundless is the opinion, inherited from pagandom, that the *fœtus*, because dependent for existence on its connections with the mother, has not a separate life, and consequently may be wilfully destroyed without incurring the guilt of murder. A moment's consideration will suffice to show the absurdity of this view. All human beings (confining ourselves for the sake of apt illustration to this genus) depend for life on the medium of their existence. Change this medium completely, and they must suddenly die. Now, what ought to be thought of a proposal to excuse murder on the ground that human adults are not indued with independent existences? Yet, monstrous though it would seem, it is the very apology offered, and among some accepted, for fœticide.

II. The point of chief interest, in a legal view of the subject under consideration, centres in the history of the word "*quick*."

Wharton informs us that, at common law, the destruction of an infant unborn, *if it had quickened*, was murder. (1 Russell on Crimes, 671; 3 Coke's Inst., 50; 1 Hawkins P. C., ch. 13, §16; 1 Hale P. C. H., 34; 1 East P. C., 90; 3 Chitty Crim. Law, 798.)

The procuring an abortion on a woman, *after* "*quickening*," was a common-law misdemeanor, although neither mother nor child perished. If it were procured upon the woman *before* "*quickening*," the offender could be convicted at common law of assault and battery upon the woman, although, if such abortion were procured with the consent of the woman *before the child had quickened*, it was not an indictable offence at common law, for the reason that *the child was not then supposed to be indued with life*. (Commonwealth *vs.* Bangs, 9 Mass. R., 387.)

The statutory laws of New York laid equal stress on the term "*quick*," making it the basis of a distinction between the degrees of guilt of criminal abortion. Thus, the Revised Statutes first enacted, which took effect in 1830, provide that "the wilful killing of an unborn *quick child* by any injury to the mother of such child, which would be murder if it resulted in the death of such mother, shall be deemed manslaughter in the first degree." (2 Rev. Stat., 1st ed., p. 661, § 8.)

This section is still in force, never having been repealed.

Section 9 provides that "every person who shall administer to any woman, pregnant with a *quick child*, any medicine, drug, or substance whatever, or shall use or employ any instrument or other means, with intent thereby to destroy such child, unless the same shall have been necessary to preserve the life of such mother, or shall have been advised by two physicians to be necessary for such purpose, shall be deemed guilty of manslaughter in the second degree."

This last section was subsequently repealed by laws of 1845, ch. 260.

Without entering into details respecting the amendments to the statutes on this subject, it will be sufficient to say generally that, from 1845 to the present day, section one of the statute against criminal abortion, defining the character and degree of the crime, has retained the obnoxious term *quick.*

In 1869 the Legislature (2 Laws of 1869, chap. 631, p. 1,502) repealed section two of the act of 1845, and section one of the act of 1846, and enacted as follows:

SECTION 1. "Any person who shall administer to any woman with child, or prescribe for any such woman, or advise or procure her to take any medicine, drug, substance, or thing whatever, or shall use or employ any instrument or other means whatever, with intent thereby to produce the miscarriage of any such woman, unless the same shall have been necessary to preserve her life, shall, in case the death of such child or of such woman be thereby produced, be guilty of manslaughter in the second degree."

This section omitted the word "*quick*," thereby relieving the prosecution from the necessity of proving a fact almost impossible. It further altered the *intent* to the production of the miscarriage of the woman, instead of to the destruction of the child. Its defects will be subsequently pointed out.

SEC. 2. "Whoever shall unlawfully supply or procure any medicine, drug, substance, or thing whatever, knowing that the same is intended to be unlawfully used or employed, with intent to procure the miscarriage of any woman, whether she be or be not pregnant, shall be deemed guilty of a misdemeanor, and shall, upon conviction, be punished by imprisonment in the county jail not less than three months, nor more than one year, or by a fine not exceeding one thousand dollars, or by both such fine and imprisonment."

SEC. 3. "Every person offending against either of the provisions of this act shall be a competent witness against any other person so offending, and may be compelled to appear and give evidence before any magistrate or grand-jury, or in any court, in the same manner as other persons; but the testimony so given shall not be used in any prosecution or proceeding, civil or criminal, against the persons so testifying."

To sum up the whole matter, the law in the State of New York at the present time, upon this subject of criminal abortion, stands as follows:

1. "The wilful killing of an unborn *quick* child by any injury to the mother of such child, which would be murder if

it resulted in the death of such mother, shall be deemed manslaughter in the first degree. (2 Rev. Stat., 1st ed., p. 661, § 8.)

"Every woman who shall solicit of any person any medicine, or drug, or substance, or thing whatever, and shall take the same, or shall submit to any operation or other means whatever, with intent thereby to procure a miscarriage, shall be guilty of a misdemeanor, and shall upon conviction be punished by imprisonment in the county jail not less than three months, nor more than one year, or by a fine not exceeding one thousand dollars, or by both such fine and imprisonment." (1 Laws of 1845, chap. 260, § 3, p. 285.)

3. "Any person who shall administer to any woman with child, or prescribe for any such woman, or advise or procure her to take any medicine, drug, substance, or thing whatever, or shall use or employ any instrument or other means whatever, with intent thereby to produce the miscarriage of any such woman, unless the same shall have been necessary to preserve her life, shall, in case the death of such child or of such woman be thereby produced, be guilty of manslaughter in the second degree." (2 Laws of 1869, chap. 631, § 1, p. 1,502.)

4. "Whoever shall unlawfully supply or procure any medicine, drug, substance, or thing whatever, knowing that the same is intended to be unlawfully used or employed with intent to procure the miscarriage of any woman, whether she be or be not pregnant, shall be deemed guilty of a misdemeanor, and shall, upon conviction, be punished by imprisonment in the county jail not less than three months, nor more than one year, or by a fine not exceeding one thousand dollars, or by both such fine and imprisonment." (2 Laws of 1869, chap. 631, § 2, p. 1,502.)

5. "Every person offending against either of the provisions of this act shall be a competent witness against any other person so offending, and may be compelled to appear and give evidence before any magistrate or grand-jury, or in any court, in the same manner as other persons; but the testimony so given shall not be used in any prosecution or proceeding, civil or criminal, against the persons so testifying." (2 Laws of 1869, chap. 631, § 3, p. 1,503.)

The defects observable in the existing law, more particularly in the first section of the act of 1869, above quoted, may now be pointed out:

1. That section does not justify the procurement of a miscarriage, *except* when necessary to save the life *of the mother.* Cases may, however, occur where, in the judgment of an experienced medical man, premature labor should be induced, and be absolutely necessary to save the life *of the child.* Hence, the statute ought properly to comprehend either contingency.

2. As the section now reads, the offence is declared to be "manslaughter in the second degree," which is a statutory felony and punishable as such (3 R. S., 5 ed., p. 941, § 20), by imprisonment in a State-prison, "for a term not less than four *and not more than seven years.*"

As the proper name for the intentional destruction of the *fœtus* is unquestionably *murder*, it is hoped the time will soon arrive for its punishment as such. It has been truly and forcibly said that an induced abortion, if undertaken before the viability of the *fœtus*, necessarily contemplates and intends its death. And it may be added, the same is nearly certain to result at a subsequent period, from injuries inflicted on itself or its mother during the operation. The subsequent death of the latter, when, as too often happens, she succumbs to the operation or its consequences, does not change the character of the crime, but rather adds to it the enormity of *a double murder.* But it is of importance, as fixing the true character of the deed in at least its moral aspect, to bear in mind that an abortionist does not intend the death of the mother. Through all his wickedness and ignorant bungling, it is at once his interest and aim to save her life. But he must always and necessarily intend the destruction of the *fœtus*, if he attempts or induces an abortion before the period of its viability. Consequently, as already observed, the crime is, in regard to the *fœtus*, an act of cool, deliberate, unrelenting murder, or attempt at murder, and the mother is often, undoubtedly, *particeps criminis.*

In view of the foregoing fact, it has been suggested by some of the most influential jurists of this city that the offence specified in the first section of the act of 1869 should be made a capital felony. One of these distinguished gentlemen, Judge Bedford, to whom the committee is indebted for valuable assistance, has since explained that his advocacy of this view was only intended for its effect on "professional" abortionists. However intrinsically just such a view may be, and is in the opinion of your committee, any serious attempt to carry it into practice at the present time would probably result in lessening the chances for a conviction in any case. This is evident from the recent trial of Rosenzweig in the New York General Sessions, when two jurors united in a recommendation to mercy, thus showing a disinclination to convict even of the felony, though the prisoner did not attempt to justify his conduct, but rested his defence upon his alleged innocence of the whole matter. And, under the section of the Revised Statutes still in force (2 R. S., 1st ed., p. 661, § 8), it is questionable whether, when the facts of the case warrant the indictment as for murder under that statute, a conviction therefor would not be upheld as good.

For these and similar considerations your committee have not thought it expedient to advocate any change in the *denomination* of the crime under consideration. They have chosen what they believe to be a more practicable course; one which, if adopted, will, they apprehend, give an effectual check to the practice of criminal abortion.

There can be no question, in a community so grievously shocked as this by the terrible deeds of certain abortionists lately exposed, that some serious defect in the existing statutes, which not even the punishment at present authorized by law to be inflicted can overcome, renders them less efficacious than they ought to be in checking child-murder. A statutory enactment punishing the crime with death would be the most effectual preventive if it were practicable; but, while there remains a doubt on this subject, such a change may be substituted in the degree of punishment already awarded as will doubtless prove of almost equal effect. The *maximum* of punishment now inflicted is clearly insufficient for this purpose. In the following act it is proposed to make the offence a felony without specific name, and to fix the *minimum* of punishment at not less than four years, leaving the *maximum* to be proportioned to the degree of guilt, as shown by the facts in the specific case, at the discretion of the court. Such discretionary power seems requisite to meet certain aggravated cases of criminal abortion. After the jury have found upon the facts of the case and returned a verdict, say of guilty, it is then the privilege of the judge, before passing sentence, to consider the circumstances peculiar to the criminal act. It may be a first offence, or other sufficient reasons may lead him to mitigate the severity of the punishment which he is called upon to inflict. On the other hand, the criminal before him may be a "professional" abortionist, a being who recognizes no higher law than his own base interests, whose heart has long ceased to know a humane feeling, whose soul is freighted with abominable crimes, whose hands are stained with the blood of innocent children, victims of his foul lust for gain. The sentiments of our common humanity revolt against so vile a wretch. Shall he be suffered to return to his old haunts and his old evil ways, with appetite whetted for more blood, after a few years spent in prison? All experience utters a solemn warning against so blind a policy.

3. The first section of the act of 1869, while providing for the punishment of any person who shall advise or procure any woman with child to take any medicine, etc., strangely enough, omits to provide for the punishment of any person *who shall advise or procure the use or* employment of any *instrument* or other means. So that, as the law stands, if the seducer, or person desirous of causing the miscarriage of the woman, intentionally advises or procures her to take any medicine, he is guilty of a felony; but quite otherwise if he advises or procures *instruments* to be used for the same purpose.

The passage of the following act will, it is believed, remedy the defects in the existing law:

An Act for the better prevention of the procurement of abortions and other like offences, and to amend the laws relative thereto.

The People of the State of New York, represented in Senate and Assembly, do enact as follows:

SECTION 1. The first section of an act entitled "An act relating to the procurement of abortions and other like offences," passed May 6, 1869, is hereby amended, and shall read as follows:

SECTION 1. Any person who shall administer to any woman with child, or prescribe for any such woman, or advise or procure her to take any medicine, drug, substance or thing whatever, or shall use or employ, or advise or procure her to submit to the use or employment of any instrument or other means whatever, with intent thereby to produce the miscarriage of any such woman, unless the same shall have been necessary to preserve her life or that of such child, shall, in case the death of such child or of such woman be thereby produced, be deemed guilty of a felony, and upon conviction shall be punished by imprisonment in a State-prison for a term not less than four years.

SEC. 2. The eighth section of the first article of the second title of the first chapter of the fourth part of the Revised Statutes is hereby repealed.

SEC. 3. This act shall take effect immediately.

Committee:
- JAMES J. O'DEA, M. D., *Chairman*,
- ELBRIDGE T. GERRY,
- GEORGE F. SHRADY, M. D.,
- WILLIAM SHRADY,
- STEPHEN ROGERS, M. D., *ex-officio member*,
- GUNNING S. BEDFORD, *member by invitation.*

Bibliographical and Literary Notes.

ART. I.—*On Dactylitis Syphilitica, with Observations on Syphilitic Lesions of the Joints.* By R. W. TAYLOR, M. D., etc. Reprinted from the *American Journal of Syphilography and Dermatology.* New York: F. W. Christern. 1871, pp. 30.

DR. TAYLOR'S position as physician for venereal and cutaneous diseases at the New York Dispensary, at which institution a vast number of cases of venereal disease come under his observation, has furnished him with an example of that rare and hitherto little-known affection, syphilitic dactylitis. This lesion, to which Chassaignac first called attention, in 1859, has found only the most meagre description in systematic treatises on syphilis, most authors contenting themselves with a bare reference to a clinical lecture devoted to it by Nélaton, in 1860. This surgeon reported a case, under the name of *panaris syphilitique;* and referred to another which had come under his observation. Cases were subsequently published by Züche, of Berne, by Archambault, by Risel, and by Berg, of Copenhagen—all in Continental journals.

After a short and excellent introduction, relating to the peculiarities of the various syphilitic lesions of the fingers and toes, Dr. Taylor details his own case of dactylitis, and then presents the leading points in the other recorded cases, adding one of recent occurrence in Dr. B. W. McCready's service at Bellevue Hospital.

Dactylitis (from δάκτυλος, a digit) "is a chronic specific inflammation, generally involving more than one, and frequently all the phalanges" of one or more of the fingers or toes, without any affection of the nails. Dr. Taylor divides the recorded cases into two classes: 1. "That in which the subcutaneous connective tissue, as well as the fibrous structures of the articulations and the phalanges, is involved. 2. That in which the morbid processes begin in the periosteum and bones, and secondarily implicate the joints, and may or may not be accompanied by deposit in the subcutaneous connective tissue." This division, adopted for the purpose of simplicity in description, and therefore more or less arbitrary, is borne out by the different clinical history of the two varieties of cases. Dr. Taylor's own case is a type of the first variety, and Dr. McCready's of the second. The lesion seems to consist primarily in the deposit of the neoplasm, known as gummy material, which, being proliferated amid the connective tissue of the fingers and toes, increases the volume of these members, and interferes with their mobility. In some cases it seems that the deposit is strictly confined to one phalanx, in others that it shades gradually off into the next, and in others again involves all the phalanges. It is deposited, as a rule, more copiously over the dorsal than over the palmar and plantar surfaces, and, at the metacarpo- or metatarso-phalangeal articulations, it shades abruptly off into the integument of the hand or foot, forming a perfect ridge, or a kind of ring. The deposit may develop slowly or quite rapidly. It is not isolable, but adheres to the corium. The swollen part retains its normal sensation, and is not attended with pain. It is not peculiarly sensitive, and its chief inconvenience arises from its interference with prehension and locomotion. The lesion shows a peculiar chronicity, and sometimes is only very slightly amenable to treatment. There is also a liability to relapse, and to augmentation in volume, even when the swelling has for a time remained indolent. The usual necrotic tendency of gummy tumor seems to be wanting.

In cases of the first variety there is a violaceous color, and resistant feel to the integument. Coincidently with, or soon after, the deposit in the connective tissue, there is a thickening of one or more phalanges, and of the articular capsule, generally but not invariably of the first phalangeal joint. There may occur a perforated condition of the ligaments, which, under favorable circumstances, is very soon repaired by a deposit of normal fibrous tissue, which may, as it becomes older, have a tendency to contract, and thus fit more tightly to the bones beneath. In general, although these structures are considerably involved, the final result is not very serious. During its progressive stage, it produces a decided impairment of motion of these members, rendering them sometimes immobile; in others preternaturally mobile, so much so that the joint-structures are flaccid, and, though the fingers or toes will, by slight force, bend in any direction, they are not at all responsive to volition: whereas, in its final stage, it may leave the joint either nearly normal or in an impaired condition; as a result of which the phalanges of the fingers and toes are sometimes in the position of superflexion, in others in superextension, or both conditions are combined in the same members.

This thickened condition and impaired nutrition of the ligaments reacts sometimes upon the synovial membrane and the articular cartilage; in this first variety of dactylitis, although there is very considerable articular trouble, there is no effusion into the joint.

The occasional occurrence of crepitus gives evidence of some change in the articular lamellæ of the cartilage, which structure undergoes more or less profound nutritional changes. This lesion generally coexists with grave lesions of the bones, joints, integument, and viscera, and is always the expression of a profound syphilitic dyscrasia. It is generally observed in patients who are past middle age, and in four out of the seven cases it was observed in men. It may occur both early and late in the tertiary period.

In the second variety the inflammatory action may begin between the periosteum and the bone, being then a specific periostitis; or it may commence in the cancellous tissue around the medulla, and is then an osteo-myelitis. The product of these specific processes is gummy material, which causes the enlargement of the bones. The swelling of the fingers and toes in this variety is very considerable. The enlargement is nearly limited to the phalanges which are involved.

The integument becomes very much stretched by the pressure from within, and the surface-markings and articular furrows in it are effaced, and it can only with difficulty be pinched between the fingers, and it may be so very tense that it can scarcely be moved over the parts beneath. Its color varies from a pink to a decided red, and, when the lesion of the bone has been very acute, it may become very much tumefied and sensitive; but this condition is only temporary. In this variety, as in the first, there is no concomitant lesion of the nail, even when the last phalanx is involved.

There are two foci of this specific bone-inflammation, the one more superficial, the other deeply seated.

The changes which Virchow describes as the dry caries of syphilis, and which are generally observed upon the cranium and tibia, have existed in some cases. After the deposit of the gummy material, no inflammatory action is excited, but it slowly produces the death of the bone which it infiltrates, and is finally absorbed, leaving a loss of substance which is not again replaced, the whole process being unattended with suppuration.

The swelling, when originally developed, is softer in the acute than in the chronic form, and this is probably due to the tissue, which is thus rapidly proliferated, being of a colloid character. This variety, of course, produces much deformity, and has a tendency to destructive change; whereas, in the chronic form, the swelling is firmer, and there is a tendency to remain indolent and infiltrate the bone, and finally to be absorbed rather than to break down and to be eliminated.

When the lesion begins as an osteo-myelitis, its course at the commencement may be quite rapid, so that very soon the finger becomes greatly enlarged. The joints may or may not be involved, and in the former case there may be effusion, with thickening of the synovial membrane.

The involution of the gummy enlargements of bone is usually accomplished, even when softening occurs, without the formation of pus. The final results are, that certain portions of the shaft may be wholly absorbed, or the whole shaft may be slightly attenuated. The shafts of the bones may also be rendered light and fragile, or local or general eburnation may take place in them. In all of the recorded cases, the sheaths of the tendons have not been involved.

The *diagnosis* of these lesions is of the utmost importance, for, when their syphilitic origin is recognized, an appropriate treatment may prevent serious destruction, and at least materially lessen the ultimate deformities.

We have thus given a brief outline of the clinical history of this affection, as presented by Dr. Taylor, and will merely add an expression of our high appreciation of the sterling service he is doing in his excellent memoirs on the various features of syphilis.

ART. II.—*Diseases of the Skin; the Recent Advances in their Pathology and Treatment.* By B. JOY JEFFRIES, A. M., M. D. Reprinted from the American Journal of Syphilography and Dermatology. Boston: Alexander Moore, 1871, pp. 79.

THIS essay, which gained the Boylston Prize for 1871, is mainly a compilation of the views and facts which have in recent years been advanced by writers on diseases of the skin. It is, moreover, in itself a review, and we shall therefore not feel it incumbent on us to give it more than a very cursory notice in these pages.

Dr. Jeffries sets out with congratulating himself (and we certainly congratulate his readers) that he is exempt from the necessity of considering the subject of the classification of skin-diseases. The "recent advances" which he has undertaken to give an account of, he defines as principally the results of the labors of Wilson, the St. Louis staff, and Hebra, during the past twenty or twenty-five years. Their imitators, *longo intervallo*, who are also prone to be book-makers, he rebukes without compunction. This he does in a very entertaining style, contriving at the same time to mingle a deal of good sense with his scolding, and, as, of course, individual offenders are not mentioned, nobody will perceive that the garment is cut for his own figure; and so the book will undoubtedly prove welcome reading to our many "dermatologists," although the author's evident straining after effect in diction may call forth an occasional shrug of the shoulders. We look for more or less of imagery in even the prosaic productions of our New England friends, and we find an abundance of it in the little volume before us. Sometimes it is very forcible, as where, for instance, Dr. Jeffries says, "There seem, also, to be a number of invisible and intangible cords, one end attached to the word *dartre*, and the other to the pens of all French dermatologists, so that these cannot move except under some reference to that potent spell."

Dr. Jeffries does not accept the impetigo contagiosa of Fox as proved; neither, apparently, does he recognize a non-syphilitic rupia, nor an acute pemphigus, remarking, in regard to the latter, that Steffen's cases, in which eight children were attacked with an affection thus described, should rather have been called "some epidemic disease with pemphigus-like bullæ." Malignant pustule is very properly dismissed with the remark that it belongs to the domain of general surgery. Reference is made, however, to three recent monographs upon that disease, and we cannot but wonder that, together with these, the very able article of Dr. A. H. Smith, recently published, in the *American Journal of the Medical Sciences*, was not referred to. Under the head of Sclerema, we are told that "Walter reports an anatomical examination of the skin." We are inclined to think that this report was taken at second-hand, and that the gentleman who really reported the examination referred to was Dr. Day, of this city, although the *Archiv für Dermatologie und Syphilis* quoted him as "Dr. Walter."

Dr. Jeffries's essay will prove of some use to the dermatologist as an index to the leading contributions recently made to the science, and will enable those practitioners, who have paid little attention to the progress made in that branch, to ascertain without much trouble what they ought to study up. To the beginner it is of little use, as its subject-matter is in no wise systematic, which indeed was not to be expected.

The book is well printed, upon heavy paper, and, with the exception of having no back-title, attractively bound.

ART. III.—*Fistula, Hæmorrhoids, Painful Ulcer, Stricture, Prolapsus, and other Diseases of the Rectum, their Diagnosis and Treatment.* By WILLIAM ALLINGHAM, Fellow of the Royal College of Surgeons of England; Surgeon to St. Mark's Hospital for Fistula, etc.; late Surgeon to the Great Northern Hospital. London: J. & D. Churchill, 1871.

IN the introductory chapter, the author gives a general idea of the manner in which patients should be interrogated, as regards their symptoms, and proceeds with a description of his method of making an examination of the bowel. No examination he would consider complete without having first administered an injection, and then "the instructed and practised finger, passed into the rectum, affords great information." In regard to specula, he states that he has had many varieties constructed, to be used with or without artificial light, but, in his opinion, none of these are of any general utility—the ordinary plated metal speculum is the one he employs at St. Mark's. For the purpose of examining the rectum very high up, he believes the ordinary glass tubular speculum answers every purpose. And for this examination he places the patient in the position recommended by Dr. Sims for using his speculum in uterine examinations. This position, Mr. Allingham states, was first taught him by Dr. Sims himself.

As regards this position, we are sure that none who have ever tried it would prefer any other; but we think that, if Sims's speculum be used in making these high explorations of the rectum, far more information will be obtained than is afforded by the ordinary glass instrument so commonly in use.

Perhaps the most interesting chapters in this work are the two relating to fistula, which is treated of in the most thorough manner; and the course of practice which is inculcated is certainly very sound. Where a fistula is inflamed, if possible an operation, in the judgment of the writer, should be avoided, as his experience has led him to believe that, if operated upon while in this condition, fresh sinuses are sure to form, and the operation prove a failure.

Contrary to the opinion of many surgeons, the advice is given, if, after having divided the fistula from the external to the internal opening, we find a sinus extending beyond the internal opening, to lay that open also. In some cases, as we have reason to know, this practice would be proper; but we know that it is not essential in every case to a speedy closure of the sinus.

That occasionally fistula will heal spontaneously, and again by means of stimulating injections, is shown by several cases that are cited; still these are very exceptional in his experience, and he, with other surgeons, is of the opinion that the cases are very few and far between that can be cured without operation. The seton is usually more painful, he says, and infinitely more dangerous, than the knife. We are glad to find that but little after-treatment is advised; indeed, he says that many sinuses, after being laid open, are prevented from healing by excessive dressing, and the use of lotions—the only dressing that is recommended being dry wool, and having the wound syringed with a little lukewarm water, all dressing being removed when granulations appear. Stimulating washes are advocated only when the wound is sluggish, or has assumed an unhealthy action.

The subject of fistula in conjunction with phthisis is thoroughly considered. From his researches in this direction, Mr. Allingham is led to believe that a very considerable percentage of fistulous patients have more or less of tubercular bony affection. In his experience he puts it down at the very lowest computation at not less than fourteen per cent. This certainly is very much greater than other authorities would lead us to suppose. In regard to the propriety of operating upon phthisical patients, our experience is quite in accord with the practice here laid down, of objecting to indiscriminate operations upon tuberculous patients, especially in cases of rapidly-advancing phthisis; and also with the statement that, where the operation is performed discreetly and with favorable surroundings, our patients will do well, and suffer no injury, by the cure of their fistula. We have not space to follow all the conclusions which are arrived at in the consideration of this subject. The advice given never to operate while a patient has frequent cough, no matter from what cause, because the jarring of the parts produced thereby is liable to prevent healing of the wound taking place, appears very sensible, to say the least.

In speaking of the operations upon hæmorrhoids, we are glad to see the *écraseur* regarded as a barbarous and unsurgical instrument, for that painful and permanent contraction of the anus is very liable to result from its use is beyond doubt; though, being no opponent to the operation by use of the clamp and cautery, our author does not agree with Mr. Smith, in the assertions which he makes regarding its perfect safety, and his remarks upon this subject are worthy of attention.

As to the use of nitric acid in small vascular piles, we do not agree with him when he says that they may be benefited, but that you cannot by any means be certain of effecting a cure by this means. Our experience with the acid leads us to entertain more favorable opinions of its efficacy, and to rank it higher than that of the strong carbolic acid and persulphate of iron, both of which are so strongly recommended, but which in our hands have never given such satisfactory results, in just these cases, as the nitric acid.

Upon the subject of inflamed piles, even when they are down, and constricted by the sphincter, we doubt very much the soundness of the advice given to operate at once. It is true an exception is made to this rule when mortification has set in, for the reason that the tissues are so broken down that the ligatures will not hold, and hæmorrhage will take place. It strikes us that there would be greater safety, if we operate *at all*, to do so after mortification has taken place, rather than when the pile is in a state of acute inflammation. With regard to the different modes of operating, we quite agree with Mr. Allingham in giving preference to the ligature in the majority of cases. The mode of applying the ligature as here recommended carries with it a great deal of force; but, for this method, we must refer our readers to the work itself. As to the safety of the ligature thus applied, the following statement speaks for itself: At St. Mark's Hospital, out of 3,210 cases, there have been but five cases of tetanus, and these five cases were prior to 1858, and not a single case of pyæmia.

In regard to fissure or painful ulcer, our author lays great stress upon the presence of polypi being frequent in connection with this disease, the polypus being situated at the upper or internal end of the fissure. That polypi are very often found in connection with this affection there is no doubt, but in our experience it has not been so frequently observed.

As to the treatment, we are surprised to find the operation of paralyzing the sphincters by forcible dilatation, after the manner first recommended by Récamier, not to have been more frequently resorted to or recommended in the treatment of fissure. The author's objections appear to have but little weight, and in far the greater number of cases we should resort to Récamier's method in preference to the use of the knife, as advocated. In the chapters on "Ulceration and Strictures of the Rectum," we quite agree with the principles laid down for the management of these cases; and must approve the author's opinion that "in the most advanced stages of ulceration and stricture where there are several fistulæ, and the whole rectum disorganized as it frequently is, nothing short of lumbar colatomy offers to the patient any chance of life." The mode of performing this operation is described with great care and minuteness.

In regard to the cause of ulceration and stricture of the rectum, we cannot quite coincide with all the views here expressed, especially in regarding it as being in many cases the result of tertiary syphilis.

There is very much in this book that will well repay the careful attention, not only of the surgeon who sees much of rectal disease, but also of the general practitioner.

The subjects are treated of in a masterly and straightforward manner, and by one who, from long and extensive experience with this class of diseases, is most competent to write such a work.

In conclusion, we must say that, in our opinion, Mr. Allingham has given to the profession one of the most interesting and instructive works ever published on diseases of the rectum.

Art. IV.—*Transactions of the Twenty-sixth Annual Meeting of the Ohio State Medical Society, held at Cincinnati, April* 4, 5, *and* 6, 1871. Cincinnati: Bosworth, Chase & Hall, 1871, pp. 351.

This is a handsome volume, and does credit to the Ohio Medical Society's liberality. In addition to the usual addresses and formal record of proceedings, we find the following papers: Some Points in Uterine Therapeutics, by E. B. Stevens, M. D.; On Physical and Vital Force, by S. S. Scoville, M. D.; On the Antagonistic Power of Opium and Belladonna, by J. A. Little, M. D.; Case of Removal of Both Superior Maxillary Bones, by W. H. Mussey, M. D.; Sanitary Science, by J. R. Black, M. D. (rather "cut up," in debate, by Dr. McIlvaine); Hydrate of Chloral, by D. D. Bramble, M. D. (a tolerable summary of facts, garnished with rhetoric of a very peculiar style); a Report on Ophthalmology, by W. W. Seely, M. D.; On the Ophthalmoscope and the Sphygmograph in the Study of the Physiological Action of Medicine, by R. Bartholow, M. D.; On Reproduction, by J. T. Whittaker, M. D. (a racy article); a Report on Prevailing Diseases in the State, by J. R. Black, M. D.; the Relations of Epilepsy to Insanity and Jurisprudence, by W. J. Conklin, M. D.; and On the Mechanical Treatment of Stricture of the Urethra, by D. S. Young, M. D. (with a wretched lithograph).

Books and Pamphlets Received.—Pulmonary Consumption: Its Nature, Varieties, and Treatment. With an Analysis of One Thousand Cases to exemplify its Duration. By C. J. B. Williams, M. D., F. R. S., etc., and Charles Theodore Williams, M. A., M. D., Oxon., etc. Philadelphia: Henry C. Lea, 1872.

On the Present State of Therapeutics. With some Suggestions for placing it upon a more Scientific Basis. By James Rogers, M. D., formerly Physician to the British Legation, and to the Abouchoff Hospital at St. Petersburg. London: John Churchill & Sons. Edinburgh: Maclaclan & Stewart, 1870.

Lectures on the Clinical Use of Electricity, delivered in University College Hospital. By J. Russell Reynolds, M. D., F. R. S., Professor of the Principles and Practice of Medicine in University College Hospital, etc. Philadelphia: Lindsay & Blakiston, 1872.

The Principles and Practice of Surgery. By John Ashurst, Jr., M. D., Surgeon to the Episcopal Hospital, Surgeon to the Children's Hospital, etc. Illustrated with Five Hundred and Thirty-three Engravings on Wood. Philadelphia: Henry C. Lea, 1871.

Report of the Surgeon-General of the United States Army, on an Improved Method of photographing Histological Preparations by Sunlight. By Assistant-Surgeon J. J. Woodward, U. S. A. Washington: Government Printing-Office, 1871.

Circular No. 3, War Department, Surgeon-General's Office, Washington. A Report of Surgical Cases in the Army of the United States from 1865 to 1871. 4to, pp. 296. Washington: Government Printing-Office, 1871.

On Clinical Education; the Introductory Address to the Clinical Session, 1871–'72, at the Queen's Hospital. By Furneaux Jordan, F. R. C. S., Surgeon to the Queen's Hospital, etc. London: J. & A. Churchill. Pp. 16.

A Practical Treatise on Bright's Diseases of the Kidneys. By T. Grainger Stewart, M. D., F. R. S. E., etc., etc. Second edition. New York: William Wood & Co., 1871.

Transactions of the Twenty-sixth Annual Meeting of the Ohio State Medical Society, held at Cincinnati, April 4, 5, and 6, 1871. Cincinnati: Chase & Hall, 1871.

Can Chloroform be used to facilitate Robbery? By Stephen Rogers, M. D. Reprint from the Journal of Psychology. New York: D. Appleton & Co., 1871.

Transactions of the American Ophthalmological Society. Eighth Annual Meeting, held at Newport, July, 1871. New York: D. Appleton & Co. Pp. 145.

A Hand-Book of Therapeutics. By Sydney Ringer, M. D., Professor of Therapeutics in University College, etc. New York: William Wood & Co., 1871.

Transactions of the Medical Society of the State of Pennsylvania, at its Twenty-Second Annual Session. Philadelphia: Collins & Co., 1871. Pp. 501.

The Virginia Clinical Record. A Monthly Journal of Medicine, Surgery, and the Collateral Sciences. Richmond, Va.: M. W. Hazlewood.

Transactions of the American Otological Society. Fourth Annual Meeting. Boston: Alfred Mudge & Son, 1871. Pp. 75.

The Dublin Quarterly Journal of Medical Science, No. CIV. November, 1871. Dublin: Fannin & Co.

Annual Announcement and Circular of the Long Island College Hospital, Brooklyn, N. Y. Session of 1872.

Transactions of the Second Annual Session of the Medical Society of Virginia, 1871. Pp. 116.

The Transactions of the American Medical Association, vol. xxii. Philadelphia, 1871.

Reports on the Progress of Medicine.

SURGERY.

1.—*A Curious Displacement of the Radius.* [Medical Press and Circular, September 13, 1871.]

Dr. Lyell has published, in the *Glasgow Quarterly Medical*, a case of partial displacement of the radius in a child. On examination the child's hand was fully prone, and the forearm semi-bent; but on examining the elbow-joint Dr. Lyell could not detect any sensible displacement of its component bones. He took hold of the child's right hand in his own right, and the elbow-joint with the left, placing his thumb over the head of the radius; then supinated the hand fully, at the same time extending the forearm. In the act of supination he could feel the head of the radius give a slight jerk. On liberating her hand the child began to use her arm and returned to her play.

Dr. Lyell says: "This is one example out of many of this species of injury that have come under my observation, so that it cannot be by any means a very rare occurrence; yet, of all the surgeons with whom I have conversed. Dr. Tannahill, of this city, is the only one who seems to have fallen in with it. Generally the accident has occurred with girls, uniformly under five years of age, and repeatedly in the same subject, for having once taken place there is a proclivity to return. In three different children I have seen it occur four or five different times. A fall on the side, or sudden pull or twist of the arm, is the usual cause of its production.

"Like Hey's accident to the knee-joint, there seems some difficulty in defining the true nature of the displacement. Dislocation of the head of the radius was pointed out by Baron Bazer to be a commoner accident than had been previously admitted, particularly among children, but then he adds, the dislocation is always complete. Now, in this sort of injury, although the position of the arm, pronation and semiflexion are the same, there is certainly no complete displacement backward of the head of the bone, nor to the touch is there any sensible displacement. Yet that such is the case is evident from the slight snap or grating felt after the manipulation recommended, and the immediate restoration of the mobility and use of the limb. It seems to me that the radius somehow from over-supination catches on the adjacent ulna and is there retained. From the laxity of the ligaments in some children, this is permitted without their rupture.

"The derangement itself is not of a serious nature, and easily rectified when recognized, although I am not aware that it has been previously noticed in any surgical works. I have known one fall produce it, and another fall put it to rights again in a child liable to the accident, for in liability to recurrence, as I have stated, it is like Hey's knee, though frequently it occurs but once.

"Faith in bone-setters seems a settled portion of the popular creed in many parts of the country, and there can be little doubt that the treatment of such cases as this tends greatly to the confirmation of the popular faith, for in true dislocation the bone-setter is usually powerless for good, especially in the elbow-joint. It is better therefore that the surgeon should in all cases anticipate the bone-setter, and to this end I have not deemed it unnecessary to submit the foregoing to the profession."

2.—*Carded Wool in the Treatment of Wounds.* By John S. Walker, M. D. [Practitioner, October, 1871.]

Dr. Walker has tried various materials for dressing wounds, but gives a decided preference to carded wool, which is known as marine lint. He says of its use:

Take now an amputation of arm or leg. After the operation, my plan of treatment is as follows: Sponge out the wound thoroughly with chloride of zinc ʒ ss to ℥ j of water, then let all applications be quite dry for the first two or three days; as soon as there is the least secretion of pus, take a Higginson's syringe, and wash out the flaps with a strong solution of Condy's fluid, say ℥ j to ℥ x of water; apply strapping, and cover the wound with a small piece of lint wetted with a lotion of ʒ ij carbolic acid ʒ ij liq. potassæ to six ounces of water; cover with a piece of gutta-percha tissue, and continue until healed, surrounding the edges or flaps with a little pad of carded lint.

Take another every-day case—an ulcer of the leg. If it is a large flabby ulcer, secreting a large quantity of pus, nothing is so good for compressing the granulations and checking the secretion of pus as the marine wool; but after it has fulfilled its object, a change of treatment, by the application of either a little dry precipitated chalk, zinc-ointment, or zinc-lotion, will heal it in a few days, with rest and bandaging.

The marine lint is a very excellent application for a sinus after the removal of necrosed bone, and facilitates their closing with very little secretion of pus. Its tarry smell seems, in my opinion, to disinfect all ulcerating surfaces, but the application must not be continued too long, as the wool seems to contract the granulations, if such a term may be used.

I do not altogether agree with Mr. Lister as to the germ-theory, but he has done good by teaching us to wash all wounds under our care with some disinfectant at each dressing; and every practitioner who will adopt this plan of treatment will be pleased at their satisfactory progress: and yet, not a mere washing—it must be a stream of fluid. Any stump, after amputation either of the leg, arm, or finger, granulates very fast under this mode of treatment.

In my opinion, all Mr. Lister's solutions of carbolic acid are too strong, and abrade the cuticle surrounding the wound; it may be through my clumsy mode of application, or not following the minute directions laid down. Thus far I can admire his treatment, that it teaches us all that we disturb the approximating surfaces of wounds too often very unnecessarily, and thereby retard Nature's efforts.

Since writing the above, my attention has been directed to a paper in the *Lancet* of the 13th May, on the use of a similar article, "Tenax." This I have not had as yet; but doubtless it produces the same therapeutical effects as the carded wool. Its chief advantages seem to arise from being so porous as to soak up all discharges, and the tar or ingredients of the tar, probably the creosote, so neutralizes and changes the character of the discharge that it becomes quite innocuous: for we see the same effect in an injection of creosote in corroding ulcer of the uterus or in cancer. But a creosote-lotion has not the same effect upon open wounds.

My chief motive in writing this paper is to direct the attention of my professional brethren to the use of some such preparation as the carded wool, as its therapeutical effects are only known to a few, and these highly appreciate its use.

3.—*Treatment of Bubo.* By W. H. McNamara, M. D., Assistant-Surgeon, 10th M. L. I. [Indian Medical Gazette, August 1, 1871.]

The importance of this subject will be readily admitted by any surgeon who has had much experience in military practice.

Patients are seen in regimental hospitals lying on their backs for months with suppurating bubo. It is remarkable to what a state of ill health these men are reduced by confinement and slow suppuration.

When an acute bubo is opened in the usual way, by a free incision, granulations spring up very slowly, principally from the sides of the incision, and they are usually pale, flabby, and unhealthy: granulation from the bottom is slow, though all kinds of stimulating applications are applied. In a situation where the skin is so movable as in the groin, the granulations of opposite sides of the wound never adhere; the consequence is that after prolonged and tedious treatment the wound is covered over by weak and unhealthy epidermis, resembling the false membrane of a sinus, leaving a sulcus which is liable to break out again when any prolonged exertion, such as a march, is performed. To remedy this state of affairs, I have for some time practised the opening of bubo by means of potassa fusa.

The bubo is covered over with several layers of sticking plaster, in which a hole, *half* the size of the intended opening, had been previously made; potassa fusa is now rubbed on the exposed skin, through the hole; sticking-plaster is then put over the hole, and a full dose of opium given. Very little irritation is produced, and the patient expresses his satisfaction that the knife has not been used. In a short time, a black *eschar* is formed, which is removed in a few days by poultices, and a healthy ulcer, easily healed by the usual treatment, is the result.

Chronic bubo, when opened by the knife, is even more troublesome than the acute disease, on account of the well-known, thin blue skin which surrounds the opening and prevents healthy action. It is usually recommended to get rid of this by means of potassa fusa, or some other caustic, but, if potassa fusa is used, as described above, in opening the bubo, this unhealthy skin does not appear.

In some cases of chronic, painless, indolent bubo, small suppurating centres occur without interfering much with the vitality of the skin which covers them. In these cases an apparatus, which lets out the matter without letting in the air, has been employed with very good effect.

As instruments are not always at hand, it occurred to me that by running a lancet under the skin, commencing a few lines from the seat of suppuration into the abscess, squeezing out the matter, and applying a pad with a bandage, a similar result would ensue. I have practised this method in several instances, with the effect of curing the patient in a few days.

4.—*Symmetrical Erysipelas.* [Medical Times and Gazette, October 21, 1871.]

The following is an abstract of some clinical observations recently delivered at the Hôtel-Dieu, by M. Noel Guéneau de Mussy, on symmetrical cutaneous affections, in relation to a case of symmetrical erysipelas:

The symmetry which prevails as a law in normal organic evolution (he observed) is not infrequently met with in the pathological condition, as manifested by the repetition of the morbid process in homologous parts of the two sides of the body. Thus, caries of a tooth is often followed by caries of the corresponding tooth, affections of the eye are often double, and in gout and rheumatism, when one joint is affected, we not infrequently find the congenerous articulation suffers likewise, or is soon invaded in its turn. In many cutaneous affections the same symmetry is observed. It seems more easy of explanation in organs to which are distributed the cerebro-spinal nerves, having their symmetrical distribution on each side of the body, and being under the dependence of the strongly-centralized innervation having its source in the encephalon. It is, perhaps, here more apparent, but it is also exhibited within the domain of the great sympathetic. Here, too, is found the pathological consensus—sympathies, in the proper sense of the word—between the two halves of simple organs, or between double organs, such as the kidneys or the ovaries. Graves has pointed out one of the most striking examples of pathological symmetry in median erysipelas; and he has laid down the rule that, when erysipelas commences at the median line, it becomes developed symmetrically on the two sides. A case which has come under my notice at the Hôtel-Dieu furnishes so striking and curious a confirmation of the law enounced by Graves, that I feel I ought to relate it:

In June, 1871, a man entered my service affected with erysipelas of the face, this having commenced on the bridge of the nose, and extended symmetrically on both sides. On the third day it occupied the forehead; but external to and below the frontal protuberances there existed two triangular spaces, having sides of two centimetres and a half, within which the skin, pale and depressed, remained perfectly healthy, its normal color contrasting with the carmine-red of the surrounding parts. The borders which limited the portion of skin unattacked by the erysipelas formed prominent elevations, indicating (as Chomel has observed) that the morbid process was not arrested, and that this portion of the integuments would also be afterward invaded. On the left side, the external boundary of this triangle exactly corresponded to a linear cicatrix remaining after an old wound of the forehead which had divided the skin throughout its thickness. The right half of the forehead had undergone no lesion, and yet the portion of the integuments unaffected by the erysipelas presented exactly the same form, seat, and dimensions, as that which on the left side was contiguous to the cicatrix The two triangles were of a perfect geometrical regularity, their positions and directions being absolutely alike. On the left side it might be supposed that the interruption of the vessels by the cicatrix may have retarded the erysipelatous fluxion; but on the right side this exemption could only be explained by the law of symmetry. The anomaly, however, was only temporary; for at the end of twenty-four hours the erysipelas had passed the obstacle, and the whole forehead was of a uniform redness, the white line of the cicatrix on the left side alone indicating the place which had been occupied by the triangle the day before.

5.—*Treatment of Traumatic Tetanus by Heat.* [Medical Times and Gazette, October 7, 1871.]

M. Demarquay recently addressed a short communication to the Académie des Sciences, giving an account of a new mode which he has adopted of treating traumatic tetanus. Having, he says, during the late siege lost many cases without being able to alleviate them, he resolved in future to try a new procedure. First bearing in mind the great susceptibility to cold manifested by these patients, and the aggravation of the suffering which this produced, he kept the two cases he now reports upon in a room heated to, and carefully kept at, a temperature of from 18° to 22° C. (64° to 72° F.). Next, in order to diminish the tonic and clonic contractions, which are in this disease so painful, causing the patient to assume such strange positions, and especially to subdue the trismus, which is one of the earliest manifestations of tetanus, as well as to relieve the pain of the wound and the convulsions of the stump, he performed, four or five times in the twenty-four hours, intra-muscular injections, as near as possible to the emergence of the nerves. These consisted of solutions of morphia diluted to a fiftieth part. At first each masseter was injected, as also the muscles of the neck on each side of the spinal column; and, when the wound which had been the occasional cause of the tetanus was painful, an injection was thrown deeply into the substance of the muscles in its vicinity. Under the influence of these remedies the sufferings were speedily assuaged, and the patient was enabled to open the mouth, and by copious drinks relieve the tormenting thirst. By aid of these, too, and the raised temperature of the room, abundant transpiration was produced. After some hours the injections were repeated, the painful contractions being pursued wherever they appeared, throwing them into the substance of the muscles concerned. They were also made over the track of the nerves of the diaphragm, to subdue the spasm of this muscle, or along the course of the pneumogastric, with the view of relieving the difficulty of deglutition, which appears to depend upon contraction of the œsophagus. In this way the pains were assuaged and the thirst relieved, while the patient was able to be fed with broths, milk, and an increasingly substantial diet. One of the two cases was suffering, when seized with tetanus, from a deep wound of the calf in process of cure, while the other had undergone amputation of the leg. In both the tetanus to all appearance was very severe; and although, of course, two cases do not say much in favor of any mode of treatment, their successful issue justifies its being made known. Frequently subcutaneous injections of morphia, atropia, and curare, have been tried, but, as far as M. Demarquay is aware, no one has hitherto thought of carrying the curative agent deeply into the substance of the muscles. This is, he believes, both a novel and rational procedure.

6.—*Pure and Odorless Carbolic Acid.* [Medical Times and Gazette, October 21, 1871.]

Mr. Church, the well-known chemist, calls attention to a method of preparing pure carbolic acid by a method laid by him before the Odontological Society eleven years ago. The best preparations of the acid of commerce are so seldom free from a disagreeable gas-like odor, entirely foreign to carbolic acid itself, as to render the use of the acid in dentistry, and as an application to the throat, objectionable. Mr. Church's method of depriving the commercial article of all naphthalic odor is so simple, and can be so readily employed by the country practitioner, that we gladly give it publicity: one pound of the best carbolic acid of commerce (the white crystallized acid) is poured into two gallons of cold distilled water, taking care not to permit the *whole* of the acid to enter into solution. With a good sample, if, after shaking repeatedly at intervals, between two and three ounces of the acid remain at the bottom of the vessel used, this will be a sufficient residue to hold and contain all the impurities; with bad samples, less water must be used, or more acid. The watery solution is to be siphoned off and filtered, if necessary, through fine filter-paper till perfectly clear. It is then placed in a tall cylinder, and pure powdered common salt added, with constant agitation, till it no longer dissolves. On standing, the greater part of the carbolic acid will be found floating as a yellow oily layer on the top of the saline liquor, and merely requires to be removed to be ready for use. As it contains five per cent. or more of water, it does not generally crystallize, but it may be made to do so by distilling it from a little lime. The portion collected up to about 365° Fahr. has at ordinary temperatures scarcely any odor, save a faint one resembling that of geranium-leaves; and advantage may be taken of this curious resemblance still further, to mask the slight odor proper to absolutely pure carbolic acid, by the addition to it of four drops per fluidounce of the French oil of geranium. This addition has the advantage of liquefying the pure crystallized product. The pure acid may be dissolved in two hundred and thirty parts of water, and used as a gargle; or in twenty-five parts of water for painting the throat, or in fifty parts for the carbolic spray. We are sure that the profession will thank Mr. Church for the introduction of such a simple method of depriving this invaluable agent of its disagreeable odor.

OBSTETRICS AND DISEASES OF WOMEN.

1.—*Pelvic Hæmatocele.*

At a meeting of the Obstetrical Society of London, held June 7th (*Lancet*), Dr. Meadows read a paper on the diagnosis and treatment of pelvic hæmatocele.

The author expressed his conviction that this affection, though not common, is not so rare as is generally supposed; nor in many cases is the diagnosis a matter of much difficulty, though in some, and especially in the less severe forms, or when seen long after the attack, the diagnosis is extremely puzzling. Allusion was made to a paper by Dr. Barnes on "Intra-peritoneal Hæmorrhage," published in the last number of St. Thomas's Hospital Reports, the author criticising some of the cases in regard to their diagnosis, and expressing some doubt as to their accuracy in this respect, judged solely by the details given. An examination of these cases in their results, when compared with others of the same kind collected from the works of MM. Bernutz and Goupil, showed a very startling contrast; for, of forty cases of the former, three only died, whereas, in sixty-two cases of the latter, only eighteen recovered. In regard to diagnosis, the author remarked that the difficulties were greatly lessened in cases where the attendant happened to know the exact condition of the parts before the occurrence of the attack; because, as swelling necessarily results from the hæmorrhage, its absence before the attack, and its immediate discovery afterward, together with the attendant symptoms, pointed at once to the nature of the case, inasmuch as no other swelling occurs thus suddenly. The differential diagnosis of uterine displacements; of tumors, either of the uterus or ovaries, becoming suddenly impacted in one or other *cul-de-sac;* of pelvic cellulitis; or of pelvi-peritonitis, was dwelt upon at some length, and their distinctive features pointed out. The author recommended the division of the cases into two groups, the first to include all those which originated in the performance of the functions of menstruation or parturition, and the second those of distinctly organic origin, not connected directly with the uterine functions. He advocated more frequent resort to puncture, grounding his recommendation on the fact that, of Bernutz's eighteen cases which recovered, nine were operated upon, seven ruptured spontaneously, and only two were left alone; while, out of twelve fatal cases, two only were tapped, one ruptured spontaneously, and nine were left alone. The author related two cases in which he had tapped successfully, and the paper ended with some directions as to the conditions, mode, and time of operating.

Dr. Barnes said that Dr. Meadows doubted his cases because they were so many; but was his experience so exceptional? Olshausen said that in 1867 Scanzoni had only seen two cases, but that he ought to have seen two hundred. Olshausen himself had seen thirty-four cases of hæmatocele in eleven hundred and forty-five gynæcological cases. Seyfert had seen sixty-six cases out of twelve hundred and seventy-two. In fact, it was only necessary to look for these cases with intelligence, in order to find them. He had expressly stated in his paper that some of the cases were, no doubt, open to criticism as to diagnosis. In some this had been drawn from the history and general symptoms, and was not established by local exploration.

Dr. Snow Beck said his experience led to the conclusion that retro-uterine hæmatocele was a comparatively rare affection. He did not think that rupture of the gravid uterus, or extra-uterine gestation cysts, or ovarian cysts with large effusion of blood into the peritonæum, ought to be included under this term, which was properly restricted to effusions of blood encysted in the pelvic peritonæum, or extravasated into the loose cellular tissue in the pelvis.

Dr. Graily Hewitt stated that during the last five years he had observed in University College Hospital altogether, he believed, twelve or fifteen cases. They had all recovered, and in no case had puncture been resorted to.

Dr. Tilt remarked that, if the German pathologists had found hæmatocele common, the French writers considered it to be very rare. Since he first drew the attention of British pathologists to hæmatocele in 1853, Dr. Tilt had only met with twelve cases. Pelvi-peritonitis was often mistaken for hæmatocele. The majority of cases required no surgical treatment, but when the collection of blood was considerable, and the tension of the sac very great, he had repeatedly punctured it through the vagina, and allowed the blood to drain away of its own accord, thereby greatly relieving the patient's suffering, and shortening the duration of the disease.

Dr. Greenhalgh agreed with Dr. Meadows that pelvic hæmatocele was by no means so frequent nor so harmless an affection as Dr. Barnes would lead the profession to believe. On a rough estimate, he (Dr. Greenhalgh) did not think that he had seen more than twenty-five indubitable cases of that affection, notwithstanding he had had extensive opportunities of meeting with such cases.

Mr. Spencer Wells said his personal experience of pelvic hæmatocele was chiefly as a sequel of ovariotomy. He believed the less severe forms, where only small quantities of blood were effused, and afterward absorbed, were very common. When the tied or cauterized pedicle was in the pelvis, a good deal of trouble was sometimes observed at each menstrual period for some months, with all the signs of hæmatocele. In the slighter cases he considered rest and opiates constituted the best treatment. But there were other cases where a high temperature, rapid pulse, loss of flesh, dry tongue and skin, with a painful, distended abdomen, and scanty concentrated urine, showed that the patient was being poisoned by absorption, and here not only puncture, but drainage, was necessary to save life. Puncture alone might only give temporary relief, or do harm by hastening decomposition of blood or pus; but, when a canula or drainage-tube maintained a free escape for fluid and gas, cases apparently hopeless did well.

2.—*Arsenic in Menorrhagia and Leucorrhœa.* [The Doctor.]

Before the British Medical Association Dr. J. H Aveling read a paper on this subject. He believes that this remedy has not received from the profession the attention it deserves. Dr. Henry Hunt used it successfully in uterine disorders, and published his experience in 1838. Dr. Aveling has employed it in cases of menorrhagia for twelve years with great advantage. Besides improving nutrition, respiration, and secretion, he finds it to have a powerful decongestive action upon all mucous membranes. He administers small doses of arsenic either in solution or in granules, and continues them for weeks or months, as the necessities of the case may require. He believes the forms of menorrhagia and leucorrhœa most amenable to the arsenic-treatment are those which have their origin in a hyperæmic condition of the uterus, which state the remedy cures by acting as a tonic and stimulant upon the vaso-motor nerves, causing the capillaries to contract and expel the superabundant blood.

Miscellaneous and Scientific Notes.

Conception under Unusual Circumstances. (*Olshausen*, *Arch. für Gynækol.*, II., 278.)—Dr. Olshausen narrates two cases of conception, in which the women wore intra-uterine pessaries at the time. In one case the menses ceased suddenly, while in the other they continued regularly for six months during which the pessary was worn. The possible dates of cohabitation were precisely given. About twenty days after this, the author removed the instrument. After barely two hundred and fifty-three days, occurred the birth of a very vigorous boy, judging from whose development the entire period of gestation (in this case two hundred and eighty days) must be allowed. The woman had also, during the first months of pregnancy, while the pessary still remained, nausea and vomiting, and therefore regarded herself as pregnant. In the other case, two hundred and forty-nine days after removal of the pessary, followed the birth of a very vigorous girl, which was much too well developed for such a short period of gestation. Dr. Olshausen believes, nevertheless, that continuity of the sexual canal, as a rule, is an absolute condition for conception, but asks, How could conception occur under such exceptional conditions? The hypothesis of Dr. Kristeller may be quoted in reply. This observer has for a long time turned his attention to the slimy mucus produced by the glands of the uterine mucous membrane, especially those of the cervix, and describes minutely its production, conditions of existence, and nature. In the normal sexual conditions, it hangs from one to six centimetres from the orifice of the uterus, clear, tough, and sticky. The author therefore rejects the continued contact of the os uteri and penis during sexual intercourse from physico-mechanical grounds, and asserts that the ejaculated sperm collects somewhat in a mass, in a species of recess of the vagina, behind the perineal groove. In this vessel of sperm, the already-produced or freshly-generated stringy mucus bathes, and carries the spermatozoa into the uterus. In fact, Dr. Kristeller, several hours after coitus, found the stringy mucus permeated far up with living spermatozoa. He therefore concludes that conception is effected by means of the string of mucus and presents as further proof a series of clinical observations.

The Variola-Varicella Question.—Eisenschitz, after regarding the question of identity as still undecided, collects the following points as confirmed: 1. That vaccination has no influence upon the course of varicella. 2. That children, soon or even immediately after having suffered from varicella, have been attacked with variola, often with fatal results (Mayer, Eisenschitz, Thomas, Heischmann, Förster). 3. That in the same individual varicella consequent upon variola appears extremely rare. 4. That there is an acute vesicular exanthem (in which also single bullæ may be interspersed) which simultaneously or in quick succession attacks several children of one family, and which by all authors is called varicella (among these should be included cases of so-called "herpes phlyctenosus"), which also at certain times appears simultaneously over a large extent of surface.

Extirpation of the Fore-part of the Tongue. (Prof. Schmidt, *Arch. f. Heilk.*, 4., 1871).—The author advises, in extirpation of the anterior portion of the tongue, to occlude the arteriæ raninæ *en masse* by puncture. On the under surface of the tongue, close above the carunculæ sublinguales, about 1½ cm. to the left of the median line, a large curved needle, with a quadruple thread, is inserted through the middle of the tongue, and toward its left border; and, about 1½ cm. from the frenulum linguæ, again pushed through. The entire soft parts included in the ligatures are then drawn tight, and the ligature fastened by a slip-knot. Then the tongue is drawn horizontally forward. The clean amputation of the tongue between the posterior limits of the disease and the affixed loops of ligature is then effected without loss of blood. The author operated in this manner without any significant hæmorrhage. This method is advisable only where no assistant can be had, for generally the hæmorrhage from the anterior half of the tongue can be easily controlled.

The Treatment of Diphtheritis with Carbolic Acid. (*Dent. Klinik.*, 26, 1871.)—Dr. Helfer employs carbolic acid in the proportions of 1 to 200 as gargle and injection, in the proportion of 1 to 50 for inhalation, and in concentrated solution with the camel's-hair pencil. He had treated altogether sixteen cases of diphtheria with carbolic acid; the majority of these cases were diphtheritis scarlatinosa, a few most probably angina diphtheritica sine exanthemata, and, as far as can be gathered from the reports of the cases, at the most, two or three were cases of idiopathic diphtheritis. The results which Dr. Helfer reports are of a nature to encourage experiments with the carbolic acid, and he refers also to Prof. Henwig and Dr. Weikert, who had obtained excellent results from the same treatment. In two cases also of pure croup, and in one of diphtheritic croup, the employment of carbolic acid resulted in a cure.—(See *Med.-Chir. Records*, No. 161, 1871.)

On the Highest Grade of Anæmia of Pregnant Women. (*Arch. f. Gynæcol.*, vol. ii., part ii., 1871.)—Prof. Gusserow observed, in his Klinic from 1868 to 1870, five cases of the most intense anæmia of pregnant women, all of which ended fatally. The history of all these cases, which is given in minutiæ, as well as the *post-mortem* evidence, entirely agrees; in all there developed during the latter half of gestation, without any perceptible cause, an intense anæmia; in every case the birth was premature (about the eighth month); and although it occurred without loss of blood or other accident, death resulted in a short time—in one case, indeed, before the expulsion of the placenta, regardless of all medication, even transfusion. The *post mortem* showed nothing but a high degree of anæmia and hydræmia.

On the Intestinal Movements. (*Med. Jahrb. Gesell. Aertz. z. Wien.* 1871.)—Drs. Mayer and Basch conclude, after a long series of experiments, that the cause by which the intestinal movements are produced is primarily the presence (especially the afflux) of the venous blood in the intestinal walls. The previously-recognized arrestive action exercised by the nervi-splanchnici upon the intestinal movements may be explained on the same principle by the vaso-motor fibres which they contain.

Library and Journal Association.—The following-named gentlemen were elected officers of this Association, at the meeting held for that purpose on 5th ult.: President, Alfred Underhill; First Vice-President, E. R. Peaslee; Second Vice-President, W. N. Blakeman; Recording Secretary, F. A. Burrall; Corresponding Secretary, J. C. Peters; Librarian, A. E. M. Purdy; Trustees for three years, S. T. Hubbard, W. R. Whitehead, L. Weber, E. S. F. Arnold, and G. M. Smith. Trustees to fill vacancies (one year), C. C. Lee, J. P. Garrish, and B. J. Raphael.

Alumni Reunion.—The Alumni Association of the Jefferson Medical College proposes to hold a social reunion during the meeting of the American Medical Association in Philadelphia, in May next. The alumni of the college are cordially invited to attend. Those who expect to be present are requested to send their names and addresses to either of the secretaries, viz.: J. Ewing Mears, M. D., 222 South Sixteenth Street, R. J. Dunglison, M. D., 636 North Eighteenth Street.

On the Formation of Caseine in the Lacteal Glands. (*Pflüger's Arch. f. Phys.*, 1870, XII.)—Dr. Daenhardt succeeded in extracting, from the lacteal glands of suckling Guinea-pigs, a body which, by the addition of a mixture of the white of egg and carbonate of soda, was capable of forming caseine. He deduces from this that, as, by a ferment of the saliva, sugar is produced from starch, so by this ferment of the lacteal glands caseine is produced from the albumen.

The St. Louis Medical and Surgical Journal.—This excellent journal begins the new year as a monthly, having hitherto been issued *bi*-monthly. The old editors are succeeded by Drs. Wm. S. Edgar and H. Z. Gill, who will doubtless sustain the high character enjoyed by the *Journal* under the management of their predecessors. The price of subscription remains unchanged.

Vesico-Vaginal Fistula.—In a recent number of the *Annales et Bulletin de la Société de Médecine de Gand*, Prof. Boddaert, of the University of Ghent, reports two cases of vesico-vaginal fistula, operated on by himself according to the "American" method of Dr. Marion Sims. In both cases a perfect cure was the result. The operations were performed without the use of anæsthetics. One or two unimportant modifications were introduced, but in all the essentials the well-known methods of Dr. Sims were strictly adhered to from beginning to end of the treatment. In view of the results of his experience as compared with those attained formerly in the treatment of this distressing disease, Prof. Boddaert characterizes the operation as the most effectual method yet presented, and, with an enthusiasm very appreciative of Dr. Sims, he exclaims, "Glorious result, a thousand times grander and nobler than the victories of armies, whose triumphs are attained only at the cost of the death of their fellow-men!"—*Boston Med. and Surg. Jour.*

Medical Students abroad.—The following is given by the *British Medical Journal* as the number of new *entrées* and total number of students registered at the London hospitals:

	New Entrées.	Total Students.
1. University College................	88	271
2. Guy's Hospital....................	86	328
3. St. Bartholomew's Hospital........	81	255
4. St. Thomas's "	58	145
5. King's College "	43	121
6. London Hospital...................	29	99
7. St. George's Hospital..............	22	85
8. St. Mary's "	21	61
9. Middlesex "	19	40
10. Charing-Cross "	11	40
11. Westminster "	10	23
Total........................	468	1,468

The Health of Baron Liebig.—From a private letter received in this city, we learn that Prof. Liebig is by no means restored to his former physical and mental activity. He spent the early part of the summer at the baths of Kissingen, and was much benefited by the treatment; later in the season, he went to meet a few choice friends, among them his life-long colleague, Prof. Woehler, at Reichenhall, where one of his sons is a physician; and here, in the invigorating mountain air, his bodily infirmities disappeared; but he complains of dizziness and suffering whenever he attempts the least mental exertion. We fear that the illustrious chemist will hardly be able to enrich our literature with many more of the brilliant writings which have rendered the science, to which he has devoted the best years of his life, so useful and so popular.—*Med. and Surgical Reporter.*

Intra-Uterine Variola.—In the *Société des Sciences Médicale* of Lyons, M. Poucet showed recently a fœtus aborted by a woman attacked by varioloid. This fœtus presented a disseminated pustular eruption over different parts of the body. Dr. Mollière mentioned that two women had variola during the first three months of their pregnancy. These children were born at full term. He vaccinated both of them on the eighth day, with some other children, but in neither of them did the vaccine take. In one of these cases the variola was confluent, in the other very slight.—*The Doctor.*

The University of Strasbourg.—According to a writer in the *Revue de Thérapeutique*, there have been under the new

(Prussian) Faculty at the University of Strasbourg eleven examinations for degrees in the scholastic year 1870–'71, against 1,014 in the scholastic year 1869–'70. The German Government have been extremely liberal to the Faculty of Medicine. Permission has been granted to the professors to constitute themselves into a self-controlling body. No salaries will be granted by the Government, but the pupils' fees will go to the maintenance of the teachers and the institution. The winter lectures have just been advertised, the old names, with a few exceptions, appearing as formerly.—*Lancet.*

The New German Pharmacopœia.—A committee of twelve, consisting of four Professors of Materia Medica and of several medical councillors and apothecaries of various of the States, are to meet under the presidency of Geh.-Med. Rath Houssele, of Berlin, in order to prepare a conjoint pharmacopœia. The committee has also the assistance of experts, as Privat-Docens Oscar Liebreich, Prof. Schwanert, etc. It has been determined, notwithstanding the opposition of the professors, to continue the adoption of the Latin language, although in South Germany the German has been long employed for this purpose; in fact, it would seem that the pharmacopœia will be little else than a new edition of the "Pharmacopœia Borussica," with the addition of some of the new remedies. If, as is said, it is to be printed by January 1, 1872, it is to be feared that its compilation will prove but a hasty and imperfect performance.—*Lancet.*

Function of the Anterior Part of the Brain.—Prof. Schiff, of Florence (*Lancet*, October 21, 1871), is said to have confirmed the announcement that the anterior portion of the brain has to do with motion; but he has found that the muscular movements do not arise from an actual motor property of the brain, but are reflex movements excited by irritation of certain parts of the cerebrum which regulate tactile sensibility (not sensitiveness to pain). The removal of such parts does not *directly* abolish the movements, but disturbs them indirectly through the destruction of tactile sensibility.

Centenarians.—No less than twelve of the deaths from all causes registered in London last week were of persons aged 90 years and upward, including two centenarians—a carpenter, who died in the Shoreditch Workhouse, aged 102; and a widow, who died in Clapham, aged 101 years 11 months and 18 days.—*Medical Times and Gazette*, December 2d.

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Original Communications.

ART. I.—*Spontaneous Generation. The Substance of Two Lectures delivered in the College of Physicians and Surgeons, New York, January 12th and 13th.* By J. C. DALTON, M. D., Professor of Physiology.

It is the object of the present lectures to give the history of our knowledge on the subject of *Spontaneous Generation;* a topic which has been at various periods, and still is, one of the most mooted questions in general physiology. I do not propose to do this in an argumentative way, or with the object of proving conclusively its truth or falsity as a scientific doctrine; but rather to offer an account of the progress of opinion and discovery in this respect from the earliest periods, and the present condition of our knowledge on the subject, so far as definite conclusions have been reached.

Such a review of scientific questions is sometimes useful in many ways. It is often difficult to appreciate the real character or value of our present knowledge without recalling the successive steps by which it has been attained; and these steps have been in many instances so slow, and separated from each other by such long intervals of time, that the connection between them is easily forgotten. Nevertheless, the combined result has often been derived from many different discoveries, each of them quite as important in its time as those which are attracting attention now.

Beside, in the case of spontaneous generation, the nature of the question at issue has varied at different times, according to the general condition of knowledge in natural history, and the greater or less facility of observation derived from the invention and use of optical instruments.

The earliest scientific period to which we can refer is that of the natural history of the Greeks. At that time it seems to have been taken for granted that a large number of animals of various kinds were produced by spontaneous generation. This was not then considered, as it is now, in the light of an exceptional and almost experimental phenomenon; but as the regular and natural method by which a large class of organized beings made their appearance on the scene of animated existence. According to Aristotle, animals are divided, in respect to their mode of production, into two great classes. Some are produced by generation from parents, that is, from other animals of the same kind; so that there is, in these cases, a successive transmission of life from one to another, forming an unbroken series of parents and offspring. Life is transmitted, in animals of this class, by the production, either of living young, as in the *vivipara;* or by that of eggs, as in the *ovipara;* or by that of grubs and larvæ, as in many kinds of insects.

On the other hand, according to this view, some animals, as well as plants, are produced without having come from parents. They originate spontaneously, by the natural reunion and combination of ingredients analogous to their own; as, for example, in decomposing mud or slime, from the substance of vegetable tissues, or in the interior of other animals, from the excrementitious or superfluous material of their organs.

A large number of these spontaneous living products were thought to come directly from the earth itself. All the shellfish, without exception, such as clams, oysters, rock-shells, cockles, limpets, etc., were said to be produced spontaneously in the slimy mud deposited from water, and to vary in kind according to the different characters of this deposit. Sea-nettles, sponges, and all similar productions, which have no shells, were said to be formed in the clefts of rocks in the same manner as shell-fish. Gnats and other small insects were known to be produced, by metamorphosis, from slender aquatic worms; but these worms were understood to be formed spontaneously in the mud at the bottom of wells, or wherever there might be a collection of standing water with an earthy deposit at the bottom. The moth-grubs, so destructive to woollen textures, Aristotle taught, were produced spontaneously in the wool itself; wood-ticks came from the wood which they inhabited; and the little transparent book-louse, the insect found between the leaves of old books, originated in a similar way from the substance of the paper or parchment.

Even some of the fishes were regarded as having been produced, without parentage, from mud, from sandy loam, or from the floating scum on the surface of the water. Eels, certain mullets, a kind of atherina (aphya spuma), and some other small fish, are all enumerated[1] as originating in this way, by the direct combination and organization of inanimate materials contained in the mud of pools, or held in watery suspension. In the case of the mullets, one particular species only is described as being produced spontaneously; the others belonging to this family being known to reproduce their kind in the ordinary way. The fishes, as a class, therefore, were considered as presenting two different modes of reproduction; some of them being naturally produced from eggs, and others, as naturally, generated from mud or decomposing slime.

With our present knowledge on the subject of reproduction, we might be disposed to regard these statements as the offspring of either extraordinary ignorance or careless observation; and yet it would be a great mistake to do so. They were the best conclusions attainable at that time. Aristotle represented, in natural science, as in so many other departments, the entire scope and successful activity of the Grecian intellect. He occupied the position which was afterward held by the Buffons, the Linnæus, and the Cuviers of more modern periods; and it is certain that the opinions which he expressed must at least have seemed reasonable from his point of view.

[1] Aristotle, History of Animals, Book VI., 15, 16.

It should be remembered, in the first place, that in all the cases now mentioned, whether among fish or other animals, the fact of spontaneous generation was never regarded as in any degree an accidental or abnormal occurrence; but always as the usual and regular mode of reproduction for those species. The belief in its existence was not due to any vulgar taste for the wonderful, or any tendency to run after extraordinary and supernatural phenomena; it was simply the result derived from an imperfect but fully scientific study of natural appearances.

Naturalists at that time investigated the reproduction of living beings as far as their means of observation would allow. They saw that some animals, such as sheep, horses, cattle, rabbits, and the like, produced living young similar to themselves. It was also plain that others, such as birds, reptiles, and many kinds of fish, produced their young by means of eggs. But *how* this generation took place in either case was entirely unknown. Naturalists were ignorant of the manner in which either the living young or the eggs were produced in the interior of the animal, and they knew nothing of the real relation between the two. For them, reproduction by eggs and reproduction by living young were two methods entirely distinct, having no resemblance to each other, and equally mysterious and incomprehensible in their nature. But they saw plainly that these two methods existed; and observation taught them that the young produced in both were directly traceable to parents of the same species.

On the other hand, there were certain kinds of animals, and these very numerous, in which no such connection between offspring and parents was perceptible. To all appearance, they were neither viviparous nor oviparous. They neither produced eggs or young themselves, nor were any parent animals of similar kind to be found in the same localities. There were worm-like larvæ which suddenly swarmed on the bottom of muddy pools; and fish, unlike any others in form and size, which were abundant at particular seasons, but contained neither ovaries nor eggs. There were maggots which appeared invariably at a certain period in putrefying meat, and others found enclosed in the centre of vegetable excrescences; beside various kinds of parasites inhabiting the cavities and even the solid organs of other animals. These creatures of doubtful origin showed themselves, for the most part, in moist and slimy deposits, containing a mixture of organic substances, and exposed to the action of the sun and air; or in some secret recess where organic material was already supplied, and where no access from without appeared to be possible. They were, therefore, supposed to be generated spontaneously from the materials on the spot, simply because no other source of production for them was visible. It was not altogether an unreasonable conclusion from the evidence then attainable; and, although mistaken in fact, it was reached by a direct course of scientific inquiry, exactly like that in use at the present day.

The mistake, as it existed, was due in great part to the inherent difficulty of the subject. For there is no point connected with the habits of animals surrounded by so many obstacles to a complete elucidation, as that of their reproduction. The secrecy of the animals in laying their eggs and in concealing the place where they are deposited; the sudden and unexpected manner in which ovulation and hatching are performed, the animal often coming from quite a different locality to deposit her eggs, and the young, on being hatched, at once dispersing to a still different one; the extreme variation in appearance, in many cases, between the parents and the young; the deposit of eggs in one season which are not to be hatched until the following spring, when the parents are all dead or have disappeared;[1] these and a multitude of other similar peculiarities often demand, in doubtful cases, an unusual exercise of skill and patience to learn the truth. Add to this the variety of metamorphoses to which many of the

[1] A remarkable instance of this is the case of the American seventeen year locust (*Cicada septendecim*), where a period of seventeen years elapses between the hatching of the larva and the appearance of the perfect insect; the larva all this time remaining buried in the ground, while the life of the insect in its perfect state does not last over six weeks. A brood of these locusts appeared in the city of New York and its immediate vicinity in 1843, and again in 1860. If they return with their accustomed regularity, their next appearance will be in 1877.

lower animals are subject before arriving at their adult condition, and the fact that in each successive stage the animal often inhabits a different locality, and sometimes even a different element, and the difficulty of tracing the connection in all cases between parents and offspring will be fully apparent.

This difficulty gradually diminished in process of time, as more knowledge was acquired in regard to the habits and growth of particular kinds of animals. In this way, by laborious research or fortunate observation, certain animals, previously supposed to originate spontaneously, were detected in their secret modes of reproduction, and were shown to be either oviparous or viviparous, like the rest. Fishes, which disappeared from their usual haunts to spawn elsewhere, were traced to their new hiding-places, and were then found to contain and produce eggs from which their young were hatched. There were other species in which the young differed so much in appearance from their parents that they had been regarded as independent and spontaneous productions; and, owing to their having no eggs or reproductive organs of their own, could not themselves, apparently, become parents. These animals were found to change in appearance, with the process of growth, so as to resemble at last the parent species; and, at the same time, to acquire reproductive organs, and become themselves capable of producing eggs. This knowledge, however, was necessarily acquired but slowly, and for one species of animal only at a time.

It was not until 1668, or more than nineteen hundred years after Aristotle, that Francis Redi, in Italy, discovered the true origin of the maggots in putrefying meat. Previous to that time the appearance of these creatures was one of the most undoubted instances of spontaneous generation. The invariable manner in which it occurred under the requisite conditions, the striking appearance of the maggots themselves, their visible size, their activity and voracity, and the great numbers in which they were produced, made the phenomenon appreciable by all, and one which could be easily brought under observation at any time. A piece of fresh meat, taken from a recently-killed animal, perfectly clean, and exposed in an open dish to the summer temperature, became in a few days, as putrefaction commenced, the habitation of an abundant colony of little maggots. They rapidly increased in size and activity, and were soon evidently engaged in devouring the softened material; so that, when putrefaction had advanced to a certain stage, the dead flesh appeared almost to be converted into an animated mass of moving worms.

These worms, furthermore, were peculiar in their appearance and all alike. They always became developed in putrefying meat, and never in any other situation. The more rapid the putrefaction, the more rapidly they were produced. No similar creatures came from without; and the maggots themselves appeared first and only in dead and decomposing substances.

Redi's experiments on this point were very simple, but wholly conclusive. He noticed, while watching the production of maggots in different putrescible substances, that before the time of their appearance certain flies always hovered round the meat, and occasionally alighted upon it. He then suspected the maggots to be the progeny of these flies. In order to determine the point, he took, in the month of July, eight wide mouthed bottles and placed in them various pieces of dead flesh. Four of these bottles were left open to the atmosphere; while the remaining four were closed by pieces of paper carefully fastened over the mouth of each, and secured by a string round its neck. A short time afterward the flesh in the uncovered bottles was filled with maggots, the flies meanwhile passing in and out by the open mouth; but not a single maggot was visible in the closed bottles, even after several months had elapsed. The experimenter varied his method in several ways, but always with a similar result. He buried pieces of flesh, for example, in the ground, keeping them carefully covered for several weeks, and found that maggots were never generated under these circumstances; while they always made their appearance in meat to which the flies had access.

The conclusion, accordingly, was fully established. The maggots were not generated by the putrefying flesh, but by flies which were attracted by its odor, and deposited their eggs upon its surface. These eggs were hatched into maggots, and the maggots were afterward developed into perfect insects similar to their parents—thus completing the round of ordinary oviparous generation.

It seems that the idea of putrefaction, or the decomposition of organic matters, as being especially favorable to the production of new forms of life, had always been a favorite one with the older naturalists. It occurs under many different forms in Aristotle, and is repeated by Pliny between three and four centuries afterward. The molecular actions, analogous to fermentation, going on in a decomposing material which had once been endowed with life, seemed not unlikely to breed new organizations of a different form. This notion evidently survived through the middle ages, since it is repeated by Fabricius in 1600, and by Harvey in 1650. It even forms the basis of the present doctrine of spontaneous generation, as it exists in our own day.

The experiments performed by Redi, therefore, by no means disproved altogether the fact of spontaneous generation, nor even its occurrence as a result of putrefaction. But it cleared up the obscurity surrounding the generation of insects produced from eggs and undergoing metamorphosis; and these species were, one after the other, withdrawn from the division of animals produced spontaneously, and transferred to that of the oviparous classes.

We now come to an epoch, however, which changed the aspect of the question very materially, and finally excluded from the field of spontaneous generation all the classes of animals formerly known to exist. This period is marked by the invention of the microscope and the discovery of the mammalian egg. This discovery, which showed that even viviparous animals really produce their young by means of eggs, took no less than one hundred and fifty years for its perfection. It was commenced by Regnier de Graaf in 1672, and was completed by Ernst von Baer in 1827. It demonstrated that reproduction takes place by means of eggs in quadrupeds and all the higher animals, as well as in birds, reptiles, fish, insects, and the like. The quadrupeds are merely distinguished, in this respect, by two peculiarities: First the egg, as formed internally in these animals, is of very small size, and needs the use of the microscope for its recognition; and, secondly, it is developed into the young animal while still contained within the body of the parent. When finally produced, therefore, it appears as a young animal, and not as an egg. But it was no less an egg originally, and corresponds, in every important particular, with the egg in other classes of animals. Consequently, there is no essential difference, in the mode of reproduction, between oviparous and viviparous animals. They all produce eggs to begin with. But the ovipara lay their eggs while still undeveloped, and the young are hatched afterward; while the vivipara develop their young internally, and produce them completely formed.

Thus the three different methods of reproduction, formerly regarded as on an equal footing and quite distinct from each other, namely, that by living young, by eggs, and by spontaneous generation, no longer existed. Hereafter, the oviparous and viviparous animals were understood to be all essentially oviparous. The mechanism of the function, also, came to be better understood, with improved means of observation, and much of its mysterious and incomprehensible character gradually disappeared. There now remained but one kind of reproduction distinct in character from the rest; namely, that by spontaneous generation. This one continued as incomprehensible in its nature as before. But the number of species supposed to be generated in this way had been in the mean while rapidly diminished, and many of them had taken their places, as time went on, among those known to produce their young by means of eggs. In this way the fact of spontaneous generation lost its rank, as a great natural division of the reproductive function; and came to be regarded rather as an exceptional phenomenon, confined to a very few species, whose existence could not be accounted for in the ordinary way. Its territory was narrowed exactly in proportion as the knowledge of natural history advanced; and it became reduced almost exclusively to the class of animals known as *entozoa*, or internal parasites.

These are organisms which live within the bodies of other living animals.

There are many kinds of these entozoa. Some of them are found in the intestines, others in the liver, the kidneys, the lungs, or the heart and blood-vessels; others on the surface of the brain, and others even in the muscles and in the globe of the eye. There is hardly an animal which is not liable to be inhabited by one or more parasites in different parts of his body. Each particular kind of parasite is peculiar to the species of animal which he inhabits, and even to a particular part of his body, often to a particular part of one organ. The ascaris lumbricoides is found in the small intestine, the oxyuris vermicularis in the rectum, the trichocephalus dispar in the cæcum. There is one kind of distoma living in the lungs of the green frog, another in those of the brown frog. The cysticercus cellulosæ is found in the cellular tissue, the trichina spiralis in the substance of the muscles. As a rule, they are not to be found except in these situations; and if they were, it is not easy to see how they could have effected an entrance, at least into the blood-vessels and the solid organs.

But the parasites, like all other animals, were at last found to come from eggs, and to reproduce their species by sexual generation. They exist only in certain organs of the living body, not because they are generated there from the animal fluids, but because it is only there that they can find the necessary conditions of their development, and the kind of nourishment that is suited for them. This is the secret of their being found in certain places and not in others. In nearly all, if not in all cases, their residence in the body of the larger animal is merely temporary, and continues only during a certain period of their growth. Both before and after that period they live in other situations; but at the time of changing their habitation, they also change their form, so that they could not be recognized as the same animal, unless carefully watched and followed through the different stages of their growth.

The proof of this is now complete, and the manner in which it has been reached forms one of the most remarkable chapters in the history of physiology. For human parasites, the class which presented the greatest difficulty in accounting for their origin may be represented by the two kinds known as

Cysticercus cellulosæ and *Trichina spiralis.* This difficulty consisted in the two following peculiarities: First, that the animals live enclosed in a distinct cyst, in the substance of solid tissues; and, secondly, that they are apparently sexless, and unprovided with generative organs. It was from 1850 to 1858 that the natural history of several kinds of cysticercus was established by the researches of Siebold, Küchenmeister, Van Beneden, and others. These parasites consist of a globular envelope, containing an involuted bag, at the bottom of which is a minute head, with four suckers and a crown of hooks, similar to those of certain varieties of *Tænia.* Experiments performed by feeding different animals with cysticerci, which resulted in the production of tænia, and by feeding others with the eggs of tænia, which in turn gave origin to cysticerci, convinced the experimenters that there was a physiological identity between the two forms, and that cysticercus was simply the imperfectly-developed embryo of tænia. This conclusion is now universally accepted. The mature articulation of a tænia, containing an abundance of fecundated eggs and embryos, is thrown off in the intestine of the animal inhabited by the parasite. After being discharged, it retains its vitality, under favorable conditions, long enough to migrate and attach itself to substances, which are swallowed as food by another animal. In the stomach and intestine of this second animal the tænia-articulation is digested and its embryos set free. They then penetrate the intestinal walls by means of the six calcareous spines with which they are provided, and thus reach other organs and sometimes distant parts of the body. Arrived at the cellular tissue, they become encysted, pass through the first stage of their development, and acquire the four suckers and double crown of hooks characteristic of the species. Here their development stops, and they remain quiescent until the animal which they inhabit is devoured by another, of the kind in which the tape-worm was originally domiciled. In this new situation their development is completed. The articulations of the tænia are produced and multiplied by successive growth, acquire sexual organs, and at last become filled with mature eggs, to repeat, as before, the process of reproduction. Thus the fact that the cysticercus lives in the cellular tissue of one animal, and the tænia in the intestine of another, is like the fact, familiar to all, that the larva of the mosquito lives in the water, and the mosquito itself in the air. Both stages of existence are equally essential to the life of the animal: only, during the first stage the creature is embryonic and sexless; during the second it is fully developed and capable of generation. Similar discoveries, made a few years later, principally by Leuckart,[1] in regard to Trichina spiralis, showed that the encysted condition of this parasite is also a temporary one; that it is the progeny of a fully-developed, viviparous, intestinal worm; and that, when itself introduced into the intestine of another animal, it goes through a similar process of growth, and becomes, in turn, provided with sexual organs. Meanwhile, the same conclusion has been reached with regard to many other parasitic worms, inhabiting insects and other of the lower animals.

In this way it became evident that none of the animals formerly supposed to be spontaneously generated were really produced in that manner. The evidence could not be resisted that, however obscure the origin of a species might be, a complete study of its natural history would always establish the fact that its reproduction took place, exactly as in other instances, by means of fertile eggs; and that its different generations would thus be connected with each other by the unbroken succession of parents and progeny. The adoption of this view was general and almost universal. In 1828 Cuvier had already published an edition of Pliny's Natural History, with commentaries of his own; and, in alluding to the author's statement, that some kinds of shell-fish are produced in sandy deposits "by the spontaneous operation of Nature," he says, in a foot-note:[2]

"It is altogether untrue that animals are ever formed by spontaneous production. We are now perfectly acquainted with the eggs of these shell-fish and their whole mode of generation. The Aphya (the fish thought by the ancients to originate from the foam of the sea) is simply the young fry of larger fish; and so of all the rest."

[1] Untersuchungen über Trichina Spiralis. Leipzig, 1860.

[2] Pliny, Historia Naturalis. Paris, 1828. IX., lxxiv.

With this conclusion we may regard the second period in the history of spontaneous generation as brought to a close. Cuvier evidently regarded the question as settled; and, in fact, it was so and still is, for all classes of animals which were known to the earlier naturalists.

But every extension of our knowledge in the natural sciences, not only adds to the information already possessed, but often brings into view, in addition, a new outlying territory in which every thing is at first doubtful and undefined. This is what happened in the present instance. For while the microscope was enabling experimenters to settle the question of spontaneous generation for all known species of animals, it discovered at the same time an entirely new and very extensive class of living beings, which had never before been known to exist. As early as 1675, Leeuwenhoek discovered in the rain-water which ran from the roof of his house, or was caught in open vases, a number of little organisms, too small to be distinguished by the unaided eye. On account of their minute size and animal organization he called them "animalcules." But they were soon found to make their appearance with astonishing rapidity in all watery infusions of decaying organic matter exposed to the air; and from this circumstance they received, some time afterward, the name of *Infusoria*.

As the construction and powers of the microscope improved with time, and a larger number of observers employed it in the investigations of natural history, these little bodies became the objects of great interest. Many different species were discovered, and their structure and habits fully described. In 1838 Ehrenberg published a magnificent work, with elaborate colored plates, entirely devoted to them, in which he described more than seven hundred different kinds. Two circumstances, relating to the infusoria, especially attracted the interest of observers, namely: 1. The fact that they constituted an entire class of living beings, previously invisible and unknown; and 2. The rapidity of their production and the immense numbers in which they exist.

In the opening chapters of his book,[1] Ehrenberg speaks of them as follows:

[1] Die Infusionsthierchen, als vollkommene Organismen. Leipzig, 1838.

"In the clearest waters, and also in the turbid, acid, or saline fluids of the most varied localities, in springs, rivers, lakes, and seas, often even in the internal parts of living plants and animals, not excepting the human body—and in all likelihood periodically mingled with the dust and watery vapor of the entire atmosphere—there exists, unperceived by the ordinary senses, a world of minute living organisms which have now for seventy years borne the name of infusoria. In the bustle of every-day life, this mysterious and immeasurable kingdom of living animalcules is passed over without interest or recognition. But for the quiet observer, who brings them under closer examination by the aid of magnifying instruments, they surpass all anticipation in the singularity and magnitude of their relations. In any single drop of standing dusty water, or solution of decaying material, we can frequently distinguish by the microscope actively-moving bodies, from $\frac{1}{1200}$ to less than $\frac{1}{20000}$ of an inch in diameter, often so crowded together that the spaces between them are hardly equal in size to their own bodies; and we can readily estimate, without exaggeration, that such a drop is inhabited by from one hundred thousand to many millions of these animalcules."

These microscopic infusoria, owing to their incalculable numbers, may even tinge extensive collections of water with well-marked colors. Ehrenberg has seen pools and ditches in the neighborhood of Berlin colored from this cause of a brick-red, and afterward of a blood-red hue. To some of them the property has been attributed of generating light; so that, although individually too small to be visible, they may produce an extensive phosphorescence on the surface of the sea. Some of the forms, originally included among the infusoria, are provided with calcareous or siliceous coverings; and when they die, these minute shells form by their agglomeration, mingled with other earthy matters, the substance of various rocks and soils. A deposit of mud has been found near the shores of Victoria Land, within the Antarctic circle,[1] chiefly composed of the siliceous shells of Diatomaceæ, which is of unknown thickness, but extending no less than four hundred miles in

[1] Carpenter on the Microscope, p. 302. Philadelphia, 1856.

length by one hundred and twenty miles in breadth. Ehrenberg regarded these creatures as forming, as a class, "by far the greatest number and perhaps also the largest mass of living animal organisms on the surface of the globe."

The infusoria exhibit among themselves a great diversity of form, size, and organization. Some of them, like *Vibrio*, *Monas*, *Spirillum*, and *Bacterium*, are either of the simplest possible organization, or else are too minute to allow their structure to be distinctly visible. They are from $\frac{1}{8000}$ to $\frac{1}{600}$ of an inch in length, and in thickness often not more than $\frac{1}{18000}$ of an inch. They are, however, in active motion, in a straight, spiral, wriggling or zigzag direction, notwithstanding that in most of them no visible organs or means of locomotion exist. Others, like *Chlamydodon*, *Ervilia*, *Paramecium*, *Kerona*, etc., from $\frac{1}{750}$ to $\frac{1}{125}$ of an inch in diameter, are covered on various parts of their surface with vibratile cilia, by which they move with repidity through the water. Others, like *Vorticella*, are attached by a slender contractile stem to some solid body beneath the surface, and have an oblique ring or crown of moving cilia upon the wide extremity of their bodies.

These animalcules are of especial interest in connection with the subject of spontaneous generation; for, by common consent, the question is now narrowed to the class of infusoria alone. No one at the present day imagines any of the higher animals to be produced spontaneously. It is only the infusoria in regard to which the doubt still exists.

For some years after the first discovery of the infusoria, a variety of fanciful characters were attributed to them which they did not in reality possess. The whole subject was so new and the field of observation so extensive, that some of the opinions adopted as to their physiology and organization were due simply to inaccurate or incomplete observation. The following notions, for instance, were occasionally entertained in regard to them:

1. They were thought to have no regular or determinate anatomical form; but to have resulted, as it was said, "from the capricious and wanton exercise of the organic powers of Nature."

2. They were thought to exhibit an indefinite or proteiform changeability of external configuration.

3. It was imagined by some that there was a frequent or even constant transition or reciprocal metamorphosis of the various infusorial forms into each other.

Ehrenberg, however, demonstrated that these ideas were erroneous. He showed that the infusoria have a regular and definite form, and that they may be classified, like other animals, in genera and species; that they are not transformed into each other, but that they have a distinct organization, which is always the same for each particular species. At the time when he published his great work on the infusoria, he had already decided against their appearance by spontaneous generation; but, before that period, the question had been discussed with considerable vivacity by two eminent observers of the last century, namely, Needham and Spallanzani.

Needham was an English priest and naturalist, born in London, in the early part of the eighteenth century. He was educated at the Catholic College of Cambray, in France, where he afterward took holy orders. He was occupied for the greater part of his life in teaching, either as professor or private tutor, and was finally appointed Director of the Academy of Maria Theresa, at Brussels. He wrote on microscopic animalcules, on polypes, on the barometric measurement of heights, and made researches on various questions of physics and natural history. In 1748 he originated the idea that the infusoria were produced by a kind of reorganization of decaying organic materials. At that time this particular theory had been abandoned for all other animals and plants, and had not yet been connected with the newly-discovered class of infusoria. This is evident from Needham's own language in the opening sentences of his first paper[1] on the subject:

"Modern naturalists," he says, "have unanimously agreed to lay down for a certain Truth, that every plant proceeds from its specific Seed, and every animal from an Egg, or something analogous, preëxistent in a Parent of the same kind."

Needham, however, believed that he had seen infusoria produced under circumstances where no eggs or germs could have

[1] Philosophical Transactions, xlv., 615.

preëxisted in the solutions. In order to provide for this, he took the juices of meat which had been extracted at a high temperature, and enclosed them, "hot from the fire," in glass phials, together with air also previously heated, and shut off from communication with the atmosphere by tightly-fitting corks. He found that in these infusions, after a few days, animalcules made their appearance; and as all living eggs or germs previously contained therein must have been destroyed by the heat, and no new ones could have been introduced through the closed necks of the bottles, he concluded that they had been generated from the dead and decomposing material by a kind of "vegetative force" residing in the solutions.

This opinion, on the other hand, was opposed by Spallanzani.

Spallanzani was an Italian naturalist, Professor of Natural History in the University of Pavia. He was a man of great acquirements, an honorary member of nearly all the academies of Europe, and universally held in the highest estimation. He wrote much and made many original and important investigations on the physiology of the lower animals, their respiration, digestion, the reproduction of lost parts, generation, the preëxistence of eggs and germs, and especially on the structure and characters of the infusoria. He thought that Needham's experiments were wanting in precision, from the fact that the degree of heat employed was not accurately determined, and the mouths of the phials not sufficiently protected from the external air.

He therefore devised and executed certain experiments in which the heat applied should be fully destructive to all preëxisting germs, and in which also the access of new germs from without should be effectually prevented.

For this purpose he took glass flasks containing air and organic infusions, and sealed them hermetically by melting together the sides of their narrow necks. He then kept these flasks immersed in boiling water for the space of an hour, and afterward exposed them to ordinary temperatures, such as were favorable to the generation of infusoria. The result of these experiments was that, "when the flasks were afterward opened, not one of them showed the least appearance of animalcules." It is a curious incident to find Spallanzani thus doing for the infusoria, in 1775, almost exactly what Redi had done for maggots and insects a little more than a hundred years before. I have given the account of these experiments with some detail, because they were the first in which the question was tested with so much rigor and exactitude. Spallanzani's conclusion was evidently this: that when the living germs already existing in the solution had been killed by boiling for an hour, and the access of others from without was absolutely prevented by hermetically sealing the flasks, there was no production of life within, and consequently no spontaneous generation.

But Needham was not satisfied with these results. He objected that so prolonged a boiling would not only kill the living germs, but that it would also impair, and perhaps destroy, the "vegetative force" of the infusion itself; and that this was the reason why animal life was not afterward developed in it.

This objection was a perfectly reasonable one. An organic solution which has been boiled is no longer in the same condition as before; and it was by no means certain whether the "vegetative force" supposed to exist in it, by which it was capable of generating animal life, might not be injured or destroyed by such a process. If so, how could the question be settled? How could the infusion be prepared in such a way as to destroy all its living germs, and yet not to interfere with its power of producing animal life?

Spallanzani undertook to surmount this difficulty; and he did so by a series of exceedingly careful and well-arranged experiments.

He first took a number of vegetable and animal infusions, which, as experience had shown, would inevitably become inhabited by infusoria, if exposed to the air at ordinary temperatures. He boiled these infusions for periods varying from half an hour to two hours, and afterward kept them in loosely-stoppered bottles, at ordinary temperatures. At the end of eight days they all contained infusoria; those which had been boiled for two hours as abundantly as those boiled for a shorter time. This showed that the previous ebullition had not, in point of fact, destroyed the vegetative force of the liquid; and that, if the infusoria were really produced by spontaneous generation, they would appear in a boiled infusion as readily as in one which had not been subjected to that process.

He then proceeded to experiment with similar fluids, boiled as before, but protected from communication with the external atmosphere. He took thirty-six flasks, containing nine different infusions, together with a full supply of atmospheric air, sealed them hermetically, and immersed them in boiling water for periods varying from half a minute to three-quarters of an hour. He then kept them under observation, in company with other flasks containing similar infusions but exposed to the air. At the end of eleven days, when the open flasks had become filled with animalcules, he opened and examined those which had been hermetically sealed. The result was, that boiling for even two minutes, in closed vessels, prevented the appearance of all animalcules of larger size and higher organization; while boiling for three-quarters of an hour prevented the appearance of all infusoria whatever, even those of the lowest order and most minute size. His conclusion was evident, as expressed in the following words: [1]

"We are, therefore," he says, "induced to believe that some of the germs included in the infusions may resist the action of heat for a certain time, but at last all are destroyed by it. And we may accordingly conclude that the crowds of animalcules which show themselves in open vessels, in organic infusions which have already been subjected to great heat, are not produced there because their germs have resisted this temperature, or because they have been generated spontaneously; but because new germs have been introduced into the infusion from the atmosphere after the boiling has ceased."

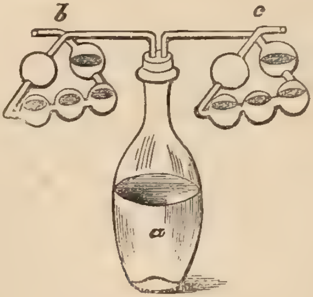
These experiments of Spallanzani carried conviction generally to the minds of naturalists at the time; especially as the discoveries in regard to the mammalian egg and its development were all tending in the same direction. Only one point remained which still seemed to admit of a doubt. This was,

[1] Opuscoli de Fisica animale e vegetabile, vol. i., p. 12. Modena, 1776.

that the experiments in question had been carried on in sealed vessels. These vessels, it is true, contained air, a substance necessary to the maintenance of all animal life; but the air was confined within a limited space. Might it not be that the production of infusoria required a *renewal* of air in the interior of the flasks, or that there should be a kind of ventilation going on during the course of the experiment? How to provide for this, and yet prevent the air, daily introduced into the flask, from bringing with it living germs, was now the object to be accomplished.

It was done in 1836 and 1837 by Franz Schultze and Theodor Schwann, in Germany, by two different methods:

Schultze took a glass flask, half filled with an infusion of various animal and vegetable matters, and which had been boiled for a long time on the sand-bath. The neck of the flask was fitted with an air-tight stopper; and through the neck of this stopper there projected two narrow glass tubes, bent at right angles, through which the renovation of the air was to take place. When the whole was thoroughly heated to the boiling-point, and all the air in the flask expelled, and steam passing out freely through the tubes, the end of each



(*a*) Flask containing the organic infusion.
(*b*) Bulbs containing sulphuric acid.
(*c*) Bulbs containing solution of caustic potassa.

tube was attached to a series of bulbs, containing on one side sulphuric acid, and on the other a strong solution of caustic potassa. All the air, accordingly, which entered the apparatus must pass through these liquids; and though the air itself would not be decomposed or injured by the passage, it would be purified from all living germs which might be present in it.

The flask was then placed at a window, exposed to the light and warmth of the summer weather, and air was drawn several times daily through the apparatus, entering through the sulphuric acid, and passing out through the potash. The result was as follows: [1]

"From the 28th of May to the beginning of August the experiment continued, with daily renewal of air, without the edges of the fluid, examined every day by the microscope, showing any living animal or vegetable forms. And when, at last, the apparatus was taken apart, there was no trace either of infusoria, or confervæ, or fungi, to be found in the whole fluid. On the other hand, all three showed themselves in great abundance after the flask had remained for a few days open to the atmosphere."

Schwann's experiment was similar to this, except that the air entering the apparatus was purified, not by sulphuric acid, but by passing through three inches of a narrow glass tube heated to over 600° F. It was thus calcined, and its living germs destroyed. The experiments lasted several weeks, and the liquid showed no sign of infusoria.[2]

Thus it was shown, beyond a reasonable doubt, that the infusoria were produced, as a general rule, not by spontaneous generation, but from their eggs or germs floating in the atmosphere. It is true, no one had ever seen these germs. They had never been recognized, even by the microscope; and their mode of origin and their development were both entirely unknown. Still they might readily exist in the atmosphere and yet escape observation. The animalcules themselves are so minute as to be invisible to the naked eye; and their germs, if any such exist, must be smaller and lighter still. Ehrenberg himself expressed this view in the following words: [3]

"Insignificant as is the weight of such invisible animalcules, yet it is calculable and has been estimated; and it is certain that the lightest current of air, which is capable of lift-

[1] Poggendorf's Annalen, 1836, xxxix., p. 487.

[2] Ibidem, 1837, xli., p. 184. [3] Die Infusionsthierchen, etc.

ing a feather, might play with these little bodies like so much vapor."

The atmosphere, in fact, as ordinary experience shows, is abundantly capable of wafting about, often to great distances, bodies much larger and more weighty than the infusoria or their germs. The phenomenon known as "sulphur-rain" is an instance of this. In the spring or early summer, after a heavy shower, the edges of standing pools are sometimes seen to be fringed with a fine yellow powder, bearing a very striking resemblance to flowers of sulphur. Microscopic examination shows this deposit to be nothing else than the yellow pollen of the blossoms of the pine. The pollen, at the time of its maturity, is scattered from the trees, and floats imperceptibly in the atmosphere, until the rain brings it down and it becomes visible on the surface of standing water. We know, too, that in point of fact, the atmosphere is everywhere pervaded by minute dust-like particles, which subside very slowly and are easily dispersed by the slightest current. These particles are usually imperceptible, on account of their minuteness and transparency, and the greater reflection of light by other surrounding objects. But if a single beam of sunlight be admitted through a narrow opening into a darkened chamber, the floating particles will at once become visible against the dark background, and can be plainly seen moving up and down in the track of the sunbeam.

In this way the scientific world gradually admitted the idea, which afterward met with general acceptance, that the air always contains an abundance of infusorial germs, carried hither and thither by the atmospheric currents, and ready to be developed into living animalcules whenever caught by the surface of standing water, or introduced into an infusion suitable for their nourishment. This conclusion was unavoidable: since it had been shown that, in boiled infusions exposed to the atmosphere, animalcules were always produced; while none made their appearance if all introduction of germs from without were rigidly excluded. Accordingly, it was generally conceded, during the earlier part of the present century, that the question of spontaneous generation was settled. Ehrenberg in 1838, Milne Edwards,[1] Longet,[2] Bergmann and Leuckart,[3] Robin,[4] Valentin,[5] Owen,[6] Carpenter,[7] all took the same view. The production of living beings without parents was a theory admitted to have no reasonable basis for its support, and was regarded simply as a curious relic of antiquity.

We have now reached the end of what may be called the third epoch in the doctrine of spontaneous generation. The first period was that in which it was regarded by the ancients as the regular and normal mode of production for a large class of animals. The second period was that from Redi to Cuvier, when it was narrowed down to a rare and exceptional mode of production for a few only of the most obscure species, and finally shown to be untenable even for them; while the third was that which opened with the discovery of the infusoria, and the experiments of Spallanzani and others on these animalcules. In the case of infusoria, however, the evidence was also found to be against the idea of their spontaneous generation, and by the year 1850 the discussion was regarded as closed.

But in 1858 it began again.

This time it was in France that the question was reopened. M. F.-A. Pouchet—corresponding member of the Institute, Professor of Zoology in the Natural History Museum of Rouen, a naturalist who had won distinction in the scientific world by his works on the anatomy and physiology of invertebrate animals, on ovulation and fecundation, on the infusoria, etc., and who was remarkable for the clearness and vigor of his style, and for his restless zeal and scientific activity—took up the subject afresh. He maintained that spontaneous generation was possible in the case of the infusoria, and that it had indeed succeeded in his hands. He was followed by several other observers who took a similar view, and for several years the de-

[1] Leçons sur la Physiologie, etc. Paris, 1857.

[2] Traité de Physiologie. Paris, 1850–1861.

[3] Vergleichende Anatomie und Physiologie. Stuttgart, 1852.

[4] Végétaux Parasites. Paris, 1853.

[5] Physiologie. Braunschweig, 1847.

[6] Comparative Anatomy and Physiology of the Invertebrates. London, 1843.

[7] General and Comparative Physiology. London, 1851.

bate on this question became, if possible, more animated than before.

The motive for this last renewal of the discussion, however, was somewhat different from any of those which had preceded it. Since the beginning of the century the science of geology had been gradually acquiring its present form, and had been particularly extended by the discovery of fossil remains of animals different from those which are now in existence. It was also found that the earliest remains of animal forms, thus discovered, belonged to the lower orders; reptiles and fish, for example, having apparently been fossilized before birds or quadrupeds, and the invertebrate animals generally before the vertebrata. The idea thus originated that animal life upon the globe had consisted in a series of different forms, following each other in a certain order of succession; the simpler forms appearing first, and those which were more highly organized coming afterward. This very naturally led to the theory that the higher forms of organization, man included, instead of being created independently by themselves, had been gradually developed during the course of ages, out of the inferior types. Furthermore, as some of the deepest strata showed no organic remains whatever, it was thence assumed that a time had existed in the history of the earth when even these lower forms of life had not yet made their appearance. In this way the conclusion was reached that the lowest and simplest of living beings, the earliest to make their appearance upon the earth, had been first formed by the spontaneous organization of inanimate materials; and that they had afterward produced, by a continuous process of gradual development, the whole series of animal life which finally peopled the surface of the globe.

These conclusions may not all have been strictly deducible from the known facts of geology; but they were very widely adopted, and no doubt gave a new stimulus to the idea of spontaneous generation. This is evident from the expressions used by Pouchet himself. "For all reflecting minds," he says,[1] "heterogeneous production is a logical consequence of the appearance and ascending development of organized beings upon

[1] Pennetier, Origine de la Vie, Paris, 1868; preface by M. Pouchet.

the globe." The most recent discussion of the question, therefore, though essentially the same as before, was yet carried on, to some extent, from a different point of view.

Some years previously a book had appeared in England which excited considerable attention, entitled "Vestiges of the Natural History of Creation;" and, in a sequel to this work, published in 1845, reference was made to certain curious experiments by a Mr. Crosse and a Mr. Weekes. These two observers claimed to have produced a new species of acarus by means of a continuous current of electricity passed for a long time through a saline solution. The experiments of Mr. Crosse were the first, and the new creature thus produced received the name of the *Acarus Crossii*. Mr. Weekes's experiments were given in detail in the sequel just mentioned. He prepared a close glass jar containing ten ounces of a solution of ferrocyanide of potassium, supplied with an atmosphere of pure oxygen, and traversed by a constant current of electricity from a galvanic battery of ten pairs. The experiment began on the 26th of May, 1842. Five months afterward a number of nearly full-grown acari were discovered on the sides of an open jar containing the same solution and also traversed by the electrical current. Afterward they appeared also in the close jar, as indicated by Mr. Weekes's description.

"In the beginning of June, 1844," he says, "rather more than two years from the commencement of these operations, in examining with a lens the deposit of oxide of iron on the bottom of the jar, I saw for the first time unequivocal proof of the existence of animal life within the close vessel. Several spinous processes of the acari and other remains were detected floating on the surface of the solution, and others attached to the surface of the glass a few lines above the liquid; while, under circumstances somewhat more obscure, several entire dead insects were perceived amid the flakes resting on the bottom of the jar."

An almost ludicrous circumstance was noticed at this point in the experiment; namely, that no mechanical provision had been made for the preservation of the living insects generated from the solution, so that, as Mr. Crosse remarked on visiting the apparatus, they would fall in and be drowned as fast as they were produced.

"This conjecture was right," the experimenter goes on to say, "for, although I have latterly watched the proceeding with diurnal care, I have never identified the presence of more than two living insects at a time within the close apparatus, and these as speedily and invariably shared the fate of their predecessors." Thus the electrified liquid, which was supposed to have given origin to living insects, proved immediately fatal to them as soon as they were produced.

These experiments were received with almost universal incredulity. The high grade of organization of the acari, a class nearly related to insects, and the fact that those appearing about the solution in open vessels soon began to lay eggs and reproduce their kind, did not invite general confidence in the idea of their spontaneous generation. Mr. Crosse sent to the French Academy of Sciences a communication describing his experiments, accompanied by a phial containing a specimen of the acarus preserved in spirit;[1] but it did not obtain from the Academy an encouraging reception. The truth is, that in such an experiment it is a capital and indispensable condition of accuracy that no possible communication should exist between the interior of the apparatus and the external atmosphere. Wherever joinings are made between its different parts they are apt to become imperfect; and the smallest crevice will, if the experiment be long continued, give rise to currents of air passing outward and inward, which may bring with them organic germs, and thus vitiate the results. This is owing to the inevitable fluctuations of temperature. For when the external atmosphere is rarefied by heat, an outward current from the jar is at once established; and when it falls again, a corresponding inward current takes place, until an equilibrium is restored between the exterior and the interior. In the celebrated experiments of the acari, it did not appear that sufficient care had been taken to avoid this source of error so familiar to other observers.

The observations of M. Pouchet, however, in 1858, commanded more attention. He insisted that he had repeated the experiments of Schultze "with every exactitude," but

[1] Comptes rendus, October 30, 1837.

with different results. He also announced the production of a fungoid vegetation (Aspergillus), from a boiled infusion of hay enclosed in an atmosphere of oxygen over mercury. His plant, like the acarus of Crosse, was said to belong to an unknown species, and the experimenter consented to adopt for it the name of *Aspergillus Pouchetii.*[1] Other similar cryptogamous plants, protozoa and infusoria, were said to have been produced in similar boiled infusions, strictly protected from external contamination.

These communications excited immediate opposition. Milne-Edwards, Payen, Quatrefages, Claude Bernard, and Dumas, all reported contrary results. It was also objected that a heat of 212° Fahr. is not always sufficient to destroy the germs of cryptogamous plants, which will sometimes resist a much higher temperature. In some experiments, accordingly, a preliminary boiling may not have been sufficient to kill all the germs contained in the closed vessels, and this accounted for the subsequent appearance of organic life. Both Milne-Edwards and Claude Bernard, however, had found in their experiments that organic infusions, thoroughly boiled, and protected from all access of external air, did not produce any living infusoria.

M. Pouchet, who, together with his intellectual agility, seems to have appreciated also the humorous side of a discussion, turned upon his opponents with an unexpected reply. "The instant you assume," he says,[2] "that a temperature of 212° is not sufficient to destroy eggs and spores, the conclusions that have been accepted for the last twenty years, from the experiments of Schultze and Schwann, become absolutely null and void. And if, for such a reason, you condemn my own experiments as invalid, the same verdict must be pronounced on those of MM. Milne-Edwards and Claude Bernard; in which case it appears very extraordinary that no infusorial plants or animalcules appeared in the experiments of the four *savants* whose names I have quoted. This leads us, you perceive, to a consequence of extreme gravity; for if that be so, every thing is to be begun over again."

[1] Comptes rendus, December 20, 1858. [2] Ibidem, January 17, 1859.

The weight of the discussion, however, soon turned upon the supposed source of infusorial organisms; namely, the general dissemination of germs in the atmosphere.

M. Pouchet denied that the atmosphere contained these germs in any abundance. He contrived an apparatus called an "aëroscope," for collecting upon a glass plate the dust-like particles of the atmosphere; and, in thirty-five cubic inches of air collected in his laboratory by this means, he "did not discover a single infusorial egg or a single spore." He collected the atmospheric *débris* from freshly-fallen snow; and in several hundred observations he found "only two encysted infusoria or eggs, two dead and deformed infusoria, three naviculæ, three bacillariæ, and two bacteria; nothing else which could be referred to either animal eggs, or spores."

He maintained, therefore that the atmosphere was in reality very poor in germs, and altogether incapable of accounting by such means for the abundant production of infusoria in organic infusions.

This part of the question was taken up by M. Pasteur, a chemist of eminence, whose attention had already been especially directed to the subject of fermentation. His experiments were intended to determine whether or no the atmosphere, in coming in contact with an organic solution, brought with it, beside its own constituent gases, any thing capable of producing living organisms. In this inquiry, the enterprise and untiring industry, exhibited by the experimenters on both sides, form one of the most remarkable chapters in the history of spontaneous generation. There is evidence that some feeling was excited in the course of the debate, and the discussion may almost be said to have kept the Academy in a turmoil for some six or seven years.

M. Pasteur took glass flasks, filled to about one-third of their capacity with a clear watery infusion of brewer's yeast. He then raised the liquid to the boiling-point; and while ebullition was actively going on, drew out the necks of the flasks to a narrow point, and sealed them in the flame of the blow-pipe. He thus had the boiled fermentible liquid, after cooling, enclosed in an air-tight vessel, with only its own rarefied vapor. He could then, by cutting off the neck of the flask in any particular locality, allow it to be refilled with the external atmosphere, which would pass in to occupy the vacant space; and, the flasks being immediately resealed, the effect of this atmosphere upon the liquid could be observed.

He prepared sixty of these flasks. Twenty of them were afterward opened and resealed in the country, at a distance from any habitations, at the foot of the heights which form the first plateau of the Jura range, thus exposing the liquid to the air of that locality alone. Twenty others were opened and resealed on one of the Jura mountains, twenty-five hundred feet above the sea-level; and the remaining twenty on the Montanvert, in the valley of Chamouni, near the "Mer de Glace" glacier, at an altitude of six thousand feet.[1] Now, the chemical constitution of the air from these different localities must have been the same; and, if the production of infusoria depended simply on its chemical influence, they should be generated indifferently in all the flasks used for experiment. On the other hand, if the atmosphere contained floating organic germs, these might easily be more abundant at the lower levels, and less so in proportion to the altitude attained.

The result was that, of the first twenty flasks, filled and resealed at the foot of the Jura, eight afterward produced living organisms; of those filled on the flanks of the mountain, at twenty-five hundred feet elevation, five flasks showed similar productions; and of those filled with air on the Montanvert, at six thousand feet, only a single one became the seat of organic life.

Pouchet, however, performed similar experiments with different results. In company with MM. Joly and Musset, of Toulouse, he took flasks containing infusions of boiled hay, and filled them with air, in the manner above described, at two different localities in the Pyrenees. Two were thus opened and resealed at La Rencluse, at six thousand two hundred and fifty feet elevation, and two others on the Maladetta glacier, at a considerably higher level. M. Pouchet even took pains to ensconce himself in a crevasse of the glacier, and with his back resting against one wall of ice

[1] Comptes rendus, November 5, 1860.

and his face fronting another, filled his flasks with the air of this unusual locality.[1] Nevertheless, in the course of a few days the infusions, after being kept at a warm temperature, contained infusorial organisms, such as Bacterium, Monas, and Spirillum, "in prodigious quantity."

He also took pains to procure flasks of air from the summits of the Buet, Monte Rosa, and Mont Blanc, altitudes varying from ten thousand to more than fifteen thousand feet; and these specimens of air, brought to Rouen, and added to boiled organic infusions, all produced infusoria.

Now, it would seem that, if any air could be regarded as absolutely pure, it would be that which had been taken from these localities. And yet M. Pouchet himself, in the course of his indefatigable researches, met with a singular proof that light bodies may be transported by the atmosphere even to the tops of the highest mountains. A phial of pure, newly-fallen snow was taken[2] by Dr. Kolbe from the summit of Mont Blanc, and brought to Pouchet. On melting, it made about one cubic inch of water, which was, to all appearance, pure and clear. But a slight deposit was formed in it on standing, and this deposit contained the following substances: a few corpuscles of a mineral nature, a dozen young cells of Protococcus nivalis, two woollen filaments, one white and one blue, a fragment of a confervoid plant, and a minute tuft of vegetable air-tubes. Whoever has experienced the occasional force of the wind on Alpine summits, will not be surprised at this result.

M. Pouchet, however, declared that all his examinations showed the atmosphere to be everywhere poor in organic germs, and often entirely destitute of them; and that its capacity for generating animal life resided, not in these germs, but in the general vivifying power of the air. M. Pasteur, on the other hand, insisted that, the chemical constitution of the air remaining the same, its power of producing organic life varied with the locality from which it was taken; and this because the number of germs contained in it varied in different places.

[1] Comptes rendus, September 21, 1863. [2] Ibidem, 1864, liv., p. 189.

Both the disputants stated their positions in definite terms.[1]

M. Pouchet said: "I assert that, from whatever region of the globe I take a quantity of atmospheric air, if this air be placed in contact with a putrescible liquid in hermetically-sealed vessels, the liquid will invariably become filled with living organisms."

M. Pasteur said: "It is always possible to obtain, in a particular locality, a notable volume of atmospheric air which, without having been subjected to any physical or chemical modification, is nevertheless incapable of exciting any change whatever in a putrescible liquid."

These assertions, emanating from two eminent observers, both members of the Academy, were so diametrically opposed to each other, that it was agreed to refer them to a committee in whose presence the requisite experiments should be performed, and who should report to the Academy on the result. Such a committee, composed of five members, was accordingly formed, and entered upon its labors in June, 1864, in the Chemical Laboratory of the Museum of Natural History, at the Garden of Plants.

M. Pasteur first presented three of his flasks which had been filled with air, four years previously, on the Moutanvert, and had remained ever since perfectly unchanged. One of them was opened under mercury; and the air which it contained, on being analyzed, was found to have the natural constitution of the atmosphere (twenty-one parts of oxygen to seventy-nine parts of nitrogen). Another flask was opened by a minute orifice at the neck; and, after being left for three days exposed to the atmosphere, it contained flakes of a cryptogamic vegetable growth, which subsequently became largely developed.

M. Pasteur then prepared and sealed, before the committee, sixty flasks, similar to those previously used. Nineteen of them, after cooling, were opened and immediately resealed in the amphitheatre of the Museum; nineteen on the top of the dome of the same building; and eighteen others at a country-house, a few miles from Paris, under a thick growth of poplars. Afterward, microscopic vegetations were devel-

[1] Comptes rendus, February 20, 1865.

oped in five flasks of the first set, six of the second, and sixteen of the third. All the remainder were unchanged at the end of over four months.

The committee subsequently reported[1] the result of their experiments, and gave as a conclusion that *the facts observed by M. Pasteur, and contested by M. Pouchet, were of the most absolute exactitude.*

It thus seems to have been placed beyond a doubt that the atmosphere is incapable, from its chemical constitution alone, of exciting organic growth in a boiled infusion; but that it often introduces with it into the solution invisible germs which do have this effect, the proportion in which these germs are present varying with the locality from which the air is derived.

But up to this time the dispersion of organic germs in the atmosphere was not an actually observed fact; but only a probable inference from the results of experiments like the above. This is what gave a certain weight to the objection of M. Pouchet, when he said in one of his communications:[2] "It seems to me that, when an experimenter declares that he can collect from the atmosphere either the eggs or spores of microscopic organisms, we have a right to demand that he should show them to us."

No one, in fact, had succeeded in collecting these germs from the air in any abundance, in such a form as to be visible and recognized.

This, however, was accomplished by Dr. Lemaire in 1864.[3] He adopted the plan of condensing the vapors of the atmosphere in glass tubes by means of artificial cold. The moisture thus obtained was then kept in the tubes, well stoppered, together with an equal or double volume of air, at a temperature of from 73° to 86° Fahr. The collections were made in the month of July, from a marshy neighborhood in the country, from the Garden of Plants in Paris, and from a village near the city, situated at two or three hundred feet higher elevation. The liquid, when first condensed, was colorless and

[1] Comptes rendus, February 20, 1865. [2] Ibidem, March, 12, 1860.
[3] Ibidem, August 17, 1864.

limpid. It contained microscopic vegetable germs or *spores ;* a great number of *pale cells*, of different dimensions; a considerable abundance of very *small semi-transparent bodies* (thought to be the germs of future infusoria) of a spherical, ovoid, or cylindrical shape, sometimes regular and sometimes irregular; certain *brownish corpuscles*, apparently of vegetable origin; *starch-grains ; dust-particles*, and *cubical crystals.* Within twenty-four hours afterward there were developed an abundance of living infusoria, bacteria, vibrios, spirilla, and monads, together with ramified cryptogamic vegetations. Exactly in proportion as the cryptogamic vegetations and the infusoria were developed, the spores and the small semi-transparent corpuscles were found to disappear.

Thus the actual existence of organic germs in the atmosphere was demonstrated; and there could no longer be any doubt that these germs, when introduced into an organic infusion, are abundantly sufficient to account for the production of infusorial and vegetative life.

For we know that these low forms of organization are especially distinguished by their power of rapid multiplication. Each individual plant of *Reticularia maxima*,[1] a species of fungoid vegetable, produces no less than ten millions of spores; and, according to Dr. Carpenter,[2] a large kind of fungus, the *Bovista giganteum*, has been known to grow so rapidly that its cells must have been produced at the rate of four thousand millions per hour. Many of the infusoria also multiply themselves by division, and in this way increase rapidly in numbers in any infusion which is suitable for their growth. Prof. Bastian [3] has seen under the microscope a bacterium, of moderate size, "divide into two, and each of these into two others, somewhat smaller, in the course of fifteen minutes." But if such an organism were only to divide into two once in every fifteen minutes, and if this process were to go on for six hours, we should have at the end of that time over sixteen millions of bacteria, produced from a single individual. There is no

[1] Quoted in Longet, Traité de Physiologie, vol. ii., part iii., p. 13.

[2] General and Comparative Physiology, p. 95.

[3] Origin of the Lowest Organisms, p. 6. London, 1871.

difficulty, therefore, in accounting for the abundance in which the infusoria may appear in a solution, after a few germs have once been introduced from the atmosphere.

And yet the question is not fully settled. Even supposing that the animalcules which appear in organic infusions come from atmospheric germs, and granting this to be their usual and regular mode of propagation, may they not also, in some instances, arise by spontaneous generation? May they not show themselves, as an exceptional circumstance, in liquids from which all access of the atmosphere has been excluded?

The only form of experiment calculated to give a direct answer to this question is that of organic solutions subjected to heat, and kept in glass vessels hermetically sealed. Here, there is no possibility of error. There are no joints which may be imperfectly secured, and no means of communication whatever with the external atmosphere. Consequently, when infusoria make their appearance in such a liquid, either they must have been produced spontaneously, or else their germs must have resisted the heat which has been applied beforehand.

Spallanzani, it will be remembered, found reason for believing this to be the case. A single boiling of the liquid in a closed flask prevented the appearance of all infusoria of the higher orders; but other and simpler forms continued to be developed even when the liquid had been boiled continuously for half an hour. It was only after boiling for three-quarters of an hour, that all infusorial life failed to appear in the solutions. Mantegazza,[1] in 1859, sealed up in a glass tube a decoction of lettuce, leaving two-thirds of the tube filled with air, and then exposed it for thirty minutes to a temperature of 212° Fahr., in one instance even for forty minutes to a temperature of 284°, and two or three days afterward found living infusoria in the liquid. Pasteur,[2] found that milk boiled for two or three minutes in a flask, and then supplied with calcined air, became afterward filled with infusoria; but, if boiled four or five minutes, the production of infusoria diminished in proportion to the time of ebullition. On the other hand, if

[1] Comptes rendus, January 31, 1859. [2] Ibidem, May 7, 1860.

the milk were boiled at a temperature of 230° Fahr. ($1\frac{1}{2}$ atmospheres), it never yielded infusoria.

But altogether the most careful and complete of the modern experiments on this point are those of Prof. Jeffries Wyman,[1] of Harvard University. In fact, the two experimenters who have pursued this part of the investigation above all others, in a thoroughly careful and rigorous manner, are Spallanzani in 1776, and Wyman in 1867. Both of these observers found that infusoria are produced in organic solutions which have been subjected to boiling. But with Spallanzani, their production was limited to a boiling of half an hour in duration. Wyman, on the other hand, succeeded in obtaining them after an ebullition of four hours; although a longer boiling than this prevented their appearance altogether. "In pushing the experiments still further," he says, "we have not found that infusoria appeared in any instance if the boiling were prolonged to five or six hours. Several experiments, in which many flasks were used, were tried; but the result was uniformly the same. Thus, a limit to the development of infusoria in boiling water was reached."

What are we to believe from these facts?

One circumstance it is important to remember. Nearly all the experimenters on this subject have found that a single exposure to 212° Fahr., in closed flasks, prevents the appearance of many of the infusoria; and that, the longer the boiling is continued, the fewer the instances or the numbers in which they appear, until at last a point is reached beyond which they do not appear at all. This looks very much as if the animalcules originated from preëxisting germs, which can resist a short boiling, but are killed by it if long continued. Otherwise, if they are produced by spontaneous generation, why should they not appear in a liquid which has been boiled five hours, as well as in one which has been boiled for half that time? We cannot say that it is because the boiling has changed the composition of the liquid, and thus unfitted it for the production of infusorial life. Spallanzani investigated this point, and showed that the same boiled infusions which

[1] American Journal of Science and Arts, xliv., September, 1867.

failed to produce infusoria in close vessels, became abundantly filled with them on exposure to the air. In Schultze's experiment, the infusion which had continued for two months in the apparatus, and had been examined daily without showing infusoria, after being opened contained vibrios and monads on the second day. In one of Pasteur's flasks which had remained four years unaltered, vegetations made their appearance in three days after it was opened.

On the other hand, if the infusorial germs preëxisted in the solutions, they must have, in many cases, a power of withstanding heat, which appears altogether exceptional. For a certain time it was taken for granted that a temperature of boiling water would destroy the infusoria and their germs, because such a heat is fatal to all animals and eggs, so far as known. But this was plainly reasoning in a vicious circle. It was assumed that, as a temperature of 212° would destroy all animal and vegetable germs, it would necessarily have that effect on those of the infusoria. But the infusoria were precisely those upon which its effect had not been tested; and, until this should be ascertained, such a general assumption, applied to all germs without exception, was evidently unfounded.

The truth is, that the limits of temperature, within which life is possible, vary with different classes of animals, even among those of higher organization. A warmth of 100° Fahr., which is the natural and even necessary temperature for most birds and quadrupeds, is fatal to frogs and other cold-blooded species. Various animals and cryptogamous vegetables have been found[1] living in hot springs, at temperatures varying from 150° to 208°. Payen satisfied himself that the spores of *Oidium aurantiacum*, a cryptogamous growth sometimes occurring in bread, would support, even in the moist condition, a temperature of from 212° to 248°, without losing their power of germination.[2] They were entirely destroyed only at a temperature of 266° or 284°. M. Pouchet found, by his own observation,[3] that the seeds of a Brazilian *Marecago*, after

[1] Wyman, American Journal of Science and Arts, xliv., September, 1867.

[2] Substances alimentaires, p. 362. Paris, 1865.

[3] Comptes rendus, 1866, lxiii., p. 939.

being boiled continuously for four hours, were, in many instances, unchanged in appearance, and afterward, on being planted, germinated freely. On the other hand, Prof. E. Frankland[1] has recently found a small kind of *ice-flea* living, full of vigor and activity, beneath stones embedded in the surface of the Morteratsch glacier, where the temperature of the enclosed air never rises above the freezing-point.

It is by no means certain, therefore, that the germs of infusoria which appear in boiled solutions, may not have resisted the action of boiling water. It is true that Prof. Wyman has shown,[2] by a series of very thorough experiments, that vibrios and similar infusoria lose their power, both of motion and reproduction, after being boiled for from five to thirty minutes. Prof. Bastian has even given evidence to show[3] that bacteria and vibrios lose their reproductive properties by being exposed for ten minutes to a temperature of 140° or 167° Fahr. But it is only the infusorial germs, not the infusoria themselves, which are supposed to preëxist in the atmosphere or the solution; and they may very possibly withstand a temperature which would be fatal to the fully-developed organism. Unless we admit, therefore, that the infusoria in question are sometimes produced in sealed vessels by spontaneous generation, the germs of these minute bodies must possess the singular power of living and reproducing their kind after having been exposed continuously for four hours to the action of boiling water.

But what are the organisms in regard to which this doubt exists?

It is by no means the infusoria, as a class. On the contrary, since the time of Ehrenberg, important progress has been made in the study of these minute animalcules, and our ideas of their structure and classification have been greatly modified.

In the first place, the whole group of Rotatoria, including *Rotifer*, *Stephanoceros*, *Floscularia*, and many others, have been entirely removed from the class of Infusoria, and assigned to that of Worms. Their complexity of organization

[1] Nature, No. 100, p. 426.

[2] *Loc. cit.*

[3] Origin of Lowest Organisms, p. 55.

showed this to be proper; and their mode of reproduction is sufficiently manifest from the fact that living embryos, in process of development, can be often seen in the interior of their bodies.

Secondly, the idea of spontaneous generation has been abandoned for all the Ciliated Infusoria, constituting, at least, nineteen-twentieths of the class, as now understood. This group includes such forms as *Paramecium*, *Colpoda*, *Chlamydodon*, *Ervilia*, *Stylonychia*, *Kerona*, *Oxytricha*, *Urostyla*, and *Vorticella*—all those, in fact, which are more or less completely covered with cilia, and which move by the regular vibration of these little appendages. Within the last ten years it has been established, beyond a doubt, that many, and probably all, of these infusoria reproduce their kind by means of eggs, regularly fertilized in the ordinary mode of sexual generation. Balbiani,[1] Stein,[2] Engelmann,[3] and Claparède and Lachmann,[4] the most recent and accomplished observers on the subject, all agree on this point. Balbiani and Stein together have observed the process of sexual generation in no less than forty-seven different genera and sixty-six different species of the ciliated infusoria. Their fertile eggs are, in many cases, abundantly visible; and the embryos produced from them, discharged from the body of the parent, become developed into similar organisms.

Furthermore, the ciliated infusoria are never produced in boiled solutions which have been hermetically sealed or otherwise protected from the access of atmospheric germs. It is always and only the minute and more lowly forms that show themselves under these circumstances. By the almost universal testimony of experimenters on both sides of the question, the only infusoria, in regard to whose mode of generation there remains at present any doubt, belong to the four genera, Vibrio, Spirillum, Bacterium, and Monas; and, of these, bacteria and vibrios are by far the most frequent, and appear with the greatest persistency, in boiled infusions.

[1] Journal de la Physiologie, Paris, January, 1861.
[2] Organismus der Infusionsthiere, Leipzig, 1859, 1867.
[3] Zeitschrift für Wissenschaftliche Zoologie, 1862, xi., p. 347.
[4] Études sur les Infusoires, etc., Genève, 1856, 1861.

Now, these are precisely the smallest and most obscure of living organisms. They stand upon the extreme limits of the microscopic world; and in most instances no internal structure can be distinguished in them, the microscope revealing nothing but their form and motions. They were cited by Ehrenberg thirty years ago as the objects which escaped all satisfactory examination, and he refers them to the same category with those of the heavenly bodies which are indistinct on account of their remoteness. "Our experience," he says, "shows the organic creation to be as unfathomable in its minuteness as the celestial universe is in its magnitude, since the powers of our optical instruments are necessarily confined within certain limits, which, however, are not the limits of Nature. A 'milky way' of the minutest organisms runs through the genera Monas, Vibrio, Bacterium, Bodo."[1]

Another fact of some interest is, that these genera, with one exception, are all now considered by common consent as belonging to the vegetable kingdom. With regard to Monas there is a doubt in this respect; but all the Vibrioniæ, including vibrios, bacteria, and spirilla, are now classed among vegetables,[2] and are regarded as incomplete and transitory forms in the development of certain aquatic fungi. These were also the organisms which were found by Dr. Lemaire to be most rapidly developed in the moisture condensed by him from atmospheric vapor. "In a single drop," he says, "we counted more than two hundred specimens of Bacterium termo."

Thus we find that now, as always, the idea of the spontaneous generation of living beings is confined to organisms of which we know the least. Exactly where our definite knowledge fails, owing either to the minute size or the imperfect organization of these bodies, there commences the obscurity which hangs around their origin. It is very justly said, in support of their spontaneous generation, that, if this mode of production exist at all, it is precisely in the case of the simplest and most imperfect organisms that we should expect it. We might imagine a bacterium or a monad to originate in this

[1] Ehrenberg's genus Bodo was afterward assigned to other genera in the family of Monads.

[2] First established by Cohn, Acta Academiæ, etc., Bonn, 1854, xxiv., part i., p. 116.

way, but not an eagle or an elephant. On the other hand, it is alleged that the imperfect organization of these minute forms is only apparent, and depends on the imperfection in our means of observation. When our microscopes and other aids to investigation have been still further improved, we shall find, it is said, that the bacterium and the vibrio possess an organization of their own, not less essential and complete in its way than that which we now know belongs to the ciliated infusoria. There is every evidence that at least their regular and normal mode of production is from germs disseminated in the atmosphere; and they themselves, as we have already seen, are embryonic or transitional forms in the development of a distinct vegetable growth. They are consequently to be regarded as an integral part of the cryptogamic vegetable organizations; and, notwithstanding the apparent simplicity of their structure, they no doubt, like other plants and animals, have their definite place in the organic world.

Art. II.—*Remarks on Strictures of the Urethra of Extreme Calibre, with Cases, and a Description of New Instruments for their Treatment.*[1] By F. N. Otis, M. D., Clinical Professor of Venereal Diseases, in the College of Physicians and Surgeons, New York.[1]

Mr. President and Gentlemen: I desire this evening to call your attention, briefly, to a few practical points in the management of stricture of the male urethra.

In a paper, which I had the honor to read before this Association nearly two years since, I called especial attention to the influence of strictures of large calibre in perpetuating a purulent urethral secretion, concluding in the following terms: "*We may, then, affirm as a most important axiom, that the slightest abnormal encroachment upon the calibre of the urethral canal, at any point in its course, is sufficient to perpetuate a urethral discharge, or even, under favoring*

[1] Read before the New York Journal Association, November 24, 1871.

circumstances, to establish it, de novo, without venereal contact."

Since the foregoing aphorism was enunciated, my experience has resulted in a daily-increasing respect for slight and usually unsuspected narrowings of the urethral calibre, as a cause of establishing local points of irritation along the course of the urinary tract.

The following case presents a common phase of the difficulty alluded to:

Mr. J. W. R., a surgeon, aged forty-eight years, came to me in June last, complaining of soreness and persistent aching in the prostatic portion of the urethra, accompanied by a slight purulent discharge from the meatus. He had been a subject of gonorrhœal inflammation several months previously, and felt confident that this had resulted in the establishment of a low grade of inflammatory action in the prostate gland. With occasional suspicions of stricture, he had attempted to verify them by the use of sounds. At one time No. 25, of the French scale, was passed through into the bladder without obstruction, but, on other occasions, no larger than 20 F. could be introduced. He was, however, very positive that no organic stricture existed, but that the irritation, caused by the passage of the instrument, excited a spasmodic contraction of the membranous portion of the urethra, which arrested its progress. Attempting the introduction of a bulbous sound of as large a size as the urethral orifice would admit, viz., 27 F., I ascertained, first, that there was a stricture near the meatus. The bulb fitted the opening, but refused to enter. After steady, gentle pressure, continued for three or four minutes, it suddenly slipped through a narrow stricture about a quarter of an inch in depth. The bulb was now easily advanced for two inches, when another obstruction was encountered; this gradually yielded for about an inch, after which the passage of the sound, onward into the bladder, was easy and natural.

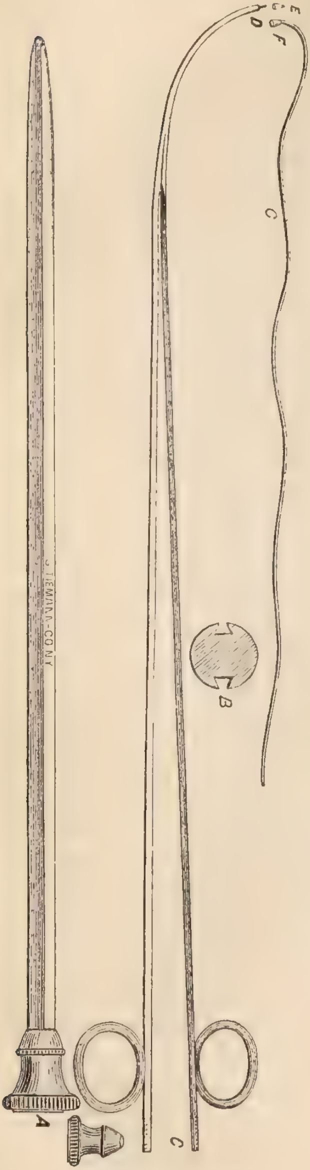
On the *withdrawal* of the instrument its bulb was arrested at a point $3\frac{1}{4}$ inches from the meatus by a stricture which presented a nearly uniform resistance for one inch, when it again glided smoothly outward until arrested by the previously-mentioned obstruction at the meatus. The handle of the exploring instrument was now permitted to fall, and dangled from the extremity of the penis, its bulb so firmly held by the stricture that not a little traction was required to withdraw it.

Here, then, we had a urethra, readily admitting the passage throughout its whole length of a No. 25 sound, of the French scale, and yet the presence of two decided strictures in its course positively demonstrated.

The stricture at the meatus was freely divided with the urethrotome of M. Civiale, and a No. 28 F. sound was passed through into the bladder. This operation was repeated, with increasing sizes, every third or fourth day, until a No. 30 F. sound was passed through the urethral canal, and continued at stated intervals for a fortnight. Still, the purulent oozing, though slight, did not cease. Believing that the full size of the urethra had been reached, and that the continuance of the discharge was due to the long-continued engorgement of the mucous membrane adjacent to the strictures, the use of the sandal-oil capsules was advised, under the influence of which it was hoped the trouble would soon disappear. The patient continued to take the capsules for a week, at the end of which time the discharge had quite ceased, but he still complained of uneasiness in the prostatic region, and still found shreds of mucus in his urine. Sound No. 30 F., of the Béniqué curve, passes quite readily, but the patient complained of unusual tenderness on its passage through the prostatic portion of the canal. From the locality and character of his sensations, he is confident that his whole trouble is now in the prostate. On the withdrawal of the sound, a little of a gray secretion was observed at its extremity, and which, under the microscope, was found to be largely purulent. This secretion, it seemed to me, had been brought from the prostatic portion of the canal. Examination per rectum revealed slight prostatic tenderness, but no hypertrophy. Endoscopic examination, half an hour after urinating, revealed nothing except a slightly-congested condition of the mucous membrane in the vicinity of the previously-mentioned points of stricture, and the presence within the prostatic portion of the canal of the secretion previously examined.

With these evidences of the existence of a chronic prostatitis, I injected five drops of a solution of nitrate of silver (grs. xxiv. to the ounce of distilled water) by means of Dr. Bigelow's prostatic syringe. Shortly following the injection, and for five or six succeeding days, the patient expressed himself as having felt a decided improvement; he also reported perceptibly less flocculi in the urine. Three injections, of the character previously used, were administered at intervals of eight days, but no further improvement resulted; on the contrary, a slight reappearance of the discharge at the meatus, with an increase of the prostatic discomfort, had occurred about the seventh day after the first application of the nitrate of silver. These symptoms again ceased upon the second application, but only to return at about the same time as on the previous occasion; a like repetition of the advance and retrograde movement occurred upon the use of the third and last injection. Suggesting the possibility of a stricture of large calibre still remaining, I introduced bulbous sound No. 28 F., and found that it accurately measured the stricture, the posterior boundary of which was $3\frac{1}{4}$ inches from the meatus, and which had been previously dilated to No. 30 F., three degrees above the supposed normal size of the urethra as indicated by the size of the meatus. I then introduced the shaft of Voillemier and passed upon it the largest dilating cylinder, measuring *thirty-two* millimetres in circumference, and corresponding with about No. 20 of the American scale. Under this distention the doctor recognized distinctly the sensation of rupture at the point of constriction. But little pain was experienced during the operation, and only slight temporary discomfort followed it. This occurred at 8 P. M., November 10th. Since that time the patient has been entirely free from the old unpleasant sensations in the prostate, and also from any sign of discharge from the urethra; the only evidence of any trouble continuing is the slight mucous flocculi that still appear in the urine.

I have now under my care another case, Mr. J. G. A., aged twenty-eight, in whose urethra some half-dozen bands of stricture from one-eighth to one-fourth of an inch in breadth are present, anterior to the bulb. These have been dilated so that conical sounds from No. 28 to No. 30 F. have been passed, with more or less difficulty, at intervals of from four to eight days, for nearly two months. A few days since I introduced Voillemier's divulsor with shaft thirty-two millimetres in circumference (the largest attainable), and with but little more discomfort to the patient than that which had followed the use of the 30 F. sound —yet bulbous sound No. 26 F. still defines the bands of stricture very distinctly. Such a degree of resiliency, in my own experience, is uncommon, although I have seen repeated instances where it was almost as great.



Voillemier's divulsor.

On a former occasion, the importance of recognizing a distinct individuality, in every urethra, was insisted on, and likewise, the measurement of the calibre of each, not by any popular standard, but by the introduction of the largest-sized bulbous sound that would pass the uncontracted meatus. With this as a guide, the discovery of urethras presenting a calibre freely admitting a 30 F. sound will not prove of so rare occurrence as at present supposed. Contractions at the meatus are a fruitful source of failure to appreciate abnormal narrowings of the urethra; the complete suppleness and resiliency of the tissues of the normal meatus is a good test of its freedom from organic stricture, but congenital contractions to a greater or less extent, are not unfrequent. Here, both the natural suppleness and resiliency may be present, and the deformity may escape notice, unless carefully sought. Wherever a bulbous sound can, by a gentle pressure of three or four minutes' duration, be made to slip into the fossæ navicularis, and in the withdrawal is abruptly arrested, the indication for the free division of the meatus is positive; without it no efficient exploration of the deeper parts can be effected.

The chief embarrassment which arises, after the demonstration of these strictures of large calibre, is from the lack of instruments of sufficient size to divide or rupture them. The largest divulsing instrument of Mr. Thompson, of London, will not expand to a size equal to more than 28 F. The largest capacity of Mr. Holt's instrument is not greater. My own Holt, purchased some years since, had only a divulsing capacity of 25 F. until I had a larger cylinder made, which brought it up to twenty-eight millimetres. The instrument of largest capacity for the internal division of stricture is that of M. Maisonneuve, and, with the widest blade, this only corresponds to a sound *twenty-eight* millimetres in circumference. It is scarcely necessary to call attention to the *entire incapacity* of dividing or divulsing instruments to deal efficiently with strictures occurring in urethras whose normal calibre exceeds their own measurement.

The divulsing shaft of Voillemier, measuring thirty-two millimetres in circumference, and which is the largest instrument of any kind at present in use for operations on stricture, failed to rupture the strictures in the case of Mr. J. G. A., previously cited. Of what possible consequence, it may be asked, is the presence of a stricture, of a calibre sufficient to permit the passage of a No. 32 F. sound, where the normal calibre of the urethra is evidently several millimetres smaller? Briefly, that experience has shown the power of such strictures to keep up irritation, and even a purulent secretion, at various points along the urinary tract, as was the case in the instances just related. Simple over-distention of such strictures, or of *any* strictures, is at best but a temporary expedient. Complete rupture or complete division is the only method by which the speedy return of a stricture to its original point of contraction can be prevented. Every practitioner of much experience in operations for stricture must have been struck with the lack of uniformity in results by any and every method, as shown by the return of patients for treatment, after variable intervals from the date of operation. Taking into consideration the difference in the regularity with which patients continue the use of dilating instruments after an operation, it is evident that data on this point must of necessity be very imperfect; but I have noticed, in cases *where no after-dilatation was practised*, more permanent results in operations upon *tight strictures* than upon those of *large calibre;* and this, it has seemed to me, was because the tight stricture was more thoroughly ruptured or divided—that the stricture of large calibre was more likely to be simply over-distended or imperfectly divided, on account of its inferior density and greater dilatability, as well as from possible insufficiency of size in the instruments employed.

The great lack in all the means now in use for operations upon the variety of stricture under present consideration, viz., those of large calibre, is their want of adaptability to the dimensions of the stricture upon which operation is required. The operation is performed on the flaccid urethra; the amount of resiliency of the stricture is undetermined; the divulsing shaft is selected without exact data, and the appropriate size of blade in the cutting instruments is a matter of judgment, and very liable to error. In small strictures a certain positiveness of result is attainable, the stricture is divulsed or divided to an extent sufficient to relieve present emergency, but there is no certainty that the rupture or the division has been complete, and, unless this result is attained, the return of the stricture to its former dimensions is certain, and, unless combated by the regular and frequent use of suitable dilating instruments, is likely to be of speedy accomplishment. I would not be understood as at all undervaluing the great advantages—nay, blessings—that have resulted, and must continue to result, from the intelligent use of the admirable instruments of Maisonneuve, Holt, and others. In their prompt and ready application for the relief of close strictures they leave little to be desired, and must always occupy the prominent place in cases of emergency, when the chief consideration is to relieve retention of urine, or the near liability to it. I would simply assert that there is an uncertainty in the extent of their action—uncertainty as to whether or not the stricture has been completely divided, or whether other tissue besides that involved in the contraction has not also been divided or otherwise injured, and that, in strictures of large calibre, they are, as at present constructed, often entirely insufficient. With the view of supplementing these important shortcomings, I have designed the accompanying instrument, which has been manufactured very perfectly by Messrs. Tiemann & Co., 64 Chatham St., under my direction, and was especially intended for operating upon the strictures of Mr. G. H. A., in whose case the 32 F. shaft of Voillemier was used without effecting their rupture.

The instrument which I term *the dilating urethrotome* consists of a pair of steel shafts (A & B), Fig. 1, connected together by short pivotal bars, on the plan of the ordinary parallel ruler, as shown in the expanded instrument at Fig. 2. Its expansion or contraction is effected by means of a screw which traverses the handle connected with the lower shaft, and is moved by means of the finger-button (C). Attached to the distal end of the screw is a pair of short, curved, registering arms, seen at D, Fig. 1, which ride through grooves on either side of the shafts (A & B), and are marked, on one side, with the divisions and corre-

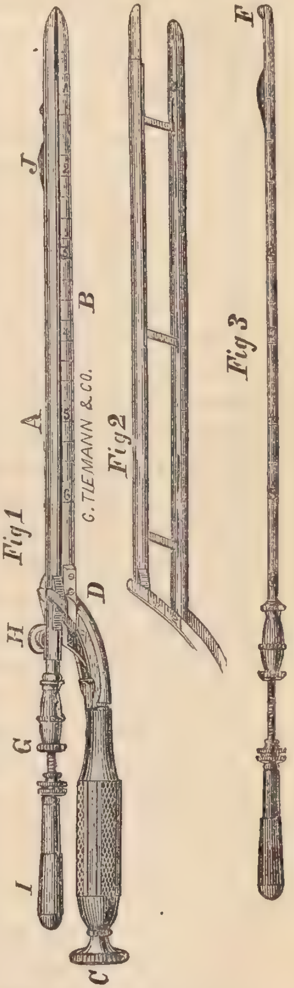


FIG. 1. Dilating urethrotome.
FIG. 2. Dilated.
FIG. 3. The urethrotome, which corresponds almost precisely with Dr. Peters's instrument.

sponding figures of the American scale, on the other with those of the French, in millimetres. Connected with the screw in the handle, the rise and decline of this register indicate exactly the degree of separation of the shafts, and consequently the precise progress of the dilatation of the instrument. Upon the inferior shaft (B) is engraved a scale of inches and quarter-inches, by which the depth of its introduction into the urethral canal may be noted. Up to this point the instrument is simply a *divulsor*, and may thus be used by introducing it into the urethra until its distal extremity is beyond the supposed point of stricture; the finger-button (C) is now turned, dilating the instrument, until, if considered desirable, the stricture is completely ruptured.

The upper bar of the instrument, however, which is hollowed out, is traversed by a urethrotome (Fig. 3)[1], the distal extremity of which terminates in a little metallic knob or indicator F, Fig. 3, by the metallic handle (G, Fig. 1) of the canula of the urethrotome, it is moved, at will, along through the entire length of the shaft (A) of the divulsor; a small button-screw, H, secures the canula at any point. Running through the canula and attached to the handle, I, is the staff of the urethrotome, terminating in a thin narrow spring blade, which, when at the extremity of the canula, is concealed in the deep groove which extends on its superior aspect through its entire length. On withdrawing the handle of the urethrotome I (its canula being fixed firmly at any given point by the button-screw, H), the spring blade (J, Fig. 1) rises out of the groove by means of a little elevation on its floor, rides over it, displaying the full width of the blade (about one and one-half millimetre) for half an inch, when it again drops down and is concealed in the groove of the canula.

The instrument, with its contained urethrotome, having then been passed down beyond the supposed or known point of stricture and dilated until the urethra is made tense, the button-screw, H, is turned, releasing the canula, which may then be drawn carefully outward until the knob or indicator,

[1] This form of urethrotome, with concealed spring blade, was invented by Dr. George A. Peters, of this city, and presented to the profession some years since.

at its extremity, is arrested by the stricture. The canula is then advanced about half an inch and secured by a turn of the button-screw, H; a rapid movement of the handle, I, of the urethrotome, *outward*, brings its blade up through the stricture, from behind it forward, incising it almost instantaneously, and passing down again into its concealment. The finger-button at the extremity of the handle of the divulsor is then turned, and the instrument is again dilated sufficiently to ascertain whether or not the stricture is completely divided; if not, the knife may be passed down, *from before backward*, completing the operation. Should other strictures present, the use of the indicator, while the urethra is kept tense, will reveal the exact locality of each, and the blade may be applied as required. The especial advantages claimed for this instrument are, that it first makes the urethra *tense*—thereby establishing the stricture as a fixed point; that it is capable of being adapted to strictures of any size within its compass; that it accurately defines their locality and extent; that it attacks a tense instead of a flaccid stricture, and hence, that its work is approached with confidence; that its incisions are made with ease, at a predetermined point, depth, and extent, instantaneously—and hence with the slightest possible discomfort to the patient; and lastly, that it combines great strength with ease and simplicity of manipulation. Since the completion of the instrument, now four weeks since, I have operated with it on six cases of stricture in the ante-bulbous portion of the urethra with complete success and satisfaction in every particular. Its compass is from 23 F. to 34 F., corresponding to 13 and 21 of the English scale. Messrs. Tiemann & Co. are confident of their ability to make one of similar pattern which shall range from 23 F. down to 18 F., corresponding to 13. and 9. of the English scale, and so curved that it may be applied to the deeper portions of the urethra; but it is for operation upon strictures of large calibre that this instrument has been constructed, and, except in such cases, especial superiority over others in use is not claimed. It will, however, I think, prove a valuable aid in completly restoring the natural calibre of urethras that have been imperfectly operated on by other instrumental means.[1] At the opposite extreme, in the range of strictures of the male urethra, we not unfrequently meet with cases which are *practically impermeable*; that is to say, in which, from the tortuous course of the urethral canal at the seat of stricture, or from a lack of instruments of sufficient tenuity or flexibility, either or both, no permeability can be demonstrated such as will permit the introduction of means through which the bladder may be emptied or the division or divulsion of the stricture can be accomplished. In this sense strictures may be permeable to-day and impermeable to-morrow. There are, I think, few surgeons who have not demonstrated the patency of a stricture by the easy introduction of a filiform bougie, and, in a day or two after, when preparing to operate by internal rupture or division, have not found the filiform guide refuse to pass the stricture, and even under complete anæsthesia, neither be coaxed nor compelled to lead the way for the shaft of the cutting or divulsing instrument. Under such circumstances no proper course is left but to allow the patient quietly to awake to the consciousness of a great disappointment, and to wait for a more favorable day.

Unfortunately, it is not always that such an operation can be postponed. For instance, in cases of stricture where, in usual health, the urethra will admit a bougie of eight or ten millimetres in circumference; in any such, a sudden cold, an excess at table, or other comparatively slight cause, may bring about a retention of urine that will not yield to general measures; and, finally, when the agony of accumulation in the bladder has gone on to the last degree of endurance, should no immediate passage through the stricture be effected, a resort to tapping above the pubis, or through the rectum, alone can save the sufferer from death. This great misfortune, and that lesser one, previously described, are often due,

[1] The only dilating urethrotome of which I find any record is that of M. Reybard (Traité Pratique des Rétrécissements du Canal de l'Urètre, par M. le Dr. Reybard, Paris, 1843, p. 205). The principles on which the instrument of M. Reybard was constructed required long and deep incisions of the urethralcanal, in consequence of which, "the instrument, never extensively used, has fallen into disuse" (Thompson on Strictures of the Urethra. Third edition. London, 1869, p. 235).

not to a want of skill, or intelligent effort, on the part of the surgeon, but simply to a lack of suitable instruments with which to afford relief.

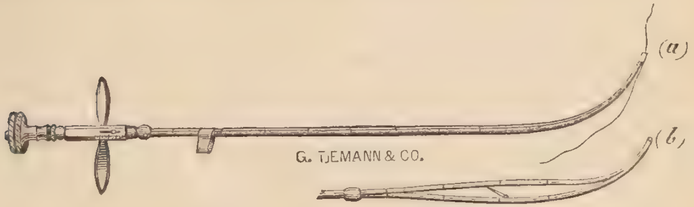
The time is within the memory of many present, when every case of close stricture, complicated with retention of urine, was considered a fair subject for *puncture of the bladder*, and when *perineal section* was the only recognized method of dividing a stricture; but (thanks to M. Maisonneuve, M. Perreve, Messrs. Holt, Thompson, Bumstead, Gouley, and others, who have invented or revived, improved and popularized, methods and instruments for the internal division or rupture of strictures) the operation of tapping the bladder occurs as a rare accident, and the performance of a perineal section is confined to cases of extreme difficulty and gravity.

Holt's dilator.

The great advantage thus gained in being able, in the majority of cases, to substitute a rapid and comparatively painless and safe operation, for one tedious, difficult, and dangerous, can scarcely be over-estimated; and hence, inventions of new means, or modifications of those already in use, which will bring a still greater proportion of cases within the reach of one or the other of the immediate and internal methods of operating upon strictures, cannot but be worthy of your attention.

I hold here in my hand the shafts of three of the most common and approved of the instruments for operation upon stricture of the urethra by divulsion and internal urethrotomy, viz., those of Thompson, Holt, and Maisonneuve, and of the smallest variety manufactured. The circumference of the shaft of each, in the order in which they have been named, is 12, 10, and 7 millimetres. Now, to make an operation possible by any one of these instruments, its shaft must first pass certainly and entirely through the stricture. For the purpose of facilitating and securing this passage, the filiform guide of M. Maisonneuve, of a somewhat smaller calibre, is attached to each by means of a delicate screw, the male thread of which is upon the extremity of the shaft, the female thread upon the filiform guide. This fine flexible bougie has but to be gently slipped along within the urethra, easily avoiding here and there the natural obstacles which oppose its passage; as these guides are manufactured as small as *three* millimetres in circumference, few, indeed, are the strictures so close as to forbid their entrance through them into the bladder. Unfortunately, however, the smallest female screw, of either foreign or domestic manufacture, which can be relied on as of sufficient strength to attach the filiform to the operating shaft, is quite *seven millimetres in circumference.* After the passage of the filiform guide—say of four, five, or six millimetres in size—through the stricture into the bladder, the next step in the operation is to screw on the operating shaft of whatever instrument it is decided best to employ. This now readily follows the guide until arrested at the point of stricture *by the female screw of seven millimetres in circumference.* The filiform has easily passed the obstruction, and its distal extremity is unequivocally coiled up in the bladder; but the shaft of the instrument will not readily follow. An important question now arises: How much force may be safely used in advancing the shaft? and, further, how shall we determine the direction in which it may be exercised? Although the guide may be well in the bladder at the commencement of the operation, this is no sufficient guarantee against a subsequent deflection of the operating shaft, as *the filiform may be dragged out of the bladder, and doubled back upon it,* which *must* take place should the shaft be forced out of the canal anterior to the stricture. This is an accident which may occur without the use of any very great amount of force. Permit me to cite a case in illustration.

In September last a gentleman (Mr. J. G. G.), aged fifty years, came under my care with the following history: Had an attack of gonorrhœa in youth, and had suffered with stricture since 1849. This had been dilated from time to time, but of late he had neglected this treatment, until, his micturition growing quite difficult, he became alarmed lest he should have an attack of retention of urine, and came on to New York for relief. Examination by an eminent surgeon of this city revealed two strictures—one three and a quarter inches from the meatus—the other five and a half inches. Filiform No. 2½ (French) was passed into the bladder, and represented the calibre of the deeper stricture. No. 17 (F.) passed the upper or anterior constriction. Immediately subsequent to this examination (which took place on



Thompson's dilator, adapted to Gouley's guide-bougie.

the 26th of August), the patient attempted to take a warm bath, but the water proved to be cold, and an attack of retention of urine followed in a few hours. For this latter difficulty he was attended by a second surgeon, who employed all the approved general means adapted to such cases, but failed to relieve the retention. Catheterism was carefully tried, but was decided to be impracticable. On August 28th—the patient then in very great suffering—a consultation was held, in which it was determined to attempt relief by an immediate operation upon the stricture. A filiform guide was readily passed into the bladder, and Thompson's dilator was screwed upon it. An effort to pass this instrument into the bladder was followed by its arrest at the deeper stricture. Hæmorrhage ensued, and the patient (who had not been etherized) complained of severe pain in the attempted advance of the instrument. It was, however, finally passed *by* the obstruction, and on, until it was supposed to have entered the bladder. A full divulsion was then made, and the instrument was withdrawn. On the passage of a catheter, only blood followed. It now became evident that the dilator had left the urethra, probably at the point of stricture, and that the bladder had not been entered. It was then decided to relieve the urgent trouble of the patient by tapping the bladder over the pubis. This operation was successfully performed, and a large amount of urine was drawn off through a No. 12 catheter, which was retained in the wound.

On the 3d of September, six days after the operation, the patient passed a small stream *through the urethra*, though with severe smarting. The retention catheter was then removed from the bladder. Since the operation there had been marked general febrile disturbance, and a persistent, dull pain in the perinæum: a free purulent discharge from the urethra had existed for three or four days.

September 14th.—Purulent discharge from the urethra continues. Examination revealed a false passage in front of a tight stricture, about six inches from the meatus. Some swelling and tenderness in the perinæum. Temperature 101½°; pulse 84.

September 21st.—Fluctuation recognized about two inches to the right of the perineal raphé. A deep incision resulted in the discharge of about two ounces of thick, sanious, offensive pus.

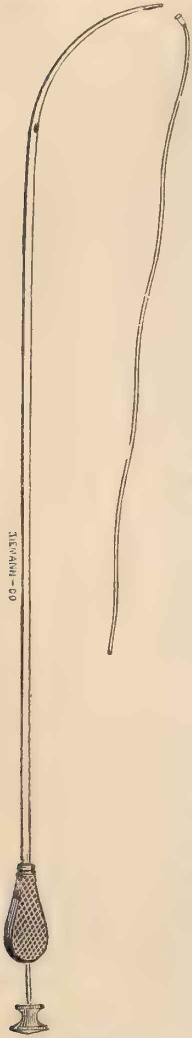
October 1st.—The patient came under my care; he was then very feeble, but free from fever. The abscess in the perinæum had nearly filled up. He stated that until within a few days he had passed a small quantity of urine through the opening in the perinæum during micturition. At this time no urethral discharge was perceptible. The wound over the pubis, resulting from the puncture of the bladder, was entirely healed. Examination, per rectum, revealed very slight enlargement of the prostate, and some induration of the tissues on the right side adjoining, but not the least tenderness at any point. Filiform No. 7 (F.) was passed through the urethral canal, and into the bladder, without the least obstruction. Further operative measures were deferred, on account of the feeble condition of the patient.

October 10*th.*—The general condition of Mr. G. having greatly improved, at 3 P. M. he was fully etherized. A filiform guide, of the size previously used (seven millimetres in circumference), was passed down through the strictures. To this was attached the shaft of the urethrotome of M. Maisonneuve, which readily followed the filiform guide. In order not to bring too great a strain upon the long, delicate staff of the instrument, the smallest blade was selected and slid down the groove in the shaft, completely through both strictures and well into the bladder. For assurance of complete division, a broader blade was then similarly employed. Directly succeeding this operation a silver catheter, No. 25 F., was introduced, by means of which the bladder was easily reached and thoroughly emptied. A quarter-grain suppository of the acetate of morphia was then passed into the rectum of the patient, and, on his recovery from the influence of the anæsthetic, ten grains of the sulphate of quinine were administered, and directions given to have this dose repeated six hours after.

October 16*th.*—Patient is up and walking about. Has not had an unfavorable symptom, nor a pulse above 84° since the operation. Sound No. 23 F., and increasing, passed every third day.

October 22*d.*—Has learned to pass an instrument himself (No. 25 F.), which is believed to be the full capacity of the normal urethra. Discharged, cured.

In reviewing this case of Mr. J. G. G., it appears that the two positive indications that the shaft of the divulsing instrument was in danger of leaving (if it had not already left) the track of the urethra, viz., *hæmorrhage* and *pain*, were disregarded, and it is in point here to remark that, as a rule, when these indications are combined in any case of attempted exploration of the urethra, it is the peremptory duty of the surgeon to withdraw the instrument. Puncture of the bladder becomes an insignificant operation, both in its performance and in its results, as compared with the probable consequences of the establishment of a false passage from the point of stricture down into the perinæum, than which, once the departure from the urethra has been made, nothing is easier to effect or more difficult to avoid. If it ever becomes necessary to use even a moderate degree of force in following up the guide, it should never be attempted, except by one who possesses the *tactus eruditus*, which can only be acquired by a long and studious experience. The filiform guide, after it has been introduced, *must run easily through the stricture, and its extremity be felt to be free, or its presence in the bladder cannot be verified.* The shaft of any operating instrument should *readily* follow the guide. It seems to me that, in the present case, the prime cause of disaster was the want of a proper correspondence between the size of the operating shaft and the previously-ascertained calibre of the stricture. With the limited means now in use, this correspondence is often impossible, as the smallest filiform bougie to which a screw can be properly attached is not less than *seven millimetres in circumference*, and the smallest shaft (that of M. Maisonneuve's instrument) is *fully seven* millimetres in circumference, while that of Holt, and also that of Voillemier, is *twelve* millimetres, and that of Thompson is *ten*.


Thompson's probe-pointed catheter (Bumstead's modification).[1]

In order to remedy this difficulty in operating upon strictures of a less calibre than these instruments, the stricture must first be forcibly dilated nearly or quite up to the size of the operating shaft. Now, the mode of accomplishing this necessary dilatation, so as to avoid danger of the accident previously described as occurring in the case of Mr. G., is the desideratum. Mr. Thompson conferred a boon on the profession when he gave to us his probe-pointed catheter for the purpose of dilating a close stricture, and through it emptying the bladder. This instrument, however, is defective, inasmuch as you get no positive evidence of its presence in the bladder until it has passed three or

[1] Modified by the addition of Maisonneuvre's filiform guide.

four inches into that viscus. Besides, the stylet of the probe-catheter extends only to its eye (the weakest part in the instrument), and at this point, after the repeated bendings, necessary in adapting it to different emergencies, it is very liable to break. Such an accident has occurred twice within my experience: once in my own hands (fortunately outside of the urethra); and again a few days since, with quite a new instrument, in the hands of one of the *internes* of the Strangers' Hospital.

Notwithstanding these imperfections, I have frequently found the probe-catheter of essential service in dilating strictures of a calibre ranging from three and four millimetres upward. Its probe-point, three millimetres in circumference, gradually increases in size, so that at the point of opening, where the stylet terminates, its size is seven millimetres, or just equal to that of the smallest filiform guide, to which a screw, for attaching it to the smallest operating shaft, can be connected. The following cases, lately occurring in my service at the Strangers' Hospital, illustrate its value: George W., New York, aged thirty-three, harness-maker, had an attack of gonorrhœa four years ago, lasting eight months. A year ago first noticed difficulty in making water; this grew gradually worse, until, on taking cold, it culminated in an attack of retention of urine lasting thirty-six hours. Two weeks ago he had another attack of retention, not so severe as the first, for in this he was always able to pass a few drops after great effort. The difficulty continued for ten days, when he passed water much more freely. At this time (April 11, 1871) he entered the Strangers' Hospital. On examination, the patient was found in good general condition, but with his bladder greatly distended, reaching within an inch of the umbilicus. Examination of the urethra shows meatus contracted to No. 14 (F.), and a stricture admitting No. 12, at a depth of two inches. Another at the bulbo-membranous junction admitted only a filiform bougie No. 1 (F.).[1] Though the bladder was so greatly distended, the patient was voiding, *guttatim*, a fair quantity of urine, and had no constitutional disturbance; it was therefore concluded, in the hope of avoiding the neces-

[1] Three millimetres in circumference.

sity of a perineal section, to attempt a gradual dilatation of the stricture until it should admit the necessary instrument for immediate operation.

April 14th.—Guide of four millimetres, closely hugged by the deeper stricture, was introduced into the bladder, and the shaft of Maisonneuve screwed on the filiform; too tightly held to be advanced by the shaft, it doubled back upon it at the meatus; operation again postponed for further deliberation.

April 19th, A. M.—Further attempts, under my direction, have failed to introduce a filiform larger than No. 4 (F.). Attempt to pass Thompson's probe-catheter also failed. Distention of bladder increased; urination more difficult; ordered one-fourth grain of morphine and hot bath. In the evening, patient passed water quite freely in the bath, and is much more comfortable.

April 21st.—Patient has had a chill. Pulse 108, temperature 103; bladder has reached the umbilicus; puncture of the bladder or perineal section seemed unavoidable. Another attempt with the probe-catheter determined on; was successful in advancing it eight inches. No blood or acute pain following, I concluded that the probe-point of the instrument was through the stricture six inches from the meatus. Guiding it with my finger in the rectum, I made steady, firm pressure for five minutes, and by this means succeeded in advancing the instrument still farther; assured, by sensations imparted to my fingers and the expressions of the patient during this effort, that I was still in the urethral canal, on withdrawing the stylet, a drop of urine announced the success of the expedient, and also the presence of the entire probe-point, three inches in length, within the bladder. I had now no hesitation, with my finger still in the rectum, in pushing the catheter directly on to its largest dimensions, viz., seven millimetres. After the withdrawal of a small quantity of urine, the eye of the catheter became occluded and was removed. Filiform guide No. 7 readily introduced, and Bumstead's modification of Thompson's catheter attached. By means of this the stricture was again passed, the over-distention of the bladder relieved, and the stricture dilated (as with the Thompson) to No. 10 (F.), the largest dimension of Bumstead's modification.

The stricture being now of sufficient size to permit it, the staff of Voillemier's dilator was screwed upon the filiform, and passed through the stricture and into the bladder without difficulty, the shaft No. 25 (F.) driven home, and the instrument withdrawn. The bladder was then emptied with catheter No. 22 (F.), the usual suppository of one-fourth grain of morphine and the ten-grain dose of quinine were administered. The case progressed favorably up to the 28th inst., when the patient, able to introduce a full-sized sound No. 25 (F.), was discharged, cured.

Case II.—John Burns, Englishman, carpenter, aged fifty-four, admitted to the Strangers' Hospital June 20, 1871. Contracted gonorrhœa in 1832, followed by stricture several months after, stream became very small, retention occurred frequently, requiring the urine to be drawn off with the catheter. Was successfully operated on in 1847 by the perineal section, and remained quite free from any trouble for twelve years. In 1869 he had a fall, striking upon his perinæum, since which time his old difficulties have returned and his attacks of retention have been frequent and severe.

June 20th.—Examination revealed, externally, the cicatricial evidences of the perineal section previously mentioned; internally, two and a half inches from the meatus, a stricture, calibre No. 15 F., a second at six inches in depth, through which a filiform bougie, three millimetres in circumference, refused to pass. Fine whalebone instruments of different curves were also tried, but with no better result. Bladder extending a couple of inches above the pubis; patient voids a small amount of urine, from time to time, *guttatim*, occasionally, by a fine, small stream. Failure of instrumental measures attributed to spasmodic action, as the bougies were hugged closely at the point of stricture.

During the following six days, general means were employed to reduce the spasmodic action, and other attempts to effect a passage of the strictures were made, but failed. An intercurrent attack of ephemeral fever delayed further interference until July 5th, when it was determined to etherize the patient and operate by such means as, in the progress of the preliminary explorations, the case might demand. Under ether, after a long trial, a small filiform guide was passed into the bladder, but closely held by the stricture. On attaching the staff of Maisonneuve, it was found impossible to advance it beyond the meatus, *the guide always doubling back upon the staff.* Perineal section then seemed the only course left to pursue, when, as a last effort to avoid this extreme measure, the fine probe-pointed catheter[1] was introduced, and by means of a finger in the rectum it was at last forced through the stricture, until, on withdrawing the stylet, a drop of urine announced its presence in the bladder. The catheter was then pushed on until its full calibre (seven millimetres in circumference) was passed through the stricture, and then withdrawn. Another of larger size was then used, and the stricture dilated to No. 8 F. No difficulty was now found in introducing the guide and staff of Maisonneuve, after which the strictures (the deeper of cartilaginous density) were divided and a No. 20 F. catheter passed into the bladder. The patient made a good recovery, and was discharged on the 18th.

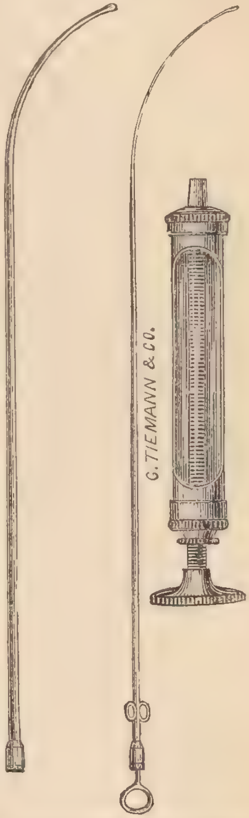
The value of proper dilating instruments of very small calibre is evident in the examples I have cited; to these, if occasion required, I could add others equally instructive. The imperfection of Thompson's probe-pointed catheter is apparent, in the necessity for its insertion some three inches into the bladder before the entrance of the eye of the catheter. The stylet reaching only to this point, is, as I have before shown, an element of very great weakness and danger. The only other instruments for the same purpose with which I am familiar are the grooved steel staff, and the retention-catheter of Dr. J. W. S. Gouley of this city. These are so contrived, that, through a groove on their inferior aspect, which terminates

Dr. Gouley's canulated staff.

at their extremity in a short canal, they may be slid down on a whalebone, or other bougie, previously introduced through the stricture. They are of exceeding service in many cases, and the *principle* in tortuous strictures is invaluable, but, as

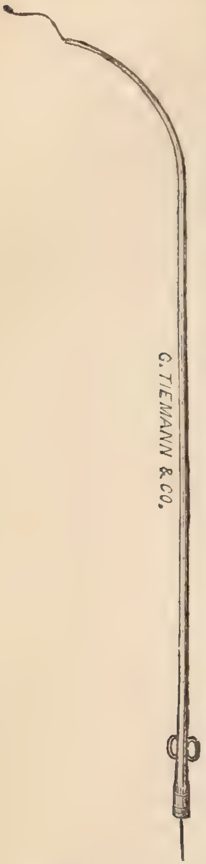
[1] Thompson's.

usually found, these instruments are relatively of *six* and *eight* millimetres in circumference. The steel staff may be made much smaller, but it gives no positive early proof of its presence in the bladder, and the retention catheter, the eye of which is three-quarters of an inch from the point, can scarcely be constructed of a size to be useful in cases such as I have cited. With the view of affording aid in the preparatory dilatation of strictures too small to admit the necessary instruments for immediate operation, I have designed the accompanying modification of Mr. Holt's probe-pointed catheter. It consists simply of a fine probe-pointed silver tube, eleven inches in length and three millimetres in circumference at its point, gradually increasing in size, so that at six inches it is six millimetres. This tube is traversed by a steel stylet throughout its length. Carefully insinuated through a close stricture, by the aid of a finger in the rectum, until its point may be supposed to have reached the bladder, the stylet is removed and a small syringe is applied to its proximal opening. If the instrument has passed the *sphincter vesicæ*, on a withdrawal of the piston, the urine will appear in the barrel of the syringe. The instrument may then be confidently pressed onward until the stricture is dilated to the largest capacity of the tube—a second tube, of corresponding form, but with dimensions ranging from four millimetres at the point to eight millimetres, may then be similarly used.



Dilating catheters and testing syringe.

In cases where, on account of the extreme closeness of the stricture, or from its divergent or tortuous course, a difficulty in passing the instrument occurs, Dr. Gouley's whalebone *guide-bougies* will prove serviceable.[1] These are used as in his grooved, canulated staff, viz., by the previous introduction of the guide-bougie into the bladder, *threading the dilating catheter upon it and following it down through the stricture.* Succeeding in this maneuvre, the guide-bougie may be removed—the presence of the dilating catheter in the bladder tested by aid of the syringe—the stylet introduced and the stricture dilated, as previously described. The whalebone guide-bougies, to be used in this manner, require to be from sixteen to eighteen inches in length. They are easily made of *any* desired length and fineness, and, by steaming, or boiling in oil, may be moulded at the extremity to any curve or angle deemed most likely to adapt itself to the eccentricity of the stricture.



Dilating catheter threaded upon the whalebone guide-bougie.

In comparison with all other instruments for the same purpose, the relatively smaller calibre of the dilating catheter must give it an important advantage in cases of stricture of extreme tenuity. Through its successful use, the earliest possible assurance of entrance into the bladder may be acquired. It is of simple construction, of flexible material, and of uniform strength. It will, I feel confident, enable the judicious and experienced surgeon to accomplish a satisfactory dilatation of many strictures, which, without its aid, would necessitate a puncture of the bladder or the perineal section.

[1] Dr. Gouley, Lectures on Stricture, New York Medical Record, March 15, 1870.

Clinical Records from Private and Hospital Practice.

I.—*Successful Applications of Dr. S. Fleet Speir's Artery Constrictor.* By Charles A. Hart, M. D., New York, Attending Surgeon to Manhattan Dispensary, Member of the American Medical Association, Fellow of the Academy of Medicine, Member of the New York Pathological Society, etc.

Case I. *Amputation of the Leg*—J. S., aged fifty-five. On March 1st, received a contused wound on the crown of the head, about two inches long, extending down to the bone, from a stake in a bale of hay, which fell upon him. The wound was dressed by a druggist, who hermetically sealed it by straps of adhesive plaster. The injury being considered a trivial matter, the dressing was undisturbed for four days, when he began to experience pain in the wound, and called in his family attendant, who found that an erysipelas had invaded the entire cellular tissue of the scalp, converting it into a big bag of pus. He removed the plaster from the wound to evacuate the collection, and also made an incision about an inch in length over the right zygoma, to facilitate drainage. This, it was supposed, would be all that would be required, but on the evening of the sixth day intense pain began in the right foot, which lasted about ten hours, and then abated. The following morning, when his physician saw him, he found that the tissues of the dorsum of the foot were attacked by *gangræna senilis.* Three days after I was called in consultation, and finding the line of demarcation established, removed the slough, which was composed of all the tissues down to the bones, except the extensor tendons of the toes, which were in a sloughing condition. The metatarsal bones were all denuded of periosteum, and necrotic; the tarsal, metatarsal, and ankle-joints open. The bone of the skull at the site of injury was found denuded of periosteum to the extent of about the size of a silver quarter. To afford the bone a chance to recover without exfoliation, I made two counter-openings low down on the back of the head, and closed the original wound by paring the edges and bringing them together with silver sutures, the result proving entirely satisfactory.

Feeling confident that the destruction of the foot was due to some cause other than metastatic, from the condition of the head, which was offered by his physician in explanation, I made careful inquiry about any antecedent illness that he might have had, and was assured by him that he had not been ill a day since the age of eighteen, when he had a light attack of variola. He had never had either rheumatism or gout, nor was any organic change discoverable.

On March 9th, his general condition being favorable, I amputated at the juncture of the middle and lower third of the leg, by the modified circular method. Some trouble being experienced in compressing the artery in the groin with the tourniquet, the thumb of an assistant was substituted. A short time previous to this, Dr. Speir had presented his instrument to the notice of the New York Academy of Medicine and the Pathological Society, together with the specimens of the carotid arteries of two sheep, a horse, and a portion of the femoral artery of a boy that had suffered an amputation of the hip-joint at the hands of Dr. Spier. All of the vessels showing such a complete closure, and being so favorably impressed with the principle, I determined upon the first opportunity to test the value of the instrument, and so employed it in the above case, where I applied it to the anterior and posterior tibial arteries, with the effect of completely arresting all bleeding. The peroneal artery, however, was so closely adherent to the vein, that I found it impossible to separate them, and, fearing that the interposition of the latter vessels would interfere with the action of the constrictor, I attempted to apply a ligature; but the vessel was in such a calcareous condition that the ligature cut directly through the coats of the artery down to the vein. The second ligature, which embraced some muscular fibres on the opposite side of the artery, succeeded in closing the vessel. When I examined the amputated portion, I was surprised to find the arteries so extensively diseased, being almost as friable as pipe-stem; this, however, verified my opinion as to there having been some other cause for the gangrene of the foot than the injury to the head, though doubtless that played no unimportant part as an exciting cause. Every thing progressed favorably up to the eighth day after the operation, when a condition simulating pyæmia was developed, but happily yielded to a tonic treatment. On the twenty-second day the ligature which I had placed on the peroneal artery came away, and *was followed by a hæmorrhage of about two ounces.* After this had ceased, I watched the patient for some time, but no recurrence taking place, I enjoined absolute quiet, in the hope that a clot would form without interference on my part; but in this I was disappointed, as his wife called at my office about eleven o'clock in the evening and stated that her husband had lost quite a quantity of blood, and was still bleeding when she left her home. I returned with her to her husband, and found the dressing of the stump saturated with arterial blood; he had lost, I should judge, about six ounces. I at once applied the tourniquet to the femoral artery in the thigh, which arrested the bleeding, and left him with the fear that I should be compelled before many hours to ligate or constrict the iliac artery. In this I was agreeably disappointed, as I had no further trouble, the tourniquet completely controlling the vessel, and not being removed until the stump was almost entirely healed, though the pressure was remitted after the third day.

Case II. *Constriction of the Common Carotid Artery.*—Four days after the above case, I was invited by a professional friend from the West to assist him in a case of ligation of the common carotid artery of the left side, in a child thirteen months old, for a congenital pulsating nævus of the left side of the jaw, about the size of a small orange. This had been previously treated by means of hot needles, but without arresting the growth.

The artery having been exposed, my friend paid me the compliment of deciding between the ligature and the artery constrictor, my decision being in favor of the constrictor. I was requested to apply it, and did so, arresting the pulsation of the tumor at once, and decreasing the size at least one-third; the division invagination of the internal coats of the artery was complete, while the external was uninjured, and lay like a flattened ribbon, in its sheath. The wound was completely closed in the usual manner, and primary union anticipated; but, owing to an attack of croup which supervened the night of the operation, was only obtained through about half of the wound. Since the operation I have been unable to obtain any details of the after-condition of the case beyond that given above, except that a perfect cure was obtained without any untoward accident.

CASE III. *Ovariotomy.*—May 30th, Dr. T. A. Emmet operated upon an ovarian tumor which was extensively adherent to the omentum and intestines. While separating the adhesions, a small artery of the omentum was lacerated and bled pretty freely. Being present at the operation, Dr. Emmet requested me to apply the constrictor to the vessel, in preference to a ligature, which I did. Notwithstanding the delicacy of the coats of the artery, the current of blood was completely arrested, and the invagination of the internal coats was visible to the extent of half an inch, while the delicate external coat remained uninjured. The portion of the omentum containing the artery I held for some time, in case of a return of the bleeding, to afford Dr. Emmet an opportunity to ligate it; but, no bleeding occurring, it was deemed safe by Dr. Emmet to return it to the abdominal cavity without further interference. Death occurred about sixty hours after the operation, the patient never having rallied from the shock due to the extensive pelvic and visceral adhesions. The autopsy was made about twelve hours after death. No blood was found in the abdominal cavity, nor could the vessel to which the constrictor had been applied be found, although carefully sought after.

CASE IV. *Amputation at the Hip-Joint.*—Besides the above cases, the instrument has been successfully applied to the following amputation at the hip-joint, October 8, 1870, to a boy who was run over by a street car, and taken to the Brooklyn City Hospital, where he suffered an amputation of the hip-joint at the hands of Dr. S. F. Speir, who, after making the anterior flap, applied the constrictor to the femoral and profunda artery, the effect of which was to arrest the hæmorrhage entirely. The vessels were left in this condition until the operation was completed, except the closing of the stump —all this time the arteries could be seen pulsating in the wound. As this was the first application to a human subject, and the case one which might involve litigation, the doctor was "induced to apply ligatures and close the wound as usual in such cases."—(*Medical Record*, April 1, 1871.)

CASE V. *Amputation of the Thigh.*—December 12, 1871, Dr. D. E. Kissam amputated at the thigh in a case of threatened gangrene following a compound comminuted fracture of the leg, in a man aged fifty-four. Dr. Speir, at the request of the operator, "constricted the femoral artery, which was the only vessel exposed, and the femoral vein, which bled more than usual. The result gave the same satisfaction as in the other cases." The wound was then closed in the usual manner. The condition of the patient being bad, and the reaction after the operation difficult, but little hope was entertained of his recovery, a condition of things which created considerable anxiety as to whether this might not prove too severe a test for the constricted artery. Finally, gangrene set in in the stump, notwithstanding which, and the open condition of the wound which followed such a condition, there was no hæmorrhage. The patient died three days and sixteen hours after the amputation. The artery was removed from the stump, and was found perfectly closed by the invaginated internal and middle coats, and a firm clot, as represented in Fig. 7.—(*Medical Record*, April 1, 1871.)

For the notes of the following cases I am indebted to Dr. Speir:

CASE VI. *Popliteal Aneurism.*—N. S., aged twenty-five, was admitted to the Brooklyn City Hospital, April, 1871, with a popliteal aneurism of the right limb, the result of an injury. Considerable œdema of the leg and foot existed; the leg was bent at a right angle to the thigh.

April 22d.—Treatment at present, opiates to relieve pain. Urine drawn off with the catheter. Knee swollen, tender, and very decided pulsation posteriorly and laterally, controlled by pressure on the femoral artery.

April 26th.—It having been decided, after consultation, to apply the "artery constrictor" to the femoral artery in Scarpa's space, Dr. Speir cut down upon the vessel, and, after separating the artery, constricted it. All pulsation in the tumor ceased at once; after the removal of the instrument the vessel was found perfectly occluded. The wound was closed with silver sutures and adhesive plaster, and an intermitting digital pressure kept up for a time, with the view to produce a large and firm coagulum in the vessel.

April 27th.—A large part of the wound appeared to be united by the first intention. One point, however, appears to be irritated by the adhesive straps where they were irregularly applied.

April 29th.—Some suppuration from the point of the wound above indicated, one of the sutures removed to-day. There has been no pulsation in the tumor since the operation. The leg has decreased in size; still keeps at right angles to the thigh. Temperature good. He has but little pain in the limb.

May 3d.—The point from which there was suppuration now discharges but little; the rest of the wound has united by primary adhesion.

May 12th.—The wound is entirely healed. The patient can straighten the limb, and sit up in bed. The swelling has disappeared from the limb, with the exception of a small, hard tumor in the popliteal space. There is no pain, and the case is considered cured.

CASE VII. *Amputation of the Breast.*—On the 20th of November last Dr. D. E. Kissam amputated the breast for cancer, in a patient thirty-eight years of age. But one vessel of sufficient size to require attention was exposed; to this Dr. Speir (by invitation) applied the constrictor. The instrument which he had at hand being rather large, he included a portion of the pectoral muscle. The bleeding was at once arrested, the wound was closed in the usual manner, and united *by first intention*, the patient being about her room after a few days.

The following cases were reported before the New York Library and Journal Association by Dr. Speir, November 17th:

CASE VIII. *Amputation at the Ankle-joint by Dr. Kissam.*—In this case there was secondary bleeding, which could not fairly be attributed to the use of the constrictor, for, on searching the wound for the bleeding-point, the internal plantar artery was found perfectly closed. The anterior tibial artery (which seemed at first to be the source of the secondary bleeding, and which had received severe bruising after its constriction) being tied above the point of constriction. The bleeding still continued, showing that the secondary hæmorrhage was in great part from the vessels which had not been constricted, and which had not bled at the time the wound was closed after the operation.

Case IX. *Amputation of the Forearm.*—In this case Dr. Speir applied the constrictor on one side and the ligature on the other, and obtained a primary union on the side where the constrictor was used, and a prolonged union by granulations on the side of the ligature.

Case X. *Amputation of the Leg.*—August 19th, Dr. Kissam amputated a leg for a gangrenous condition following a severe compound fracture. Dr. Speir closed the two tibial arteries with the constrictor. The gangrenous condition extending after the operation, the patient died, on the sixth day after the amputation, of pyæmia. No accident occurred from the vessels, notwithstanding the unfavorable condition of the stump.

Case XI. *Popliteal Aneurism.*—August 26th, Dr. D. E. Kissam operated upon a popliteal aneurism at the Brooklyn City Hospital. He had first tried compression and flexion, without avail. The application of the constrictor to the femoral artery was followed by immediate relief from pain, and a perfect cure of the aneurism. In this case there was no union by first intention. The patient was a mulatto, fifty years of age, of a cachectic and broken-down appearance. The 5th of December he left the hospital cured.

In all of the cases contained in this paper it will be noticed that but two occurred which at the time of the operation presented any thing like favorable conditions for the application of any method which would be likely to control the vessels permanently through their progress to recovery or death. In the first case cited, the vessels were in an extreme state of disease, and one of such a character as to excite very grave fears on the part of any surgeon for the recovery of his patient. It also furnishes a good example of the relative merits of the ligature and the artery constrictor, and demonstrating the superiority of the latter method over the former, *as secondary hæmorrhage occurred from the vessel to which the ligature had been applied, while the constricted vessels remained closed.* Case II., I had hoped, would have proved another advantage, viz., by the wound closing entirely by primary adhesion. This was partially obtained, and I have no doubt would have been perfect, had not the attack of croup, which was developed the night of the operation, prevented. The cure of the case, however, was accomplished. Case III. demonstrated how groundless is one of the objections urged against this instrument by some of the older surgeons, viz., that "the force employed would divide the external coat of an artery, or at least injure it so as to cause it to slough;" in this case the artery belonged to the most delicate class, and, notwithstanding it was drawn into the sheath of the instrument to the extent of between a quarter and half an inch, with sufficient force to divide and invaginate the internal coats, the external remained uninjured. Its position, also, was one favorable to any consecutive bleeding, being in loose floating tissues, and unsupported by the pressure of any dressing. How the above objection could be urged against the use of the constrictor by practical surgeons, I can hardly understand, for a moment's reflection on the anatomical construction of arteries should preclude such an objection, as every professional man of any surgical experience can hardly fail to know that the external coat, composed as it is of connective tissue and elastic fibres, is extremely tough and resistant. As to the force used in the application of the instrument, it is far less in reality than that exerted by the ligature, and which is, I may say, always much more of a cutting instrument, from the fact of the ligature being much smaller in circumference than the beak or hook of the constrictor. The ligature, again, is frequently applied with such force as to break, even though generally composed of good, firm silk, while the cutting through of a vessel by it is far from common. The notes of the cases furnished me by Dr. Speir all speak favorably for the method. In one case complete union by first intention was obtained; in two others a partial primary union. One of these, I have very little doubt, would have united by first intention, had a ligature not been present in the wound. The valuable practical points which this method presents over the ligature, and which, I think, is to some extent demonstrated by the cases contained in this paper, are:

1. *Its greater certainty of permanently controlling arteries in a healthy condition, as well as in extreme states of disease.*

2. *The wound being free from any absorbing or retaining agent like the ligature in which discharges can decompose, there is less liability to septæmic poisoning.*

3. *The wound being entirely free from any foreign body, there will be a greater probability of obtaining primary union, and which we can materially assist by hermetic closure of even as large wounds as in amputation of the thigh, by covering the entire stump with collodion.*

4. *Far less liability to secondary hæmorrhage when applied to a vessel in its course. Should the invagination of the coats or clots give way, the current of blood will simply flow on in the old channel, and not exsanguinate the patient as in secondary hæmorrhage after the ligature.*

II.—*Ovarian Dropsy, with Invagination and Sloughing of Six and one-half Inches of Small Intestine.* By William McCollom, M. D., Brooklyn, N. Y.

Mrs. H. B. R., mother of four children, the youngest four years old, in delicate health, attended church three miles from home; rode home in a carriage after the service, feeling as well as usual, and took a full meal with a keen relish at 4 p. m. Soon after dinner she felt a severe pain in epigastrium, followed by vomiting, and the stomach was relieved of what she had taken. Retching and vomiting continued as often as she took drink, until I visited her, at 10 o'clock in the evening, October 27th. I found her suffering severely from pain referred to the epigastric region, with very marked distention of the small intestines, and elicited from her the following history of an abdominal enlargement I noticed, in attempting to discover the cause of obstruction: she remarked that she had suffered for two years from a fibrous tumor of the uterus, as diagnosticated by two physicians who had examined her a number of times, and that the growth had increased very considerably during the past year. I found, on examination per vaginum, the uterus low in the pelvis, and not receding from pressure against the os. The well-defined ovarian cyst extended two inches above the umbilicus, and felt not unlike a large fibroma. Very marked tenderness was noticed immediately above the cyst, but none in other regions of the abdomen.

I ordered copious warm-water enemas, and administered one-fourth grain acetate of morphia, which was immediately rejected; but after repeating the dose a number of times the retching was less frequent, the pain less severe, and she slept toward morning. During the 28th, 29th, and 30th, she required the frequent repetition of morphine to allay nausea and to relieve pain. The injections passed, with very little fecal matter.

On the 2d of November, six days after the invagination, she took hyd. cum creta, with pulv. rhei, and had a copious evacuation, and for two days seemed better, was dressed, and left her room. The extreme distention of the intestines, which had existed from the first, passed away on free action of the bowels. From the 4th to the 9th her condition remained much the same, vomiting occasionally, complaining of distress, which was referred to the same locality, and daily growing weaker. On the 9th the bowels acted freely, and the intussuscepted portion of intestine, six and one-half inches in length, and very offensive from advanced decomposition, passed away.

From the 9th to the evening of the 11th she suffered from occasional nausea and vomiting, and had frequent copious, dark-colored, very offensive, watery discharges from the bowels. Found her on the morning of the 12th more comfortable; diarrhœa checked; some appetite. At this time the tumor had entirely disappeared, the abdomen was flat, the uterus higher in pelvis, and receded from pressure against os.

On the 13th and 14th, appetite good, bowels soluble. Found her on the morning of the 15th dressed and walking about the house. She suffered from nausea at times; from the 15th to the 19th there had been diarrhœa for two days, which was checked when I next visited her, on the 21st. At this time the hypogastrium was full and tympanitic; from the 21st, through the month, she vomited occasionally, and suffered from abdominal pain. Subsequent to this time, until the time of her death, twenty-four days later, her condition was variable; during the most of this period she was constipated, and rejected, by vomiting, a large part of the nourishment taken, though she took food with considerable relish. She continued to lose strength and flesh, and died on the 24th of December, fifty-seven days after the invagination of the intestine.

Autopsy twenty-four hours after death. The contents of the abdomen examined.

The intestinal and other viscera were found extensively agglutinated, evidently by both old and recent adhesions; considerable puriform fluid found in peritoneal cavity. There was a collapsed ovarian cyst of right side, which contained three drachms of hair, and which had become adherent to the small intestine near the ilio-cæcal junction, and communication had been established between the cyst and intestine, through which the fluid contents of the cyst had drained away, and been discharged from the bowels.

The uterus and left ovary were normal, and there appeared to be no structural lesions of any of the other viscera.

Proceedings of Societies.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, December 21, 1871.

DR. E. R. PEASLEE, President, in the chair.

AFTER the reception of several communications, Dr. Elisha Harris reported from the committee appointed to examine into the merits of the bovine virus furnished by Dr. V. L. Ferrer, of Havana, Cuba, and preserved by his peculiar method. Dr. Janes and Dr. F. P. Foster followed with detailed accounts of their experiments with it; and the matter was left *sub judice* pending the results of further trial. The Academy then resumed the discussion of

YELLOW FEVER.

DR. CHARLES P. RUSSELL remarked:

There are some interesting points in connection with yellow fever as it prevailed last year upon Governor's Island and in this city. First, as to its origin. Although the committee appointed by the Board of Health to investigate its cause, were inclined to ascribe it to infected articles thrown overboard from vessels lying at the Brooklyn docks, opposite Governor's Island, yet that explanation would hardly seem adequate. Such articles would, in most stages of the tide, more naturally float with the powerful current in Buttermilk Channel than directly across it, and would consequently lodge, if anywhere in the vicinity, along the Brooklyn wharves or the Gowanus shore. The stringent military *surveillance* maintained over the island, day and night, would, moreover, have prevented infected materials from being smuggled across. The history of Rotten Row is suggestive of some pernicious local influence. If we accept Dr. Nott's assurance that the poison cannot be wafted by the wind, and if we admit the non-contagiousness of yellow fever, we are strongly driven toward one of two conclusions: either that the disease originated *de novo* within the tenements alluded to, or that it was due to the awakening of dormant germs deposited there in former years. Practically, the two hypotheses amount to the same thing, and point to the destruction, by radical measures, of that probable nest of still slumbering infection. The recommendation of the Board of Health to this end, although partially founded upon reports by army-surgeons, was not met in the proper spirit by General McDowell, and Rotten Row yet stands.

Secondly, as to the diagnosis of the disease. Although six fatal cases were demonstrated to have occurred in the city previous to October 1, 1870, yet in no one instance was the cause of death detected by the attending physician. Two were returned as "remittent fever," one as "typhus," one as "hepatitis," one as "coma," and one as "paralysis." This list is quite similar to one given in 1822 by Dr. Cuming, then city inspector, which stated that three deaths by yellow fever had been reported as simply "fever," three as "bilious fever," two as "remittent fever," one as "inflammatory fever," one as "liver-complaint," one as "inflammation of the brain," one as "gastritis," one as "inflammatory bilious fever," and one as "insanity." Last year the very competent surgeon at Governor's Island was equally at a loss; and, although the disease had broken out on August 13th, its proper character was first discovered by Dr. Nott six weeks subsequently. During that period it had occasioned twelve deaths upon the island. Such facts are very significant in teaching us how essential is a large experience to the recognition of this insidious disease, which assumes forms so similar to more familiar affections.

Altogether, in 1870, there were brought to light twelve cases of the fever in this city, of which nine proved fatal. Taking into consideration the circumstances of its unsuspected existence upon Governor's Island for six weeks, and the unrestrained intercourse for that period between the city and the infected locality, which was visited by hundreds of the patients' friends, I think we cannot escape the conclusion that many more cases must have occurred among such visitors, and that a number of them probably died without their disease having been recognized. I believe, also, that such facts justify the assumption that undetected cases, particularly among sailors and 'longshoremen, are much more frequent in the city than is generally supposed. Two sailors have recently died here of yellow fever—one at Bellevue, and the other at the Fever Hospital. In each the diagnosis was somewhat equivocal, but was considered to have been confirmed by autopsy, which revealed, in both cases, coffee-colored fluid in the stomach and intestines, fatty liver, and acute desquamative nephritis.

Dr. Harris said that cases like the last two referred to occurred here almost every year, though they were often not regarded as cases of genuine yellow fever, because appearing out of season. The Bellevue patient was a man who had, about three weeks ago, passed quarantine without question on the ship Cleopatra. He had speedily sickened and been sent by the surgeon of the Marine Hospital to Bellevue, where his disease was recognized from the black vomit and other characteristic phenomena. The quarantine officers were naturally disinclined to admit that the case was one of yellow fever; but since the Cleopatra had again come into port, after another voyage, the history obtained had placed the matter beyond all doubt. It turned out that she had had other cases of yellow fever on board during the voyage, and that this man had been sick nearly the whole time. He was, however, with much difficulty, mustered on deck with the other passengers, and made to present a fair appearance during the visit of the inspecting-officer.

The speaker could say, of his own knowledge, that in 1856 no less than fifty-one cases of this disease, nearly all fatal, were brought to New York, the most of them coming into the Quarantine Hospital under his care.

Dr. J. C. Peters thought it by no means settled that the yellow-fever poison could not be carried, for a certain distance at least, by the wind; and he cited again the case of Mr. Chandler White, of Bay Ridge, to which he had referred at the last meeting. He knew that material thrown overboard might readily be washed upon the shore of Governor's Island, as he had often seen in 1856. Then the rigid sanitary regulations concerning it were not established until after the outbreak of the fever. He could not tell whether this was so last year, so as to justify the Health Board committee's report of the cause of the epidemic; but he was firmly convinced that the disease was always imported here; that it had thus been brought to Governor's Island within a year or two, and had not lain dormant there for a longer period.

Dr. Harris stated that for ten or eleven days previous to Mr. Chandler White's sickness, in 1856, the ship Jane H. Gliddon, from Havana—perhaps the most noteworthy yellow-fever vessel he had ever encountered—had been discharging her cargo, consisting in part of rags, within five hundred yards of Mr. White's residence. She had dragged her anchor, and thus come so near the shore. During this time of discharging cargo, she had developed thirteen cases of the fever. Dr. Harris exhibited a map prepared at that time to show the relative positions of the infected ships and the districts invaded.

Dr. W. C. Roberts, in an elaborate essay, discussed the question of the contagiousness of yellow fever, inclining to agree with Rush, Aitken, and others whom he quoted, that the fever-germ was reproduced in the body, and might be personally communicated, at least in epidemic seasons. He was chary, however, about expressing a positive opinion.

Dr. Nott was called upon to give his experience in the practical management of the disease. He said that the clinical history of the affection was well defined; and, though different cases varied much, the following might be taken as an example of its typical course: The subject, apparently in perfect health, is attacked suddenly, oftenest perhaps at night, awaking with a chill, severe headache, violent pains in back, limbs, and other parts of the body. In the course of fifteen to thirty minutes reaction comes on, and fever sets in—not very high, the pulse ranging from 90 to 110. This pyrexia may last from thirty-six to seventy-two hours, its usual duration, in the disease as it appears at Mobile and New Orleans, being forty to forty-eight hours. Then succeeds the stage of calm. The feverishness is gone, the pulse normal, the skin naturally moist, the tongue healthy in appearance. This continues about forty-eight hours longer, bringing the case to the end of the fourth day. Now occurs the stage of collapse, attended with black vomit, and ending in death, which begins at the heart.

As to treatment, there was no specific. Bleeding was out of date. Mercury was useless, and so were purgatives. Quinine was of value only to relieve the muscular pains, which made so large a part of the patient's suffering. The two great indications were, first, to disturb the stomach by drugs as little as possible; second, to preserve the patient's strength against the stage of collapse, which must come. He must be nursed through, not forced through. Careful nursing in the earlier stages, judicious support in the latter, were about the whole treatment. In most cases alcoholic stimulants were required by the third or fourth day, the special form being a matter of taste. Iced mint-juleps were commonly grateful; but sometimes champagne had the best effect. If the stomach was too irritable to take stimulus and nutriment, these must be administered by the rectum. It was very important to keep the patient in the recumbent position, and to avoid every source of exhaustion. If, after he had passed the fourth day, you could only ward off death for forty-eight hours longer, he was pretty sure to rally, and then to enter upon a rapid convalescence. It was remarkable that a man could come through so terrible a disease with so little sign of wear and tear. Often you might find him heartily enjoying a beefsteak or a mutton-chop eight or ten days after he had been lying at the point of death. Herein was a marked distinction between this fever and intermittent or remittent.

The older writers on yellow fever spoke of taking with advantage even one hundred or one hundred and fifty ounces of blood. At the present day, certainly, such treatment could not be borne. In his earliest practice the speaker had bled two yellow-fever patients, and one fainted at ten ounces, the other at eight. This had taught him a lesson; and since then he had seen not more than one case in forty or fifty that would justify the lancet. It might be used when brain-symptoms were manifested; but it was a characteristic of the fever that the mind was generally clear throughout. Yet the men who practised those heavy bleedings had as good minds, as good powers of observation, as ourselves. It was impossible they should have been wholly deceived about the effect of the remedy; and the only reasonable supposition was, that the type of the disease had changed.

Dr. Walser remarked upon the relations of yellow fever to quarantine, speaking from a long experience. Owing to the lateness of the hour, he was unable to state his views in full, and was requested by the President to read a paper upon the subject at a future meeting. He desires us, therefore, to give no report at present.

The meeting adjourned.

Bibliographical and Literary Notes.

Art. I.—*The Science and Practice of Surgery. Illustrated by Four Hundred and Seventy Wood Engravings.* By Frederick James Gant, F. R. C. S., Surgeon to the Royal Free Hospital; formerly Surgeon to Her Majesty's Military Hospitals, Crimea and Scutari. Philadelphia: Lindsay & Blakiston, 1871, 8vo, pp. 1,265.

We take up this bulky but elegant book with some trepidation, both on account of its size and the lofty claims of its author, and the vast range of subjects presented to our consideration.

A carefully-considered, well-studied introduction, full of correct observation and sound sense, agreeably and instructively opens the way for the discussion of the special departments of surgery which follow in due order.

The work is ambitious in design, and aims at an exhaustive statement of the science and art of surgery. Inflammation is treated *in extenso*, and the various theories, especially with reference to its essential pathology and treatment, fairly set forth, examined, and balanced. The author cautiously refrains from giving a decided opinion of his own. He is timid in respect to authorities, and hesitates before accepting the modern and philosophic views in respect to the treatment of Bennat, Anstie, Forbes, and others, to say nothing of American surgeons. There is a truth, however, to explain this peculiar proscriptive conservatism in English and Continental writers, in the fact that they see far less intellectual activity and nervous irritability than we do, and their patients bear depletion by bloodletting, purgatives, and low diet, better than ours. Whether it does them any good or not we are not prepared to say, but we incline to the negative. Mr. Gant makes little account of the arterial sedatives in the therapeutical treatment of inflammation, and gives altogether too much weight to the mechanical function and action of the heart. The heart and arteries beat synchronously in obedience to the same law, responsive to the same power. As Simon correctly observes, more blood goes to the inflamed part because it is inflamed, not because the heart beats harder; soothe the local distress, and quiet the irritated sympathetic, and the heart responds by more gentle and moderate movement. No notice is taken of the theories of Cohnheim and others, in reference to the amœboid movement of the white corpuscles, which certainly are worthy of consideration and discussion, although their pathological significance may not be regarded as settled. The degrees and kinds of inflammation, the modes of repair, and the forms and consequences of destructive action, are well arranged and carefully considered, evidently from light obtained by extensive observation and experience. The conditions and methods of local treatment are carefully explained and satisfactory. The chapter, however, cannot be compared with Mr. Simon's admirable essay in the second edition of Holmes's Surgery. We see nothing to object to in the section on Tumors and Morbid Growths. The classification is in our view unexceptionable, and the pathology and practice in harmony with the views of the best English pathologists, of whom Mr. Paget is the representative mind.

We remark, in passing, as an omission in the article on Hospital Gangrene, which is otherwise well considered, no mention of Goldsmith's method of treatment by pure bromine. This method was the precursor in the order of ideas of the antiseptic treatment of wounds, which is compelling its own adoption by the force of good surgical authority, and its intrinsic excellence.

Passing over the interesting and instructive chapters on Ulceration and Ulcers (in which, however, we remark the absence of any allusion to the important subject of skin-grafting), Gangrene, Scrofula, Scurvy, and Purpura, Gout and Rheumatism, we reach the section which treats of the "Diseases of Contagious Origin," beginning with syphilis. The learned author gives a *résumé* of English and foreign opinion, takes the ground that soft chancres, or, as we call them here, chancroids, may produce constitutional infection and secondary lesions. He takes no notice of the excellent work of our distinguished townsman Bumstead, who has certainly written as well on this subject as anybody else. A recent American writer, Ashhurst, of Philadelphia, assumes decidedly the opposite position to Mr. Gant, following or coinciding with Bumstead, in the duality of the two forms of at least similar diseases. All that a reviewer can say is, that neither party proves its case. The question is unsettled, and we do not see very clearly, after the infinite labor and pains taken by competent, conscientious, and scientific observers, how it is to be settled. The absolute untrustworthiness of the statements made by syphilitic patients, the fact that obviously or apparently obviously specific sores are not always followed by external evidences, constitutional poisoning, and that a chancroid is liable and likely to be inoculated with syphilitic virus before it comes under observation, complicate and invalidate experiments to such a degree that equally honest and competent diagnosticians arrive at diametrically contradictory conclusions. This point among others is important as affecting practice, especially with respect to prophylactic treatment. All observers agree that both forms of the contagion have a positive tendency to produce anæmia—chancroid (so called) in our experience—more rapidly and decidedly than the more slowly-developed form which is usually followed by definite constitutional symptoms of a depraved and degenerated blood, and a consequently impaired nutrition. The obvious conclusion is, that nutrition, hygiene, and *iron*, should be employed from the very outset of every suspicious case, and that the specific character of the local sore should be changed by a potent escharotic as soon as possible. The English writers do not urge and insist upon *iron;* and American writers, while recognizing its value, do not give it the importance which in our judgment it deserves. We thank our author for his condemnation of the practice of syphilization, which, while we have the highest respect for one of its apostles, the gentle and accomplished Norwegian who lately honored us by his presence, we consider barbarous and unjustifiable in the extreme.

The general principles of "Gunshot-wounds" are stated clearly, succinctly, and satisfactorily. Credit is given to American surgery for an excellent bullet-forceps. In this connection, we take the opportunity to record the fact that the idea embodied in Nélaton's diagnostic bullet-probe was anticipated by Dr. Highaway, of Cincinnati, who used the stem of a clay tobacco-pipe to distinguish between bone and lead during the Mexican War.

"Aneurism" is well treated, according to the standard of authority and experience. The methods by galvano-caustic puncture and by injection are interesting because applicable to a class of otherwise necessarily hopeless cases. The statistics of galvano-puncture are brought down to 1866 only, since which time many new experiments and some successful cases have been reported. Both these methods are justifiable in the hope of giving relief, and from the fact that they do not obviously hasten death when the ultimate result is unfavorable.

We refer our readers to Dr. Keyes's interesting and candid report of a case treated in Charity Hospital by galvano-puncture. Dislocations and fractures give nothing which is not as well and better given in other works. We are refreshed, however, here and elsewhere, by the sight of time-honored engravings which recall the days of our youth, and awaken pathetic as well as harrowing recollections. Liston's *board* and the abominable perineal barrel are reproduced for the misery of future sufferers with fractured thigh, and Nélaton's pistol is again presented with dire intent. Happily the illustration is omitted, probably because the block was lost.

There is a new American splint for fractures of the forearm, especially adapted to those near the wrist-joint, which we shall, when we describe it, call the American revolver in contradistinction. If excoriation from the perineal barrel occurs, as it always does, our author advises the suspension of a weight from the *end* of the *splint.* It is customary in New York to apply extension to the leg of the patient. The plan recommended might perhaps be well in case of the fracture of a wooden leg.

In chapter xxxvii., p. 606, on Excisional Surgery of the Joints and Bones, we find our author at home upon his favorite ground. It is a study of the subject from the points of view of thorough research and extensive practical experience, stimulated by enthusiasm and sincere humanity. It leaves little to be added or desired from the knowledge at present in our profession, and it seems to us that his appreciation of the value of the excisions with reference to life, and secondarily the preservation of useful limbs, is accurate, scientific, and methodical. We are not prepared to go quite so far as he does in reference to excision of the knee-joint, but we hold ourselves open to conviction. In well-selected cases it is no doubt an admirable resource against otherwise unavoidable mutilation. Some surgeons regard mutilation with horror, and never resort to it excepting in the performance of an imperative duty.

Due weight and value are given to excisions of the hip-joint, which are a means of cure in intractable disease, and in most cases of traumatic origin a substitute for amputation at the coxo-femoral articulation. If not successful, they promote, with the aid of morphine, anæsthesia.

We are glad to observe in a previous chapter upon Diseases of Joints, that credit is given to our skilful townsman, Dr. Henry G. Davis, for the principle of elastic extension in diseases of the joints of the lower extremities. This idea originated, and was practically demonstrated, by Dr. Davis. The hip-joint splint, generally called Sayre's, was adopted from Davis, improved by Prof. Erskine Mason, then an *interne* at Bellevue, and *popularized* by Prof. Sayre.

The section on Amputation is satisfactory. The appreciation of Symes's and Pirogoff's amputations, about which there has been much dispute and some very absurd manifestations of unprofessional irritability, is correct. Pirogoff's operation is adapted to traumatic cases in young and healthy subjects, Symes's to pathological cases especially. The flap will not often slough, if due regard is had to support and position, and the maintenance of reparative power by nutrition.

Hernia and the operations required are exhaustively treated.

We consider it an important omission that the ether-spray is not recommended as an adjuvant to taxis, and as the only proper method of applying cold. If a perineal tumor does not yield to gentle manipulation after an anæsthetic, and the application of the spray, the gut should be considered strangulated and immediately liberated by the knife. More people have died as a consequence of the abuse of taxis and cold than from the effect of operation. Ether-spray is particularly valuable as affording a safe and rapid method of emptying engorged vessels and controlling muscular fibre or demonstrating the necessities for a relieving incision.

Lithotomy and lithotrity are fairly contrasted, and their respective advantages candidly set forth. We owe a debt to the English surgeons for giving the latter proceeding its true importance, which is one of the distinguishing honors of modern surgery.

In discussing retention when relief is required by an artificial opening, no mention is made of the easy, safe, and surgical method advised by Dr. Dieulafoy, of Paris, and also recommended by the venerable Dr. Nott, of this city, by means of the capillary trocar and suction apparatus. Mr. Gant, likewise, holds on to the exploded idea of retaining a catheter in the urethra after rupture of the urethra on incision for the relief of retention. We would advise him to remark, in his next case of tied-in catheter, how long it takes for the urine to make its way between the outside of the foreign body and the internal surface of the urethra. We know of but one case in which it is worth while to tie in a catheter under these circumstances, and that is after perineal bruise, when we anticipate sloughing of the urethra and extravasation, and are afraid, if we take it out, we cannot get it back again. No reference is made to the labors of New York surgeons in the department of urethrotomy. It is not too much to say that this class of operations is as thoroughly appreciated and probably better performed here than anywhere else.

In the section upon the surgical treatment of Lesions of the Organs of the Female Pelvis, no mention is made of Dr. Emmett, who has done more than any one else to make the methods pursued and the operations practised intelligible, scientific, and surgically exact. No work professing to treat of this branch of surgery can afford to ignore his admirable and practical treatise in which the results speak for themselves and the author.

Mr. Gant's book, as a whole, is methodical, conscientious, learned, and painstaking. It is thoroughly English in tone, and somewhat hyperconservative and deferential to authority. It is an excellent compilation of received opinions, and a correct guide to established modes of practice. It is a better volume for a surgeon than for a student's text-book. It is a good guide to the study of surgery, and abounds in valuable facts and statistics. The style is generally clear and elegant, although we encounter occasionally a puzzling ambiguity of expression. We advise surgeons who can afford it to buy the book.

Art. II.—*The Principles and Practice of Surgery.* By John Ashurst, Jr., M. D., Surgeon to the Episcopal Hospital, Surgeon to the Children's Hospital, etc. Illustrated with Five Hundred and Thirty-three Engravings on Wood. Philadelphia: Henry C. Lea, 1871.

The general arrangement of this work is very much the same as we are accustomed to see in the various text-books on surgery. It is divided into three parts. The first part treats of inflammations, anæsthetics, the minor operations, and amputations; the second part treats of surgical injuries; while the third portion is devoted to the consideration of surgical diseases. We are told in the preface that the object of the work is to furnish, in as concise a manner as may be compatible with clearness, a condensed but comprehensive description of the modes of practice now generally employed in the treatment of surgical affections, together with a plain exposition of the principles upon which this practice is based. The treatment which is recommended in almost all instances is such, the author tells us, as has proved satisfactory in his own hands in a not very limited hospital experience.

In this treatise, which we must regard as chiefly a compilation, with here and there some little originality, we have a very good digest of the works of our modern surgical writers, including the publications from the surgeon-general's office.

In the article on the pathology of inflammation, especially under the head of the formative changes, we are glad to see so good a condensation of the modern views upon this subject. Indeed, we know of no surgical text-book in which they are so clearly set forth. The remarks upon the clinical view of inflammation, and the chapter upon the treatment of inflammation, are excellent. While speaking of the preliminary arrangements for an operation we are told that the best hour for operating, in this region of country, is from 11 a. m. to noon. We must confess that we do not see why this period is better than one a few hours earlier or later. The directions for the administration of anæsthetics, and the decided preference expressed for ether in surgical cases over chloroform, or a mixture of ether and chloroform, meet our entire approval.

In the article upon minor surgery, regarding the subject of immovable bandages, we are told that the starch-bandage requires two rollers, the inner one of which is saturated with thick starch. Are we to believe that *only two* rollers are required to give us a firm and immovable apparatus? And in describing the plaster-bandage, the student, or those having no experience with this mode of dressing, would be led to suppose that one roller was all that was required. The description of this dressing is not sufficiently minute.

The subject of wounds, especially gunshot-injuries, is very well treated, and gives us a good *résumé* of these accidents and their results, as observed in our late war.

In the treatment of arterial hæmorrhage, which is well considered, we find but little to criticise. The first rule given for ligating wounded arteries might be somewhat modified. In cases of primary hæmorrhage, no operation should be performed upon an artery, we are told, unless it is at the moment actually bleeding. If a wounded artery is seen, we should consider it much safer to tie it at once, whether it be bleeding or not, than to trust to Nature to prevent a recurrence of bleeding, especially if the vessel were of any size.

More space should have been given to the subject of injuries of the nerves, which is a matter entirely too important to be touched upon so superficially. We regret that our author did not avail himself more fully of the researches of Drs. Mitchell, Morehouse, and Keen, in this connection. Chapter XI., upon the general consideration of the subject of fractures, is very clearly written, and a most excellent compend. With the chapter on special fractures we are not so well satisfied. It is very defective in some points. Various apparatuses are spoken of, but no description is given of them, nor are we told how to apply them. This is a very important matter to the student. In speaking of fractures of the leg, the author says he knows of no apparatus which presents so many advantages as the old-fashioned fracture-box. Is it possible that he prefers to keep his patient in bed with his limb in a fracture-box for several weeks, when the immovable apparatus of the plaster-bandage will permit of his going about, and secure just as good a result? We do not find these immovable dressings spoken of at all in the chapter on special fractures.

The article on dislocations is a decided improvement upon that of fractures, and in every way much more satisfactory. Though the author believes that in the action of the muscles we are to find one of the chief obstacles against the reduction of luxations, we are glad to find that, when speaking on the subject of dislocations of the hip, he embodies and advocates the views so clearly set forth by Prof. Bigelow in his excellent monograph upon this subject.

Injuries of the head and spine are treated of in a clear and comprehensive manner, and we are furnished with the views of the most recent investigators regarding the pathology of these affections.

In regard to trephining for depressed bone, we must protest against the advice given to refrain from operating in impacted fracture, even though compound and depressed, with symptoms of compression. Nor can we agree with the author in considering the operation of trephining for epilepsy as always unadvisable.

On the subject of wounds of the iris, we are told that if the iris protrude, an effort should be made to replace it by means of a fine probe; or, if this be impossible, the projecting portion should be snipped off with curved scissors. This is, to say the least of it, very injudicious advice in the majority of such cases. As for the use of the probe, the farther it is kept from the wounded iris the better. The chapters on the diseases of the eye and ear, however, are much better than those found in the ordinary text-books on surgery, and are in accordance with the views now entertained by the leading specialists on these subjects. The same remarks will apply to the chapters on venereal affections.

The description of tumors is for the most part in accordance with the views and classification of Mr. Paget, though reference is made to the group of sarcomata as described by Virchow and other German pathologists, and a brief account of each variety is given. Of epithelioma we are told that, when occurring on the fingers, lips, or tongue, it may occasionally be mistaken for a chancre in the same situation. This is very true, but its resemblance to the *chancroid*, in our experience, has been far more striking than that to the chancre; and we prefer to apply the test of inoculation in cases where there is any doubt, before resorting to antisyphilitic treatment as here advised.

Of the chapters on aneurism, and on diseases of the bones and joints, we cannot speak too favorably, and a better *résumé* of the literature on these affections will not be found in any book that a student is likely to possess. It would have been well, however, while dealing with the pathology of synovitis, if the author had entered a little more fully into this subject, and availed himself of the researches of Billroth and other German investigators in this field. And on page 572, when referring to the treatment of synovitis, we are surprised that no mention is made of the use of extension, which, in our experience, has proved of the greatest value.

With the chapter on orthopedic surgery we are not so well pleased. In reference to the treatment recommended for the majority of affections that are embraced under this head, we think Dr. Ashurst entirely too much inclined to tenotomy. Nor are we at all prepared to indorse the statement that tenotomy has not been proved to do any harm. We believe that, if the author would make a fair trial of those means that have been advocated by Mr. Barwell, especially in the treatment of club-foot, his views would be materially changed. The treatment of lateral curvature, as recommended, would have been decidedly improved had the views and apparatus of Mr. Barwell been given; we believe his treatment to be far more efficacious, in the management of these cases, than any heretofore suggested. With regard to the management of cases of antero-posterior curvature, especially in reference to the form of mechanical support those cases require, little or nothing is said. We cannot indorse the remark that, "in ordinary cases, the patient may be confined to the *horizontal position* on a suitable couch, the prone being more desirable than the supine posture." This certainly is not in accordance with the views of those who see or treat many cases of this disease at the present day. It certainly would have been far better for the student had some of the mechanical contrivances now in use been more fully described and illustrated.

The remaining subjects are particularly well treated, and especially those on the subject of hernia and the diseases of the genito-urinary organs.

Indeed, the work as a whole must be regarded as an excellent and concise exponent of modern surgery, and as such it will be found a valuable text-book for the student, and a useful book of reference for the general practitioner.

ART. III.—*The Journal of Anatomy and Physiology.* Conducted by G. M. HUMPHREY, M. D., F. R. S., and WILLIAM TURNER, M. B., Professor of Anatomy in the University of Edinburgh. Second series, No. ix. November, 1871. Cambridge & London: Macmillan & Co.

THE present number of this journal fully maintains its high standing among scientific periodicals. It contains seventeen original articles, the first of which is by Prof. Humphrey, on the Muscles and Nerves of the Cryptobranchus Japonicus. Dr. Bradley has an exceedingly interesting description of the brain of an idiot, which weighed only twenty-eight ounces. Prof. Struthers contributes an article on the Anatomy of the Great Fin-whale; Dr. Murie one on that of the Malayan Tapir; and Mr. Champneys one on the Structure of the Chimpanzee and Anubis. Dr. Blake furnishes the Results of his Experiments on the Injection of Salts of Platinum and Palladium into the Blood. Several other elaborate scientific papers, and a report on Physiology, complete the November series.

ART. IV.—*Photographic Review of Medicine and Surgery: a Bi-monthly Illustration of Interesting Cases, accompanied by Notes.* Edited by F. F. MAURY, M. D., and L. A. DUHRING, M. D. October and December, 1871. Nos. 1 and 2, vol. ii. Philadelphia: J. B. Lippincott & Co.

WE notice with pleasure the excellence of the photographs in these two numbers of the *Review.* The subjects too, are all of great interest, and well worthy of the skill dis-displayed in their illustration. The October number contains photographs and notes as follows: Pseudo-hypertrophic Muscular Paralysis, S. Weir Mitchell, M. D.; Deformity of the Leg, S. D. Gross, M. D.; Epithelioma of the Face, T. D. Davis, M. D.; Aneurism of the Aorta, J. B. Howard Gittings, M. D. The December number contains the following: Condylomata, R. W. Taylor, M. D.; Fatty Tumor of the Neck, Elliot Richardson, M. D.; Venous Tumor, T. H. Andrews, M. D.; Extirpation of Thyroid Gland, F. F. Maury, M. D.

Announcements.—Messrs. J. B. Lippincott & Co. announce the following medical books: Atlee's Diagnosis of Ovarian Tumors, with special reference to the operation of Ovariotomy, by Washington L. Atlee, M. D.; and Injuries of Nerves, and their Consequences, by S. Weir Mitchell, M. D. Messrs. Lindsay & Blakiston announce: Gamgee on Fractures; Le Gros Clark's Outlines of Surgery, second edition; Harley on the Urine and its Derangements; a Hand-book for the Laboratory, by Profs. Sanderson and Foster; Brunton's Investigation into the Action of Medicines; and Black's Functional Diseases of the Renal, Urinary, and Reproductive Organs.

L'Union Médicale du Canada is the title of a monthly journal of medicine and surgery issued in Montreal, and of which we have received No. 1, vol. i., dated January, 1872. It is published in the French language, and is therefore especially adapted to the wants of the profession in Canada East. The editor is J. P. Rottot, M. D., and the assistant editors are A. Dagenais, M. D., and L. J. P. Desrosiers, M. D. The journal promises well, and we wish it success.

Books and Pamphlets Received.—Medical Thermometry, and Human Temperature. By C. A. Wunderlich, Professor of Clinic at the University of Leipsic, etc., etc., and Edward Seguin, M. D. New York: William Wood & Co., 1871.

Materialism in its Relations to the Causes, Conditions, and Treatment of Insanity. By H. B. Wilbur, M. D., Superintendent of the New York Asylum for Idiots. Reprinted from the JOURNAL OF PSYCHOLOGICAL MEDICINE, January, 1872, pp. 35.

Observations upon Medication, by the Use of Uterine Cloth-tents in Diseases of the Body and Cavity of the Uterus. By V. H. Taliaferro, M. D., Columbus, Ga. Atlanta, 1871, pp. 15.

On Vascular Nævi, and their Treatment by the Actual Cautery. By B. F. Dawson, M. D. Reprinted from the *American Journal of Obstetrics and Diseases of Women.* Pp. 20.

Alopecia. By Edward Wigglesworth, Jr., A. M., M. D. Read at the Annual Meeting of the Massachusetts Medical Society, June 6, 1871. Pp. 28.

An Address delivered September 19th, at the Annual Exhibition of the Farmers' Club, Princeton. By Nathan Allen, M. D. Lowell, 1871.

A Clinical Manual of Diseases of the Ear. By Lawrence Turnbull, M. D. Philadelphia: J. B. Lippincott & Co., 1872. 8vo, pp. 486.

Address by Thomas E. Vermilye, D. D., LL. D., at the opening of the Roosevelt Hospital, November 23, 1871. New York, 1871, pp. 27.

Announcement, Charter, and By-Laws, etc., of the Woman's Hospital of the State of Illinois. Chicago: Horton & Leonard, 1871.

Lessons on Population, suggested by Grecian and Roman History. By Nathan Allen, M. D., Lowell, Mass. Boston, 1871, pp. 16.

Transactions of the Medical Society of the State of West Virginia. Wheeling: Frew, Hagans & Hall, 1871. Pp. 312.

Sixteenth Annual Report of the Trustees of the State Lunatic Asylum, at Northampton, Mass. Boston, 1872, pp. 55.

Small-pox in Lowell. Reports of the Board of Health, and of the Consulting Physicians. Lowell, 1871, pp. 18.

Transactions of the Medical Society of the State of New Jersey. Newark: Jennings & Hardham, 1871.

Medical Supply-table of the United States Marine Hospitals. Washington, 1871, pp. 15.

Annual Report of the Home for Incurables, Westchester County. New York, 1871.

Proceedings of the Academy of Natural Sciences of Philadelphia. Part II., 1871.

Complete Report of the Board of Health, of Lowell, Mass. Pp. 18.

Anæsthetics. By Walter Coles, M. D. St. Louis, Mo., pp. 14.

Proceedings of the Royal Society, vol. xix., Nos. 123–129.

Reports on the Progress of Medicine.

MATERIA MEDICA.

1.—*Nitrite of Amyl; its Physiological Action and Medicinal Uses.* By TALFOURD JONES, M. B., Lond., University Medical Scholar. [Practitioner.]

My attention was first drawn to this new remedy by reading in the *Medical Times and Gazette* for September 3, 1870, an editorial paper on "The Progress of Therapeutical Science." It gave the results of Dr. Richardson's experiments with nitrite of amyl, and described a case of Dr. Brunton's, in which the remedy had been most successfully employed in angina pectoris; also one of Dr. Anstie's, in which it had given most marked relief to an asthmatic gentleman who suffered from angina. Two or three other instances of its therapeutical value were mentioned. Happening at the time to have under my care a most distressing case of angina in an elderly lady, I sent for a couple of ounces of the nitrite; but, unfortunately, the day before the supply arrived my patient died, and I had no opportumity of trying it upon her, and of affording her that relief from pain which my subsequent use of the remedy tells me she would most certainly have had. Shortly after, October 23d, I had an opportunity of trying it in a severe case of spasmodic asthma; and the immediate and almost magical relief it afforded convinced me that in nitrite of amyl we have a most powerful and valuable medicinal agent.

Nitrite of amyl, C_{10}, H_{11} O, NO_3, is an amber-colored fluid, smelling like the essence of ripe pears. The experiments of Dr. Richardson show that it causes paralysis of the chain of organic nerves which supplies the contractile power of the blood-vessels; that, applied to the olfactory filaments by inhalation, the impression is conveyed along the ganglionic nervous tract, and causes more or less paralysis of the vaso-motor nerves, and induces muscular and arterial relaxation.

Though it produces its characteristic effects directly upon the walls of small arteries. yet it is highly probable that it also acts upon most, if not upon all, unstriped muscular fibres.

I have now given it experimentally to some fifty friends and patients, and have found that its inhalation invariably causes increased frequency of cardiac pulsation, accompanied with flushing of the face, warmth of head, face, and neck, and perspiration—the warmth and perspiration often being general. The pulse is the first tell-tale of its effects: in from eight to twelve seconds its frequency rapidly increases. A pulse of twenty in the quarter-minute will often rise in ten or fifteen seconds to forty. The beating of the heart and of the carotids is in some persons very marked. The reddening of the face sets in later; it usually takes from thirty to forty seconds before there is much flushing. It sometimes causes a little breathlessness and coughing. Now and then it gives rise to giddiness; and, in some, to a feeling of intoxication—girls who have inhaled it have often complained that it has given them a headache.

It may be administered by inhalation, by the mouth, or by subcutaneous injection. The best and safest way is by inhalation. I usually pour five drops on a piece of lint the size of a crown-piece, or larger, and hold it close to the nostrils for ten or twenty seconds, or until I feel an acceleration of the pulse, or see the face beginning to redden; or the nitrite may be dropped on to a pocket-handkerchief, and then held to the nose just like chloroform; or, lastly, it may be inhaled directly from the bottle. I have never seen any bad effect result from its use. There is just one important point that should be borne in mind, and this has been insisted upon by Dr. Anstie—it is, that judgment and caution should be exercised in its employment in the aged or in persons the subjects of arterial degeneration. It must not be forgotten that, just as the cerebral congestion induced by stooping down may cause rupture of weakened or damaged cerebral arteries, so also may the sudden alteration in the cerebral circulation by amyl be followed by similar ill results. I will now successively describe every case in which I have employed it in disease:

I. Spasmodic Asthma.—At twelve o'clock on the night of October 23, 1870, a woman begged I would instantly go and see her daughter, who, she said, "was in a dying state." On entering her bedroom, I saw the patient, a young married woman, half undressed, sitting on the corner of the bed, and holding on to the bed-post. There was a dusky, leaden hue about her face, neck, chest, and hands, and a cold, damp sweat clung to her. Her body generally was cold, but her feet and legs were of an icy coldness. Her pulse could scarcely be felt. She was making violent efforts to breathe, and each inspiration was accompanied with marked recession of the supra-clavicular and the intercostal spaces. Loud sibilant *râles* with sonorous rhonchus could be heard over the greater part of the chest. She tried to speak, but could only make faint gasps. The thought instantly occurred to me that the nitrite of amyl which I had procured only a short while before might be of use. I ran back to my house, which was close by, and returned with the bottle. Five drops of amyl were applied, on a piece of lint, to her nostrils. In half a minute her face began to redden, and in less than a minute it was deeply flushed; her heart palpitated, her carotids throbbed, warmth of body quickly returned, and her breathing became easy. The effect was marvellous, and I felt nearly as much astonished as the patient and her mother. She now became able to converse, and told me that she had been subject to asthma for many years, that her father also was asthmatical, and that she had never before had such a severe attack as this. She accounted for it thus: In the early part of the day she was as well as usual; but that evening she remained out for some time in the wet, and returned home feeling damp, cold, and chilly.

In about ten minutes after the inhalation, the breathing became a little asthmatical, so we reapplied the amyl, and again she became perfectly easy, and went to bed. Next morning she told me that she had had a most comfortable night, the asthma had quite gone, and she was attending to her household duties.

About five months after this attack, March 26, 1871, I was again summoned to her. This time it was nothing more than an ordinarily severe bout of asthma, and wanting the signs of severe collapse that accompanied the first. A repetition of the amyl treatment was followed by results as speedy and effectual as in the first instance.

II. Spasmodic Asthma.—Mr. G——, aged forty-four, had been subject to occasional attacks of asthma for some years. On the 6th of last March he had a rather bad attack of it, and late in the evening he sent for me. He had caught cold, and when I saw him there was some bronchitis as well.

Inhalation of the nitrite of amyl instantly relieved his asthmatic breathing. In six or seven minutes after, it returned, though in a lessened degree. Again he inhaled the amyl, and again with relief; but for the next hour there was every now and then a tendency to the recurrence of the bronchial spasm, which, however, continued to be relieved at every inhalation. He went to bed, and had a good night. Next day the asthma was gone, and a little bronchial catarrh only remained.

III. Cardiac Dyspnœa.—Mrs. T. T—— suffered from dilated hypertrophy of both ventricles, the dilatation being in excess, accompanied by a very feeble circulation, and anasarca to an extreme degree extending upward to the abdomen, slight pulmonary œdema, some lividity of face, but not much shortness of breath.

Hydragogue cathartics and infusion of digitalis, acupuncturation, and vapor-baths, were used, with temporary relief. The digitalis, given in full and frequently-repeated doses at intervals of a week, had invariably induced a copious discharge of urine, but at last its diuretic effects seemed to fail, and one day last November I thought I would see whether cupping the renal region would be of service. It should here be stated that during the preceding week she had been inhaling the nitrite of amyl, five drops at a time, four or five times a day, with some relief to her breathing. On each occasion she felt easier, more comfortable about the chest, and experienced a pleasant glow all over her body. The comforting effects of the amyl usually lasted about an hour. I proceeded one evening to cup the loins. Two glasses were applied, one over each kidney. The blood flowed slowly, and when there was about a wineglassful in each glass it ceased to run. At that moment it occurred to me that it would be interesting to watch just then the effects of nitrite of amyl upon her. I immediately applied about ten drops on some lint to her nostrils. Soon the radial pulse throbbed, then the face became flushed, and *at the same instant blood flowed freely into the cupping-glasses.* The experiment was most striking, and clearly showed the influence of amyl on the circulation.

IV. Angina Pectoris.—Mr. P——, aged twenty-one. When seventeen years old he had a severe and prolonged attack of acute rheumatism, which caused the cardiac mischief from which he now suffers. Toward the end of a long convalescence he began to be subject to præcordial pains, which have ever since continued to trouble him. For the fortnight immediately preceding my first inteview with him, he had not had during any night an hour's sleep. His pains were terrible, cramp-like, and suffocative, chiefly situated about the left mammary and infra-mammary regions, but most intense near the left nipple, and radiating to the shoulder, the neck, and down the left arm to the elbow. He often felt that he must die. Latterly the pains had increased in frequency and severity. For months past he had not passed a night without a paroxysm, and rarely had he escaped during the day. An attack usually lasted from half an hour to an hour, but sometimes held on for two hours; it was generally made up of several paroxysms, with, perhaps, respites from pain of a minute or so in duration. He had consulted several doctors, and had tried all sorts of remedies, but never got any substantial relief.

I first saw him on the 26th of last January. He was then sitting in a chair, was easy, and free from pain—face very pale, pulse abrupt, rather tense, irregular in force, 90. Cardiac impulse heaving, extensive; apex-beat three inches below nipple level, and an inch and a half outside nipple-line. Systolic and diastolic basic murmurs. Systolic murmur left apex; systolic thrill at base. While I was examining his heart, he was seized with a fit. He seemed to suffer most agonizing pain about the præcordial region, which he pressed and squeezed with his left hand; he threw his head back, inspired deeply, stretched his neck, held his breath for about half a minute, and while doing so kept most of the muscles of his neck, shoulders, and chest, strained to the uttermost. He afterward stated that the posture described and the straining were in most part voluntarily assumed, since they gave him some slight comfort during the paroxysm. Cold and clammy perspiration on his face; distressing eructation set in, accompanied with loud, gulping noises. He continued in this state for about four minutes. There was nothing to indicate that the paroxysm was about to abate. I then poured five drops of nitrite of amyl on a piece of lint, and held it to his nostrils. In about thirty seconds his face and neck became deeply flushed, and all his pains vanished. He looked at me with the utmost astonishment, scarcely knowing what to make of his sudden relief. In answer to my question, he said the pain was gone. That night he slept soundly. A bottle of the nitrite was left with him, with directions that he was to pour out five drops and inhale it whenever an attack came on.

Ever since, the amyl has invariably given him relief, and no ill effects or unpleasant symptoms due to its repeated use have been noticed.

Altogether he has used more than thirty ounces of the nitrite since the 26th of January. He now notices this difference in its action. Latterly he finds that though the pain gives way as quickly after the inhalation as at first, yet in a few minutes he feels that it is about to recur; he then takes a few more whiffs from his bottle and is easy. For this reason the inhalation has to be repeated sometimes three or four times before the attack completely gives way.

In the case I have described, the pallor, coldness, and arterial tension, followed on the inhalation of amyl by redness of surface, warmth of body, and relaxation of the arterial circulation, indicate that a tonic condition of the arterioles existed, and that the good effects of amyl were due to its antagonistic effects on the muscular tissue of the arteries, probably though to medium of the vaso-motor nerves.

V. Syncope.—While puncturing a small abscess in the upper eyelid of Mrs. T——, aged forty, her face became pale, the muscles around the mouth twitched in a convulsive manner, and she would have fallen had I not kept her up. She was carried to a sofa close by, and in a few seconds was perfectly unconscious. The radial pulse could scarcely be felt. I hurriedly poured some nitrite of amyl upon a pocket-handkerchief, and applied it to her nostrils. It seemed for a minute to have no effect, but then color reappeared in the face, and the radial pulse was felt beating rapidly and strongly. At the time this happened I felt certain that the nitrite of amyl was of considerable service: the syncope would probably have continued longer but for its use. How soon she would have rallied after she was put on the sofa in a recumbent posture had the amyl not been employed, it is impossible for me to say. She had never before had a similar attack.

There are many forms of syncope in which this remedy will probably be found to be useful.

VI. Epilepsy.—What is known of the pathology of epilepsy leads one to think that the inhalation of amyl may be of service, both in preventing a fit that is about to occur, and likewise in stopping or diminishing the actual fit itself.

There seems to be in epilepsy some perverted nervous action causing spasm of the contractile fibres of the cerebral vessels, and those also of the muscles of the face, pharynx, larynx, respiratory apparatus, and the limbs generally.

The loss of consciousness is probably due to the fact that, by the contraction of the cerebral vessels, the brain is deprived of its due supply of blood.

I have but once tried nitrite of amyl during an epileptic fit, and the evidence I have to offer is not precise and complete enough to entitle the observation to be considered as a proof of the value of amyl in epilepsy.

A little boy named John Davies, aged three years, was brought to the Brecon Infirmary on the 5th of September. He had been suffering from fits for twelve months past at the rate of six or eight in the twenty-four hours. A fit lasted from ten to twenty minutes. The history of his case was not simply that of convulsions. The suddenness, both of invasion and of loss of consciousness, and the average duration of the paroxysm, pointed to epilepsy—to "le haut mal." While the mother was in the waiting-room the boy had a fit, and she at once brought him to me. I applied nitrite of amyl to his nostrils; he got exceedingly red in the face, and in about two minutes was unconscious.

The mother said she was "sure that it helped him out of his fit." I, too, am pretty certain that it did. He was ordered to take bromide of potassium. From the 5th to the 9th of September he continued to have fits, but from the 9th to the 16th he had not had one.

VII. Laryngeal Spasm.—In laryngismus stridulus or spasmodic croup, the spasm of the adductors of the vocal cords and the carpo-pedal contractions seem to point to nitrite of amyl as a valuable therapeutical agent in the treatment of the paroxysm. I have not yet had a chance of trying it.

In whooping-cough also it may perhaps render good service, by relaxing spasm of the glottis. Apart from physiological reasons, the only case I have met with that illustrates the action of amyl in laryngeal spasm is one which, I acknowledge, does not entitle me to speak with any authority on the subject.

VIII. Colic and Enteralgia.—In these affections such symptoms as the cool skin, pale face, slow pulse, and spasmodic pains of an ordinary attack, indicate the employment of amyl. More especially would it be likely to be useful in those severe attacks which are accompanied with clammy perspiration, dusky hue, and the general signs of collapse. Dr. Anstie last year stated that he had used it in two or three cases of spasmodic cramps associated with flatulence; and he is reported as having said, "There can be little doubt in my mind that amyl is a prompt relaxer of spasm in the alimentary canal."

Spasm of the pharynx or œsophagus, biliary and renal colic, vesical and urethral spasm, may perhaps yield to the action of amyl. Hiccough, too, judging from its spasmodic nature, may also be expected to be checked by this remedy.

IX. Headache and Facial Neuralgia.—I have, during the last week, had under my care a good example of that form of hemicrania designated "sun-pain," the latter name being confined to those instances of the affection in which the pain continues only so long as the sun is above the horizon.

Elizabeth G——, a cook, aged forty, came to me on Wednesday, the 13th September. For the preceding fourteen days she had suffered severely from headache and face-ache. The severe pain was chiefly localized about the right forehead, brow, eyeball, and upper half of cheek, but there was some pain about the left brow and the left molar region. The pains began every morning about nine o'clock and disappeared about five in the evening. She had comfortable nights. She had been sick; vomiting once, or oftener, every day. When seen by me her face was pale, pulse feeble, tongue red. She was suffering severe pain, chiefly in the course of the right supra- and infra-orbital nerves. She was given five drops of amyl to smell. While the characteristic reddening of the face was setting in, she said she felt giddy; she seemed inclined to fall, and probably would have fallen, had I not helped her to a seat. I have frequently known a giddy feeling occur from its use, and look upon it as due to the suddenly-induced alteration in the cerebral circulation, or to muscular relaxation. In the present case the giddiness was more marked than usual. In less than two minutes the face resumed its pale color, but the pain, with the exception of a slight "soreness" about the forehead, had gone. In ten minutes afterward pain returned in the right forehead, but not in the cheek. I then applied, for five minutes, to the right temple, some bisulphide of carbon, as recommended by the late Dr. Kennion, of Harrogate. This gave additional relief, and when she left me there was only a little soreness over the right eyebrow. I prescribed for her nothing more than the ordinary hospital white mixture.

Next day, Thursday, the 14th, she suffered nearly as much pain as on any previous day. On the 15th she came a second time to me. Amyl was reapplied, and this time with complete relief. I then gave her some in a bottle, to take home, with instructions that she was to use it whenever the pain came on.

On the 16th, at 9.30 A. M., the pain returned. She used the amyl, and one inhalation gave her complete ease for the rest of the day.

On the 17th, at 9 A. M., the pain again set in. One application of the amyl sufficed to stave off the attack.

On the 18th, at 10 A. M., she felt some pain, but not much. She used amyl, and no further pain troubled her.

On the 19th, she visited me, for the third time, in order to tell me the result. The pain had quite left her, and she felt as well as usual. She was very thankful for the relief she had obtained, and spoke much in praise of the remedy.

In some forms of hemicrania and headaches the pain is probably in some way connected with cerebral anæmia, the result of arterial spasm. In *migraine* and most neuralgias there probably exists some imperfect nutrition of the nervous tissue, atrophic or anæmic. In such cases it is possible that the increased supply of blood caused by inhalation of amyl may supply the posterior roots of the implicated cerebral nerves with that pabulum which they need.

This case leads me to think that nitrite of amyl will prove a valuable remedy in that form of tic called by Trousseau "epileptiform neuralgia."

Nitrite of Amyl as an Antidote to Certain Poisons.—I must leave, for future publication, some remarks on this head, which, had space permitted, would now be included in this paper. I will briefly mention that there are well-founded physiological reasons for believing that amyl is in many respects antagonistic to chloral and ergot; that it is likely to be a valuable agent in cases of overdosing by chloroform; and that it is capable, generally, under certain conditions, of counteracting the tendency to death by anæmia, syncope, and apnœa.

2.—*Therapeutic Action and Uses of Turpentine.* [Medical Press and Circular, September 6, 1871.]

AT the last meeting of the Edinburgh Medico-Chirurgical Society—Dr. H. Bennett in the chair—Dr. Warburton Begbie read a paper on the above subject. He gave a brief sketch of the ancient history of the drug from the time of Hippocrates, with a notice of the various forms in which the oleo-resins of the coniferæ are used or have been used in therapeutics. Oil of turpentine was described as being irritant and stimulant, quickening the circulation and augmenting the temperature of the body. In larger doses it produces a sort of intoxication; in drachm-doses it is hypnotic. Externally it is a valuable rubefacient, and is absorbed by the skin so as very soon to be recognized in the breath, and by its characteristic violaceous odor in the urine. The production of this violaceous odor in its perfection seems to be a test of the integrity of the urinary organs, as it is less marked or absent in disease of the kidneys. The therapeutic actions and uses of turpentine are various: 1. As a cathartic it is uncertain, but along with castor-oil it is useful in cases of obstinate obstruction and tympanitis. 2. As an anthelmintic it is chiefly used as a cure for tape-worm; also, in the form of enema, it destroys ascarides and lumbrici. 3. Though turpentine sometimes causes hæmaturia, it cures certain passive hæmorrhages. It is useful in purpura, probably acting through the nervous system; and is useful also in hæmoptysis, hæmaturia, and uterine hæmorrhages. 4. As a stimulant, it is especially valuable in adynamic fevers; as in the stupor of typhus, in certain kinds of delirium, and in the later stages of enteric fever with a dry tongue. 5. In certain nervous diseases, such as epilepsy and chorea, it is said to be very useful; but in epilepsy it is supplanted by bromide of potassium, and in chorea by arsenic. In certain forms of sciatica and crural or brachial neuralgia in the aged, twenty minim-doses thrice daily have a very good effect. In the nervous headache of delicate females, and the headache which is induced by fatigue, it is a better stimulant even than strong tea, and without the effect which tea so often has of banishing sleep. 6. In all chronic discharges from mucous membranes, such as chronic and fetid bronchitis, it is very useful, and even is advantageous in gangrene of the lung in checking the fetor. Under this head some interesting cases were given of gangrene of lung depending on the presence of foreign bodies.—A discussion followed, in which the chairman, Mr. B. Bell, Dr. T. R. Fraser, Mr. Lister, Dr. Smart, Dr. T. G. Stewart, and Dr. Joseph Bell, took part. Additional evidence as to the value of turpentine in hæmorrhage and in chronic mucous discharges was elicited.

3.—*Physiological Action of the Codeia Derivatives.* [Medical Press and Circular.]

Dr. Michael Foster has investigated the physiological properties of the derivatives of codeia. As we have given an account of their chemical properties as stated in Prof. Wright's paper, we complete the subject by noting some of Dr. Foster's experiments:

The hydrochlorate of chlorotetracodeia, and the hydrobromate of bromotetramorphia, in doses of a decigramme by subcutaneous injection or by the mouth, produced in adult cats in a very few minutes a condition of great excitement, almost amounting to delirium, accompanied by a copious flow of saliva and great dilatation of the pupils. Micturition and defecation occurred in some instances, and vomiting was observed on two occasions with the morphia-salt, but was very slight. The excitement was very peculiar, being apparently due partly to increased sensitiveness to noises, and partly to an impulse to rush about.

The same doses of the morphia-salt given to a young kitten produced the same flow of saliva, dilatation of pupils, and excitement (without vomiting): but the stage of excitement, which in adult cats passed gradually off in a few hours, was followed by a condition marked by a want of coördination of muscular movements, and presenting the most grotesque resemblance to certain stages of alcoholic intoxication. This stage was followed in turn by sleepiness and stupor, in which the kitten was left at night; in the morning it was found dead.

Two observations have shown that these salts paralyze (in dogs and cats) the inhibitory fibres of the pneumogastric; they also seem to lower the internal tension, but want of material has prevented from ascertaining how this is brought about.

On *rabbits* neither salt, even in doses of a decigramme, seems to have any effect, except perhaps a slight excitement. There is no dilatation of the pupils, no flow of saliva, and, if one observation can be trusted, no paralysis of the inhibitory fibres of the pneumogastric.

No marked difference was observable between the two salts, except that the morphia salts seemed rather more potent than the corresponding codeia bodies.

The salts of deoxycodeia and deoxymorphia given by mouth or by subcutaneous injection, in doses of a decigramme, produced in adult cats, almost immediately after exhibition, a series of convulsions much more epileptic in character than tetanic. In one case there was a distinct rotatory movement. In a few minutes these convulsions passed away, leaving the animal exhausted and frightened. Then followed a state of excitement, with dilated pupils and flow of saliva, very similar to the effects of the tetracodeia and tetramorphia salts, but less marked.

Miscellaneous and Scientific Notes.

Billroth on Acupressure, Acutorsion, and Torsion.—In fifty cases of amputation Prof. Billroth performed acupressure or acutorsion, using on an average four needles in each case; he has performed it on two hundred arteries, including fifteen cases of amputation of the thigh; he has had no opportunity in cases of amputation of hip-joint, having had no cases lately; in exarticulation of shoulder he never succeeded in fixing the needles firmly enough to rely on methods above named. Acutorsion was performed more, acupressure less frequently, acufilapressure in no case. Hæmorrhage following removal of needles occurred in one case only; he accounts for it by his having performed acupressure according to the first English method, compressing against the integument, the brachial artery, the median nerve, and belly of the biceps muscle; this caused fluxion to the compressed parts; removing the needle, they receded, tearing the adhesions which had compressed the artery. He therefore abstains from acupressure *en masse*, and recommends careful acutorsion. In acutorsion, he considers one half turn sufficient, and preferable to a whole turn or more, since the needle is removed more readily. Gold needles occasionally become bent if very long; but they can be removed without causing irritation, and are therefore preferable to all others.

Prof. Billroth attempted torsion in several cases of amputation of the breast, with such negative results that he abstained from it until again led to it by English surgeons, and Porta, who performed it four hundred times without secondary hæmorrhage (out of twenty-three attempts of torsion of the femoral artery he failed four times, and applied ligatures).

During last summer Billroth performed torsion in five amputations of the leg, two of the foot, two of the fore, and one of the upper arm; also upon the greater number of arteries in several amputations of the thigh.

Secondary hæmorrhage occurred in one case, from the posterior tibial; he believes this was due to his having forced the torsion. To perform torsion successfully, he considers it necessary—1. To isolate the vessel thoroughly; 2. To grasp the end of the same with a strong, well-fitting pinzette; 3. There must be a certain length of vessel from the end to the nearest branch. These conditions are not given in the mammæ, hence his previous failure.

Although Porta recommends making but slight traction, four to five turns in small, and six to eight in large vessels, Billroth has in every case drawn out the vessel from one to one and a half inch, and turned the pincers until the vessel was torn asunder, one portion remaining in the instrument, the other receding; this was done to alter the walls as much and as far as possible, in order to gain an extensive thrombus.

Post-mortem examination has proved these views to be correct; the advantage of an extensive thrombus is, however, counterbalanced by the fact that vessels isolated for a greater distance, and denuded of their sheath, become necrotic. In cases below the knee and elbow, Billroth considers torsion advisable, if not too near a larger branch.

Appointments, Honors, etc.—Dr. Billod, chief physician to the Lunatic Asylum of the Seine, at Epinay-sur-Orge, has received from the inhabitants of his commune a gold medal, purchased by subscription, in recognition of his services during the siege of Paris. The Cross of the Legion of Honor has been sent by the French Government to Madame Pochet, of Havre, for services rendered during the war. The honorary membership of the Royal Society of Sciences of Brussels has been conferred on Sir James Paget and Sir William Fergusson. Dr. Tilbury Fox has been made a member of the Leprosy Committee of the Royal College of Physicians. Mr. John Hilton has been elected President of the Pathological Society of London for the year 1872. A subscription is on foot among the profession in Italy to strike a gold medal to be presented to Virchow. M. Barth has been elected President of the Academy of Medicine, of Paris, for the present year. The degree of LL. D. has been conferred by the Faculty of Princeton College on Drs. Abraham Coles, of Newark, and Hugh L. Hodge, of Philadelphia. Dr. Bamberger has been elected to the chair of Medicine in the University of Vienna, to succeed the late Prof. Oppolzer. Dr. Tyson has retired from the position of Assistant Editor of the *Philadelphia Medical Times.* Dr. Thomas W. Evans, of Paris (an American), has been promoted to the rank of Commander in the Legion of Honor.

Small-pox in Hamburg.—According to information furnished by the Hamburg Board of Health, there were in that city, from August 19 to November 18, 1871, 5,707 cases of small-pox, with 1,047 deaths, 696 cases remaining under treatment. The following are the statistics as regards vaccination :

	Vaccinated.	Unvaccinated.
Recoveries...	2,954	1,010
Deaths........................	347	700

Cundurango in Germany.—The curative powers of this root for cancer, on both sides of the Atlantic, begin to be viewed in the light of another huge mare's-nest. The German papers have received a communication from Prussia House, to say that the reports of the English practitioners, applied to by the German embassy, go to deny entirely the curative power of the cundurango.—*Medical Times and Gazette.*

Infallible Cure for Chills.—In Pleasanton, Texas, says the *Commercial Advertiser*, one Frank Polica, who had been afflicted with chills, until life had become a burden to him, vowed that, if he had another, he would kill himself. The next day he felt the chill coming on, and before he could be prevented he put his pistol to his head and fired, killing himself instantly.

A Memorial to Jenner.—It is proposed to erect a memorial window in the old church at Berkeley, Gloucestershire, England, to perpetuate the fame of Jenner, who lived and died in Berkeley. The sum of £500 is needed for the purpose, and the subscription-list is headed by the names of three earls.

Antidote to Carbolic Acid.—Dr. T. Hasemann, from numerous careful experiments, both chemical and medicinal, advocates the use of a strong solution of saccharate of lime, of course to be taken as soon as possible.—*Medical Times and Gazette.*

Proposed Medical Congress.—The medical profession of Lyons are discussing the propriety of holding a congress in that city next year, and there is every probability that the scheme will be carried out.—*Lancet.*

Billroth on Ovariotomy. — This eminent surgeon, in his "Reminiscences," published in the *Wiener Med. Wochenschrift*, says of ovariotomy:

First of all, surgeons must dismiss from their minds that ovariotomy is a dangerous operation; and, through the medium of well-informed practitioners, this conviction must make its way with the public. After ovariotomy, skilfully performed according to the rules of art, recovery is the general rule, and a fatal issue the constantly-diminishing exception. Comparing it with some other operations, ovariotomy, taking the mass of cases, is shown by statistics to be less dangerous than amputation of the thigh, disarticulation of the shoulder- and hip-joints, or excision of the hip or knee. Its danger is about the same as that of amputation of the arm, excision of the shoulder, partial excision of the jaw, lithotomy in the young, and similar operations. We must, however, perform ovariotomy strictly according to the rules laid down by the English operators in their classical works; and only after having attained the same results should we venture to practically put into force our own ideas, in order to improve upon these. I had the good fortune to see Spencer Wells operate upon two complicated cases, and from them, as well as from oral communication with this remarkable man, I learned much. I constantly follow his precepts, knowing that he has long since thoroughly thought out and tested all that can happen to myself. I shall willingly regard myself during my lifetime as his scholar; and contented shall I be if it falls to my lot, by means of this operation, to snatch from certain death one-half of the number of lives he has been enabled to save.

Up to the present time I am tolerably contented with my results. I give here a short account of them, in order to encourage the performance of these operations, and especially to inform the colleagues into whose hands these lines may fall that I have, personally, no reason for supposing that the results attendant upon ovariotomy will be less cheering in Vienna than they are in London. Hitherto, I have performed it nine times; and of these patients only two have died, giving, therefore, only a mortality of 22.2 per cent. The first four cases recovered one after another; then two fatal cases occurred, to be followed again by three recoveries. The first case is related in my Zurich "Chirurgische Klinik," and the second, third, and fourth cases in the "Chirurgische Klinik," published at Vienna in 1868.

Medical Declaration respecting Alcohol.—The following declaration is published, signed by about two hundred and fifty of the leading medical men in London and the provinces:

As it is believed that the inconsiderate prescription of large quantities of alcoholic liquids by medical men for their patients has given rise, in many instances, to the formation of intemperate habits, the undersigned, while unable to abandon the use of alcohol in the treatment of certain cases of disease, are yet of the opinion that no medical practitioner should prescribe it without a sense of grave responsibility. They believe that alcohol, in whatever form, should be prescribed with as much care as any powerful drug, and that the directions for its use should be so framed as not to be interpreted as a sanction for excess, or necessarily for the continuance of its use when the occasion is past.

They are also of opinion that many people immensely exaggerate the value of alcohol as an article of diet, and since no class of men see so much of its ill effects, and possess such power to restrain its abuse, as members of their own profession, they hold that every medical practitioner is bound to exert his utmost influence to inculcate habits of great moderation in the use of alcoholic liquids.

Being also firmly convinced that the great amount of drinking of alcoholic liquors among the working-classes of this country is one of the greatest evils of the day, destroying—more than any thing else—the health, happiness, and welfare of those classes, and neutralizing, to a large extent, the great industrial prosperity which Providence has placed within the reach of this nation, the undersigned would gladly support any wise legislation which would tend to restrict, within proper limits, the use of alcoholic beverages, and gradually introduce habits of temperance.—*Lancet.*

Dr. Lionel S. Beale protests against some of the statements of this declaration, and withholds his signature, doubting whether the public can be made temperate by such means. Dr. Anstie also declines to sign the declaration.

Prevalence of Small-pox in England.—We are about to make a statement which, if it were not based upon stubborn undeniable facts at this moment before us, we should certainly neither believe ourselves nor ask credence for in our pages, so lamentable is it and so discreditable to the intelligence of the people and rulers of this country. In the year 1870 there were registered in the seventeen principal cities and towns of England 1,259 deaths from small-pox; in the year just closed there have fallen victims to that disease in the same towns no less than 13,174 persons. The details for the several towns have been derived from a collation of the weekly returns for 1871, and are as follows: London, 7,876 fatal cases; Portsmouth, 39; Norwich, 245; Bristol, 45; Wolverhampton, 284; Birmingham, 61; Leicester, 11; Nottingham, 144; Liverpool, 1,919; Manchester, 267; Salford, 227; Bradford, 5; Leeds, 43; Sheffield, 406; Hull, 57; Sunderland, 850; Newcastle-on-Tyne, 695. The proportion of fatal cases to the population of the seventeen towns taken in the aggregate was 18 per 10,000; the ratio in London was 24, in Norwich 30, in Liverpool 39, in Wolverhampton 41, in Newcastle-on-Tyne 54, and in Sunderland 86.

The highest small-pox mortality in London during the thirty-one years 1840–'70 was in 1863, when 2,012 fatal cases were registered; in the subsequent seven years (1864–'70) the annual deaths were successively—537, 646, 1,388, 1,332, 606, 273, and 958. Last year they rose to the unprecedented number of 7,876, whereof 2,400 occurred in the first, 3,241 in the second, 1,255 in the third, and 980 in the fourth quarters. This epidemic began in the latter weeks of 1870, and rose rapidly through the first quarter of 1871 until its maximum fatality was attained in the week ending 6th May, after which period it declined—at first steadily, but afterward with considerable rapidity—to less than 100 deaths weekly in August, and the minimum of 51 deaths was touched in the last week of September. Since that time, and especially within the last month, there have been indications of recrudescence which demand the serious consideration of all who are intrusted with the care of the public health.—*Lancet*, January 6, 1872.

Resection of the Œsophagus.—To the current number of Langenbeck's *Archiv*, Prof. Billroth, of Vienna, contributes a most interesting and suggestive paper bearing the title, "Ueber die Resection des Œsophagus." He states that some time ago, after a *post-mortem* examination of his first patient affected with carcinoma of the œsophagus, the possibility suggested itself of making a resection of this part of the alimentary tube. The fact that the lymphatic glands in the neighborhood of the diseased part are not generally affected, and the partial success which had hitherto attended the operation of œsophagotomy in this disease, together with the analogy of external urethrotomy in cases of gangrene or ulceration of the urethra, seemed to lend support to such an idea. The passing, moreover, of bougies through cicatricial tissue was far preferable to the manipulation of such instruments in a tube with ulcerated and weakened walls.

On April 21st of last year, a large dog was put under the influence of chloroform, and a piece, about an inch and a half in length, was cut out of the whole circumference of the œsophagus. The lower end of the divided tube was then fastened by a couple of sutures to the skin at the margin of the external wound. Up to the 26th of the same month the animal was fed with milk through a tube passed into the wound, but on and after this date the tube was passed *via* the mouth. A week after the operation the sutures were removed. By the end of June the fistulous opening had completely closed, and the process of healing would have been quicker if it had not been that the dog, like human patients, dissatisfied with "milk diet," purloined the more solid food of neighboring victims to science. After the closure of the œsophageal fistula, which took place at the end of June, the tube was daily dilated by a bougie of the diameter of a large index-finger. After the healing of the wound the dog was in capital condition, eating meat, potatoes, etc., but the variety of fare was not allowed to extend to bones. On July 26th the animal was killed with cyanide of potassium; and all that was found as a trace of the operation was an annular scar, scarcely half a line in width, and moreover easily dilatable. —*Lancet.*

A Victory for the Ladies.—New-Year's Day in Edinburgh was signalized by a victory for the lady medical students. At the annual meeting of contributors to the Royal Infirmary, the question of admitting ladies to the wards for purposes of clinical instruction came up in connection with the election of managers. The retiring managers whose reëlection was proposed were confronted by nominees favorable to the admission of lady students into the infirmary. After an animated discussion a division was taken, when the nominees were declared duly elected by 177 to 168. A scrutiny was demanded on behalf of the defeated managers. Thereafter it was proposed that the meeting should consider the advisability of enacting that henceforward all registered students of medicine shall have full admission to all the educational advantages of the infirmary without reference to sex. The defeated minority objected to this resolution, that they had not received the statutory month's notice of the intention to introduce it. The friends of the ladies, however, persisted in moving the resolution, which was unanimously adopted—their opponents having previously left the room.—*Lancet.*

Transfusion of Blood.—At a late meeting of the Obstetrical Society of Dublin (*Lancet*), two cases of transfusion of blood were detailed by Dr. Ringland, one of which proved successful. In both cases the instrument used was that invented by Dr. McDonnell, of Dublin, in which an arrangement exists to obviate the danger of air entering the circulation during the operation. The *Gaz. Med. Ital. Prov. Venet.* published a case of gastric hæmorrhage in a woman sixty-three years of age, who, being in *articulo mortis*, was restored by the injection of one and a half ounce of defibrinated blood taken from the arm of her son. In seven days she was discharged cured from the hospital. In the *Centralblatt* some cases of tranfusion have been published by Juergensen, and extensively quoted. In a case of gastric ulcer, complicated with pleurisy, in which the patient was reduced to the last extremity, the operation was without benefit. Two other patients recovered, one a case of phosphorus-poisoning, in which there was much hæmorrhage; the other a case of asphyxia from carbonic acid, in which the benefit was most marked, and for which accident Juergensen looks upon transfusion as an important remedy.

Royal Victims to Small-Pox.—Dr. John Gardiner (*Edinburgh Medical Journal*) has collected the following history of the ravages of small-pox in some of the royal houses of Europe, hoping thus to impress the public mind more forcibly as to the advantages of vaccination:

Among the family of Charles I., of Great Britain, of his forty-two lineal descendants up to the date of 1712, five were killed outright by small-pox; viz., his son Henry, Duke of Gloucester; and his daughter Mary, wife of the Prince of Orange, and mother of William III.; and three of the children of James II.; viz., Charles, Duke of Cambridge, in 1677; Mary, Queen of England, and wife of William III., in 1694; and the Princess Maria Louisa, in April, 1712. This does not include, of course, severe attacks not fatal, such as those from which both Queen Anne and William III. suffered. Of the immediate descendants of his contemporary, Louis XIV. of France (who himself survived a severe attack of small-pox), five also died of it in the interval between 1711 and 1774; viz., his son Louis, the Dauphin of France, in April of 1711; Louis, Duke of Burgundy, son of the preceding, and also Dauphin, and the Dauphiness, his wife, in 1712; their son, the Duc de Bretagne, and Louis XV., the great-grandson of Louis XIV. Among other royal deaths from small-pox in the same period were those of Joseph I., Emperor of Germany, in 1711; Peter II., Emperor of Russia, in 1730; Henry, Prince of Prussia, in 1767; Maximilian Joseph, Elector of Bavaria, December 30, 1777.

Influence of Tobacco on the Nerve-centres.—In the *Bulletin de l'Association Franc. cont. l'Abus du Tabac*, M. Tamisier states that, out of fifty-nine grave affections of the nerve-centres observed from 1860 to 1869 among men, forty occurred in smokers. In fifteen cases of hemiplegia, nine abused tobacco, and two used it moderately; four did not smoke. Of eighteen cases of paraplegia, five were great smokers, three moderate smokers, and ten abstained from tobacco. Out of twenty cases of locomotor ataxia, fourteen were great smokers, five moderate, and one abstainer. Tamisier thinks that it is especially, if not wholly to this cause, that we must attribute the disease in the majority of cases of hemiplegia and of ataxia he has noticed since 1860. M. Lefevre, of Louvain, thinks it indubitable that excessive smoking causes paralytic mania: because—1. Nicotine causes in animals progressive enfeeblement of the muscles of motion up to paralysis, and congestion of the nerve-centres. 2. Analogous symptoms have been noticed in numbers of persons who abuse tobacco in smoking or chewing. 3. It has been found in all countries that there is a constant relation between the consumption of tobacco and the increase of general paralysis.—*The Doctor*.

The Beauties of Homœopathy.—Dr. C. W. Earle, in a paper read before the Chicago Medical Society, made the following severe remarks regarding homœopathic therapeutics:

Homœopathy came, and with one mighty bound went *back* five hundred years. Its advocates invaded the *loathsome* for remedies with which to humbug the people. Take Jahr's Pharmacopœia, and look at their remedies. On page 96 you will find an account of the American polecat or skunk. The learned author of the advance school says: "Near the *anus* there is a pouch where the follicular glands secrete an odorous matter; the animal squirts his liquor," etc. "We make the first three attenuations by trituration." The black spider (see page 83) is prepared for homœopathic medication by putting the whole animal in alcohol; "macerating it for weeks, and even months, and then decanting the clear liquor, which is the mother tincture." "The first three attenuations of the common wood-louse" are prepared by trituration; the tincture, by twenty parts of alcohol. "Lachesis (snake-poison) is procured from the poison-bags which are found in the upper jaw of these reptiles."

The Naval Hospital at Annapolis.—The United States Naval Hospital, near Annapolis, has finally reached its completion, after the expenditure of almost two years in labor and more than two hundred thousand dollars in money. The hospital in the Naval Academy, intended at first merely for the use of sick midshipmen, but afterward, through necessity, appropriated to indiscriminate naval service in its particular province, was found to be too small for the demands after the transfer of so many ships and their accompanying officers and crews to the Academy, and hence the erection of the present building. In all, the building contains one hundred and seventy apartments, and is competent to furnish accommodation for nearly three hundred patients. Surgeon Van Ripen is in charge, and D. T. Streets is chief steward. The outer hall is of fine pressed brick, neatly ornamented. The edifice was constructed under contract with a Baltimore firm. Their original contract was for one hundred and ninety-six thousand dollars, but the additions and alterations, and other extra work, have swelled this amount by several thousand more.—*Medical and Surgical Reporter.*

Treatment of Blistered Feet.—Inspector-General Lawson, with a view of obviating the ineffectiveness resulting from soldiers getting their feet blistered by marching, has directed (*Lancet*) that medical officers are to apply to their commanding officers, to have every man suffering in this manner taken at evening parade to the medical officer, who should cause him to wash his feet, and then to pass a needle with a worsted thread through each blister, cutting off the thread a little distance outside the blister at each side, and leaving a portion in it. The part is then to be rubbed with common soap, the sock put on, and wetted over all prominent points, and the soap again rubbed over them freely. When properly attended to, no man should be unable to march the following day on account of blistered feet, unless the cuticle has actually been removed, leaving a raw surface exposed.

Bichloride of Methylene in Italy.—The anæsthetic use of bichloride of methylene is not very general at the present time in this country, but it would appear that it is in great favor at Padua, where it has been regularly employed since July, 1868, to the exclusion of ether and chloroform. Mr. Robbins, of Oxford Street, sends a supply twice a year, and since the time above stated, one hundred and eight operations have been performed with this anæsthetic. M. Rossi states that the patients may be thus divided: Fifty-two became narcotized quietly and naturally in a very short time, and without any struggling; thirty-two showed some excitement, and fell asleep after inhaling from eight to ten minutes; four only were violently agitated, and were from fifteen to twenty minutes before they slept; twenty were proof against the anæsthetic after breathing it for forty or fifty minutes; and eight had severe vomiting.—*Lancet.*

Obituary.

The death of Dr. Henry D. Bulkley, which it becomes our duty to announce, occurred on the 4th of January, 1872. Dr. Bulkley for nearly half a century had been identified with the medical profession of this city, and might be considered as one of the links which connected the physicians of old New York with those now living among us.

In early life he was doubtless familiar with the features of Hosack, of Mitchell, and of Post; while during mature manhood he enjoyed the companionship and counsel of Mott, of Stevens, of Francis, of Joseph M. Smith, of Wood—his seniors in years, and who preceded him to the grave; and again, up to the time of his decease, we find him active in medical association with junior members of our profession who are worthily filling the places of departed worthies.

Dr. Bulkley was born in New Haven, Conn., on the 4th of April, 1804. After passing through the ordinary preparatory course of studies, he entered Yale College, from which institution he graduated in 1821. Selecting medicine as a profession, he entered the office of Dr. Jonathan Knight, and in due season obtained the degree of Doctor of Medicine from his *alma mater*. In 1831 he visited Europe, and while there paid special attention to cutaneous affections, availing himself particularly of the clinical lectures of Cazenave and Biett at the Hôpital St. Louis, Paris. During the summer of 1841 he again visited Europe, seeking rest from professional duties, and returned to this country during the autumn, having been invigorated by his journey. In 1846 and again in 1853 he edited editions of the work of Cazenave and Schedel, on Diseases of the Skin; and in 1851 also edited "Lectures on the Eruptive Fevers by George Gregory." From 1853 to 1856 he was editor of the *New York Medical Times*, during a part of this period being associated in the management with his friend Dr. John G. Adams. He was one of the earliest writers in this country on "Infantile Syphilis," and one of the first in this city to give a special course of lectures on cutaneous affections. In 1848 Dr. Bulkley was appointed one of the attending physicians to the New York Hospital, a position which he honorably filled, and retained to the close of his life. At the time of his decease he was consulting physician to the New York Dispensary; of the New York Dispensary for Diseases of the Skin; of the Nursery and Child's Hospital; trustee of the New York Academy of Medicine; of the New York Medical Library and Journal Association; President of the New York Dermatological Society, etc. In 1869–'70 he was President of the New York Academy of Medicine, as he had also been of the Medical Society of the County of New York. Into both of these Societies he infused life, and they flourished under his administration.

Dr. Bulkley had not been in robust health for some months before his decease, but was sufficiently well to attend to professional duties. On the Saturday preceding his death, viz., December 30, 1871, he was able to visit his patients, but at the time was suffering from a cold. His disorder culminated in pneumonia, from which affection he died on the following Thursday morning, in the sixty-eighth year of his age.

Accurate in diagnosis, skilful in the management of disease, tender in the care of the sick, and possessing the traits of a Christian gentleman, Dr. Bulkley merited and acquired the esteem of his professional brethren and of the community in which he lived.

At a stated meeting of the New York Academy of Medicine, held January 4, 1872, Drs. William C. Roberts, James Anderson, and Gouverneur M. Smith, were appointed a committee to take action in reference to the death of Dr. Henry D. Bulkley, and, in accordance with such appointment, presented the following preamble and resolutions, which were unanimously adopted:

Whereas, It has pleased an All-wise Providence to remove by death Dr. Henry D. Bulkley, late trustee and former President of the New York Academy of Medicine, after brief illness:

Resolved, That this Academy is called upon to mourn the loss of a Fellow who was early identified with its interests, and to the close of life active in the promotion of its welfare.

Resolved, That, in the various official positions to which he was elected by his colleagues, he has exhibited integrity of character, and contributed by his assiduous labors in elevating the Academy to its present position among scientific associations.

Resolved, That his contributions to medical literature, enriched as they were by practical conclusions derived from clinical observation in hospitals and infirmaries which he had long and faithfully served, will remain permanent monuments to his professional industry and ability, especially in the department of cutaneous medicine, to which he particularly devoted himself, and in which he had long been considered an expert.

Resolved, That, while this Academy thus bears witness to his eminence as a physician, it also records its recognition of the fact that both in his public and private life he exhibited the traits of character which conspicuously mark the Christian gentleman.

Resolved, That, as a tribute of respect to his memory, the Fellows of this Academy attend his funeral on the 7th inst., and that a copy of these resolutions be presented to his family, and published in the medical and daily journals.

EDMUND R. PEASLEE, M. D., *President.*
WILLIAM T. WHITE, M. D., *Secretary.*

At a special meeting of the Medical Society of the County of New York, held January 7, 1872, at the chapel of the Fourth Avenue Presbyterian Church, the following preamble and resolutions, presented by Dr. William C. Roberts, were unanimously adopted:

Whereas, It has pleased the All-Wise Disposer of human events to take unto Himself the soul of our departed friend, associate, and professional brother, Henry D. Bulkley, M. D.: therefore—

Resolved, That, while expressing its profound sorrow at the severance of the tie that has so long bound them, the Medical Society of the County of New York is desirous of placing on record its high appreciation of Dr. Bulkley's services as its former President, his eminence as a practitioner of medicine, and his virtues as a Christian and a man.

Resolved, That Dr. Bulkley's life-long labors in the cause of medical science, as an editor, writer, teacher, and as physician to the New York Hospital, and to several other charitable and benevolent medical institutions in this city, have entitled his name to be long and affectionately cherished in the memories of his professional brethren and of the public; while his erudition, genial courtesy, strict integrity, and observance of medical ethics, had acquired for him the respect and esteem of all who knew him.

Resolved, That, in the demise of the late Dr. H. D. Bulkley, this Society, and the profession generally, are called upon to mourn the loss of a distinguished member; and the public, of a righteous man, a kind friend, and faithful servant and physician.

Resolved, That a copy of these resolutions be transmitted to the family of the deceased, and published in the medical and daily journals.

(Signed) ABRAHAM JACOBI, M.D.,
President.

BRADFORD S. THOMPSON, M. D., *Corresponding Secretary.*

At a meeting of the New York Medical and Surgical Society, held January 13, 1872, the following resolutions, offered by Dr. A. C. Post, were unanimously adopted:

Resolved, That this Society has heard, with profound regret, of the death of Dr. Henry D. Bulkley, one of its original members.

Resolved, That we cherish his memory as that of an earnest and diligent worker in the promotion of medical science, and in the advancement of the healing art.

Resolved, That our long association with him has deepened our conviction of the excellence of his character as a faithful and conscientious physician, as a true and warm-hearted friend, and as a pure and upright man in all the relations of life.

Resolved, That we sympathize with his family in their bereavement, and that we rejoice, with them, that he was cheered in his dying hour by the hope of a blessed immortality.

Resolved, That a copy of these resolutions be forwarded to the family of the deceased, and that they be published in the medical journals of this city.

THOMAS F. COCK, M. D., *President.*

ROBERT WATTS, M. D., *Secretary.*

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Original Communications.

Art. I.—*Remarks on some of the New Methods of Treatment for Fracture of the Femur.* By Frederic D. Lente, M. D., Cold Spring, N. Y.

The treatment of fracture of the shaft of the *femur* has justly attracted more attention from surgeons than that of any other bone; and various have been the plans devised, and great has been the mechanical ingenuity brought to bear upon it, in order to avoid the long and irksome, and often injurious confinement, formerly considered necessary, and also the shortening so annoying to the patient, and frequently so injurious to the future comfort of the surgeon.

Over twenty years ago, when resident surgeon of the "late lamented" New York Hospital, where the treatment of fracture was, at that time, generally considered in advance of most of the institutions of this or any other country, Physick's modification of Desault's splint, with the occasional use of the double-inclined plane for exceptional cases, was the most effective and comfortable appliance at hand. It soon, however, received important improvements from the surgeons of the hospital. The introduction of *adhesive plaster* for extension, by Drs. E. Wallace and Crosby, which I first adopted in the hospital, and, I believe, in the city, was so important an improvement that it seemed to render the apparatus almost perfect. Soon after, I contrived a still further modification, as regards the length of the splint and the distribution of the counter-extending pressure, thus enhancing the comfort of the patient. This apparatus is described and figured in the first edition of Hamilton's work on Fractures. It was soon, however, superseded, to a great extent, by the appliance which my friend Dr. Gurdon Buck, who has done so much for the improvement of the treatment of fracture in general, has the credit of introducing to the profession, and which is now in very general use by surgeons, and will probably continue to be so.

Within the last two years, however, a still further and very important modification of the treatment of fractured femur has been introduced, the advantages of which, in *simple fracture*, have been most prominently set forth by Prof. Sands in the June (1871) number of this JOURNAL; also by Dr. Bryant, one of the resident surgeons of Bellevue Hospital, at a later date. Both speak in the highest terms of the success of the "plaster-of-Paris splint," and present incontestable evidence of its excellence, in the statistics which they publish. The application of the plaster-bandage, however, and the preliminary arrangement of the patient as described, though sufficiently simple for those engaged in the constant use of surgical appliances, will, no doubt, appear more troublesome and complicated to the general practitioner, who is in the habit of treating only occasional cases of fracture, than the apparatus of Buck, in the application of which the merest tyro in surgery can scarcely fail to succeed, and which, with any decent degree of watchfulness and skill on the part of the surgeon, leaves little else to be desired in the great majority of cases. Still, the plaster-splint, in its application to the *femur*, as in all its other applications, has a certain attractiveness, which will lead many surgeons to adopt it; and, in certain cases, where exercise and fresh air are of special importance, and where the conveyance of the patient from one locality to another is necessary, will undoubtedly prove superior to all others.

A great feature in all our modern improvements in surgery is simplicity, and it would seem that the necessary appliances, as described by Prof. Sands, for the proper application of this splint, are sufficiently simple, and such as any person, in any locality, may always procure. Nevertheless, the general practitioner, who but rarely treats fracture of the *femur*, is not apt to provide himself with the upright bar with its binding-screw; and when the emergency presents itself, would consequently be apt to defer the application of the plaster-bandage to some future occasion. It may, therefore, not be superfluous to remind him that he may not only dispense with this contrivance without disadvantage, but probably with a positive gain, in the facility and rapidity of application, and perhaps in the comfort of the apparatus also. By discarding the bar, we may also dispense with the second table, provided we have a single one of sufficient length. But this is a detail of little moment, except when we are working, as we sometimes are among the poorer classes, in their own apartments, which are very contracted. I would then suggest that the patient be placed on a long table, or on a broad board of sufficient length placed on a table;[1] that three layers of old blanket, or an equivalent of flannel, be placed on the injured side, between the genitals and thigh (not over the *perinæum*, which is entirely out of the way), extending down several inches upon the latter; that over this be placed the counter-extending band, consisting, as most readily accessible, of a strip of strong, unbleached muslin about six inches wide, folded twice upon itself longitudinally, and long enough to pass above and below the body to above the shoulder, where it may be tied in a knot. This is to be secured either to the upper end of the table or board, as the case may be, or attached to a cord and fastened to a staple in the wall, or some such fixture which may happen to present itself. The limb having now been drawn out, in the manner described by Dr. Sands, to its proper

[1] He may, if more convenient, be laid on a single bedstead without a foot-board, or a "bunk" made for the occasion.

length, as ascertained *by actual measurement*, is to be protected by carrying a flannel roller from the toes to the groin, and around the pelvis. Before carrying it about the latter, this must be raised a few inches from the table. To do this conveniently, carry a band folded like the counter-extending one, but wider, around the pelvis, just above the greater trochanter, tie in a knot about eighteen inches above the body, pass a stick under it, by means of which, an assistant, standing on the table astride the pelvis, may easily raise it to any required height. If it be preferred, or in case the table is not sufficiently firm to bear the weight of an additional person, the body may be raised, and kept at a convenient height by a simple mechanical contrivance always at hand. Place a piece of board four or five inches wide, and long enough to reach a couple of feet or so above the table, so that one end will rest on the floor on a line with the pelvis, and at a distance of a foot or so from the table, to be held temporarily by an assistant; place a similar piece with one end resting on the upper extremity of the first; this is to be used as a lever for raising the body from the table; and the pelvic band must be so attached to it that, when it is raised to a parallelism with the table, the pelvis shall be at a convenient height above the latter, which need not exceed a few inches. To maintain the lever in this position, place under its extremity a vertical prop similar to the first-described piece. The pelvis will then be swung clear of the table, as upon a gallows. Extension may now be made in the manner described by Prof. Sands. If weights be used, the extending cord may pass over the top of a chair-back, the chair being kept firm by a suitable weight on its seat. If adhesive plaster be used for extension, each band opposite the *malleoli* had better be folded upon itself longitudinally, so as to make it as narrow as possible, in order that it may interfere as little as possible with the proper covering of the foot by the turns of the roller. It is stated that only modeller's plaster should be used; but this is seldom to be procured in small villages and in the country; it is therefore well to know that *any fresh* plaster of Paris will answer; though the coarser variety may be longer in "setting," which process generally requires at least half an hour, and from one to two hours to become perfectly hard and dry.

It will be perceived that I have not gone into all the details of the application of the plaster-bandage, as it has already been well described in this JOURNAL (June, 1871), by Prof. Sands, and subsequently by Dr. Bryant, of Bellevue Hospital, but only such as, in my opinion, may simplify the process still further, and perhaps render the splint still more comfortable as well as more efficient, and thus induce the more general introduction into the surgery of the country of one of the most valuable improvements of the day.

Prof. Sands, in his paper, remarks that " the ordinary perineal band is a very indifferent substitute for the iron bar." I think, on a trial of both, that the reverse will be found to be the fact. All the bar can do is to make an efficient counter-extension, and this can as well be done by any inelastic substance which is sufficiently strong, especially for the short time it is in action. The objection to the bar, it appears to me, is, that it occupies the very space where we wish to place our *permanent* counter-extension, in applying the roller which, from not having been spread over this space, when the bar is used, is reduced almost to an edge; and is, as he remarks, liable to produce excoriations, rendering an excess of padding necessary. When the bandage has become dry, the counter-extending strap is to be cut off above the pelvis, behind and before, the lower portion, as well as the pelvic or elevating strap, remaining as a part of the apparatus. A single layer of flannel *roller* over the limb is sufficient for its protection; indeed, a roller of ordinary bandage will do very well, and is less clumsy and difficult of application than the layers of blanket, and the splint is less liable to get loose, and thus to require opening and replacing. But several layers should be placed about the pelvis, and, if the patient is very thin, perhaps some cotton wadding.

As regards the period after the injury at which the apparatus should be applied, some would prefer waiting for several days, until it is ascertained whether inflammation and swelling will ensue, or until these subside; and one will not lose any thing by waiting if, in the mean time, he applies Buck's apparatus, and thus maintains the fragments in good position. But, in the great majority of cases of fracture of this bone, there never occurs any considerable tumefaction, and it may be prevented by methodical bandaging immediately after the injury. Dr. Valentine Mott used often to remark that "a roller-bandage is a good antiphlogistic."

This apparatus, in my opinion, may prove of even more eminent service in compound fracture of the femur, in gun-shot-wounds, for instance, than in simple; for here, the process of consolidation, if it occur at all, is always tedious, and apt to be retarded, especially by the long confinement of the patient, and want of exercise. If the sinuses open on the posterior aspect of the thigh, it is troublesome to maintain cleanliness; and, if on the anterior or lateral, the pus is apt to burrow—whereas, if the patient has the power of changing his position, the matter can be made to gravitate, and discharge at a dependent point. No matter how many openings there may be, the splint is still applicable; for, after it has hardened, *fenestræ* may be cut of any required size, in any part of the splint, without materially impairing its strength. If, however, the *fenestræ* are to be near each other, wires of sufficient size should be laid between them during the application of the roller, to insure requisite solidity. Every time a fresh dressing is applied to the openings for the absorption of the pus, the edges of the *fenestræ* should be well "calked" with "patent oakum" or some such absorbent, to prevent the pus insinuating itself between the splint and the limb. In the case of a compound fracture of the *humerus*, the treatment of which I have just completed, although there were three sinuses communicating with the bone, necessitating three large openings in the splint above its middle, with the soft parts in a very unfavorable condition from a protracted attack of phlegmonous erysipelas, the splint remained perfectly firm for six weeks, during which it was kept on without once being removed, with the result of perfect consolidation; and perfect cleanliness was maintained throughout, the dressings being all managed by the patient and his wife. Dr. Burnett, house-surgeon of Bellevue Hospital, assisted me in the management of the case.

This apparatus finds another valuable application in the management of severe sprains and strains in and about the large joints. The firm and methodical application of strips of diachylon-plaster in at least three layers, immediately after the injury, and, if necessary, the subsequent application of the light plaster-splint, save more pain, and effect a much more rapid cure, than any other method. Even in those severe cases of articular rheumatism in the knee, ankle, or elbow, where the mere idea of movement, or the touch of the couch by an attendant, produces a cry of pain, and where it is impossible, by any position of the body or arrangement of the couch, to obviate movement of the joints, a light plaster-splint would effectually relieve all distress from this cause, and thus tend to expedite the cure; just as a plaster or muslin bandage around the thorax in pleuritis gives more relief even than an anodyne, and favorably influences the treatment. I have used it, on the recommendation of other surgeons, also with great satisfaction in the treatment of club-foot in the youngest infant; here it is the safest and most effective of all appliances.

Art. II.—*Prolonged Expiration: its Physical Cause, and its Relation to the Pretubercular Stage of Consumption.*[1] By Samuel G. Armor, M. D.

In the study of physical science we constantly encounter facts of observation, and have only hypotheses to explain them. Now, it may be that, in attempting an explanation of a frequently-observed clinical fact, I shall be charged with indulging in mere speculation; not, I hope, as to the fact itself—for I cannot doubt that, in a certain proportion of cases, and that more than has been generally supposed, prolonged expiratory murmur is an early and prominent feature in the forming stage of phthisis. And I am equally certain that the sign may be recognized, both in tracheal and vesicular respiration, independently of any very marked variation in *intensity*, *quality*, or *pitch*. This general statement brings the subject-matter of discussion at once before the mind.

It will be remembered that prolonged vesicular breathing, in the absence of vesicular emphysema, has been generally in-

[1] Read before the Kings County Medical Society, Brooklyn, N. Y.

terpreted as indicating the "presence of tubercle," and that it does so, in many instances, cannot be doubted. To Dr. James Jackson, Jr., of Boston, is due the honor of having first attracted the attention of the profession to this subject. He published a communication in 1833, on the subject of a prolonged expiratory sound as frequently constituting an important physical sign of the first stage of phthisis, and now, so far as I know, the sign is recognized, with varied degrees of value attached to it, in all the standard text-books of Europe and America.

"The expiratory murmur," says Skoda, "is constantly increased in duration from the normal standard of two to eight or upward."

Niemeyer speaks of different degrees of value attached to the sound, and adds, "When prolonged and altered in *quality*, it is very significant."

Flint says: "The earliest and most obvious of the auscultatory evidences of tubercle, in a certain proportion of cases, undoubtedly, are incident to expiration."

And this observation is fully confirmed by Dr. Theophilus Thompson, in his "Clinical Lectures on Pulmonary Consumption." From the analysis of a large number of cases, he was led to conclude that "a prolonged expiratory murmur frequently takes precedence of other characteristic signs."

Loomis also recognizes its value, but makes this qualification: "Prolonged expiration, if unattended with alterations of quality, is insignificant; or, if it is low-pitched, it furnishes no evidence of tubercle."

But, while there is general agreement as to the value of the sign, with certain qualifications, there is diversity of opinion as to the immediate physical cause.

The most generally-accepted theory is, that the vesicular structure of the lung is so substituted by tubercular deposit that it becomes a better conductor of expiratory sounds, which, in normal states of the vesicular murmur, are soon lost after the commencement of the expiratory act. The normal vesicular murmur is simply converted into bronchial respiration, which is frequently as long or longer than inspiration.

Skoda, however, suggests the theory of "some obstruction to the egress of the air from the lungs," and he expresses his belief that "the impediment is, in most cases, caused by a swelling of the lining membrane of the bronchial tubes." Exceptionally, he explains it by what he calls "a law of consonance," and this is manifested only in cases of consolidation of lung-tissue.

These are, I believe, the general outline views held by auscultators as to the immediate physical cause of prolonged expiration when heard in connection with suspected tubercle. The normal vesicular expiration, which is low in pitch, and averaging, according to Fournet, about one-fifth the duration of inspiration, is, by virtue of consolidated lung-structure, converted into bronchial expiration, which is relatively more prolonged, more intense, and of higher pitch, than the vesicular.

According to this generally-received view, prolonged expiration is heard only in connection with tubercular deposit; and, while the theories of "conduction" and "consonance" may be rational and philosophical, when such deposit is present, they are nevertheless unsatisfactory, as a general statement, from the fact that the abnormal murmur is often present when tubercular infiltration is extremely limited; nay, more, it is frequently observed when no tubercles can be detected, and when no sibilant or sonorous *râles* indicate swelling of the bronchial mucous membrane.

In such cases, what is the value of the sign? and what does its presence indicate? These questions constitute the text of my paper, and to their answer I shall briefly direct attention.

In the first place, I may be permitted to state that I am quite sure there are two errors copied into the majority of our text-books on physical diagnosis in reference to prolonged expiratory murmur: the *first* is in attaching too much value to it as indicating the *actual presence of tubercle;* the *second* is in ascribing, without exceptional qualification, the immediate physical cause of the sound to the presence of tubercle.

In rejecting, as a general unqualified statement, both the "tubercular" and the "bronchial congestion" hypotheses, I offer, as a substitute, the more rational one, in my judgment, of the *want of pulmonary elasticity.* This is certainly more in harmony with anatomical facts, with the physiology of the respiratory act, and with the clinical history of many cases in which prolonged expiration is observed.

It will be remembered, in connection with this explanation, that the substance of the lung is highly elastic; no other portion of the body is so largely composed of elastic fibres.

"The sub-serous areolar tissue," says Gray, "contains a large portion of elastic fibres. . . . Between the vessels and in the interstices between the lobules, there is a quantity of yellow elastic tissue, which gives firmness and resiliency to the pulmonary structure."

The walls of the terminal bronchial tubes, the air-cells, and the intercellular passages, are formed by a peculiar interlacing of longitudinal elastic bundles with fibrous tissue, within which is placed a continuous series of circular muscular fibres; so that the entire parenchyma of the lung may be said to be a mass of elastic and contractile tissue.

Now, in the condition of normal elasticity of the air-cells and intercellular passages, inspiration is relatively longer than expiration; for the reason, perhaps, that the air, when it enters the smaller bronchial tubes and air-cells, meets with resistance from their *contractility;* whereas, in atonic states of this elastic property, inspiration and expiration become more nearly alike in duration; in other words, the vesicular approaches more nearly to the bronchial expiration. And, in proportion as they become more alike in duration, they are also, of course, more or less mixed in quality.

In accounting, therefore, for the physiological disturbance between these two acts—the faintly-heard vesicular murmur gradually approaching the bronchial, and finally becoming more prolonged than inspiration—we naturally inquire into the forces and functions of ordinary respiration. What are these forces? And how are they disturbed?

The limit of my paper will only allow me to inquire into the one point before us, namely, the cause of *prolonged expiration.*

Physiologists are, I believe, agreed upon the fact that the movement of ordinary expiration is entirely a *passive* one. The causes of expulsion of air from the lungs embrace mainly: 1 The restoration of the chest to its former dimensions; and

2. The elastic recoil of the tissues composing the parenchyma of the lungs. Of these the most important factor is undoubtedly the latter. "The pulmonary tissue," says Dalton, "is abundantly supplied with yellow elastic fibres, which retract by virtue of their elasticity in every part of the lungs after they have been forcibly distended, and, compressing the pulmonary vesicles, drive out a portion of air which they contain."

It will be thus seen, from this physiological statement, that the perfection and rapidity of the *expiratory* murmur depend largely on the elastic properties of the lungs themselves, and to defect of this property must be due, in a great degree at least, the inverted ratio between normal inspiration and expiration.

But, notwithstanding these accepted physiological facts, and the logical inferences that would seem to naturally flow from them, the statement continues to be copied into most of the text-books, that "the most common cause of prolonged expiration is tubercular deposit in the lungs."

Now, I do not propose to unqualifiedly controvert this statement. There is too great weight of authority against me to justify such a position. Moreover, I doubt not that a prolonged expiratory murmur is frequently—if not "most commonly"—associated with tubercular deposit, and not only associated with, but doubtless caused by it. But that it is not peculiar to pulmonary consumption alone is a fact that may be easily verified at the bedside. Indeed, the most marked and typical illustration of excessively-prolonged expiration is to be met with in vesicular emphysema—a condition in which the air-cells, from over-distention, have lost their elastic and therefore physiological property of driving out the air with which they have been forcibly distended in inspiration.

In normal conditions of lung-structure, the power of the elastic and muscular fibres is so proportionate to the amount of air to be expelled, that expiration is of short duration, i. e., as compared with inspiration; and, when prolonged from general depressed conditions of the system, affecting the contractile power of the muscular and elastic fibres, the pitch is low because the lung-tissue is normal.

It will be thus seen that two distinct conditions may give rise to prolonged expiration: the one a general condition of atony, in which the elastic and muscular fibres participate; the other, consolidation from tubercular deposit. These two conditions are represented by but one sign in common, viz., prolonged expiration. And the cause of the sign, in the one case and the other, differs as widely as the conditions themselves. In the *constitutional* condition already referred to, the amount of air in the vesicles being the same, while the elastic and contractile power of expulsion is impaired, it follows that the *duration* of the expulsion must be increased—that is, provided the orifices through which the air is expelled remain unchanged; in the case of *tubercular deposit*, the expiratory murmur being caused by the passage of air through the rima-glottis, it is better conducted by a more or less solidified lung, and becomes, of course, more tubular in quality and higher in pitch. *Pitch*, *quality*, and *intensity*, therefore, determine at once the physical cause of *duration*.

Let me be distinctly understood, however, as not intending to embrace, in the present paper, the tubercular theory of prolonged expiration. It is rather my purpose to attract the attention of clinical observers to the general law of prolonged expiration, and to affirm that the sign may be distinctly recognized in a class of cases not at all tuberculous. In the cases to which I refer the respiratory murmur is, as a rule, diminished or feeble in intensity, and the prolonged expiration is heard with more or less distinctness over the entire chest, presenting a contrast, in this respect, with the more circumscribed sounds heard in cases of tubercular deposit. Allowance will be made, of course, for the normal and relative distinctions between *tracheal*, *bronchial*, and *vesicular* respiration. These being carefully noted, the most delicate test of the *duration* of expiration is often over the larynx or trachea; and it is interesting to note, in this connection, the relatively low pitch of expiration caused by feeble elastic recoil of the lungs and expiratory muscles. The higher pitch is evidently partly due, at least, to the greater force of the act, and partly to the greater contraction of the glottis by the approximation of the vocal cords which takes place with expiration. And this approximation is, of course, greater in proportion to the elastic recoil of expiration. Hence, again, the necessity of carefully apply ing the tests of *intensity* and *pitch* to suspected *prolongation* of sound.

This disturbed relation between inspiration and expiration, when it takes place independently of tubercular, bronchial, or emphysematous complications, has its origin in an *error of nutrition.* And this error may be *general* or *local.* If general, the contractile power of the muscular and elastic fibres participates in the universal failure of nutrition; the prolonged expiratory murmur is simply one of the manifestations of weak vital power, just as an atonic muscle, a certain quality of an elastic artery, or the non-contractile state of a feeble capillary, is an indication of constitutional debility. Thus, I have frequently observed a morbidly-prolonged expiration in general anæmia, in cases of exhaustive discharges of long standing, in advanced stages of typhoid fever, Bright's disease of the kidney, and other constitutional states characterized by general prostration of the vital powers. In all such cases, vital contractility—including the natural resiliency of lung-tissue—is everywhere disturbed by the general failure of nutrition, and this may exist, I repeat, without reference to tubercle.

Practically considered, therefore, a prolonged expiration is frequently characteristic of the *pretubercular stage* of consumption — a stage in which auscultation and percussion give us no clew to the disease other than the sign referred to. In such early stage of the disease we simply affirm, from a morbidly-prolonged expiratory murmur, that the patient has "weak lungs," and this we do on a principle as easily understood as that of weak digestion or weak circulation.

Nevertheless, clinically considered, as already stated, morbidly-prolonged expiration may be often detected in constitutions not at all tuberculous, so far at least as we are able to discover. In rare instances, indeed, it is normally present, particularly on the right side. Hence, unless other evidences accompany it, it furnishes no reliable test of the actual presence of tubercle.

As indicating a *tendency*, however, to tubercular deposit, the sign is extremely valuable in its groupings with the rational symptoms generally present. If, for instance, with a prolonged expiratory murmur, the patient has the peculiar build of body indicative of the want of proper *nutrition* and *development*, and if to these be added lax fibre, waste of muscles of inspiration, gradually progressive emaciation without any known cause, want of fatty matter in the subcutaneous connective tissue, general pallor, and, perhaps, morning and evening fluctuations of temperature, we have every reason to suspect what the older authors described as the "consumptive habit of body," and to fear the speedy development of other signs indicating absolute presence of tubercle.

The importance of grouping together such general signs and symptoms as indicate, in their correlations, a tuberculous tendency of constitution, before there is consolidation of lung of sufficient magnitude to be recognized by ordinary physical signs, can scarcely be over-estimated. The refinements of physical diagnosis are often indulged in at a time when there is no hope for the patient.

I have spoken of a general lesion of nutrition. If the error, however, be more decidedly *local*—whether from local anæmia, loss of power from the effused products of inflammation, fatty degeneration of the air-cells, or stiffening of their walls from the infiltrated deposit of tuberculous matter—we are apt to have, with prolonged expiratory murmur, a great variety of local and circumscribed sounds, corresponding with the location and extent of the tuberculous deposit. Thus, it is often found associated with the extremes of feeble and rude respiration, bronchial and cavernous. It may exist in connection with high or low pitch, moist or dry *râles*, sonorous or sibilant. But, in all instances, *intensity* and *pitch* will determine whether the prolonged expiration indicates solidification of lung, or mere retardation of reflux of air from loss of power of its muscular and elastic fibres; and the predominant bronchial or vesicular character of the sound, as the case may be, will also throw light on the nature of the cause. Moreover, if tubercles be actually present, the diagnosis rarely hinges exclusively on prolonged expiration.

The study of these more local signs, however, does not come within the scope of the present paper. It has been my aim to present, in the briefest possible space, the simple statement, that prolonged expiration may be caused by *defective tonicity of the muscular and elastic fibres of lung-tissue*, irrespective of vesicular emphysema, tubercular deposit, or bronchial congestion; that this results from atonic conditions of the general system; and, when taken in connection with certain rational symptoms, is a most significant sign of the *pre-tubercular stage of consumption.*

The views here expressed as to clinical facts have been formed from personal observation, which may or may not accord with the opinion of others; the *cause* of the facts, I desire to repeat, is only an opinion earnestly entertained, and believed to be in harmony with the facts, and with the physiology of the respiratory act.

ART. III.—*Chronic Diffuse Nephritis.* By FRANCIS DELAFIELD, M. D.

THE name of Bright's disease of the kidney, like that of tubercles of the lungs, has had the unfortunate effect of leading to confused and undefined ideas concerning the lesions of those organs. Thus, we find persons, otherwise well informed, speaking of hydronephrosis, acute interstitial nephritis, and various other kidney-lesions, as if they were diseases of the same class and character.

The first step toward a clinical differentiation of the different renal diseases is an accurate knowledge of the morbid changes which take place in them.

In the present paper I wish to call attention to those chronic diseases of the kidney which are most frequently met with in practice, and to which the name of chronic Bright's disease is most commonly applied.

For the study of these lesions the Bellevue Dead-House affords a peculiarly rich field.

The descriptions of the course and character of this disease, given by different authors, do not by any means correspond. The following quotations are taken from the works of the standard authors on this subject:

Frerichs distinguishes three stages of Bright's disease: 1. The stage of hyperæmia and of commencing exudation. 2. The stage of exudation and of commencing transformation of the exudation. 3. The stage of atrophy.

The first stage is characterized by increase of the size of the kidney, especially of the cortex; by general congestion; by extravasations of blood in the Malpighian bodies, the tubes, and the kidney-tissue; and by the filling of the tubes with coagulated fibrine. The epithelium of the tubes is unaltered. This is the form of disease which occurs after scarlet fever. In the chronic form of the disease all these changes are less marked.

In the second stage, the congestion diminishes while the exudation increases. The exudation is found in the tubules and in the interstitial tissue. The epithelium of the tubes and the exuded fibrine within them breaks down into fatty granules. The exudation between the tubes is sometimes organized into connective tissue. The cortex becomes of a whitish-yellow color, and remains thickened. The surface of the kidney is smooth or slightly granular. The pyramids are of a reddish color. Some of the Malpighian bodies are normal, others are enlarged, and filled with exudation. In the cortex the epithelium of the tubes is swollen and granular, and may break down altogether, or it simply shrivels and atrophies. The tubes are filled with degenerated epithelium, granular matter, and fat-globules, or with homogeneous exudation. The tubes are dilated. The dilatation of the tubes is the principal or only cause of the increased size of the kidney.

In the third stage, the kidneys are smaller, or of normal size, or even larger than normal. The capsule is adherent. The surface of the kidney is irregular and granular, its color a dusky yellow. Its consistence is hard. The cortex is thinned. The pyramids are smaller. The fat about the pelvis is increased in amount. The tubes are dilated and filled as in the second stage, or are collapsed and folded together. Most of the Malpighian bodies are shrivelled and fatty. If the exudation between the tubes has become organized, we find masses of connective-tissue cells and fibres.

Virchow says that, in Bright's disease, the first change in the kidney is in the epithelium of the convoluted tubes. The epithelial cells become swollen and granular. The process is, therefore, a parenchymatous nephritis.

George Johnson distinguishes five forms of renal disease (excluding the suppurative, scrofulous, and cancerous diseases):

1. *Acute Desquamative Nephritis.*—The form of disease occurring after scarlet fever, exposures to cold, etc. This corresponds to Frerichs's first stage. Johnson, however, lays most stress upon the desquamation of the epithelium, and but little on the exudation in the tubes. Exudation between the tubes he does not mention.

2. *Chronic Desquamative Nephritis.*—This corresponds to the second and third stages of Frerichs. Johnson describes the degeneration of the epithelium, the denudation of the tubes of their epithelium, their dilatation and collapse, and the presence of coagulated material within them. The Malpighian tufts are thickened or atrophied. The arteries are thickened. He regards the production of new fibrous tissue as an accidental and unessential phenomenon.

3. *Waxy Degeneration of the Kidney.*—Under this name Johnson describes kidneys which are of large size, their cortex thick and white, their tubes filled with waxy material. This waxy material he supposes to be produced by a degeneration of the epithelium. The large hyaline casts found in the urine he calls waxy, and seems to consider them diagnostic of this form of kidney-disease.

4. *Acute Non-desquamative Disease of the Kidney.*—This is characterized during life by scanty or suppressed urine, but containing no albumen, and no casts, or only a few waxy ones. The kidneys are of normal size; the epithelium of the tubes is somewhat altered.

5. *Chronic Non-desquamative Disease.*—The kidneys are usually large, very rarely atrophied. The cortex is thick and white. The convoluted tubes are more opaque than usual. The Malpighian bodies and arteries are thickened.

6. *The Granular Fat Kidney.*—This form may be a consequence of the non-desquamative disease, of acute desquamative inflammation, and rarely of chronic desquamative disease. The kidneys are large, the cortex white, mottled with yellowish granulations. These yellow granulations are formed of tubes, containing oil-globules. The vessels and Malpighian

bodies are thickened. Sometimes the same yellow, fatty granulations are found in atrophied kidneys.

7. *The Mottled Fat Kidney.*—All the tubes of the cortex contain oil-globules, and there are red spots of congestion or extravasation.

Grainger Stewart distinguishes:

1. *The Inflammatory Form.*—This has three stages: (1.) That of inflammation; (2.) That of fatty transformation; (3.) That of atrophy. These correspond very closely with the three stages described by Frerichs, except that the existence of exudation between the tubes, and of new connective tissue in the stage of atrophy, is not noticed.

2. *The Waxy Form.*—This also has three stages: (1.) That of simple degeneration of the vessels; (2.) That in which a secondary alteration of the tubes is superadded; and (3.) That of atrophy.

In the first stage, the kidney is of normal size, the tubes are unaltered; only the Malpighian bodies and small arteries have undergone waxy degeneration.

In the second stage, the kidney is enlarged, the cortex thick and white, the Malpighian bodies and small vessels waxy; the tubes contain hyaline casts; their epithelium is swollen; their basement membrane may be waxy.

In the third stage, the kidney is small. The surface is rough, granular and pale. The tubular structures are swollen. The tufts and vessels are waxy. A few tubes are distended, most are collapsed, and are represented only by fibrous tissues.

3. *The Cirrhotic, or Contracting Form.*—This consists of an hypertrophy of the connective tissue of the organ, and a consequent atrophy of all the other structures.

There is at first little diminution of the size of the organ, but the capsule is thickened and adherent, and the surface is rough and granular. The color is pale and reddish. The arteries are prominent, their walls thickened, and their cavities often dilated. On the surface, and in the substance, cysts are often seen. Some are produced by dilatation of the Malpighian capsules, some by dilatation of the tubes, some from morbid growth of epithelial elements. The tubes are compressed and atrophied by the new fibrous tissue. They contain little opaque material, but often hyaline matter. Sometimes urate of soda is found in the stroma and tubes of the pyramids. The disease is a non-inflammatory increase of connective tissue.

Both the waxy and contracting forms may be secondarily affected with the inflammatory disease.

4. *Simple Fatty Degeneration.*—The kidneys are of about the normal size. The surface is smooth, the capsule not adherent. Their texture is soft, the cortex is pale and mottled, with sebaceous-looking deposits. The epithelium of the tubes is fatty.

Dickinson describes tubal nephritis, granular degeneration, and depurative infiltration:

1. *Acute Tubal Nephritis.*—This, the nephritis of scarlet fever and of exposure to cold, is described in very much the same terms as the acute desquamative nephritis of Johnson.

2. *Chronic Tubal Nephritis.*—The kidney is large, the cortex of an opaque white or buff-color, the pyramids pink. The surface is smooth, the capsule not adherent. The convoluted tubes are distended with granular and fatty epithelium and with fibrinous exudation. The straight tubes are packed with the products of epithelial growth, while others contain transparent fibrine. The tubes are not changed, save as regards their contents. The Malpighian bodies are normal or somewhat dilated. There is no increase of inter-tubular tissue. These kidneys remain large and smooth to the last, unless complicated with the depurative change.

Sometimes the cortex is sprinkled with white, sharply-defined specks, like bits of bran. This change is characteristic of a great amount of fatty change in the accumulated epithelium.

3. *Granular Degeneration.*—The kidneys may be of normal, or even increased size, but are usually small. The capsule is adherent. The surface is irregular and covered with little rounded nodules. The cortex is thin. Cysts are often found in the cortex and cones. There is an increase of fibrous tissue around the Malpighian bodies and vessels, and beneath the capsule and deeper in the cortex. The

cortical tubes are atrophied or dilated, but many tubes may remain unchanged. The tubes may be filled with epithelium, or with transparent, fibrinous material. In the majority of cases the epithelium is exactly such as is found in normal kidneys. When changed, it is by an alteration in its regularity of form, becoming somewhat angular, as if cramped in growing space. The circulation through the blood-vessels is much obstructed. The formation of cysts is due to dilatation of the tubes, or of the Malpighian capsules.

4. *Depurative Infiltration.*—The kidney is at first of normal size, pale, and its surface smooth. The only change is in the Malpighian tufts, which react with iodine. As the disease goes on, the kidney becomes larger and its capsule adherent. The cortex is of a pale, opaque fawn-color, or has a pinkish or gray translucency. Afterward the kidney atrophies and its surface becomes nodulated. There may be small cysts. In cases of long standing, almost the entire organ gives the characteristic reaction with iodine. The first change is the infiltration of the Malpighian bodies and vessels. Afterward new fibrous tissue is formed between the tubes, the epithelium degenerates; the tubes are dilated and contain fibrinous casts.

It will be seen that the name of "depurative infiltration," is given to the same form of kidney-disease which is called by others waxy or amyloid. This is done to carry out the author's hobby, as to the nature of the waxy change.

Klebs describes:

1. *Diffuse Granular Degeneration of the Epithelium.*—This condition is found by itself, and in connection with lesions, in the interstitial tissue. By itself, it occurs with pyæmia, phthisis, rheumatism, typhoid and typhus fevers, the malarious fevers, the acute exanthemata, extensive burns, poisoning with phosphorus, and the mineral acids. During life the urine may contain granular casts and albumen. The kidney is somewhat enlarged, the cortex grayish yellow, the pyramids bluish red. There may be little extravasations of blood in the convoluted tubes. The epithelium of the tubes is granular and may distend them. The tubes may contain casts. These changes are most frequent in the convoluted

tubes, but are sometimes confined to the straight tubes of the pyramids. The entire process is a degenerative and not an inflammatory one.

2. *Cyanotic Induration of the Kidneys.*—This condition is produced by any long-continued obstruction to the escape of venous blood from the kidneys, most frequently by heart-disease. The kidneys are increased in size, the surface smooth, the capsule not adherent. The organ is hard, the cortex and pyramids congested, and of a dark-red color. The epithelium of the tubes is not altered. The interstitial tissue is harder, but not increased in amount. The continued congestion may, after a time, produce further changes. The epithelium of the convoluted tubes may undergo granular degeneration, and the cortex becomes paler. Or there may be an increase of interstitial tissue, and the surface becomes nodular.

3. *Interstitial Nephritis.*—This has two stages: (1.) That of cell-infiltration; (2.) That of atrophy.

(*a.*) The Stage of Cellular Infiltration of the Interstitial Connective Tissue.—The kidney is increased in size. The surface is smooth, the capsule not adherent. The cortex is of a whitish or yellow color, the pyramids red. In the cortex the tissue between the tubes is everywhere increased from two to four fold. This increase is due to the pressure of lymphatic elements and of clear serum. There is at first an exudation of lymphatic fluid, which dilates the lymphatic vessels of the interstitial tissue, and is accompanied by an emigration of white blood-globules, which finally fill all the spaces in the interstitial tissue. The epithelium of the convoluted tubes undergoes granular degeneration in consequence of its disturbed nutrition. The increased pressure of blood causes an exudation of the elements of the blood from the Malpighian tufts, namely, fibrinogenic material which coagulates in the tubes, albumen, and red blood-globules. The lymphatic cells perforate the basement membrane of the tubes, and become adherent to the fibrinous casts.

(*b.*) The Stage of Atrophy.—The preceding stage may terminate in resolution and recovery. If it does not, it is succeeded either by a hyperplasia of connective tissue or by granular atrophy.

If there is a hyperplasia of connective tissue, the kidneys are of normal size, or slightly atrophied. The capsule is somewhat adherent. The cortex is whitish, yellowish, or mottled. The pyramids are congested. There is a uniform increase of connective tissue between the tubes. The tubes are unaltered or somewhat narrowed.

Granular atrophy is more common. The kidney is atrophied. The capsule is very adherent. The surface is uneven and nodular. The change of the lymphatic cells into connective tissue is accompanied by fatty degeneration of the cells. In the atrophied spots the tubes and glomeruli become impervious. The tubes contain hyaline casts. The basement membrane of the atrophied tubes becomes thick and fibrous. The glomeruli are atrophied, their capsules thickened, their vessels obliterated. The larger arteries are thickened.

Glomerulo-nephritis.—Klebs gives this name to a form of disease which he has observed in scarlatina cases. The kidneys are of medium size, the capsule not adherent, the surface smooth, the parenchyma congested. There are no changes except in the glomeruli. These appear as opaque, white points. On minute examination, it is found that there are large numbers of small, rounded cells about the loops of the Malpighian tuft, while the epithelium of the capsule is unaltered.

Amyloid degeneration is described in much the same way as by other authors.

Rindfleisch describes:

1. *Acute Parenchymatous Nephritis.*—In the milder form the kidney is of normal size, the surface smooth, the cortex of a yellowish-gray color. There is a moderate degree of cloudy swelling of the epithelium of the convoluted tubes.

In the severer form the kidney has the same appearance, but is increased in size and the cortex thickened.

Both these forms occur with the acute exanthemata, typhus, pyæmia, etc.

2. *Diffuse Interstitial Nephritis.*—This corresponds very closely with the description given by Klebs.

He states that the disease may begin as a parenchymatous nephritis, and afterward become interstitial, but that the two rms also occur independently of each other.

Amyloid degeneration is usually accompanied by interstitial nephritis. The amyloid degeneration is the primary change, and the nephritis follows it as a secondary lesion.

Rosenstein, who has written the most complete and best monograph on kidney diseases, describes:

1. *Chronic Congestion of the Kidney.*—This condition is described in much the same way as by the preceding authors. He brings under this class, however, not only the kidneys which are congested by heart-disease, but those which are congested by the presence of the pregnant uterus. The cases in which kidney-disease did not exist anterior to the pregnancy are cases of congestion, not of inflammation of the kidney.

2. *Catarrhal Nephritis.*—The kidney is of normal size, or slightly enlarged; in severe cases congested and mottled with small ecchymoses. The process begins at the apices of the pyramids, which are at first congested, afterward pale. After a time we find the pyramids divided into red and white striæ, running from the apex to the base of the pyramids. The red striæ are the portions more recently congested; the white are the tubes distended by an increase of epithelium.

The urine contains a little albumen, hyaline, granular and epithelial casts, and blood-globules.

The symptoms during life are not marked. The lesion is seldom primary. It may follow catarrhal inflammation of the urethra, bladder, or ureters; the use of cantharides, copaiba, and cubebs, typhoid and typhus fever, cholera, etc.

3. *Diffuse Nephritis, Parenchymatous Nephritis, Bright's Disease, Granular Degeneration of the Kidney.*—This form has three stages.

The first stage is that of hyperæmia. The kidney is of normal size, or enlarged, congested, and red; there are ecchymoses in the tubes, and swelling of the epithelium of the convoluted tubes.

The second stage is that of exudation. The kidney is enlarged, the cortex pale, the pyramids red. The epithelium of the convoluted tubes is swollen and granular. The tubes are dilated and contain casts. There is usually an increase of cells in the interstitial tissue.

The third stage is that of atrophy. The kidney becomes smaller, its surface nodular. The atrophy may take place without any change in the interstitial tissue, simply as a result of the destruction of the epithelium. Usually, however, the retraction of the new interstitial tissue assists in producing the atrophy.

The epithelium is granular or fatty. The Malpighian bodies are atrophied, their capsules thickened and surrounded with new connective tissue. The basement membranes of the tubes are thickened, and are accompanied by bands of connective tissue. The intertubular capillaries are partly dilated, partly small and fatty.

The atrophy consists, therefore, in a suppression of the function of a number of the tubes, with obliteration of some of the blood-vessels and increase of the interstitial tissue.

Both processes, that in the epithelium and that in the connective tissue, can occur separately, but are usually combined.

4. *Amyloid Degeneration.*—He describes this form in much the same way as other authors. He regards the degen eration of the vessels only as a complication of the parenchymatous and interstitial change.

5. *The Fatty Kidney.*—There is an infiltration of the epithelium with fat, or a fatty degeneration. The condition is described in the same way as the diffuse granular degeneration of Klebs.

It will be seen from the preceding abstracts that, while these authors have apparently observed the same forms of kidney-diseases, their interpretation of them is very different.

1. They all describe a form of disease in which the kidney is large and the cortex white, with or without hæmorrhagic spots, and which occurs with scarlet fever, and after exposure to cold.

Frerichs describes this as the first and second stage of Bright's disease. The principal feature of the lesion is the exudation of fibrine into and between the tubes. The degeneration of the epithelium is secondary to this.

Virchow gives the most prominent share to the changes in the epithelial cells and their parenchymatous inflammation.

George Johnson considers the lesion to consist in a desquamation of the renal epithelium, and does not speak of any exudation between the tubes

Grainger Stewart describes the lesion in the same way as Frerichs, but does not put any exudation between the tubes.

Dickinson describes the lesion under the name of acute tubal nephritis, in the same way as Johnson does.

Klebs calls it interstitial nephritis. There is an accumulation of serum in the lymphatics and interstitial tissue, and an emigration of white blood-globules into the interstitial tissue. The degeneration of the epithelium is secondary to this.

Rindfleisch admits two forms, one in which the principal changes are in the epithelium, and one in which they are in the interstitial tissue.

Rosenstein, like Rindfleisch, admits two forms.

2. They describe a chronic form of disease, occurring in connection with exhausting diseases, and not attended by acute symptoms. The kidney is of medium size, or large; the cortex white and opaque.

Frerichs does not describe this.

Johnson describes the condition under the names of chronic non-desquamative disease, granular fat kidney, and mottled fat kidney. In the granular fat kidney the white cortex is mottled with yellow spots; in the mottled fat kidney the cortex is mottled red and yellow. The lesion consists in a fatty and granular degeneration of the tubes.

Stewart calls it simple fatty degeneration.

Dickinson calls it chronic tubal nephritis.

Klebs calls it diffuse granular degeneration, and the process a degeneration and not an inflammation.

Rindfleisch includes it under the name of parenchymatous nephritis.

Rosenstein describes two varieties under the names of catarrhal nephritis and fatty degeneration.

3. They describe a form of disease characterized by atrophy of the kidney, with thinning and roughening of the cortex.

Frerichs makes this the third stage of Bright's disease.

Johnson calls it chronic desquamative nephritis.

Stewart describes it under two different heads, as the third stage of the inflammatory form, and as the cirrhotic or contracting kidney.

Dickinson calls it granular degeneration.

Klebs calls it the third stage of interstitial nephritis.

Rindfleisch says it may be the result of parenchymatous or of interstitial nephritis, or of both combined.

Rosenstein calls it the third stage of diffuse nephritis.

4. They describe a form of disease in which the Malpighian tufts and the vessels are the seat of waxy degeneration.

Johnson describes, under this name, large white kidneys, of which the tubes are filled with "waxy" casts.

Stewart describes three stages: First, waxy degeneration of the vessels; second, degeneration of the epithelium; third, atrophy of the organ.

Dickinson calls it "depurative infiltration." The first change is in the Malpighian tufts and vessels; afterward there is formation of new interstitial tissue and degeneration of the epithelium; lastly, atrophy of the organ. He considers the waxy material to be dealkalized fibrine, and to follow chronic suppuration.

Klebs does not speak very decidedly as to the connection between the waxy degeneration of the vessels and the interstitial and parenchymatous changes. He admits that the interstitial nephritis may be the first change, and that it may begin and go on at the same time as the waxy degeneration.

Rindfleisch assumes that the waxy degeneration takes place first, and that the interstitial and parenchymatous changes are secondary to it.

Rosenstein regards the degeneration of the vessels only as a complication of the parenchymatous and interstitial changes.

5. Chronic congestion of the kidneys from heart-disease is not described as a separate lesion by the English authors.

Klebs and *Rosenstein* describe it in the same way.

Glomerulo-nephritis is only described by Klebs.

Catarrhal nephritis, as described by Rosenstein, is not exactly described by the other authors.

It will be seen that the principal points of difference are in relation to the comparative importance of the changes in the interstitial tissue and the epithelium of the tubes, and whether the latter are inflammatory or degenerative.

The chronic diseases of the kidney, which I have met with in the Bellevue Dead-house, may be conveniently classed under three heads:

I. Chronic Congestion of the Kidney.
II. Chronic Diffuse Nephritis.
III. Chronic Parenchymatous Nephritis.

I. **Chronic Congestion of the Kidney.**—I have nothing to add to the descriptions given of this form of disease by Rosenstein and Klebs. The lesion is a very common accompaniment of heart-disease; its gross appearances are very characteristic; it may pass into either of the forms of chronic diffuse nephritis which will be presently described.

II. **Chronic Diffuse Nephritis.**—To speak of this disease as the latter stages of an acute inflammatory nephritis, seems to me to be in the highest degree erroneous. Neither the clinical histories nor *post-mortem* appearances give any good grounds for such an opinion. It seems to be rather the result of a wish for a systematic division into a first, second, and third stage, than of observation, which has engrafted this idea into the descriptions of nearly all authors.

The disease is, as a rule, from its beginning to its close, a chronic affection. In many cases it is impossible to fix the period at which the disease commenced, so insidious is its approach, and, in autopsies of persons dying at all stages of the disease, no first stage of congestion and inflammation is to be observed.

The disease may begin as a chronic diffuse nephritis; may succeed chronic congestion of the kidney; or may follow the acute lesions of the kidney which occur with scarlatina and after exposures to cold. The first course is the ordinary one.

The lesions of chronic diffuse nephritis are: 1. Granular and fatty degeneration of the epithelium of the tubes, especially in the cortex. To judge of the condition of the epithelium of the tubes, it is necessary to remember its normal character. The tubes commence at the papillæ as ducts of large size, which soon divide and subdivide into smaller tubes. These smaller tubes run in straight bundles nearly to the surface of the cortex. At that point they bend downward, and run back in a straight line to a variable depth in the pyramids. Here they again curve upward, run into the cortex, become convoluted, and terminate in Malpighian bodies.

The epithelium varies in the different portions of this long course of the tubes. The papillæ are covered with cylindrical epithelium. This epithelium dips into the open mouths of the tubes, and forms a continuous layer with their epithelium. The epithelium of the large tubes has the same appearance as that covering the papillæ, except that the long axes of the cells are shorter, and the cells have consequently a more cuboidal shape. As these large tubes divide and subdivide in the pyramids, their epithelium retains the same character, but the long axes of the cells become still shorter to correspond with the diminished size of the tubes. As these straight tubes run in bundles into the cortex, the epithelium retains the same character. After the tube has bent downward, passed into the pyramid, and again bent upward (forming Henle's loops), its calibre is much diminished, and its epithelium is different. It is lined with flat pavement epithelium, the cells thin and flat, the nuclei large. When the tube has again passed into the cortex, and become convoluted, its calibre is increased, and its epithelium is still different. The tubes are no longer lined with a layer of distinct cells, of which each cell can be distinguished, but by a continuous layer of finely-granular matter in which nuclei are embedded at regular intervals.

The same appearances, therefore, which are perfectly healthy in the convoluted tubes, may indicate advanced disease in the straight tubes.

Again, it is only in very marked degrees of fatty and granular degeneration that we can be certain of the abnormal character of isolated epithelial cells. In the great majority of cases we can only determine the condition of the epithelium by comparison. To do this, we must make thin sections, embracing the greater part of a pyramid, and the cortex belonging to it. This section should then be examined with a magnifying power of not over 100 diameters. We can then see whether in all the tubes the epithelium has the same transparency, whether the tubes are filled by it in an equal degree, whether it is swollen or flattened, whether it is in place or detached.

If the kidney is examined in this careful manner, it is very seldom that some changes will not be found in the epithelium.

2. *Dilatation of the Tubes.*—This is a common but not a constant change. Both the tubes in the pyramids and in the

cortex may be dilated. Nearly all the tubes may be enlarged or only a few. The dilatation may be uniform for a considerable length of a tube, or it may form abrupt pouches. In the pyramids the tubes may not only be larger, but also more tortuous. In extreme cases the dilatation is so great as to form cysts visible with the naked eye. The dilatation of the tubes may be accompanied with a flattening and thinning of the epithelium on their walls, until it may look like a nucleated membrane.

3. *Formation of Cast-Material in the Tubes.*—This condition is nearly always present, but varies in its extent. The straight tubes, and Henle's loops in the pyramids, are the most common situation, but the cortex-tubes also often contain it. This cast-matter is a pale, homogeneous, translucent substance, filling the tube within the epithelium, or it is mixed with granular matter, or with detached epithelial cells. It seems to be identical with the hyaline, granular, and epithelial casts found in the urine.

4. *Increase of the Interstitial Tissue.*—This is a constant lesion. We find around the Malpighian bodies, and near the surface, irregular patches of tissue between the tubes. These patches are composed of small, round cells, resembling white blood-globules, and of fibrous tissue in variable proportion. The amount of this new interstitial tissue varies very much in different cases. It is usually confined to the cortex. I have only seen it in the pyramids, with excessive cystic dilatation of the tubes.

5. *Changes in the Blood-vessels.*—It is usually impossible to inject the kidney completely by the artery. Through the vein, however, the entire venous system can be completely filled. The walls of the arteries may be thickened, and their lumen diminished. The Malpighian tufts may be shrunken and atrophied, their vessels impervious. The veins, especially the capillary plexus in the cortex, may be dilated. The Malpighian tufts, the small arteries, and the veins in the pyramids, may be the seat of waxy degeneration.

These lesions may correspond to very different gross appearances of the kidney. The organ may be of normal size, may be enlarged or atrophied.

If the kidney is of normal size, its capsule will be somewhat adherent; its surface smooth or covered with little nodules; its cortex of an opaque white, or mottled red, white, and yellow; the red and white streaks, indicating the position of the straight and convoluted tubes, are lost; the pyramids have their natural appearance.

If the kidney is enlarged, the appearances are much the same, except for the increased thickness of the cortex.

If the kidney is atrophied, the capsule is more strongly adherent; the surface is more irregular and nodular; the cortex is thinner, and the pyramids smaller; the kidney may be not only smaller, but there may be an increase of fat about its pelvis, giving a sort of central atrophy.

Waxy degeneration of the vessels is most frequent in the large kidneys and those of medium size—rare in the small, atrophied kidneys.

Cystic dilatation of the tubes is more common in the atrophied kidneys, but occurs also in the large.

Atrophy of the Malpighian tufts is more frequent in the small kidneys.

Deposits of urate of soda are found in the pyramids of some atrophied kidneys, especially with gout.

These various changes constitute the lesions of chronic diffuse nephritis, the most common form of kidney-disease. The clinical symptoms and the condition of the urine vary with the extent and manner in which these lesions are developed. Albumen and casts in the urine, œdema of the face and legs, or general anasarca, nausea, diarrhœa, loss of vision, convulsions, and coma, are the symptoms with which all physicians are familiar.

The tendency of the disease is to a constantly-increasing impairment of the functions of the kidney, but life and comparative health may be prolonged for many years.

III. **Chronic Parenchymatous Nephritis.**—This condition of the kidney is met with in the course of severe and exhausting diseases. Every physician knows that in typhus fever, in pyæmia, in pneumonia, in cholera, in yellow fever, in alcoholism, and in other severe constitutional diseases, the urine of some patients will contain albumen and casts. If the patient recovers, the symptoms of renal disease also disappear.

In these cases the kidneys are found to be of normal size, or slightly larger. They are neither congested nor anæmic. The cortex is opaque and yellowish white, the markings of the straight and convoluted tubes are lost, or may be nearly perfect. The pyramids appear normal, or their papillæ are a little whiter than usual. The Malpighian tufts and vessels are unchanged. In the convoluted tubes the epithelium may appear normal, or may be swollen so as to nearly fill the tubes, and appear stiffer and more opaque than usual. In the straight tubes of the cortex, the epithelial cells are granular, sometimes broken down, and detached so as partly to fill the tubes. In the straight tubes of the pyramids the epithelial cells are granular, detached from the basement-membrane, sometimes glued together by hyaline-cast matter. In some of these tubes, and in Henle's loops, there are hyaline casts.

Whether this lesion is to be regarded as a parenchymatous inflammation or degeneration is, in the present state of our knowledge, hardly to be decided.

Art. IV.—*A Synopsis of Recent Contributions to Mental Science in Great Britain.—Treatment of Insanity.—Cold Applications in Cerebral Rheumatism.—Locomotor Ataxia.—Epilepsy.—Hydrophobia.—Dextral Preëminence.—Dr. Richardson's Discourses.* By George E. Day, M. D., F. R. S.

The West Riding Lunatic Asylum Reports contain three articles on the Treatment of Insanity, viz.: "On Artificial Feeding of the Insane," by Dr. Lawrence; "On the Treatment of Insanity by the Hypodermic Injection of Morphia," by Dr. Ward; and "On Ergot of Rye in the Treatment of Mental Diseases," by Dr. Fox.

Dr. Lawrence begins with a summary of the various causes that lead insane patients to refuse food. These are: 1. Delirium of a fanciful or absurd character, as the idea that they cannot pay for it, that they do not deserve it, that they are dead, and do not need it, that there are serpents in the stomach that must be killed by starvation, etc.; 2. Organic disease of the stomach or liver; and 3. Perversity, stupidity, and such extreme helplessness that they cannot even swallow what is put into their mouths.

The characteristic symptoms of want of food are briefly but clearly given. The most marked symptom is the disagreeable fetor which the breath acquires. It is something so peculiarly *sui generis*, and so distinct from the fetid breath which marks ordinary dyspeptic derangements, that it can be easily recognized by all who have perceived it for a limited number of times. It arises, doubtless, from the altered state of the secretions of the mouth and stomach. The mouth becomes clammy, the teeth become foul, and, in the worst cases, are covered with sordes; the tongue is coated with a thick white fur of old epithelium; little saliva enters the mouth, and what does so is allowed to escape by the lips. The offensiveness of the breath soon disappears in most cases, if a regular and sufficient supply of nourishment is administered, without the aid of medicine at all—a circumstance which shows that it mainly arises from the want of food, and consequent state of the mouth, and the body generally, in which the wasting process far outstrips and exceeds the renewing; but so marked and constant is it that, in the case of newly-admitted patients, about whom no very definite information can be obtained, it may be taken as a good guide as to whether the patient requires to be artificially fed or not.

When moral influences — persuasions, entreaties, commands, bribes, threats, etc.—have failed in inducing a refractory patient to take food, the spoon or the feeding-jug is to be tried, and the way in which the latter is most effectually employed—by laying the patient on his back, and compelling him to swallow one mouthful after another by compressing the nose, and so preventing respiration until deglutition has taken place—is so well known, and has been so fully described by Dr. Williams, in the *Journal of Mental Science*, that it is unnecessary here to describe it. Neither need the method of feeding, which Dr. H. Tuke practises, by means of a catheter passed through the nose, and entering the stomach—a plan which is not altogether free from danger—be detailed at length. Feeding through the nose has very rarely been found necessary in the West Riding Asylum, and almost every case is easily managed with the stomach-pump, as it is employed there.

There are different ways of using the stomach-pump; and, first, as to the position in which the patient is placed. Dr. Blandford, in his recently-published "Lectures on Insanity and its Treatment," has described minutely the method by which violent patients were fed by Dr. Stevens, of St. Luke's Hospital. He, as is generally done, had the patient seated in an arm-chair, and restrained in that position by means of sheets, which rendered him incapable of any sudden movement, by being passed round the body and legs, and then drawn through the arms and legs of the chair. The chief advantage of the upright position is, that there is little risk of choking, should there be any regurgitation of the food while the stomach-pump is being used, as what fluid is returned readily passes out of the mouth, and the patient can again respire freely. In cases where there is not much resistance offered, this position is, perhaps, preferable to any other; but if the patient is very strong, and resists vigorously, it is almost impossible to hold him quiet in a chair by any means, which, however, can be easily done if he is placed in the supine position upon a bed, and held by a sufficient number of assistants, and that without any risk of bruising or inflicting any injury. The recumbent position is the one in which patients are always fed at the West Riding Asylum; and, notwithstanding the frequency with which the stomach-pump has to be used, among nearly 1,500 inmates, there has been no accident from choking, nor any untoward result whatever; and, indeed, this position seems to be as safe as the upright, provided the œsophageal tube is withdrawn, if the food is returned by the side of the tube to any extent. Moreover, whether the patient is in the upright or recumbent position, it is generally found to be useless to go on sending food into the stomach after it has commenced to reject it; so that, in either case, the tube should be withdrawn when this occurs, to prevent the whole being returned by vomiting. The great advantage of having the patient completely overpowered, and held perfectly quiet, is obvious, seeing that there is then no struggling or resistance, and, the abdominal muscles being thus set at rest, there is much less chance of vomiting; whereas, if there is much struggling, the breath is held in, the diaphragm and the abdominal muscles are in a state of tension, and what is forced into the stomach by the pump is as speedily rejected.

The patient being in position, and held by attendants, the mouth has to be opened by one or other of the various forms of gags. The gag most frequently employed is the one with which the ordinary stomach-pump case is furnished, and consists of a straight piece of wood with a hole in the centre of it, through which the tube is passed. Though this gag is almost in general use, there are several objections to it, and, for this reason, in the West Riding Asylum the mouth is always opened by one of the screw-keys, of which there are many varieties, and the wooden gag is dispensed with. These instruments should be light but strong, and the screw should be tolerably quick in its action, not for the purpose of forcing open rapidly jaws that are firmly clinched, but for the purpose of quickly taking advantage of any voluntary separation on the part of the patient. An instrument of this kind is easily applied, as it can be inserted wherever there is an open space between the teeth, and the mouth is then readily opened by turning the screw; no matter with what determination it is shut. When the mouth has been opened, the key should be committed to an assistant, whose only duty should be to hold it in its place; and this there is no difficulty in doing, as the head is held perfectly steady. In this way the greatest freedom is afforded in passing the œsophageal tube, and the finger may be inserted to give the tube the necessary inclination, and guide it over the root of the tongue into the œsophagus, should there be any difficulty in passing it.

The calibre of the œsophageal tube depends upon the size of the pump that is used. Too large a one is of no advantage whatever, and is decidedly objectionable on account of its inflexibility, and the difficulty with which it adapts itself to the curved passage which it has to traverse, though it may be considered the safest because it cannot enter the larynx or produce laceration of the parts so easily. A medium-sized tube, open at the end, and without a wooden point, besides being introduced more readily, gives a freer passage for the fluid than one of large calibre, with two small openings a little distance from the extremity. In passing the tube there is sometimes a little pressure required to make it enter the œsophagus, on account of its having to follow a slightly obtuse curve, and coming in contact with the bodies of the vertebræ, which become prominent if the head is held far back. This pressure may be reduced to a minimum by directing the tube a little to the left side, as the œsophagus inclines to the left, in the upper third of its course, and by moving the head forward when once the tube has reached the entrance of the œsophagus. In using the double-actioned stomach-pump, it is profitable to have the tube attached to the orbital extremity, so that the pump may be held horizontally; in this way all air from it can be excluded by drawing the piston two or three times and depressing the end next the operator, before directing the current into the stomach. Instead of the pump, an India-rubber bottle, with a nozzle fitting the tube, is employed by some. In the West Riding Asylum the funnel, as well as the pump, is in frequent use. It is of a size large enough to hold a pint and a half, and the whole quantity which is to be administered may be poured into it at once. The funnel is attached to the tube by about eighteen inches of gutta-percha tubing. The fluid, by the force of gravity, flows slowly but steadily down, and, by elevating the funnel above the body, it is made to exercise a greater distending force on the stomach, as this force is equal to the weight of a column of fluid, of the area of the stomach, by the height of the fluid in the tube.

The frequency with which a patient requires to be fed depends upon the degree of exhaustion which is present, and the quantity which it is desirable to administer at a time. When the exhaustion is great, and the patient rapidly emaciating, or, when the stomach is irritable and can only retain small quantities, it is necessary to feed three or four times a day; but in most cases twice is sufficient, as there are few patients who cannot be prevailed upon to take something in the intervals.

The most convenient articles for administration with the stomach-pump are a pint of new milk, with a flipped egg and a glass of sherry in it, or an equal quantity of beef-tea, thickened with arrow-root; but the diet, as well as the medicine which it is proper to give, must vary with the nature of the case, and at the discretion of the medical officer. The many cases of complete recovery of patients who have required feeding with the stomach-pump for a long period are, perhaps, those upon which the asylum physician looks back with the greatest feeling of satisfaction, as being due to his care and attention. Other patients might have got well had they been properly attended to and nursed at their own homes; but these would certainly have perished had he not assiduously sustained the powers of life during mental aberration.

Cases of recovery are not uncommon of patients who have been fed with the stomach-pump daily for a period of three months; and such a case, in which the disease was acute dementia, has only lately been discharged from the West Riding Asylum. Other instances, in which the disease has resulted in a less fortunate termination, have shown that the œsophagus need present no lesion, nor any abnormal appearance, after the protracted use of the œsophageal tube.

Dr. Ward's paper on the treatment of insanity by the hypodermic injection of morphia is based on eighteen observations; and the following are his general conclusions: "The above cases will be, I think, sufficient to show that in this mode of treatment we have at least a valuable adjunct to our other remedies in the treatment of insanity, and especially in those cases where there is refusal or great unwillingness to take medicine, as by this means that amount of exhaustion is avoided, of which a struggle between the nurses and patient in administering a dose of medicine must always be productive. In some of the cases of chronic mania the effect was indeed almost magical, and it is in these where I have seen most special benefit from its use. In the cases of puerperal mania the general effects appear to be good, as, indeed, we should expect, from the effect in them of opium given by the mouth. In one case the injection of a third of a grain of morphia acted almost as a charm where seventy grains of chloral had failed to produce any effect on the previous night.

Another case is also interesting, as illustrating the deferred action of morphia by injection, and showing that the delayed effect of the drug cannot always be ascribed, when given in the ordinary manner, to its non-absorption into the system. The results in melancholia have not been so good as might have been expected, but three of the cases in which it was tried proved extremely unfavorable ones."

He adds that, in the cases in which the injections were continued for a long time, no constipation or unpleasant symptom of any kind, except once a small abcess, was induced.

Dr. Fox's paper on "Ergot of Rye, in the Treatment of Mental Diseases," is little more than a supplement to the previous researches of Dr. Browne, described in his last letter. He gives six cases in which the effect of the remedy was specially marked; and they are certainly very convincing ones. I extract as a specimen one of the most briefly recorded of these cases:

T. B., male, aged forty-four, admitted 12th of May, 1870, from Bradford, having been discharged about two months previously. For several years past he has suffered at wide intervals, from outbreaks of furious excitement, in which he is destructive and dangerous. At the time of his admission he had just experienced one of these outbursts. The next came on on the 24th of December. It continued for nearly three weeks, and occasioned considerable damage to property, being but little affected by antimony and ipecacuanha, which were administered. Excitement again appeared suddenly at 3 A. M. on the morning of the 4th of June. The patient got out of bed, broke into fragments the window-shutter and door of his room, and the furniture which it contained, and bellowed horribly and uninterruptedly. His complexion was scarlet, his pupils were contracted, his pulse bounding; while the superficial veins of his neck were raised like cords above the surface. Liquid ergot (in ʒ iss.-doses) was administered at 3 P. M., and again at 7 P. M. It seemed to soothe him immediately. During the night he was noisy at times, but the following morning, after two more doses, he was quiet and docile. In the afternoon he announced himself quite well, only a little tired and dizzy. The next day he returned to his usual occupation. He fully appreciated the effects of the ergot, and said that he "never felt any thing take such a hold of his head."

In another case, E. B., a female, aged twenty-nine, who had been turbulently and dangerously maniacal for eleven years, with occasional intervals of comparative lucidity, and who had "exhausted the pharmacopœia," ergot of rye has this year been given with gratifying results. The excitement is now thoroughly controlled by a few doses. Since taking it she has exhibited unusual somnolency.

Dr. Fox believes that he has seen it secure rapid recovery from the *status epilepticus*; and he adds that he has remarked that bromide of potassium in some cases aggravates the attacks of epileptic mania. This fact is of interest, as in a certain degree corroborating Dr. Hammond's views regarding the danger of giving this salt in large doses in certain cerebral states.

The form in which the ergot is given is that of the fluid extract, given in doses of a drachm or a drachm and a half, three times daily.

Dr. Moxon, of Guy's Hospital, has published (in the *Medical Times* for August 26th) a "case of cerebral rheumatism treated by the cold bath," which, although it terminated fatally, is deserving of notice for the commentary which that able physician has attached to it.

"R. B. H., aged twenty-three, a policeman, was admitted with all the symptoms of acute articular rheumatism, on November 27, 1871, having been suffering for three days previously. He was at once put under alkaline treatment; and nothing worthy of special notice was observed till the 13th of December, when a sudaminous rash appeared on the trunk, and a rub was heard over the heart's surface synchronous with its action, but only during inspiration. He was too ill to be raised in bed. Although his whole condition was worse than ever, he now said he was much better and free from pain; but his eyes glistened with a singular appearance, and his manner was excited. In the night he was delirious, and the next morning his temperature was 105°, pulse 102, respiration 321; perspiration and urine were acid in reaction.

"The next day the temperature was 106°, pulse 104, and he was quite delirious. A slight systolic rub was to be heard over the heart, but only on inspiration, so that the probability of pleurisy outside the pericardium was inferred. A grain of antimony was given every quarter of an hour until four grains were given, but the temperature rather rose than fell.

"The condition of the patient was now very threatening. Muttering delirium, carphosis, and intense heat, showed that he would soon be sinking. He was then taken from his bed and put into a cold bath, the temperature of which was 64°. The water was about twelve and a half inches deep in the bath, and its temperature rose during the immersion from 64° to 68°, while the patient's temperature fell from 106.2° to 98°. When in the bath, water was poured over his head to prevent cerebral congestion, if it be so caused. On removal from the bath he was put to bed, and rubbed dry and wrapped in woollen blankets. He was quite conscious at the time he was in the bath, and continued so for a quarter of an hour afterward, but his delirium and carphology then returned gradually, and his temperature rose slowly and gradually, reaching, at five in the morning, 105°; the patient very delirious. Dr. De Liefde, the house-physician (who carried out my wishes with his usual ability and firmness), then gave brandy up to three ounces in the course of three hours, but without any benefit. At half-past nine the next morning Dr. De Liefde saw him, and thus described his condition: 'He was very delirious, caught hold of the bedclothes, the curtains, or my coat and hands. The pulse was 172 per minute, as far as I could ascertain, but full; his face looked congested; he mumbled with his lips, and his skin was of an arid heat; the temperature of axilla 108°. Considering that, in my opinion, he was materially benefited by the former bath, which, I believe, had already prolonged his life, I determined to follow out the instructions of Dr. Moxon, and immersed him again. He looked very blue in the face and was very dilirious, so I did not keep him in longer than four minutes, and, on his removal, had him rubbed again by the nurses. His condition was not much changed by the second bath; he remained blue; temperature, 103.2°; pulse, 152; and continued to be delirious. On my return to the ward after an absence of half an hour, he had died ten minutes ago.' "

Dr. Moxon adds that he was led to employ the cold bath, in this case, through the belief " that patients, in cerebral rheumatism with intense fever, die of the heat immediately; indeed, are, in a way, seethed to death in their own fluid." In a case in which the temperature was 109°, the patient was ordered to be bled, and died during the operation; in another case where the temperature was 104.5°, and the patient was becoming excited, frequent doses of brandy (℥j for a dose) were administered, but the temperature rose rather than fell. Hence he was led to the trial of the heroic treatment adopted in the present instance; and " the history of the case shows an immediate, decided, and great relief from the use of the cold bath, all the active symptoms disappearing with the fall in temperature. It is true this benefit was only temporary, but so was the remedy; and though, when the remedy was repeated, a less effect was obtained, yet it will be seen that the patient was then so far gone that the house-physician did not think it safe to reduce his temperature below 103.5°, itself a temperature of high fever with delirium. The use of cold suddenly to such an extent was determined on in this case partly on account of the urgency of the case, and partly because it seemed to me desirable to get the effect of it decidedly, so that as little doubt of its efficacy as possible should remain in the minds of those who saw the trial. But it is possible that an equally good effect would be more surely and safely obtained by cold sponging or continuous cool bath. As we know that such a temperature as we were dealing with actually causes the white blood-corpuscles to cease their movements and contract closely, while at 113° some elements of the blood begin to coagulate, it is certainly necessary to prevent the temperature reaching these heights by all possible means, and we do possess entirely efficient means." [As the words *carphology* and *carphosi* may be new to many of our readers, I may observe that they seem to refer to " picking at the bedclothes," there being a Greek verb, *καρφολογεω*, which has that meaning.—G. E. D.]

In the *British Medical Journal* of the same date Dr. Wilson Fox reports two similar cases of hyperpyrexia in acute rheumatism, treated by cold applications. In one of these the temperature was 110°, and in the other 107.3°—there being deep coma in the former, and delirium in the latter.

In the former case there was, on the fourteenth day of illness, a rise of 8° within twelve hours. Ice-water, poured over the body, and ice applied to the chest, abdomen, and spine, reduced the temperature within an hour to 97.4°. The treatment by cold, in various forms, was continued for four days. This patient (a woman) from that time gradually improved. In the latter case delirium set in, with a rise of temperature to 107.3° on the seventeenth day of the disease. A bath at 86°, of twenty minutes' duration, reduced the temperature in an hour by 9.5°. Cold applications, consisting of baths, ice to the spine, and packing in wet sheets wrung out of iced water, and changed every half hour, were continued for seven days.

Dr. Fox observes that no treatment is nearly so efficacious in arresting the rapid rise of temperature as the present plan; and that, of the methods employed for the reduction of temperature, total immersion in a bath of from 60° to 80° is the most successful; but that it requires caution, because the fall of temperature persists long after removal from the bath to bed.

Dr. Nicol has contributed to the West Riding Lunatic Asylum Reports a paper of considerable value, and of much ingenuity, on "Progressive Locomotor Ataxia, and some other Forms of Locomotor Deficiency, as found in the Insane." He begins by giving in detail six cases in which these diseases were combined. It will be sufficient if I give the brief commentary he appends to the series:

"In the first of these cases it is noticeable that the form of mental disorder was at first in the direction of depression. The man had lost his employment owing to his advancing blindness, and a master whom he had long served died about that time, so that he had no resource but the workhouse, and there he became melancholic. In this state he was admitted to the asylum, but he does not appear to have continued long in a very depressed frame of mind. In October, 1869, a year and a half after admission, the melancholia seems to have left only its traces in a delusion of a hypochondriacal nature. By this time symptoms of ataxia were observable, and he became unable to stand. At the time of examination again his spirits were good, almost expansive in character; he was voluble, and spoke about getting better in an eager, hopeful manner. There is, therefore, in this case a decided transition in the mental state as the ataxia becomes developed, and the change is from depression to exaltation. In the second case ataxia is present to a great degree, and with it are delusions of an expansive nature, bearing the character of mental exaltation. In the third case there is again a state of mental exaltation with the ataxia. In the fourth case neither the bodily nor the mental symptoms are of a sort easily defined; the former point to commencing ataxia, especially the state of the optic disks, the frequency of urination, the analgesia of the lower extremities, the gait, and the difficulty in getting started to walk; the latter point to a sort of moral insanity. There has evidently not been time in this case for the spinal affection, if present, to impress its character on the mental disease. In the fifth case there are ataxic symptoms, with a certain degree of the same elevation of spirits over what would be the healthy average under the patient's circumstances."

These facts seem to point to at least one accompaniment, in the mental centres—that of lesion in the subordinate machinery which appears to us as ataxia. That accompaniment is the state of buoyancy—high spirits. Nor is there any thing improbable in this, even if we were to suppose that mind is in some respects out of the reach of body, for there can be no condition mentioned which would be more easily proved to depend often on grossly material conditions than precisely this one of buoyancy or high spirits. In animals it can be produced almost at will by certain physical treatment, and in men it can be controlled by drugs to a very considerable extent, even with our present knowledge.

But it is well to add at once what additional evidence can be obtained. Three cases have been put on record by Westphal, all of which had undoubted symptoms of locomotor ataxia, and, supervening upon this, insanity; the latter affection took the form of "mental excitement and *manie des* *grandeurs.*" In all three cases the spinal affection had shown itself (two, nine, and fifteen years respectively) before the patients were admitted under Dr. Westphal's care for mental alienation.

Here, therefore, is a certain amount of evidence—enough at least to justify us in asking whether there are any *a priori* grounds on which we might expect to find these two forms of disease linked to one another. To answer such a question it is necessary to insert some remarks as to the nature of what we designate "ataxia."

I cannot follow him through the long argument which leads him to conclude that "the afferent fibres for locomotive purposes are separate from those for cutaneous sensibility," or through the secondary conclusions at which he arrives. His observations on the connection between the disease of unstable powers and insanity reveal to a certain degree his "scheme of the physical framework of locomotor ataxia, drawn partly from the imagination, but in good part from facts."

"It can now be seen in what direction an explanation of the psychical phenomena of locomotor ataxy promoted to the brain is attempted. Granted that locomotor ataxia is a disease in which running away of power is an essential element, and granted that there is that openness for transmission of organic impressions toward the mental centres, which (as in hypochondria) is often so marked in the insane diathesis; or, on the other hand, that the necessity for decay which lies upon the afferent fibres of locomotion, and produced (as we say) the lesion there, may extend by anatomical analogy to other fibres which subserve the comparisons between objective circumstances constantly going on in our minds subordinately to our volitional life—fibres, namely, which, with help, collect all the impressions for us, carry them toward the brain, and often are privileged to draw power for an "ideo-motor" action of their own; granted that these too are involved in the ruin, and that the stored-up power they formerly set in motion is left to rush off wildly with each mad attempt to volitionize apart from a true sense of externals, and it will no longer remain a matter of surprise that exaltation is the phase of emotional life, which locomotor ataxia determines in the brain.

"Hitherto I have considered ataxia as developing into *manie des grandeurs.* It will be remembered, however, that, from the cases I have myself observed, the only general conclusion that could be drawn was that of a certain buoyancy, a state of high spirits. It will suffice very shortly to point to what the reader will no doubt remember for himself, that buoyancy of spirits is even a more general accompaniment of outflowing power than is the proper emotion of power, this last reposing, as it does, on a certain amount of education. Not by any means *all* outflowing power is so accompanied, but much is so, and especially when an excess runs off. Buoyancy, therefore, is very much to be expected, with any ataxia involving the conscious centres, in a patient who has in his previous life enjoyed the exercise of his powers to a fair extent. In this regard the case of W. K., who displayed ataxic movements on his right side especially, but also on his left, which had previously been paralyzed, and in whom a boyish height of spirits was apparent, is a very interesting and instructive one.

As the mental exaltation is the point mainly insisted on by the writer, I may briefly mention that in Case I. the patient "seems decidedly to take a happy view of things;" that in Case II. he (being a laborer) "believes that he has a great deal of property up and down; that the railway company owes him $2,200, etc.; that in Case III. he (being a workhouse patient) "says he has property in houses, and a crown of glory which his heavenly Father has given him, but no money, and fancies that he is very strong;" while in Case V. he says "he is happy and well, and is inclined to look at every thing in a happy light."

The paper concludes with full details of three other cases of locomotor deficiency, of a different sort from those commonly described under this disease. I have no space for saying more regarding them than that "the first arises not so much from loss of power as from disease of the power-storing centres, showing itself as instability of the latent force; the second from loss of power, and probably from degeneration of the cells to produce it; and the third partly from debility, partly from disorder of sensation, acting on a weak intellect. In the first there is ataxia, in the last two there is none; but the first has an ataxia of a different intimate nature, probably, from that producing the locomotor disease, which has, for a prominent feature, a running away of the nervous force which ought to be stored up till the will requires it."

Dr. Thompson Dickson, one of the physicians to the Infirmary for Epilepsy and Paralysis, has contributed three remarkable cases of *le petit mal* to the *Medical Times*. They are of interest as illustrations of the extreme mobility of some nervous systems, when brought within the influence of very slight subjective impressions.

Two of the patients were young ladies of an age between fifteen and sixteen, and the other was a boy twelve years old. In all the cases the operation of vaccination induced the attack. In the first case, just as Dr. Dickson began to scarify the arm, the girl, whose physical and mental development are good, became ashy white, and the next instant unconscious, and fell forward on a table against which she was standing. Almost the next moment she stood up, gazed vacantly around her, and re-presented her arm. When asked what was the matter, she said "she did not know, she only felt a little faint." Three or four years previously she had had a fit, but experienced no sort of attack in the intervening time.

"This girl's mother," says Dr. Dickson, "is epileptic; I have known the mother to be in the *status epilepticus* for twenty-four hours at a time. This child, however, was born before the malady had shown itself in the mother. But a brother of the mother was insane, and a maternal uncle of the mother was an epileptic; thus in the third generation the hereditary predisposition has, at an early age, become manifest in the development of a highly-mobile organization, and, though the positive manifestations have been but slight, the consequences hereafter may be very great."

The second case occurred in the other girl, in whom the signs of puberty are incomplete, though her physical development is rapidly maturing. She, however, partakes more of the child and less of the woman than my first case. "The extreme mobility of this young lady's nervous system I was quite aware of, and I was more than ordinarily struck with an observation I made upon it a few days before I vaccinated her. I was dining one evening at her father's house, and she playfully asked me to mesmerize her, and I, willing to gratify and amuse the child, made an attempt to do so, but for some time without any effect. After some minutes she laughed, saying, 'It is impossible to influence me;' but within the next minute she was almost completely under the nervous influence. Of course, I instantly desisted, and refrained from pushing the experiment further; but I noted the extreme suddenness with which her nervous organization had changed from a state of great excitement to one of very passive control. But to return. While vaccinating her, just as I had completed the scarification of the skin, I observed her turn deathly pale, and the next instant she fell into the arms of her mother, who was standing by. Her mother placed her on the floor, and for about forty seconds the patient was death-like and motionless; she was then seized with a convulsion, in which all the flexors were thrown into spasm (the fit exactly resembled one of the eclamptic attacks I have often produced in guinea-pigs by section of the spinal cord). She then turned on her right side, and then sat up, asking what the matter was. I gave her my hand, and she stood up, looked round the room, and again asked what the matter was. I desired her to lie down on a sofa, which she refused to do, saying, 'I'm all right.' She was, however, easily persuaded, and, when lying down, said, 'I hope I have not had a fit.' On questioning her, she told me that she had a headache, and felt giddy and confused, and that it was not the first time that she had felt so, though she had never told anybody. A few days afterward I learned from her that she is subject to frequent attacks of vertigo, and to occasional headache of an almost overpowering kind. She is a girl of very brilliant intellect, and possessed of abilities of a very high order; she has extraordinary vivacity. At the same time, she is devoted to reading and study, and will brood over books for many hours together. I learned, too, that she was occasionally subject to somnambulism; and I also learned that a brother of her mother had died insane. A sister of my patient died of cerebral disease which had manifested itself as epilepsy; and her father, although a man of large mental calibre, is the subject of vertigo. I have not been able to learn for certain whether the taint extended back to a former generation (the subject is one of extreme delicacy with the family), but the mother hinted to me on one occasion that her grandfather was at one time insane."

The third case occurred in a boy, aged twelve years, who had been epileptic from infancy, the *status epilepticus* sometimes lasting twenty-four hours. He had, however, experienced no attack for eighteen months before he was brought to be vaccinated. "His mother," says Dr. Dickson, "brought him in compliance with his own wish, and after he was vaccinated he sat down on a chair while his mother was being operated upon. After some four or five minutes, he put his hand up to his right ear, and said he had a pain in the ear. On placing my hand on that organ I found it intensely cold. After sitting for one or two minutes longer I saw him become blanched. He then got up, and ran toward the door, and had just touched the handle, when he reeled round on the right foot, and fell forward. I reached him in time to catch him in my arms, and I laid him, rigid and insensible, but unhurt, upon the floor. He was insensible about thirty seconds. He then became a little flushed, was seized with one very slight convulsion, opened his eyes, and the next instant fell asleep. He slept about ten minutes, after which he sat up, and said he felt very ill. He did not know what the matter had been with him; he said all he remembered was that somebody had shaken him. His mother asked me the question whether it was a fit or whether he had 'only fainted.' On the morning of the day on which the occurrence took place it was noticed that the child refused his breakfast. His breath had a fetid odor, and he admitted that his bowels had been confined for some days.

"In this case the family history pointed to hereditary taint. The child's maternal grandfather died of cerebral disease, though the malady did not appear in any of his children or grandchildren, or in any member of their families, until this child's first attack. On the paternal side the family history is good. Although the exciting cause of the fit I have described was undoubtedly the intestinal irregularity, the case teaches much from the fact of the slightness of the determining circumstances. He admitted that he did not feel the vaccination; it was to him, therefore, merely an idea. The phenomenon was, consequently, subjective, but subjective impressions are as exhausting to the cerebral cells as objective, and, as shown by the cases I have given, readily upset the nervous equilibrium or balance of control.

"The special interest attaching to the two first-mentioned is the fact that they were first, or almost first attacks, and therefore there was perhaps some excuse for their friends mistaking the manifestations for faintings. The mother of the second-mentioned, however, recognized and at first admitted the epileptic character of the affection, though she afterward persuaded herself, and insisted among her relations, that the attack was only a faint. The cases, however, were all instances of genuine epileptic seizure, and all reached the degree of complete loss of consciousness. It is certainly remarkable how unwilling relations and friends are to admit the fact of epilepsy, and how ready they often are to stifle their fears with the assumption that the patient has 'only fainted.' It is certain that the form of epilepsy we call *le petit mal* bears in its outward form so strong a resemblance to syncope that it is of the highest importance to study minutely all the circumstances of a patient the subject of fainting, lest, under the assumption that the attack is a mere faint, we overlook the graver condition."

After noticing the confirmation which these cases afford of the fact that the proximate cause of the epileptic phenomenon is cerebral anæmia, and observing that both girls, while professing to be brave, were afraid of the operation, he comes to the question of treatment, of which the essential part, in his opinion, "is to endeavor to teach the patients themselves to exercise such judgment in all their actions that they may, if possible, prevent recurrence of the attacks. Every effort must be made to ward off attacks and to nourish the brain-tissue in such subjects. All exhausting influences, particularly excitement, and long abstinence from food, must be carefully avoided, and the due performance of all natural functions adequately and normally sustained. Epileptic patients, particularly young girls, are often self-willed. It is, therefore, highly necessary to win their confidence, in order to persuade them of the necessity of adopting rules and habits of living which are essential to their good."

Girls are often captious and capricious about their food, and neglect to take it with that regularity which in an epileptic is necessary to avoid exhaustion. To starve at one meal-time and eat inordinately at another is a certain means of nursing epilepsy, since it brings about alternate irritation and exhaustion. Irregularity in attendance to the calls of Nature, also, is a neglectful habit to be watched and remedied. Constipation is a frequent attendant of epilepsy, is often increased by neglect, and is a very frequent excitant of an epileptic seizure.

"Precaution also must be observed in regard to direct excitement. Not long since I witnessed a young lady in a ball-room drop, as though dead, into her partner's arms while she was waltzing. Her loss of consciousness was complete, and, although she was able almost instantly afterward to walk to a chair, it was some minutes before she recollected where she was. She told me that she was quite aware of her malady, that she was subject to seizures of the kind, and she never could waltz for many minutes without feeling giddy; but that the temptation to waltz whenever she had the opportunity was so great that she found resistance of it almost impossible. In *le petit mal*, as indeed in epilepsy of every degree, there is no circumstance too small to be unworthy of notice."

"Hydrophobia in Dominica" is the title of a recent article in the *Medical Times*, by Dr. Imray, who has long practised on that island. This paper presents several points of great interest. In the first place, we learn that till the present year hydrophobia in man, and rabies in the dog, were unknown in the island; secondly, that rabies seems to have been spontaneously produced in a dog, which formed a focus for the disease; thirdly, that, in the case of hydrophobia attended by Dr. Imray, there was no evidence that the patient (a robust negro, aged about forty-five) had ever been bitten by any animal; and, fourthly, that, although he could not bear the sight of water, he waded through a stream in escaping from the hospital little more than twenty-four hours before death.

It appears that about the beginning of the year a dog was brought from a distant part of the island, and in the course of a week after arriving at its new quarters exhibited signs of rabies, which, as the disease was unknown, were not recognized at the time. It bit at the dogs in its reach, and the distemper soon spread among them. These rabid dogs attacked cattle, sheep, and pigs, and one of them bit four children on the the same day (in April). Twenty dogs died on the estate where the outbreak occurred, and many more were killed as soon as the symptoms appeared.

Six weeks after the children were bitten, one was taken ill and died in two or three days, with all the symptoms of hydrophobia, and a week later another was attacked and died in a similar manner. The locality being remote, they were beyond the reach of medical assistance, but there could be no doubt regarding the nature of the disease.

We now come to the history of the first case that ever came under professional observation on the island, and which is further remarkable in consequence of the uncertainty in regard to the source of the virus. Moreover, the fear that is regarded by some skeptical physicians as the cause of the disease cannot have acted here, since the patient had never heard of the existence of such a disease.

The early part of the history of his illness, previous to his admisssion into the hospital, was obtained from his wife:

"On Friday, July 28, 1871, M. N. went to work in his garden. After putting out some guano, he felt something itching his right hand, and he began scratching it. When he got home he complained that, although he had only scratched his hand, it was becoming painful. During the night he felt as if there were something running up the arm to the shoulder. On Saturday he remained ill at home, the pain in the arm increasing. On Sunday he was worse, the pain extending to the head, and there being attacks of trembling. During the day his hand trembled when he took water, and he did not know what was the matter with him: it seemed as if there was something inside afraid of the water; he felt as if there was a bar across his throat and chest, and his heart 'jumped up when he tried to drink.' On Sunday night he often called for water, but ordered it away when it was brought. 'The thing inside,' he said, 'don't want to see water.' Said he was thirsty and hungry, but could not drink or eat. On Monday he was in bed all day, complaining of weakness and faintness. He had the same horror of water, and yet a craving for it; he asked for some in a phial instead of a cup, and said to his wife, 'Don't give it to me to my face, but bring it behind my back.' He took it quickly, and, with a struggle, carried it to his mouth and swallowed it with great difficulty." On Tuesday he was removed to the infirmary at Roseau, and came under the care of Dr. Imray, who describes his case as follows:

"August 1st, M. N., a tall, spare, strong-looking black man, apparently between forty and fifty years of age. He is extremely agitated and frightened; his eyes wild and staring. He speaks rapidly and eagerly; says he is very ill, but does not know what is the matter with him; describes minutely his sufferings, and hopes I shall be able to do something for him. Surface cool; free perspiration; tongue moist, with yellowish coating; pulse quick. His great restlessness and agitation were quieted somewhat by speaking encouragingly and soothingly to him. He stated that, while putting out some manure in his grounds, ants crawled upon his hand; he brushed them off, but his hand and arm hurt him afterward. He does not remember being bitten by a dog, or any other animal. Some thin arrow-root ordered; part taken with great difficulty. Any attempt to swallow is accompanied with most distressing agitation; he rises from his bed with terror in his countenance, calls for people to hold him, and, unless there are two or three people to take hold of his arms and body, he will not make the effort. With the left hand he suddenly grasps the cup or glass with a great effort, and, assisted by some one also holding the vessel, it is brought to his mouth, the head is thrown back, a quick gulp of the liquid is taken, and, with a most painful struggle, it is swallowed. This is repeated two or three times, and he sinks down in a state of utter exhaustion, and bathed in perspiration. In the evening he was able to swallow a draught with half a grain of muriate of morphia. He slept for several hours after this draught. Toward morning he awoke; he sprang from his bed, rushed out of the infirmary, and ran to his house at the outskirts of the town, wading through a stream to reach it. Soon after, he was brought back by his wife. He was then in a state of great agitation and terror. He swallowed a draught containing a quarter of a grain of morphia, which had a slightly quieting effect. Every attempt to swallow was accompanied by the phenomena already described, only with augmented violence. About 11 o'clock P. M. I administered to him a small glass of sangaree (wine, water, sugar, and nutmeg), which he thought he would try. In this were dissolved forty grains of hydrate of chloral. I held the glass to his lips while the fluid was swallowed in repeated portions. The unfortunate patient took the draught mouthful by mouthful with great resolution, but with pain, horror, and agitation, very distressing to witness, the whole surface being almost washed with perspiration. He would not even look at the glass until he had been firmly held by several persons. The only effect of the chloral hydrate was, to make him a little drowsy. The same violent paroxysms recurring at short intervals, another dose of fifteen grains of the chloral hydrate was given, but with no good effect; the paroxysms still continued, and he died, exhausted, about eight o'clock in the morning of August 3d."

There was no *post-mortem* examination.

If the poison of the ants, to which he referred, had set up inflammation of the lymphatics of the arm, the local symptoms and feverishness might be thus explained; but on this hypothesis we have no clew to the special hydrophobic symptoms.

"Dextral Preëminence, or Right-handedness," formed the subject of a paper lately read to the Medical and Chirurgical Society by Dr. Ogle: 1. After a very brief account of the chief explanations which have been given of right-handedness, the author advances numerous arguments against the most generally-accepted doctrine, that it is based on conventional argument, enforced by educational influence, and has no natural foundation in our physical conformation. Of these arguments the following are the chief: The preferential use of one side is not limited to the arm, but extends to the leg, which is not subjected to education as the arm. The tendency to use one side preferentially manifests itself before education begins, and often persists in spite of the effort made to overcome it. Left-handedness resembles many physical malformations in being hereditary, in running in families, and in attaching itself rather to the male sex than to the female. Statistics are given of its relative frequency in the two sexes. Men are not the only animals with a tendency to use one side preferentially. The author gives an account of his observations in this matter on monkeys and on parrots. 2. Having shown that there must be some one or other structural foundation for right-handedness, the author next considers what this may be. He shows that in right-handed persons the left hemisphere is proved to be preëminent over the right by its lodging the faculties concerned in speech, etc., and that in left-handed persons the right hemisphere has a similar superiority. This latter statement, the probability of which was suggested by the author several years ago (St. George's Hospital Reports, vol. ii., 1867), is supported by three cases of asphasia in left-handed persons, accompanied by left hemiplegia, which the author has himself seen, and a fourth recorded by Dr. Jackson. So that right- and left-handedness would seem doubtless to depend on a natural predominance of the left and of the right hemispheres respectively. 3. Inquiry is then made, whether any structural differences between the two hemispheres can be detected; and it is shown that while the left is the more complex in right-handed persons, the contrary is the case in left-handed individuals. This latter statement is based on the examination of the brain, in two left-handed subjects. The specimens were exhibited, and also tracings of them by Dr. Broadbent. 4. Finally, the question is considered, What is the cause of the greater development, as a rule, of the left hemisphere? It is argued that it depends probably on the left hemisphere receiving a freer supply of blood than the right one. The results of the author's observations as to the relative sizes of the arteries on the two sides of the neck are given; from which it appears that the left arteries are, as a rule, slightly larger than the right ones. It is also shown that, independently of the size of the vessels, the stream of blood is less hindered on the left side than on the right. Lastly, it is shown that this explanation is consistent with and corroborated by the peculiarities of the cerebral blood-supply in those other animals which, like man, manifest a tendency to use one side preferentially to the other—such as parrots.

Mr. Savory expressed his surprise that the question of dextral preëminence had been treated with reference to the extremities only. We found a corresponding departure from symmetry in every part of the body. Every microscopist knew that he had a favorite eye; and it was always more easy to wink with one eyelid than with the other. The septum nasi was not in the median line; and the power of smell of the larger nostril was superior to that of the other. Mastication, without any reference to decayed or painful teeth, was performed usually on only one side of the mouth. Every nursing woman suckled her child more at one breast than at the other; and almost every one slept constantly upon the same side. With regard to the complexity of the cerebral structure, he should have liked to hear something about the ganglia at the base of the brain, the so-called sensory ganglia, as well as about the hemispheres; although he admitted the great difficulty of such an investigation. In respect of the blood-supply, he thought that the general teaching of physiology was opposed to considering this as a cause of growth, and led us to regard it rather as an effect of nutrition. The well-known transplantation of a cock's spur, by Hunter, as well as the periodical activity of certain organs, seemed to point in this direction.

Dr. Charlton Bastian, like Mr. Savory, felt doubtful of the correctness of Dr. Ogle's suggestion, that greater blood-supply might be a cause of increased growth. With regard to the general question, he thought the view taken about the reason of dextral preëminence would depend upon whether we regarded man as the result of a single creative act, or of a complex process of evolution. He had lately made a *post-mortem* examination of the head of a man, who had in his lifetime been remarkable for great intellectual power, and who had been from childhood blind of the right eye. In that case there was a very remarkable excess of size of the right over the left hemisphere, the former measuring longitudinally, over the vertex, five-eighths of an inch more than the latter. Four or five years ago, he had made and published a series of observations on the specific gravity of the brain-substance, and had satisfied himself that the gray matter of the left hemisphere was specifically heavier than that of the anterior lobes; on account of the greater admixture of white communicating tissue in the former; he thought that the greater weight of the gray matter of the left hemisphere might also possibly be due to the larger proportion of communicating fibres, required by its greater complexity of structure and greater functional activity.

Dr. Ogle, in reply, said that he had not been unmindful of the desirableness of investigating the condition of the sensory ganglia, but that the difficulties in the way of doing so had as yet been too considerable to be overcome. As regarded the question of blood-supply, he had plainly stated in his paper that it might be either a cause or a consequence of increased growth, and he thought the balance of evidence was in favor of the former supposition. In very young rabbits, after section of the vaso-motor nerve in the neck, he had observed hypertrophy of the ear on the side operated upon, attended, in some instances, by increased growth of hair. He acknowledged the importance of Dr. Bastian's observations about the different specific gravities of different parts of the brain, and thought that these observations told in favor of his argument.

Dr. Richardson's "Discourses on Practical Physic" (London, 1871), especially the first of them, treating of "Physical Disease from Mental Strain," deserve a notice in this letter. He divides the *subjects of mental strain* into six classes, all of whom are mental workers, but in whom the character of the work is different. First, there is the mere copyist, including the clerk, the compositor, and the reporter. Secondly, there is the thinker and writer. Thirdly, there is the speculative man. Fourthly, there is the man who thinks and works for others more than for himself, as the professional man in his different form of statesman, clergyman, medical man, and lawyer. Fifthly, there is the artist. Lastly, there is "the learner and the student; the child or youth, whose will is hardly his own, who works when he is bidden, and plays when he is permitted; who is fed too often with flattery or blows, and, between or by one or the other, is at length turned out in life prepared, as it is thought, by education and training, to fight the great battle of life."

Passing over his remarks on the diseases characteristic of the first two classes, I shall briefly notice his remarks on the ailments of speculators and of the professional class.

"The ailments of the speculator are usually compound in character; for he is, in most cases, a man of active life, and the whole of his organism, muscular and nervous, is equally taxed. If he be a betting-man, the race-course or some other out-door pursuit calls him into the open air. If he be a gambler, he is subjected to considerable muscular fatigue. Hence it follows that he is exposed to a variety of exhausting influences. His first symptoms usually commence with irregular action of the heart, and this is followed by results pertaining to a failure of that organ. In the majority of cases he succumbs, after exposure, to some subacute inflammatory disorder. He takes cold, suffers from congestion of the lungs or kidneys, and, unable to bear the shock, sinks rapidly under it, his mind becoming intensely irritable, or even losing its balance. Often he does some foolish thing, trips in his calculation, and is pronounced 'insane.'

"In the members of the professional class the brain is constantly being exercised without enthusiasm, and the body is daily being exercised without any sufficient rest. The result is, that the excitement of brain, which leads to insanity, is exceedingly rare, and that those physical ailments which follow as secondary to the overworked brain become developed. The professional class suffer largely, therefore, perhaps mainly, from physical affections. Diabetes, a physical nervous malady, is so common among them that I once had six gentlemen, following learned professions, under my care for this disease, at the same time; and, for many years, I have never been without one or more of such cases Paralysis of the limbs, with little interference of the mental faculties, is another common type of disease. Affection of the kidneys, degeneration of the structure of those organs, is a third condition; and disorganization of the structure of the heart is a last and by no means a rare occurrence."

The extent to which over-mental strain is injurious to the young varies according to the kind and character of the work. The endeavor to fill the minds of children with artificial information leads to one of two results. Not unfrequently in the very young it gives rise to direct disease of the brain itself, to deposit of tubercle if there be any predisposition to that disease, to convulsive attacks, or even to epilepsy. In less extreme cases it causes simple weakness and exhaustion of the mental organs, with irregularity of power. The child may grow up with a memory taxed with technicals, and impressed so forcibly that it is hard to make way for other knowledge; and, added to these mischiefs, there may be, and often is, the further evil that the brain, owing to the labor put upon it, becomes too fully and easily developed, too firm, and too soon mature, so that it remains throughout manhood always a large child's brain, very wonderful in a child, and equally ridiculous in a man or woman. The development in an excessive degree of one particular faculty is also a common cause of feebleness.

As an illustration of the danger of constantly forcing a single faculty, he tells the story of a boy whose originally good memory was cultivated to such a degree that he could learn off fifty lines of "Paradise Lost" at a single reading. On going from school to the university he was beaten by every fellow-student in the learning of detailed and detached facts. For a long time he made mistakes that were most annoying; he was unable, for instance, to cast up accurately any column of figures, he forgot dates, he ran over or under important appointments, misnamed authors in speaking of works of art or letters, and, in reasoning, his want of analytical power was painfully felt. It took him full ten long years to unlearn his wonderful technical art.

"For the reasons given," says Dr. Richardson, "I have always persistently opposed the special prize system in schools.

A teacher, with some experience of results of teaching, I can recall no single instance in which noted prize-men *in early youth* bore away more than other men the prizes, that is to say, the successes, of after-life. I have, however, many, many times known the successful prize-man in the class the least successful afterward, and as often have known the most ordinary youths, in class, come out as the best men in life. Overwork in the child and in the student defeats its own object; it does not develop the powerful brain so necessary for the man: for life is ever a new and great lesson, and some young brain must be left free for the reception of lesson on lesson. Of this there need be no doubt, and there we may leave the first and leading fact. But the danger of overwork is, unfortunately, not confined to the brain; it extends to the body as a whole. When the brain is overworked in the growing child, however well the child may be fed, and clothed, and cared for, there will be overwaste of substance in proportion to the overwork. There will be stunted growth, and the formation of a bad physical body."

Among the special diseases, produced by and followed upon mental strain generally (in all his six classes), he specially notices diabetes, paralysis (local or genaral), intermittent pulse, and arterial relaxation with arterial murmur; while among the diseases in which it excites latent or intensifies actual symptoms, he places certain chronic diseases of the skin, cancer, epilepsy, and insanity itself.

"Respecting insanity," he observes, "I doubt whether it is ever the result of simple mental overstrain; on the contrary, I take it rather to be an upshot of extreme mental inactivity; but when the tendency to it is pronounced, then mental strain excites the malady.

"The psychological physician is accustomed to look at mental as evolved from physical, or social, or transmitted causes, acting on the mind through the body. I have been trying to indicate, in the present effort, physical devastations as evolved from agencies acting on the body through the mind. I think I see the reverse side of a subject which has not often been discussed, the relation of mental to physical disease, and the picture thus presented is singularly instructive. It tells me that the origin of insanity, as a concrete fact, is rather to be sought for in inactivity, hereditary and individual inactivity of brain, than in exercise of brain; and that excessive exercise of brain is a cause not of mental, but of physical derangement. Our uneducated, cloddish populations are, in short, as I venture to assume, the breeders of our abstract insanity, while our educated, ambitious, overstraining, untiring, mental workers are the breeders and intensifiers of the worst types of physical disease."

Art. V.—*Preputial Calculi.* By J. G. Kerr, M. D., Canton, China.

The cases of preputial calculi in the accompanying table are specimens of a complaint comparatively rare in Western countries. A paper, in a previous number of this Journal, will have shown the comparative frequency of urinary calculus in Southern China. Many cases of *urethral* calculus have been met with, and some specimens are in my possession, in which the diameters are one and one-fourth by one-half inch. Coexistent with these, as to latitude and climate, is the form of calculus now under consideration.

Phymosis is not an uncommon deformity among the Chinese, and it is, of course, only in cases where this abnormal condition existed that preputial calculi could form.

The cases included in the table were operated on in the Medical Missionary Society's Hospital in Canton, and I need but refer to the simplicity and entire freedom from danger of the operation, required for the relief of these cases, to show how utterly destitute of surgical skill the physicians of the oldest nation on the earth are, and how much need there is of giving them the knowledge and appliances of western science and art.

A reference to the table will show that in every case except one the calculi were multiple. In two cases the number exceeded one hundred; in one there were forty, and in three others there were between twenty and thirty. In one or two cases the calculi were uniform in size and shape, but, in most of those where they were numerous, there were a few large and many small ones. Some had smooth, even surfaces, while others were angular, with facets.

The most remarkable of the cases was No. 7, operated on in 1862. The patient was thirty-eight years old. The prepuce was much thickened and enlarged, and the presence of calculi was easily discovered by a probe. A crucial incision was made in order to remove the stones, and the superfluous thickened skin was removed by circumcision. The calculi were two in number, and the weight one ounce and a quarter. The diameters of one were one and five-eighth by one and one-eighth inch. It had two concave facets, on opposite sides. One facet which received the second stone was smooth and polished. The concave facet on the other side was precisely like the first, except that it was slightly roughened by recent deposit. The second stone was one and a half inch in diameter, and was almost a perfect double convex lens in shape. One of its sides, where it rested on the other stone, was worn quite smooth, and it had evidently been shifted from one side of the first stone to the other, thus maintaining the completeness of the two concave facets. The calculi from case No. 2 are in the possession of Prof. S. D. Gross, of Jefferson Medical College, Philadelphia.

I will add an account of two more cases, not included in the table, because neither of them is strictly either preputial, urethral, or urinary calculus :

Yip Akwong, aged twenty-one, was admitted into the Medical Missionary Society's Hospital, July 27, 1867 (then in charge of F. Wong, M. D.), with an accumulation of calculi, in a sac half the size of a walnut, on the under side of the penis, about its middle, and communicating by a small opening with the urethra. It had existed since his childhood, but had given rise to no difficulty in passing water until recently. The sac was laid open by a free incision, and a large number of calculi removed. They were of various sizes, whitish, smooth, and shining. Four of them were about the size of pigeons'-eggs, with irregular surfaces and angles. Six were smaller, oval in shape, and angular. There were two hundred and eighty one small pieces, varying in size from that of a pea to a millet seed. Total number, two hundred and ninety-one.

The other case is, strictly speaking, one of lithotomy, but the stone was in an unusual situation:

A patient, aged fifty years, from Hoi-ping District, was admitted with stricture of long standing and urinary fistulas, which he persistently asserted had existed only about one year, but the appearance of the parts indicated a much longer duration. On probing the fistulas, a stone was discovered, located on the right side of the contracted scrotum, near the groin. The fistula was enlarged toward the perinæum, and the stone extracted. It weighed two and a half drachms, and its diameters measured one and a half by three-quarters of an inch. Its appearance indicated that it was composed of uric acid.

Cases of Preputial Calculi.

No. of Case.	Age, years.	Date of Operation.	Number of Calculi.	Weight.			REMARKS.
				oz.	dr.	sc.	
1	58	1856	1		2		
2		"	103		2		All smooth.
3		"	24		2		Smooth and angular.
4		"	2			2⅓	
5		"	12				
6	63	1860	40		1	2	Uniform in size and shape.
7	38	1862	2	1	2	2	One had two facets in which the other (being double convex) fitted.
8	47	"	4				
9		1863	6			1	
10		"	6				
11		1866	4		2		Surface rough.
12	72	1867	116	1			" "
13	42	1869	6		1	1	
14	56	1871	5				Also had stone in bladder.
15	8	"	22			1	" "
16	39	"	28			1½	
17	75	"	2			½	
18	39	"	11		2		Three large and eight small.

Clinical Records from Private and Hospital Practice.

I.—*A Case of Complete Placenta Prævia, complicated with Breech-Presentation; Hydrocephalus, and Death of the Fœtus.* By W. W. Hewlett, M. D., Babylon, L. I.

Mrs. N., aged forty-one, experienced a moderately profuse vaginal hæmorrhage at the termination of the ninth month of gestation, which was accompanied with only a few slight uterine pains. The hæmorrhage recurred three times during the succeeding three weeks, alternating with a very offensive mucous discharge. On December 17th, *over three hundred days* from the date of the last catamenial discharge, the patient was suddenly seized with uterine pains and a profuse and alarming hæmorrhage from the vagina.

The attendant was summoned in haste. On vaginal examination, the os was found to be dilated to about the size of a modern penny, through the opening of which a soft fleshy mass, having a corrugated feel, could be detected. No presenting part of the fœtus could be felt. The finger was carried around the uterine rim in the adjacent vicinity of the internal os; then withdrawing it, I proceeded as quickly as possible to tampon the vagina with a long strip of roller that had been previously soaked in vinegar-and-water. In the momentary space of time that elapsed from the withdrawal of the finger to the effectual introduction of the tampon, the hæmorrhage was exceedingly profuse. The patient fainted; the lips were thoroughly blanched, and no pulsation could be detected in the radial artery. After elevating the hips with the pillow that had been lying under the head, and applying an abdominal bandage with a view to forcing the presenting part of the fœtus down against the placenta, I sent for a consulting physician, J. R. Mowbray, of Bay Shore, Long Island. Reaction soon ensued, and during an interval of two hours, embracing the doctor's arrival, the patient was in a condition of comparative safety. When more rigorous pains came on and the blood began to flow around and through the jammed-up folds of the tampon, the doctor easily and rapidly withdrew it; finding the os considerably dilated, he protruded two fingers *through* the placenta, intending to rupture the membranes and bring down the presenting part. (I felt quite certain that it was a breech-presentation, from the result of several previous explorations of the abdomen.) Ascertaining that two fingers were not sufficient, the whole hand was introduced, the membranes were ruptured with a good deal of difficulty, and a foot was brought down which was secured with a fillet. After allowing the patient an interval of rest, during which the leg acted as an efficient tampon, the hand was again introduced and the other leg was seized and the foot brought down. Delivery of a dead fœtus with a flaccid and enormous cephalic extremity was soon effected through the placental rent, leaving the placenta *in utero.*

The placenta was adherent anteriorly, and detached with some little difficulty. The organ, in structure, looked more like a liver than a normal placenta. It would measure scarcely more than seven inches in either diameter, and was fully two and three-quarter inches thick; the maternal surface presenting a perfect cast of the lower fifth of the internal uterine ovoid.

The mother made a slow but complete recovery.

II.—*Cases of Ovariotomy.* By A. S. Woodward, M. D., Brandon, Vt.

On the 19th day of December, 1870, I was called to Mrs. O'Connel, a native of Ireland, fifty-three years old, and the mother of thirteen children. She claimed to be quite healthy up to last June, when she noticed her strength was failing. In July, menstruation suddenly ceased, and never appeared afterward. For the past few weeks she has been confined to her bed, with entire loss of appetite, and rapidly-wasting strength. Her frame is greatly emaciated; pulse 90; constant nausea with occasional vomiting, expression anxious and dejected. The abdomen is greatly distended, to which her attention was first directed last June. The neighboring women, however, say that they had suspected pregnancy for more than a year. Palpation of the abdomen revealed the presence of something of greater density than air; fluctuation was not distinct. The uterus could not be reached through the vagina. The roof of the pelvis was occupied by a resisting symmetrical mass that completely blocked up the superior strait, offering an obstruction to the passage of the finger beyond the pubes, against which the vagina was pressed and flattened. The girth of the abdomen at the umbilicus was forty-seven inches. Patient has never been tapped. Diagnosis, ovarian cyst. She was removed to an accessible room, and an attempt made to rally her appetite and strength, but she soon became unmanageable and impatient for the operation. On the 26th of December, while under the anæsthetic influence of sulphuric ether, and in the presence of Drs. Perkins, Wakefield, Peck, Northrop, Pond, Bassit, Carpenter, and Atwood, an incision was made in the usual manner, and with ordinary care, from the umbilicus to the pubes, opening into the peritoneal cavity. Neither the sound, nor hand, as far as it could be introduced, revealed any formidable adhesions. A large-sized trocar was introduced into the cyst just below the umbilicus, and about one pint of yellowish fluid containing lymph-shreds evacuated, which was all that could be made to escape. The trocar was withdrawn and introduced again and again, in several different places, with no better success. The walls of the cyst were now incised sufficiently to admit the hand, which served to break up the internal structure and evacuate several quarts of fluid. It was quite impossible to get at the upper portion of the tumor without extending the abdominal incision, which was done as far upward as the cuneiform cartilage. A strong fibrous band passed across that portion of the cyst exposed by extending the abdominal incision, segmentating it, and presenting the appearance of having the transverse colon firmly bound to it. Each segment was incised and the hand introduced, and twelve quarts of water collected in a pail was the sum total from the entire cyst. It now became evident that the remaining portion of the tumor, which was composed above of the partially-collapsed cyst, and below of fibrous tissue (upon which, and embedded into it, was the womb, four inches long), must be removed as expeditiously as possible and at all hazard, as a pretty free hæmorrhage was gushing up from the interior of the cyst, and from the incised walls. About this time also, from getting too much ether, the patient rapidly sank, and for a brief space her immediate death seemed inevitable; happily, on withdrawing the ether, this state passed off. Two assistants, one on either side, by grasping the tumor, could only raise it two or three inches out of its bed, in consequence of adhesions which seemed to involve much of the posterior wall of the tumor; they were strong and spread out laterally into broad bands, like the broad ligaments of the womb. As much as possible they were removed by the hands, but the knife was required at many points. Nine ligatures were required before the pedicle (if it may be called such) was reached and isolated. Storer's clamp was placed around the upper portion of the vagina, and the vagina severed above the clamp. It may on first thought seem quite impracticable to apply a clamp to the vagina, and have sufficient length to bring the clamp out and rest it cleverly upon the abdomen. It must be borne in mind, however, that the womb had been by the growth of the tumor, carried up high above the pubis, elongating the vagina as a result. After cleanly sponging, the abdominal walls were closed by needles, which were made to penetrate the peritonæum, and the clamp placed transversely. As soon as the woman rallied from the ether, one-third of a grain of sulphate of morphine was injected under the skin, and repeated at intervals of three hours during the night.

The tumor proved to be fibro-cystic, and, as before stated, twelve quarts of serum were caught in a vessel; while a quantity, variously estimated from two to six quarts, escaped upon the floor. It is to be regretted that the fibrous mass was not weighed. It was not convenient at the moment to do so, and, when preparation was made to do it, it was discovered that the tumor had been buried by the superstitious husband, who declared, after a shammed search, that it could not be found. It was larger than the water-pail which held the serum, and in the opinion of all present could not have weighed less than twenty pounds.

December 27th.—Patient slept several hours during the night, without pain. Pulse 100; surface cool; no thirst.

Ordered the morphine to be continued, and beef-tea and brandy injections every third hour. *Evening.*—Has slight pains in the abdomen and some tenderness. Pulse 104; treatment continued the same.

December 28th, morning.—Slept very well through the night, slight vomiting, stomach too irritable to retain food; indeed, from that time it continued so irritable that little or no food was retained ever after. The only sustenance retained was given by the rectum. Pulse 110, skin moist and cool; treatment the same. *Evening.*—Has slept quietly all day, skin moist, pulse 110; some tension and tenderness of the abdomen. Wound suppurating slightly. Solution of carbolic acid applied over the stump of the pedicle. Tr. opii was substituted for morphine, and given by the rectum. *Evening.*—Less tenderness and tension of the abdomen; pulse 100.

December 30th, morning.—Pulse 108; pain and slight soreness of the abdomen; treatment the same.

December 31st.—Condition remains the same. Clamp and needles removed; wound dressed with adhesive straps.

January 1st.—Condition remains unchanged.

January 2d.—Pulse 104; bowels moved by an enema of warm water; the evacuation of wind and water was large, and relieved the abdominal distention very much.

January 3d.—Pulse 110. More restlessness, sleep disturbed; attributed to the change from morphine to opium; changed back to morphine. A sero-purulent discharge is issuing through the opening in the abdominal wall. At the lower angle of the wound which has not closed, the pedicle retracting after the clamp was removed, is the supposed cause of the non-union. A solution of chloride of soda was injected warm through the opening into the cavity of the peritonæum.

January 4th, morning.—Slept quietly through the night; pulse 104. The soda again injected. Morphine, brandy, and beef-tea, continued the same. *Evening.*—Very comfortable; pulse 100.

January 5th.—Prof. Perkins (to whom I am indebted for this record of the treatment, and who had immediate care of the case, as I reside too far from the patient to visit her daily) was taken ill yesterday, and, a knowledge of the fact coming to the ears of the family, they all took fright, and gave up in despair, so that the patient went twenty hours without nourishment or medicine. Patient's pulse, 125; good deal of nausea; nourishment taken into the stomach is raised without much effort; enema, followed by the passage of a large quantity of liquid fæces and gas. Treatment the same, with the addition of cream-and-brandy administered by the stomach.

January 6th, morning.—Pulse 115; slept quietly all night; has passed much gas by the rectum. *Evening.*—Pulse 110.

January 7th.—Pulse rapidly increasing in frequency, and losing strength and force; abdominal tympanites greatly on the increase; frequent vomiting and delirium. From this time the patient sank rapidly, and died on the morning of the 8th, thirteen days after the operation. To those who were in attendance it did seem as though the result would have been more favorable had the nursing been better. The loss of twenty hours' attention on the tenth day of the treatment, with the attendant fright, was the feather which broke the camel's back.

Remarks.—The subsequent retraction of the vagina after the removal of the clamp, which left an opening in the line of incision, I consider as only one instance of an inevitable occurrence, and one that would of a certainty occur again and again if the experiment were tried. I believe it, because in the first place there was sufficient length to the vagina to warrant any one in the first experimental trial; and, secondly, the retraction was too great to allow any one who witnessed it to believe in its practicability.

Mrs. Foster, widow, aged thirty-four years, native of Vermont, and resident of Virginia, consulted me on the 18th February, 1869, about a tumor which she had discovered in the pelvic region. She was never a strong person, yet considered herself during her girlhood as strong as most of her mates. Dates her decline from the time of an attack of enteric fever in 1863. Convalescence was very tardy, and attended for three consecutive months with metrorrhagia. She is small of stature, thin in flesh and muscle, mother of one living child, and a three-months' fœtus. By abdominal palpation and vaginal touch, a symmetrical tumor about three inches in diameter was revealed, occupying a position behind and above the uterus. Both the tumor and womb could be moved about quite freely; neither was tender to the touch.

Diagnosis, Ovarian Cyst.—*September* 15, 1869.—The patient passed the intervening time in Virginia. Her sufferings at this time were greater than at the time of the first examination; has "bad spells, attacks of colic-pain," as she calls them, lasting from a few minutes to from one to two hours, not so severe as to call for anodynes, as, by simply resting on her couch for a brief period, they would subside. The abdomen was quite as large as it would have been at full term of pregnancy. Vaginal touch revealed the presence of the tumor much enlarged since the last examination. From that time till the 24th of November she was placed on a meat-and-milk diet, with plenty of apples. The last two weeks of the time she took daily four grs. of quinine, and ʒj ferri tr. murias, divided into three doses.

At this date she was put under the influence of sulphuric ether, and, in the presence of Drs. Perkins, Northrop, Peck, and Atwood, an ovarian cyst and fluid contents, weighing twenty-seven pounds, were removed through an abdominal incision, extending from the umbilicus to the pubes. The pedicle, which was long and small (not exceeding two and a half inches in circumference), was tied with a double silken ligature, cut short and returned into the abdomen, and the wound closed by needles which were made to embrace the peritonæum. The only medicine required during her recovery was morphine. The needles were removed on the sixth day. The bowels were moved on the eighth. Was not allowed to turn herself in bed till the sixteenth day, and sat up on the twenty-ninth.

From this time the patient was in comfortable health till February 1, 1871, when she began to fail in strength, and was troubled with occasional attacks of "colic-pains," as she expressed it, not severe, but much like the attacks of pain suffered before the removal of the ovarian cyst. I saw her about the middle of April; she had been confined to her bed for two days by pain and soreness in the lower part of the abdomen. An examination by abdominal palpation and vaginal touch discovered a tumor situated in the same region (behind and above the womb) and about the size of the ovarian cyst when it was first examined.

Diagnosis, Ovarian Cyst.—My next visit was made about the 1st of June, when I found the cavity of the peritonæum filled with serum, which was unmistakably external to the tumor; pulse weak, and strength greatly reduced. It was the opinion of all the physicians consulted that the ascites was in some way dependent on the pressure of the tumor; and inasmuch as the patient was fast losing strength, and no way offering for the removal of the ascites without the removal of the tumor, it was determined upon for the earliest opportunity, which did not present, in consequence of several untoward circumstances, till the 15th of June. Sulphuric ether was again administered, and, assisted by Drs. Perkins, Peck, Mead, Northrop, Pond, and Atwood, the old cicatrix was followed by an incision extending its entire length. A trocar was thrust into the tumor, which proved to be solid. The walls of the incision were now drawn apart to the utmost extent, the hand introduced, and innumerable slight adhesions broken up; the pedicle reached, ligated with a double silken ligature, cut, and returned into the abdomen. The hæmorrhage, which at first was quite free, ceased after exposing the surface to the atmosphere for a short time. The abdomen was thoroughly sponged out, and the wound closed by needles, embracing the peritonæum. One-third of a grain of morphine was injected under the skin, as soon as the patient returned to consciousness, and repeated every fourth hour through the night.

June 16th, morning.—slept quietly through the night; suffering a little pain, and has some tenderness of the abdomen; pulse 120.

June 17th.—Has been restless all night; pulse 130; thirsty, bowels distended, painful, and tender. Morphine given freely with iced milk-and-brandy.

June 18th.—Has been failing all night. Died at 11 A. M.

Remarks.—The tumor weighed two and a half pounds. By a microscopical examination made by Dr. Mead, it was decided to be an encephaloid degeneration of the ovary Notwithstanding rapid waning of strength and the existence of ascites, a class of symptoms that has been considered characteristic of malignant degeneration, the diagnosis, as stated above, was ovarian cyst—because, in the first place, ascites was distinctly traceable in the family, while cancer was not. In the second place, the pain had been inconsiderable; and, thirdly, the patient had previously been the subject of an ovarian cyst.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, January 22, 1872.

DR. ABRAHAM JACOBI, President, in the chair.

THE following physicians were reported from the Comitia Minora as recommended for membership, and, by vote of the Society, they were admitted, viz.: Alexander Perry, Charles M. Fairbrother, Nicholas L. Campbell, Julius H. Tyndale, Gustavus Treskatis, Herman Loewenthal, and L. De Forrest Woodruff.

The President announced the death of Dr. Henry D. Bulkley and of Dr. Alexander N. Gunn. On motion, Drs. D. C. Carr, D. A. Hedges, and S. W. Roof, were appointed a committee to draft appropriate resolutions in the case of Dr. Gunn. Resolutions on Dr. Bulkley's death had been adopted at the Special Meeting which we have already recorded.

DISEASES OF THE PAST YEAR.

The report of the Committee on Diseases was read by its chairman, as follows:

The Committee on Diseases has the honor to present the following report upon the mortality and prevailing diseases of the past year:

The total mortality of this city for 1871 amounted to 26,976—a decrease of 199 from the previous year. It was equivalent to a death-rate of 28.6 in each thousand inhabitants.

Zymotic diseases destroyed in the aggregate 8,365 lives—being 31 per cent. of the total mortality—a slight increase upon 1870. The most prominent epidemic of the year was that of small-pox. This disease had prevailed to an unusual extent in 1870, having produced 293 deaths, but it had so subsided in the autumn of that year that in the three months ending December 31, 1870, it occasioned but 12 deaths. In the early part of January, 1871, it broke out afresh with extraordinary vigor, and rapidly increased during the ensuing six months. It caused 208 deaths in the first quarter of the year, and 304 in the second quarter. In the summer it began slowly to decline, its mortality being 164 for the quarter, and but 23 for the month of September. Up to that time the work of vaccination had been so energetically pursued by the vaccinating corps of the Health Department, that they had vaccinated 100,000 persons in nine months. At the very time, however, when the prospect was most favorable for entirely arresting the disease, the Board of Health was obliged, on account of the failure of its pecuniary resources, to dispense with its extra inspectors for some two months. The result was soon apparent in the extension of the epidemic, as, although only 23 deaths were due to small-pox in October, it occasioned 36 in November, and 70 in December. On November 24th the work of vaccination was resumed, but the disease had acquired too much new force to be readily overcome. Its total mortality for the year amounted to 805, its greatest yearly mortality recorded in this city during the present century, the next largest number of deaths, 681, having occurred in 1853. During the year 2,085 cases were reported to the Bureau of Sanitary Inspection. According to these figures, its mortality reached 39 per cent. About 47 per cent. of the deaths were among children of five years old or less. The greatest number of deaths, according to locality, were as follows: 17th Ward, 41; 22d Ward, 37; 20th Ward, 26; 21st Ward, 23. The Small-pox Hospital furnished 572. Its severity here during the past year, although remarkable for New York, was insignificant as compared with its ravages in many other places throughout the world. We may simply mention, for example, among foreign cities, the Hague and London. In the former (whose inhabitants have always evinced a peculiar antipathy toward vaccination) the mortality from small-pox during the first quarter of 1871 was enormous, reaching the extraordinary annual death-rate of 48 in the thousand inhabitants—a rate which would have produced in New York 7,500 deaths from small-pox in three months. In London there were registered during the year 7,876 deaths from small-pox in a total from all causes of 80,332. In a single week the fatal cases amounted to 288. The same death-rate here would have given us 2,300 deaths instead of 805. The most remarkable epidemic of small-pox upon this side of the Atlantic has devastated the city of Philadelphia. Nearly 2,000 fatal cases were registered during the past year, of which 1,812 occurred in the three months ending December 31st; 233 were recorded in a single week.

The next zymotic disease which displayed an unusual activity here during the past year was whooping-cough. It carried off 465 victims, being the greatest number of lives destroyed by this disease in any year of the present century, the next largest number having been 377 in 1855. Of the 465 deaths, 196 were males, and 269 females. The large preponderance of females over males is curious, but, as the death-records of former years show, is quite characteristic of this affection. As far as we are aware, it is unaccountable.

Remittent fever during the year caused 165 deaths; intermittent, 110; and typho-malarial, 12. These fevers were never so fatal before. They prevailed most extensively in the upper wards of the city.

There is little of interest to record in regard to the other zymotic affections. The following statement presents the mortality of the principal ones for the years 1870 and 1871:

	1870.	1871.
Scarlatina	975	791
Measles	298	409
Diphtheria and croup	729	704
Typhus fever	96	65
Typhoid fever	422	239
Diarrhœal diseases	3,956	3,653

The principal feature of the above statement is a considerable falling off during 1871 in the mortality from typhus and typhoid fevers and diarrhœal complaints. Six deaths were ascribed to cholera; 6 to relapsing fever; 2 to yellow fever; 142 to syphilis; 7 to hydrophobia, and 220 to intemperance. Typhus and typhoid fevers were most prevalent and fatal in the 17th Ward.

The following cases of contagious disease were reported to the Bureau of Sanitary Inspection during the year:

Typhus fever, 126; typhoid fever, 492; scarlatina, 2,850; relapsing fever, 80; measles, 1,626; diphtheria, 408; small-pox, 2,085.

Constitutional diseases produced 6,263 deaths, about 23 per cent. of the whole, against 6,329 in 1870. Of these, rheumatism caused 103; cancer, 339; marasmus and scrofula, 757; hydrocephalus and tubercular meningitis, 755; and phthisis pulmonalis, 4,186. The mortality from phthisis was equivalent to 15½ per cent. of the total, and was both positively and relatively large as compared with that of late years. In 1866, it was equal to 12.2 per cent.; in 1867, to 14.4; in 1868, to 13.7; in 1869, to 13.3; in 1870, to 14.8; and in 1871, to 15.5. We thus perceive that this formidable disease has been constantly increasing in fatality during the past six years. About one-quarter of its deaths took place in hospitals, and a large majority among the tenement-house population. The migratory character of that class of our inhabitants renders it almost impossible to observe and localize those regional causes which may influence consumption in this city.

Local diseases, or affections of special organs, occasioned 9,282 deaths, or 34 per cent. of the whole—a decrease of 617 from the previous year. Of these there were due to diseases of the nervous system, 2,677 deaths; circulatory system, 894; respiratory system, 3,248; digestive system, 1,052; urinary system, 1,163; generative system, 54; locomotory and osseous system, 120; and integumentary, 74. Among these there was little worthy of extended mention. There were 964 deaths from bronchitis, and 1,834 from pneumonia, against 855 and 1,836 respectively in the preceding year. Urinary diseases were uncommonly fatal, and they appear to have been becoming more and more so during the past few years. Thus the deaths by Bright's disease within four years have been as follows, commencing with 1868: 534; 557; 787 and 947—an increase of 77 per cent. during the period mentioned.

Developmental diseases (or those arising from abnormal action of the formative, reproductive, or nutritive processes) produced 1,763 deaths, or 6½ per cent. of the whole: 888 were in infants newly born; 312 in women; 333 were due to senile decay; and 230 to atrophy and asthenia. These figures were somewhat in excess over the previous year.

Finally, the deaths from violent causes amounted to 1,303, or 5 per cent. of the whole, against 1,000 in the previous year. The increase was owing principally to the July riot, which caused 53 deaths, and the Westfield explosion, 82. 1,070 deaths were ascribed to accident and negligence; 65 to homicide; 114 to suicide; and one was a judicial execution.

Of the total number of deaths, 7,994 occurred among infants less than one year old; 10,700 among those less than two years old; and 12,970 among children less than five years old; the last being 40 per cent. of the total mortality. In 1870, the deaths among such children were equal to 49 per cent. of the whole; in 1869 to 51 per cent.; in 1868 to 52½ per cent.; and in 1867 to 53 per cent. Altogether, therefore, between 1867 and 1871, there has been a gain among children less than five years old of 13 per cent. upon the total mortality. There were 1,296 deaths among persons 70 years old and upward, a gain of 74 upon the previous year; 14 were reputed to have been 100 years old or more, of whom 3 were males and 11 females.

Respectfully submitted,

Chas. P. Russel, M. D.,

Chairman Com. on Diseases.

CANTHOPLASTY IN CONJUNCTIVAL AND CORNEAL AFFECTIONS.

Dr. H. Althof read a paper entitled "Clinical Notes on Diseases of Conjunctiva and Cornea." Its main object was to call attention to an important element, hitherto rather neglected, in the therapeutics of these affections, viz., the diminishing of the pressure of the lids upon the globe. After a review of the present methods of treating the more serious forms of conjunctival disorder—granular lids, diphtheria, blennorrhœa, etc.—all of them only partially under the control of the physician, it entered more directly upon its special subject.

Thus far, three different procedures had been occasionally followed for the purpose of relieving the ball of the eye from excessive pressure, viz.: incisions or excisions of chemotic conjunctiva; removal of a triangular piece of skin (down to the muscle) from the upper lid; and splitting of the outer angle. This last method undoubtedly suited best; but its good effect was very transitory, the wound healing up in too short a time (a couple of hours) to give any permanent relief. If, therefore, a way could be found to modify this remedy, so as to make its results lasting, the therapeutic means at our disposal would receive a valuable addition. This Dr. Althof thought to have been found in the little operation known under the name of *canthoplasty*—the splitting of the external commissure down to the conjunctival sac, and careful uniting of the conjunctiva to the outer integument.

The results of this simple procedure had been very favorable, a large number of cases at present testifying to its efficiency. It had rapidly become popular in our great infirmaries, and was now, after five years' trial, considered by American oculists a very welcome remedy in cases which had baffled the most skilful efforts, and in diseases against which the prevailing treatment was almost powerless. Granular lids, and phlyctenular conjunctivitis and keratitis, furnished the great majority of cases in which canthoplasty might be employed with advantage — to cut off acute attacks, to diminish the number of relapses or prevent them altogether, and to give other remedies a better chance for effecting a perfect cure. In genuine diphtheria the doctor thought it almost indispensable; and blennorrhœa in its most acute and destructive forms lost most of its dangers.

Dr. H. D. Noyes agreed with Dr. Althof in his high estimate of the operation of canthoplasty. It had become as much a *sine qua non* in the treatment of trachoma as had iridectomy in that of glaucoma and diseases of the iris.

The remarks upon diphtheritic conjunctivitis had especially interested him from the fact that he had, within three weeks, had in his practice four cases of the affection, one of them very severe, involving the ocular as well as the palpebral conjunctiva. Up to six months ago his experience had furnished him with but few examples of this disease. Since that time he had had perhaps a dozen; and he believed the occurrence of these cases in such close succession was coincident with an unusual prevalence of scarlet fever, and of croupous affections generally.

As to the therapeutics of conjunctival disorders, there was less need of discovering new means of treatment than of learning precision in the application of those now known; for their number was already bewildering.

Dr. Agnew's experience had confirmed his belief in the bold use of canthoplasty. He had been led to modify the operation by division of the edge of the external tarsal ligament of the upper lid only, as exposed in the wound. It was the pressure of the tarsal cartilage of the upper lid which caused the chief trouble; and the incision of skin and mucous membrane alone would not always make such pressure harmless. He felt confident that, since the introduction of iridectomy, no other addition so important as canthoplasty had been made to the resources of ophthalmic therapeutics.

Dr. Edward Curtis could testify to the value of the procedure in phlyctenular conjunctivitis, the promptness of its effect seeming at times almost magical. He related a case, attended with great photophobia, cured by its means in a couple of days.

Dr. R. H. Derby thought that the advantage of the operation lay, perhaps, as much in its enabling the surgeon to reach the retro-tarsal fold with his applications, as in the relief from pressure it afforded. Von Graefe objected to its employment in diphtheritic conjunctivitis, from the danger that any wound in such a case might itself become the seat of diphtheritic deposit.

Dr. Althof had had only seventeen cases of diphtheritic conjunctivitis in five years, up to September last, though since then he had seen at least twenty. He would not set his own slight experience against Von Graefe's; but, although he had always found the diphtheritic deposit infiltrating the incision, as it does wounds of other parts, yet he regarded this as of slight consequence when compared with the great advantages conferred by the operation. By the time the diphtheritic trouble was cured, the wound was well healed, and no harm had come of it.

Dr. Roosa had never seen any grave consequences from the operation as performed by Dr. Althof, though he had several times seen ectropion of the lower lid follow, as a result, perhaps, of the incision having been made not horizontally outward, but obliquely downward. He had known, however, one or two instances of serious injury from the operation, where the dissection of the conjunctiva was made very thorough and carried into the orbital tissue. In one case the eye was lost.

Four years ago he had had a dispensary-case of phylctenular keratitis relieved most remarkably, in twelve hours, by the operation. He was not aware, at the time, that it had been previously performed for this form of disease.

Dr. Loring had been anticipated by the last speaker in most of what he was about to say. He had observed some half a dozen cases of ectropion due to the operation; and had several times seen trouble from too free a dissection of the parts. He thought the division of the external tarsal ligament, suggested by Dr. Agnew, should be limited to those cases in which trachoma had progressed to the extent of converting the inner surface of the lid into a hard, cicatricial tissue, which, by its contraction, had warped the tarsal cartilage, increasing its natural curvature, so as to bring its sharp edge against the cornea. He had himself formerly employed the method, originally proposed by Pagenstecher, of dissecting up the conjunctiva so as to make a considerable flap, the sides of which were so stitched to the skin, near the outer extremity of the horizontal incision, as to convert this into a wound, with its long diameter vertical. But this produced much greater deformity than the method followed by Dr. Althof; and, moreover, as before remarked, the liberal incision into the orbit was by no means free from danger. He had seen two instances of severe inflammation from it, one of them being that referred to by Dr. Roosa, in which the eye was destroyed.

He had, therefore, abandoned the plan in favor of the simple operation recommended in the paper. In this, as there stated, the stitch at the angle was the main point upon which success would depend.

Dr. Agnew did not mean to say that he would cut the tarsal ligament in every case, or that he would ever cut it in the lower lid. But he would incise it oftener than recommended by Dr. Loring, on account of the danger of relapse when this was omitted. Indeed, he had more than once been obliged to divide the ligament by a secondary operation, where the ordinary horizontal incision, previously made by another surgeon, had proved insufficient. There was no difficult dissection required, and no danger of orbital cellulitis. It usually sufficed to make the incision through the edge of the ligament, near its insertion into the periosteum of the orbital rim. It need not be cut in such simple cases as those of phlyctenular ophthalmia with photophobia.

CEREBRO-SPINAL MENINGITIS.

Dr. Russel said that, within three days, seven cases of cerebro-spinal meningitis had occurred in the city, three of which had proved fatal. At the suggestion of Dr. Peters, he read the report of an interesting case furnished by Dr. C. E. Simmons. The Health Board were anxious to study the disease, and asked, as a matter of favor, an early report of any cases which might be discovered.

The President had seen two cases on the east side of the city, near Dr. Simmons's case, one of them in the Hebrew Orphan Asylum, at Third Avenue and Seventy-seventh Street. He had also, by invitation of Dr. Rodenstein, been three times to see the cases under his charge at the Catholic Protectory in Westchester. There were some twenty of them; and five or six had ended fatally. The speaker pointed to the presence of stagnant water between Second and Third Avenues, in the upper part of the town, as connected with the prevalence of endemic diseases in that neighborhood.

CRIMINAL ABORTION.

On motion of Dr. S. Rogers, the Society

Resolved, That the Medical Society of the County of New York indorses the amendment to the law of 1869, relative to criminal abortion, proposed by the New York Medico-Legal Society.

Adjourned.

Special Meeting, January 29, 1872.

DR. A. JACOBI, President, in the chair.

OBITUARIES.

THE meeting was called to hear memorial notices of some deceased members, which had been prepared, under authority of the Comitia Minora, for transmission to the State Society. Dr. Fordyce Barker, to whom had been committed the notice of Dr. George T. Elliot, was unavoidably absent. Dr. Ellsworth Eliot gave an account of the life and labors of Dr. William B. Bibbins; and Dr. William C. Roberts reviewed the career of Dr. Henry D. Bulkley. The leading points of these memorials have already appeared in our pages. Dr. David C. Carr recounted the professional and other services of Dr. Alexander N. Gunn, dwelling especially upon the assistance he had always been ready to proffer to young men. As chairman of the Committee on Resolutions, Dr. Carr reported the following, which were adopted:

Whereas, It has pleased an All-wise Providence to remove from his earthly labors Dr. Alexander N. Gunn, a beloved associate, and an honored member of the Medical Society of the County of New York:

Resolved, That, in his death, all have lost a valued friend and counsellor, and this Society one of its brightest ornaments, distinguished alike for his eminent social qualities, his Christian virtues, and his professional ability.

Resolved, That, while we bow in submission to the Divine Will, we deplore the loss of an active worker in the profession to which he was an honor; of a man who, by his genial, sunshiny nature, won not only the hearts of his patients, but the confidence and esteem of all who knew him.

Resolved, That a copy of these resolutions be preserved in the archives of the Society, and also that a copy, signed by the President and Secretary, be sent to the family of the deceased.

The meeting adjourned.

A special meeting, called for February 3d, "to consider and take suitable action upon the bill, recently introduced into the Assembly by Mr. Flammer, entitled *An Act to protect the People against Quackery and Crime*," was very thinly attended, and adjourned without action.

Bibliographical and Literary Notes.

ART. I.—*A Clinical Manual of the Diseases of the Ear.* By LAURENCE TURNBULL, M. D. Philadelphia: J. B. Lippincott & Co., 1872, pp. 486.

IN his preface, the author says, "he has not simply recorded the views and opinions of others, but has added the results of his personal experience in seventeen years' private practice and in the treatment of several thousand cases in the Aural Department of Howard Hospital."

After reading the above we are rather surprised to find him speaking, on page 319, "of some fourteen hundred (!!) cases of ear-disease, which I have treated in the Howard Hospital, and in private practice. . . ."

From the freedom with which quotation-marks are used, we might imagine that the book *was* "simply a record of the views," etc., but we must admire the honesty which tells of so many quotations and translations which the great majority of readers would not have suspected. Pages 176–188, and 227–238, seem to have been translated entire from Gruber's work.

On p. 228, Gruber is represented as preferring *excision* of the tonsils "by application of caustics or astringents. It is easily performed by Fahnestock's instrument."

Among other specimens of loose English, he speaks of a glass jar (also made of tin)," p. 88.

Every book of the present day is to some extent a *résumé* of the opinions of others; but of the present work this seems to be particularly true.

One wishing to see what work has been done in the line of aural disease, may find this work very useful; but, regarding its arrangement and the limited amount of original matter contained in its pages, we are tempted to ask, Why was it written? or, having been written, why was it not called "Extracts from Recent Writings on Otology?"

Art. II.—*Diagrams of the Nerves of the Human Body.* By William Henry Flower. Edited, with Additions, by William W. Keen, M. D. Philadelphia: Turner Hamilton, 1872.

Considering the present and future importance of the nervous system in practical medicine, nothing could be more welcome to the profession than the cheap and excellent edition of Flower's admirable work which Dr. Keen has given us. In these diagrams we have presented the principal nerves of the body with their distributions. The distributions to the muscles are lettered in red, while those to the sensitive surfaces, as well as the names of the nerves themselves, are designated by black letters. To one moderately familiar with the anatomy of the nervous system, the arrangement is such as to present at a glance those intricacies which are so apt to elude the memory. Dr. Keen's own additions and alterations are, as he himself states, slight; yet, coming from one who is a thorough and practised anatomist, they fairly enhance the value of the original work.

Announcements.—S. W. Butler, M. D., of Philadelphia, has in preparation a Medical Register and Directory of the United States, by J. M. Toner, M. D. Lindsay & Blakiston announce a new edition of Bloxam's Chemistry; a third edition of Grant's work on Irritable Bladder; a second edition of Macnamara's Manual of Diseases of the Eye; and a Manual of Microscopic Mounting, by J. H. Martin. Alexander Moore, of Boston, is preparing a work on Consumption, by Carl Both, and one on the Leprosy of the Bible, by that indefatigable writer, B. Joy Jeffries, M. D.

The Chicago Medical Examiner is now published on the 1st and 15th of each month, instead of once a month, as formerly.

The National Medical Journal is in trouble. Both the editors have suddenly resigned, in consequence of some disagreement with the publishers.

Books and Pamphlets Received.—Cancerous and other Intra-thoracic Growths, their Natural History and Diagnosis. Being the substance of the Lumleian Lectures, delivered before the Royal College of Physicians. By James Risdon Bennett, Consulting Physician to St. Thomas's Hospital, etc. Five plates. London: J. & A. Churchill, 1872.

Plain Talk about Insanity: Its Causes, Forms, Symptoms, and the Treatment of Mental Diseases. With Remarks on Hospitals and Asylums, and the Medico-legal Aspect of Insanity. By T. W. Fisher, M. D., late of the Boston Hospital for the Insane. Boston: Alexander Moore, 1872.

On the Treatment of Fractures of the Limbs. By Samson Gamgee, Fellow of the Royal Society of Edinburgh; Surgeon to the Queen's Hospital, Birmingham. London: J. & A. Churchill, 1871.

First Semi-Annual Report of the Board for the Examination of and Licensing Druggists and Prescription Clerks, in the City of New York. New York Printing Company, 1872, pp. 24.

A Plea for Scientific Reform. A Letter to Rev. Theodore L. Cuyler, D. D., on the Attitude of Physicians and Scientists toward the Temperance Cause. By George M. Beard, M. D.

Report of the Special Committee on Meteorology and Epidemics, of the Philadelphia County Medical Society, for the year 1870. By Benjamin Lee, M. D., pp. 38.

The Detection of Criminal Abortion, and a Study of Fœticidal Drugs. By Ely Van de Warker, M. D., Syracuse, N. Y. Boston: James Campbell, 1872, pp. 88.

Medico-Legal Considerations upon Alcoholism, and the Moral and Criminal Responsibility of Inebriates. By Paluel De Marmon, M. D., pp. 24.

Electricity in the Treatment of Diseases of the Skin. By George M. Beard, M. D. Reprinted from the *American Journal of Syphilography*, pp. 13.

Proceedings of the Second Meeting of the American Association for the Cure of Inebriates, held in New York, November, 1871, pp. 115.

Annual Report of the Board of Governors and Board of Lady Supervisors of the Woman's Hospital of the State of New York, 1872.

Third Annual Report of the Trustees of the Willard Asylum for the Insane, for the Year 1871. Albany: Argus Co., 1872, pp. 42.

Animal and Vegetable Parasites of the Human Skin and Hair. By B. Joy Jeffries, A. M., M. D. Boston: Alexander Moore, 1872.

Report of the Foundling Asylum of the Sisters of Charity, in the City of New York, from October 11, 1869, to October 1, 1871.

Braithwaite's Retrospect of Practical Medicine and Surgery. Part LXIV., January. New York: W. A. Townsend, 1871.

On the Method of Creation of Organic Types. By Edward D. Cope, A. M. Philadelphia: McCalla & Stavely, 1872, pp. 35.

The Journal of the Gynæcological Society of Boston, vol. v. July to December, 1871. Boston: James Campbell, 1872.

Small-pox: The Predisposing Conditions, and their Prevention. By Dr. Carl Both. Boston: Alexander Moore, 1872.

Forty-Sixth Annual Report of the President of Harvard College, 1870 '71. Cambridge, 1872, pp. 87.

Transactions of the Iowa State Medical Society. Davenport: Griggs, Watson & Day, 1871, pp. 272.

The Influence of Speculative Beliefs in Medicine. By Z. C. McElroy, M. D., pp. 13.

Fourth Annual Report of the New York Orthopædic Dispensary. New York, 1872.

Reports on the Progress of Medicine.

OPHTHALMOLOGY FOR 1871.

By Henry D. Noyes, M. D., New York.

1.—*The Support of the Eyes during Expiratory Blood-pressure.* By Prof. F. C. Donders. [Archiv. f. Oph., xvii., 1, 80.]

On the occasion of a recent visit to England, Prof. Donders had his attention called by Darwin to a theory propounded by Sir Charles Bell that the closure of the eyelids serves to protect the globes against injurious pressure (during the act of coughing or sneezing) of the sudden rush of blood, caused by these expiratory acts. After some investigations Prof. Donders was convinced that in these acts the arterial pressure is increased, but quickly abates. Further, during expiration, the jugular vein becomes distended, and is emptied during inspiration; and its distention is not due to regurgitation of blood from the chest. On examining the vessels in the fundus oculi, it was found that, during forced expiration, the veins become larger, and light pressure on the globe with the finger makes the veins smaller. When the superficial ocular vessels are examined with the microscope, the arteries are little changed, while the veins increase in size during violent expiration.

It may justly be inferred that the lids under these circumstances give support to the circulation of the eye. The internal ocular circulation finds its greatest support in the normally tense condition of the globe and in the elasticity of its coats.

Other effects of forced expiration are distention of the orbital veins, and consequent protrusion of the ball. The degree of prominence of the eye varies with the width to which the lids are separated, and in each act of winking the globe moves to a slight degree backward or forward. In efforts of accommodation, the globe slightly advances.

2.—*The Eye in Acephalous Monsters.* By Prof. W. MANZ, Freiberg [Virchow's Archiv., Bd. li., Hft. iii., S. 313–349.]

By careful study of eight embryos born without brain, Prof. Manz endeavors to determine the relations of the optic nerve to the brain in its development.

If the accepted theory, that the opticus is merely an outgrowth from the brain, be tenable, it is greatly interesting to know what becomes of the nerve if the brain is wanting. The result of his inquiry was, that the structure which in these cases represents the nerve contains no nerve-tissue, and is in fact no nerve. In the retina, only the opticus fibres and the ganglion cells are deficient. The rest of the retina and of the eye present no anomalies.

After making this statement, an elaborate account is given of the peculiarities of the embryonic eye in its various structures. A few items may be selected:

The epithelium of the membrane of Descemet passes as a continuous layer from the cornea over the ligamentum pectinatum upon the iris. The stroma of the choroid has no pigment. The superjacent epithelium is well developed and filled with pigment-granules. The hyaline membrane of the vitreous is distinct from the capsule of the lens and from the membrana limitans of the retina; cell-forms pervade the whole corpus; there are no other vessels than the central artery, saving in one specimen, in which a branch of a posterior ciliary artery came directly through the membranes into the vitreous. The optic nerve looks normal, and has the usual double sheath; the intervaginal space is clothed with epithelium. As the nerve passes into the sclera, it does not undergo the constriction which belongs to the developed state, but continues of the same diameter as when outside the bulb. The nerve-sheaths pass into the sclera, and the nerve adheres intimately with the choroid, and nothing but the vessels mark the place of its connection with the retina. There is no border to denote the outline of the optic disk. The vessels spread out equally in all directions, not as when fully grown, the larger vessels going upward and downward. The arching toward the macula does not exist, and no macula lutea appears. All the layers of the retina except optic fibres and ganglion cells could be identified.

As the opticus was traced into the skull, it became flatter and was lost in a mesh-work of blood-vessels and connective tissue. The time when nervous structure appears in this cord is undetermined. It is clear, however, that the retina attains its development independently, and the nervous connection between it and the brain is established much later—the fibrous cord, which is first produced, serving as a nidus for the growth of nerve-fibres, but whence these come is not clear.

3.—*Historical Notice upon Basedow's Disease* (*Heart-Disease and Bronchocele, with Occasional Exophthalmus.*) By Dr. E. EMMERT. [Archiv. f. Oph., xvii., 1, 203.]

The writer, who has spent some time in England, where, according to his observation, the disease prevails more than in Germany, discusses the question as to whom the right of priority in discovery belongs. Graves's name is attached to it in England, and Basedow's in Germany and France. It appears, however, that in the library of Guy's Hospital is a work in two volumes, by Dr. Parry, printed in 1825, who gives a special chapter to

"Enlargement of the Thyroid Gland in Connection with Enlargement or Palpitation of the Heart." He gives histories of eight cases, in one of which there was exophthalmus; their striking features were heart-palpitations, strong carotid and weak radial pulse, and its frequency 113 to the minute, sometimes intermittent. In five more cases there were in addition epilepsy, deafness, headache, vertigo, and other nervous symptoms. The histories are next quoted in detail and are easily recognizable as the malady described by Basedow in 1840, and by Graves in 1843, but to which, if any man's name is to be attached, it should be Dr. Caleb Hillier Parry's.

4.—*Xanthelasma Palpebrarum.* By Dr. MANZ, of Freiberg. [Klin. Monats, August-September, 1871, 251.]

This case, a woman sixty years old, exhibited the yellow blotches on the skin of all the lids, and they were so large as to require removal from one upper lid. The microscopic examination showed them to consist of an hypertrophy of fatty tissue interspersed among fibres of connective tissue. The fat-globules were most abundant about the hair and other follicles. Cases of the above are common enough, but seldom call for operation.

5.—*Cases of Temporary Anchyloblepharon.* By Mr. C. J. WORKMAN. [Oph. Hosp. Reports, vol. vii., part i., February, 1871, p. 1.]

The above title does not convey the real idea of the paper, which is to set forth a simple and ingenious method devised by Mr. Bowman, to increase the effect of operations for ectropium, by temporarily uniting the upper and lower lids to each other. The method may consist in turning down or up, as the case may be, a tongue of skin 1½‴ line wide and uniting it to a prepared surface of the opposing lid, or the opposed edges of the lids may be carefully denuded on the inner edge for a narrow space and fastened to each other by fine sutures set deeply into the fibro-cartilage, and not removed for from five to seven days.

One of the cases had necrosis of the orbit and a fistulous opening, with subsequent ectropium, by slow contraction of the tissues. A plastic operation was done, and the lids united by a bridge. Several operations were needed, which were greatly assisted by the intercalated flap. This was maintained for two years, and, when severed, the eyelids were able to keep themselves in proper position.

The temporary adhesion of the lids by paring a part of their edges was successfully used in treating ulceration of the cornea provoked by ectropium, and also in a case of paralysis of the orbicularis during the time needful for recovery.

6.—*Kerato-Conus and its Treatment.* By Dr. STEINHEIM, of Bielfeld. [Archiv. f. Oph. und Otol., ii., 1, 166–176.]

We have the history of three cases treated by the method which Graefe proposed, whose purpose is to change the shape of the cornea, although its flattening is secured at the expense of a central leucoma. The summit of the cone is carefully sliced off to the extent of ¾ to ⅘ of a line, avoiding perforation. The ulcer is touched several times daily with a crayon of nitrate of silver, for two to four days, to set up inflammation and destruction of tissue, until, being made very thin, paracentesis is performed and frequently repeated. The ulcer is not allowed to heal until by contraction the form of the cornea is seen to approximate to the normal. After healing occurs, which will be in from three to eight weeks, an iridectomy for purely optical purposes completes the case.

Results in the three cases related were very satisfactory, and in one this is made graphically evident by the patient's skill in drawing. He made sketches of the appearances to himself, before the operation, of a dot, of a line, of a circle, and of other figures, and sketched them again after the operation. The cuts are interesting to study, as showing how strange were the dispersive forms, and how they were modified by the change in shape of his cornea.

7.—*Intermittent Blepharospasm.* By Dr. SEELIGMÜLLER, in Halle. [Klin. Monatsbl., Jahr. ix., August and September, 203.]

A farmer's wife, aged fifty-nine, had suffered for thirty years from blepharospasm. Her eyelids would from time to time close, and remain shut for some minutes, despite any efforts she could make to open them. After a while she would lift them with her finger. The attacks happened sometimes two or three times an hour, again as often as ten times an hour. The circumstances which provoked the occurrence were movements of the eyes, especially downward, movements of the head, especially to rotate it, when the patient would feel a cracking in the joints of the cervical vertebræ. Chewing hard substances often brought on the attacks. In the attempt to read or sew she would be constantly interrupted by the closure of the lids, and on some days could not work at all. The attacks would come more frequently when she had something in both hands than when one hand was free. Besides these and other physical causes, mental excitement was a sufficient provocative, such as anger or hearing "dreadful stories." A sudden noise or loud voice would excite the trouble.

Rarely, when the lids shut, could she open them without the help of her hand. Their appearance, when closed, did not suggest spasmodic action of the orbicularis, but resembled more nearly paralysis of the levator palpebræ, because there was no contraction of the neighboring facial muscles. She would sit as if merely asleep.

The history was as follows: She was perfectly well until the age of twenty-six, when she had severe toothache, chiefly on the left side, and the teeth fell out of themselves. Headache, too, took place, and more on the left side. Exposure to the weather, and a long journey on foot during a cold night, when she had toothache, were the direct causes of the beginning of the trouble. She began to have dimness of sight in near vision, and, while sewing, would have to stop, and rub eyes and forehead to clear her sight. From these frequent stoppages gradually the blepharospasm developed. Continuing to suffer from these symptoms, notwithstanding various liniments applied to the forehead, she submitted to neurotomy of both supra-orbitales. But this operation, as well as injection of morphia, faradization, and blisters, proved inefficacious. After its performance the disease was worse.

She applied to Prof. Graefe, who discovered a point behind the last molar of the lower jaw which, when pressed, dissolved the spasm, and the eyelids sprang open. He, however, declined to attempt any operation. Patient has been regular in menstruation, was married at the age of thirty-four, at forty-one had twins. Since menstruation has ceased, the spasm of the lids has increased. She is not in the slightest degree nervous or hysterical. Beside the spot behind the jaw, discovered by Graefe, other spots were found which, when pressed, would relieve the spasm. In some the pressure was painful, in others not. The first correspond in part with the painful points of Valleix—they were the supra-orbital foramina, the parietal bosses, the vertex, the transverse processes of the cervical vertebræ, the superior ganglion of the sympathetic in the neck, the brachial plexus under the clavicle, the spinous processes of the upper and dorsal vertebræ. Pressure rather light on these places would soon open the eyes. But strong and longer pressure on other places not painful, such as the left wrist, and the prominences of the radius and ulna, would have the same effect.

Treatment adopted was to endeavor by galvanization to cure the hyperæsthesia of the above painful spots. The negative-pole plate was placed on the middle of the dorsal vertebræ, and the bulb-shaped positive pole was applied to the painful spots for one to two minutes. From six to sixteen elements of a Siemens-Halske battery, with constant current, were used. The effect of the first sitting was highly gratifying. Patient could move the head and eyes in all directions, and could follow the finger as before she could not. At subsequent sittings, if the eyelids closed while the current was in action, a little stronger pressure with the electrode would open them. After fourteen days' treatment, with two sittings daily, the hyperæsthetic spots were not perfectly cured, but were much less sensitive, and the lid-spasm greatly mollified. I could not expect perfect cure from the electricity, and, as the patient wished to go home, I directed a blister to the nape of the neck, to be followed by cantharidal ointment, and the sore to be kept running. I heard, some weeks afterward, that this treatment did no good, and the patient relapsed into her previous condition. Cases similar to the above are cited from Arlt, Wecker, Mackenzie, from Handfield Jones ("Studies on Functional Nervous Disorders," London, 1870, p. 390), and from Dr. Broadbent, *Medical Times and Gazette*, July 9, 1870.

The case is not to considered one of local reflex spasm of the orbicularis, but rather a general disease of the nervous system.

8.—*Case of Blepharospasm.* Dr. LENNICH, Bonn. [Klin. Monatsbl., January to March, 1871, p. 55.]

A soldier was wounded by a piece of shell on the top of the head, tearing up the scalp for two inches behind the coronal suture on the left side. As soon as the wound healed, spasm of the lids of the left eye took place, but ceased when the cicatrix was incised. If the wound were prevented from healing, the spasm did not occur, but, when this was completed, the disease returned. Pressure on the scar relieved it, and it was fully cured by division of the supra-orbital nerve.

This case contrasts with that given above, being one of purely local reflex action.

9.—*Acquired Nystagmus in Mountaineers.* Dr. PAUL SCHRŒTER. [Klin. Monatsbl., June and July, 1871, p. 135.]

In both cases, one a man of forty-three, the other a man of thirty, the tremulous movement of the globes was first observed at evening, and it became so bad as not only to trouble vision by day, but to render it impossible for him to walk the streets at night. The trouble was greatest with fixation upward and straight forward. By looking far to the side, the eyes were for a time steady. Vision normal—a slight hypermetropia. Treatment was by induced electrical current to the eyes daily, and blisters behind the ears. After six weeks, considerable improvement occurred, but lasted only a short time, when the patient returned, and was subjected to the use of strychnia, by which he was cured, and remained well.

Both cases were much alike in history and symptoms, but the second case did not remain under treatment.

10.—*Upon the Method of Formation of New Blood-vessels in the Inflamed Cornea.* By Dr. W. H. CARMALT, of New York, and Prof. S. STRICKER. [Medizinische Jahrbücher, Wien, 1871, P. 3, S. 428.]

The experiments were undertaken upon the eyes of frogs and rabbits, in which corneal inflammation was excited by drawing a thread through the membrane, and leaving it until the desired effect occurred. As soon as vessels could be found, the cornea was examined by the microscope. The new-formed vessels were very numerous, and within them was found a mass of shining bodies of about the size of the nucleus of a blood-corpuscle. Outside of the vessels, and separate from them, were certain oblong, spindle-shaped bodies, standing perpendicular to the vessels, and filled with precisely similar elements to those contained in the vessels. Furthermore, these minute elements were found in the corneal substance.

These bodies were not precisely like blood-corpuscles, and are regarded as being formed in the inflamed tissue, and similar to embryonic blood-globules.

The blood-vessels may be found in the new tissue, both with and without continuity from the surrounding marginal vessels. They originate sometimes certainly in the fusiform cells above alluded to. Stricker himself says that he is convinced that vascular tubes are originally solid cylinders of protoplasm, which afterward become hollow.

11.—*The Treatment of Sämisch's Ulcus Corneæ Serpens.* By Dr. Hermann Pagenstecher. [Oph. Hosp. Reports, vii., 1, 21.]

The result of the experience of Dr. Pagenstecher is, on the whole, favorable to the method commended by Sämisch. The large incision through the ulcer gives great relief, and the wound is afterward kept open by pressure of the finger on the lids. The aqueous evacuated on the evening of the first day, and sometimes its outflow procured as many as three times in a day, usually twice daily. Seldom was it needful to use a probe or other instrument in the wound. But the reopening is not kept up so long as Sämisch indicates, being done rarely longer than a week, usually for only four days. Pressure bandage, atropine, and hot water resorted to as usual.

It may be added that the repeated opening of the wound by an instrument may give rise to serious irritation; a secondary purulent keratitis may be then excited, as in some cases I have seen. The value of the remark about effecting the escape of aqueous by means of light pressure on the lids will be therefore better appreciated.

12.—*Paralysis of Trigeminus followed by Sloughing of the Cornea.* By Dr. W. F. Norris, Philadelphia. [Trans. Am. Oph. Soc., 1871, 138.]

Cases of Paralysis of the Fifth Cerebral Nerve. By Dr. H. D. Noyes, New York. [New York Medical Journal, August, 1871.]

13.—*Report of Thirty-eight Cases of Paralysis of Accommodation, from the Clinic at Kiel.* By Dr. Scheby-Buch. [Von Graefe's Archives, xvii., 1, 265.]

Observations in Cases of Paralyzed Accommodation. By Dr. Colsman, of Barmen. [Archives für Oph. und Otol., ii., 1, 154.]

14.—*Peculiar Forms of Exudation in Iritis.* By Dr. H. Schmidt, Marburg. [Klin. Monatsblat., ix. Jahr., 1871, 94.]

Two cases are given. A man, twenty-three years old, had rheumatic iritis. On the second day a grayish semi-transparent exudation filled the inner half of the anterior chamber, and ten days after it had been again absorbed. Now a bluish-white transparent vesicle appeared on the previously-covered part of the iris, and reached a little distance up on the lens capsule. This continued two days, then vanished, leaving only a slight synechia. It is not stated that the mass was really a cyst, but that it resembled one.

Another man, aged twenty-six, with iritis, which is not said to be syphilitic, but for which mercurial inunction was prescribed. The anterior chamber very deep, and occupied by a gray, opaque mass, which looked like a luxated lens, above whose upper edge the pupil could be seen in a crescent about ¼ line high and 1½ line long, after atropia had been freely used. It was impossible to use the ophthalmoscope. Patient can count hands at 3½ feet. The diagnosis was luxation and cataractous opacity of the lens, with secondary irido-choroiditis. Two days after the anterior chamber was clearer. On the third day a hæmorrhage into the chamber took place. The upper edge of the mass now appeared to be serrated, and the free pupillary space had enlarged. It was now evident that there was no dislocation of the lens. In a week the mass was nearly absorbed. The last portions occupied the lens capsule, and were surrounded by free pupillary space. At a later period the lens were seen to be *in situ*. The vitreous filled with flocculi; and, finally, patient discharged with v. = ¼. The duration of treatment was six weeks.

15.—*On the Tables given by Loring and Knapp to show the Displacement of the Retina in Ametropia.* By Dr. O. F. Wadsworth, of Boston. [Transactions American Oph. Soc., 1871, p. 87.]

In the report made last year it was remarked that a table given by Dr. Knapp, to show the amount of actual displacement of the retina for any given degrees of ametropia as expressed by convex or concave glasses, differed in an important way from a table given by Dr. Loring, based on formula worked up by Dr. Mauthner. The remark was made that the discrepancy might be only apparent, yet, if it were, the subject is of such practical importance as to deserve elucidation. Dr. Wadsworth has been kind enough to investigate the calculations, and shows that the separate results may be harmonized. To quote his words:

"Loring, following Mauthner, in the use of a formula given by Helmholz, shows in his table the amount of displacement in certain degrees of ametropia. Knapp, using another formula given by Helmholz, shows the amount of displacement in degrees of ametropia, which are corrected by a lens, of a certain number of inches focal distance (positive or negative), placed at the anterior principal focus of the eye. The anterior principal focus of the eye being 20.29mm., almost exactly ¾″ in front of the second nodal point, we must therefore subtract ¾ of an inch from the number of the glasses given by Knapp, to obtain the degrees of hypermetropia, and add ¾ to obtain the degrees of myopia referred to in his table."

It is shown that there is no important error in the calculations of either gentleman, although, as the figures appear to the tolerably careful scrutiny of an unmathematical reader, there is a puzzling difference. A practical suggestion, which we hope to see carried into effect, is to form a complete table of the amount of displacement of the retina corresponding to all the degrees of ametropia between $\frac{1}{2}$ and $\frac{1}{48}$, taking the intervals, as Javal has done, at $\frac{1}{48}$. This to be done on the plan of Knapp's table, which includes the distance of the spectacle-glass from the patient's eye. An ophthalmoscopic observer can bring himself almost to as close approximation to the eye as the spectacles, and the formulæ calculated for spectacles would be almost correct for the ophthalmoscopist. Another table could be made in which the observer's eye should be allowed to be one inch from the cornea, and this would be constantly referred to in estimating the location of foreign bodies, the height of tumors, the thickness of exudations, the swelling of the papilla, etc. For example, take both H and M; for

$$\frac{1}{2},\ \frac{1}{2\frac{1}{4}},\ \frac{1}{2\frac{1}{2}},\ \frac{1}{2\frac{3}{4}},\ \frac{1}{3},\ \frac{1}{3\frac{1}{4}},\ \frac{1}{3\frac{1}{2}},\ \frac{1}{4},\ \frac{1}{4\frac{1}{2}},\ \frac{1}{5},\ \frac{1}{6},\ \frac{1}{7},\ \frac{1}{8},\ \frac{1}{10},$$

$$\frac{1}{12},\ \frac{1}{16},\ \frac{1}{24},\ \frac{1}{36},\ \frac{1}{48}.$$

Do this for spectacles at $\frac{1}{2}$ inch from the cornea, and for an ophthalmoscopic observer at 1 inch from the cornea.

16.—*On the Effect of Atropine upon Myopic Eyes.* FRED. HOREH. [Inaug. Dissert. Basel. 1871.]

There are 57 observations upon 29 myopic persons belonging to Prof. Schiess's practice in Basel, for whom a solution of atropine gr. iv ad ℥j was prescribed. The same practice had been followed by Dobrowlsky. In 11 instances no immediate benefit ensued. In 46 cases immediate good effect was produced. In 31 cases permanent improvement was obtained. This was decided by an examination of the refraction and vision, not only after the treatment had been suspended, but after the patient had resumed his usual work for weeks or months. After the lapse of a still longer time, about half of these patients continued to show the improved condition which treatment had effected: 10 had relapsed to the *statu quo*, and in 5 only had myopia progressed. The result shows that, by suspending the accommodation, one of the most potent agents in making myopia progressive is set aside. The treatment was maintained for various periods from four weeks to six months.

17.—*Remarks on Cataract.* By Dr. E. G. LORING. [Trans. Am. Oph. Soc., 1871, 105.]

To correct the astigmatism which often results after cataract extraction, Dr. Loring proposes to cement together a spherical and cylindrical glass. To reduce the weight, he makes the spherical a plano-convex glass whose outline is circular, and its diameter that of the spectacle-frame, the plano-cylindrical has an oval outline, and the two together are lighter than a common biconvex spherical glass of two or three inches' focus.

The glasses may be cemented so as to be very strong in their adhesion, and no more liable to separate than telescope-glasses.

Another point which the paper discusses is, that the standard of perfect success after cataract extraction has in late years undergone a change; that, for flap-extraction, Graefe in his first 1,500 cases adopted v. = $\frac{1}{2}$ as "perfect" and v. = $\frac{1}{3}$ as also "perfect," if patients were over seventy-five years old; that now Dr. Knapp, Dr. Becker, Dr. Derby, of Boston, and others, give statistics in which first-class or "perfect results" are based upon v. = $\frac{1}{10}$.

Dr. Loring attempts to show that this change of standard must have a modifying effect upon the prevailing opinion of the great superiority of the new peripherical linear over the old flap operation: that, estimated by the acuity of sight, the flap sustains its prestige better than it has lately been considered that it could.

Following Dr. Loring's paper, an article appeared, by Dr. H. Derby, in the *Boston Medical and Surgical Journal*, November 23, 1871, claiming that Von Graefe did not apply the standard of v. = $\frac{1}{2}$ to all his 1,000 flap-extractions, but only to about the last 100 of them. To this Dr. Loring rejoins in the *Boston Medical and Surgical Journal* of December 28, 1871.

Without following either of the disputants in the details of their argument, this point at issue whether Graefe in 1863, meant by "perfect success," v. = $\frac{1}{2}$ or "ability to read fine print," which Dr. Derby thinks may permit v. = $\frac{1}{10}$. This point is easily decided by reference to Zehender. Klin. Monatsbl., for 1864, page 343, which gives a discussion of the Heidelberg Congress on cataract operation, and certain remarks of Graefe. His language is: "I am now occupied in preparing statistics of 1,000 flap-extractions, and I divide their results into four classes:

"1. Perfect result; vision equals at least $\frac{1}{2}$, the patients can also read the finest print.

"2. Vision is greater than $\frac{1}{30}$; patients can only recognize the largest test-type, but, if the eccentric vision is in due proportion, easily get about.

"3. Patients can at least count fingers at one foot.

"4. Patients unable to see as much as this, but have either the minimum of qualitative perception or quantitative perception of light, or can see no light whatever."

The statement that perfect vision means $\frac{1}{4}$, and better, is extremely explicit, and must make every candid reader feel sure that v. $= \frac{1}{4}$ is what Graefe says he obtained in his first class of cases. If this flat statement needs any confirmation, the second category, which descends at once to v. $= \frac{1}{30}$, would abolish all doubt.

It is evident that, in comparing methods, we must criticise results with greater care. At the same time, that a larger number of less absolutely perfect results may be attained by the peripheral linear than by flap is, if it can be proved to be a fact, a very strong argument in its favor when the issue between one method and another is to be tested.

It remains to be seen whether by Graefe's operation the number saved from entire failure or useless vision is not greater than by the flap. But the flap may be able, perhaps, to claim that its measure of success, in scale of sight, exceeds that of the peripheral linear.

Hence we may yet arrive at the conclusion to adopt one or the other kind of operation, according to the chances of success in a particular patient.

We seem destined to an endless succession of suggestion, as to the mode of making incision for extraction. Dr. Liebreich tells us that three hundred times he has made a cut transversely across the cornea just below the horizontal meridian, using Graefe's knife and not doing iridectomy. Statistics and visual acuity not given.

Dr. Galizowski, in his late treatise, finds reason for an incision at the outer margin of the cornea with a lance-shaped knife—a modified Weber method.

Dr. Canstatt combines reclination and extraction—thus: a slightly-curved needle is passed into the sclera close to the lens, corneal section is made upward, the aforesaid needle is used to pry the lens and capsule through the pupil; by light pressure they come out. Five cases have been thus treated, and "with perfectly satisfactory result." That is all we know about them.

18.—*The Modern Operation for Cataract.* A Lecture by Dr. H. Derby, of Boston, with an Analysis of Sixty-one Operations, 1871.

This is a lucid and detailed account of Graefe's mode of operating, and subsequent treatment of the cases. Besides quoting statistics from Graefe, Becker, and Knapp, Dr. Derby adds the results of sixty-one cases on which he operated, and they are as follows: Failures, 3; undetermined, 3; unrecorded, 5; vision $\frac{2}{3}$ to $\frac{1}{10}$, 44; vision $\frac{1}{11}$ to $\frac{1}{30}$, 6. Total, 61. Of the 44 "entire successes," v. $\frac{1}{2}$ and better $= 19$; between $\frac{1}{2}$ and $\frac{1}{10} = 25$. There were 8 with v. $= \frac{2}{3}$, and also 8 with v. $= \frac{1}{10}$, and 9 with v. $= \frac{1}{5}$.

19.—*On Extraction of Cataract.* By Prof. von Hasner, [Vierteljahrschrift für die Praktische Heilkunde, xxviii., 1871, Bd. 11, 73.]

The drift of this article is to call attention to the fact that the most serious dangers threatening success after cataract extractions are not from suppuration of the wound or of the cornea, but spring much more commonly from inflammations of the iris, ciliary body, capsule, vitreous, and choroid. An important factor leading to these complications is imperfect opening of the capsule, and consequent partial retention of lens-substance.

Instead of a simple linear incision either vertical or horizontal, Prof. von H. favors an inverted V-shaped incision, the apex of which shall go far down toward the equator. (Prof. Hasner usually does the simple flap-extraction without iridectomy.)

In spite, however, of this precaution, capsular cataract may ensue by the folding in or wrinkling of the torn edges, or their pushing again into the pupil. The posterior capsule may also become thickened. As an attempt to get rid of this annoyance, Prof. H. makes a linear cut into the posterior capsule after the lens has been removed. The vitreous comes forward and pushes asunder the flaps of the torn capsule, and in most cases secures a clear pupil. This manœuvre has been done in more than a thousand patients within seven years.

It was published several years ago, and the author, while strongly commending it, hopes that now the suggestion will meet more attention than it could at that time, when the stream of ophthalmology ran so violently in another direction. What is meant by this allusion will be understood by recurring to his controversy with Graefe. So far as we know, no one has adopted this proceeding, probably because it would seem to invite an unpleasant prolapse of vitreous. The pointed way in which Prof. Hasner insists that this danger is not a real one for the *practised* operator may not suffice to remove even from such minds all fears. Certainly a large capsular opening is highly important.

20.—*Neuritis Optica from Lead-Poisoning.* By Dr. SCHNELLER, Danzig. [Klin. Monats., August and September, ix., 1871, p. 240.]

The case recorded is clearly one of lead-poisoning, and in this respect has decided clinical value. The man, forty-four years old, was a painter, and his general symptoms were the blue line on the gums, slight constipation, the skin of grayish tint. Only the left eye was seriously affected. There was central scotoma, v. = $\frac{11}{100}$. Right, v. = $\frac{12}{20}$. The optic nerves were reddened, not swollen, a little hazy, borders indistinct, covered by a thin veil which also rendered the retinal vessels dim. The arteries of normal size, and very tortuous; the veins so much like them as to be difficult to distinguish. The case improved under iodide of potassium, derivatives, blue glasses, and rest. The author also adds another case, and attempts to show that the features of this kind of neuritis may be differentiated from other forms by certain peculiarities—especially because the veins so closely resemble the arteries in size and outline. He thinks their moderate size is due to contraction of the muscular coat, and that this establishes an analogy between the neuritis and the usual symptoms of lead-colic. We can hardly admit the conclusiveness of the reasoning by which the neuritis is asserted to be typical, but we are glad to have a case so well reported, in which the etiology is less conjectural than it has been in some so-called cases of saturnine neuritis or atrophy.

21.—*Detachment of the Retina under the Influence of Chloroform.* By Dr. R. SCHERNIER, Greifswald. [Klin. Monats., ix., August and September, 246.]

A seamstress, age twenty-seven, with m. $\frac{1}{6}$, and staphyloma posticum, in whom the above accident occurred, upon awakening from chloroform narcosis.

22.—*On Lead-Poisoning as a Cause of Optic Neuritis.* By JONATHAN HUTCHINSON, F. R. C. S. [Oph. Hosp. Reports, February, 1871, 6.]

The five cases quoted were all distinct cases of the general malady; in all, both eyes were affected, but in one of them the eyes were not both taken at once, but at an interval of three years. The first case, which was published before, was peculiar in that there were retinal apoplexies, in addition to neuritis. In the other cases, the nerves presented the pale and dirty-gray look of the latter stages of neuritis. In some, the edges of the disk were "fluffy," and, in others, the edge is said to be concealed by lymph. But in no case is it said that the exudation looked recent, or that there was hyperæmia of the nerve, as in Dr. Schneller's case. The inflammatory action is evidently subacute, and the result atrophy to a greater or less extent. Two cases remained permanently blind, two were under treatment, and, of the other, the result is not stated.

23.—*Atrophy of the Optic Nerve within the Brain, associated with Pressure. Excavation of the Papilla.* By Prof. HERMANN SCHMIDT, Marburg. [Archiv. f. Oph., xvii., 1, 117.]

A woman, aged sixty-one, had been blind in one eye for five years, and a similar trouble appeared in the other. There were none of the outward signs of glaucoma, such as diminished sensibility of the cornea, shallow anterior chamber, etc.; but the tension was on the extreme of the physiological limit, and the papilla was deeply excavated as in glaucoma, the sides of the pit being steep, the vessels greatly displaced, the phenomena of parallax marked, the tissue of the nerve a pale gray; the vessels were nicked as they passed over the edge, but there was no spontaneous arterial pulsation. Both eyes were greatly alike—only one retained v. $= \frac{2}{5}$, the other blind. In the better eye the visual field was contracted in the inner, outer, and upper sides.

The diagnosis made by Graefe was glaucoma simplex, and iridectomy intended, but the patient died of pleuro-pneumonia.

Autopsy showed the pia mater in the neighborhood of the cavernous sinus to be hazy, a little thickened, and decidedly œdematous. The optic nerves, for their whole extent to the brain flattened and small. Microscopic examination showed extreme atrophy of the nerve-structure. A section through the left papilla displayed a deep excavation of the nerve, with recession of the lamina cribrosa. The floor of the pit was 4 mm. deep from the inner surface of the sclera. The optic fibres and ganglion layer of the retina were atrophied, its outer layers normal. While the atrophy of the left opticus could be traced up to the chiasm, and the right optic showed partial atrophy, no examination seems to have been made of the brain-tissue, or effort to find some other focus of nerve-degeneration. It is only stated that there were no signs of apoplexy. It is clearly shown that primary atrophy of the optic nerves may sometimes coincide with such an excavation of intra-ocular ends as to perfectly simulate the characteristics of glaucomatous cupping, and a similar case is quoted from Stellwag. The author's explanation is, that advanced atrophy, by diminishing the bulk of the nerve, leaves the lamina cribrosa weaker than before, because its interstices are not so completely filled, and, under the extreme of physiological tension, it may give way, as in real glaucoma. This is not perfectly satisfactory any more than Stellwag's suggestion of an inflammatory process softening the tissues about the lamina, and allowing it to recede. Is it not quite as likely that a natural deficiency in the strength of the lamina cribrosa, and its attachments to the sclera, may, in these cases, have permitted a pressure excavation to develop, which, if the parts had their usual conformation, would only have been the ordinary shallow or saucer-like cupping?

24.—*Anomalous Forms of Retinitis Pigmentosa.* By TH. LEBER. [Archiv. f. Oph., xvii., 1, 314.]

It has been sufficiently established that the disease, to which the above name has been given, is not a distinct individual, but includes a variety of lesions. The name will, doubtless, for some time to come, be retained, but out of it, as from a nebula, the separate forms will be isolated, and have their individual names. The existence of the pigment in the retina has been a matter of surprise; but, with our more recent notions of the true relations of the pigment, as belonging to the retina and not to the choroid, the wonder will cease, although we may no better understand the real nature of the morbid process. A paper in Schultze's Archives, by Dr. Morano (vol. viii., part i., p. 80), shows this, and is condensed below.

Dr. Leber has done good service in bringing together a series of cases under the following heads:

1. Typical retinitis pigmentosa.

2. Retinitis pigmentosa, with typical visual impairment, and anomalous ophthalmoscopic appearances. To this belong (a) the well-known retinitis pigmentosa without pigment, of which the essential part is interstitial hypertrophy of the retinal neuroglia and atrophy of the nerve-elements.

b. Retinitis pigmentosa, with spots of choroiditis disseminata.

3. Retinitis pigmentosa, with typical appearances to the ophthalmoscope and anomalous visual disturbances.

a. Retinitis pigmentosa, with predominant abatement of central vision. This is due to lesion of the centre of the retina, and is often accompanied by nystagmus. Under this head are described four cases, in each of which some symptoms were present, such as hemeralopia, posterior polar cataract, atrophied nerve, small vessels, pigment in the retina, and in all there was marked central amblyopia.

b. Retinitis pigmentosa, with typical ophthalmoscopic appearances and good central vision, but with anomalies in other characteristics of sight; that is, irregular forms of the visual field, such as fields limited more on one side than another, or with the ring-shaped scotomata. Under this head come the cases where hemeralopia is wanting; and in one case Haase found nyctalopia.

4. Retinitis pigmentosa, anomalous ophthalmoscopic appearances, and with anomalous visual disturbances.

a. Congenital amblyopia, or amaurosis from retinitis pigmentosa. In many of the cases reported, the sight was very bad at an early age, but no pigment could be found in the retina until several years had elapsed. Out of fourteen cases of this class, four were complicated with deafness. In eleven cases, about which note was made on this point three times, the parents were blood-relations.

b. Retinitis pigmentosa, not congenital, with central amblyopia, and no retinal pigment.

c. Chorio-retinitis pigmentosa, with anomalous symptoms. The hereditary character of the cases quoted is the reason for putting it under this head.

5. Retinitis pigmentosa, with unusual course or unequal participation of both eyes, etc. Sometimes the disease progresses rapidly, sometimes with extreme slowness; or, especially with central amblyopia, it may become stationary.

Of all the cases, typical and anomalous, out of thirty-seven, parental consanguinity was found nine times; only twice was it hereditary; in eight families, there was more than one child affected. The influence of syphilis in etiology could not be well made out. In only two cases were mercurials of any service. In all other cases no treatment did any good.

25.—*The Pigment Layer of the Retina.* By Dr. Franz Morano, of Naples, with a plate. [Schultze Archiv für Mikroskop. Anat., viii., 1, 81.]

The author, under direction of Prof. Boll, in the laboratory of Berlin, has studied the so-called choroidal epithelium, and gives the following description: That the cells are not flat, but are cylinders of small height, consisting of two parts, viz.: an outer, toward the choroid, which is colorless, and made of pale granular protoplasm, and an inner part, which is pigmented. These are to each other as 1 to 3. The nucleus lies in the colorless part, and has one large nucleolus. Oil-drops of an orange hue also belong to the outer part. The pigmented part of the cell seems to be striated longitudinally. In good preparations this part is seen to terminate in long fine hairs running into the retina. The pigment-granules are never round, but needle-shaped, and lie in rows parallel to the fibres. The very tip of the fibres is often colorless. There may be thirty or forty hairs terminating one cell. In other cases the inner extremity of the cell has a pointed form, or a membranous expansion. The hairs thus described penetrate between the rods of the retina as far as to the end of their inner member, that is, to the membrana limitans externa. From three to five rods abut against one epithelial cell at the periphery of the retina, but at its centre the cells are smaller, and to each only a single rod will correspond. The above observations were made on frogs and other amphibia.

26.—*The Abuse of Alcohol and Tobacco as a Cause of Amblyopia.* By Dr. L. Hirschler, of Pesth. [Archiv. für Oph., xvii., 1, 221.]

The author appears to have had large experience among the intemperate, and gives the following *résumé* of symptoms of *amblyopia a potu:*

Objects appear as if under a veil, and this dimness comes on suddenly. They are apt to swim and tremble; persons' faces seem to the patient to be bluish or yellow; glistening, metallic surfaces are badly discriminated; only large print (Jaeger 14–20) can be read; at first it seems clearer than it does after looking a short time; objects soon grow hazy and utterly confused. Vision is worst at mid day, and best at twilight and by artificial light.

To external inspection there is nothing abnormal, except perhaps slight conjunctival catarrh. The pupil is contractile, and is quite indisposed to expand. Visual field is intact. Central scotoma is rare. By the ophthalmoscope the nerve may be normal, or slightly reddened, or of a dirty gray; at the later stages it may be white, and indicate beginning atrophy. The amblyopia soon reaches its maximum, and remains for a long time unchanged.

Amblyopia from tobacco differs little in symptoms from the above. The proneness of the pupil to contract is sometimes remarkable.

Amblyopia from other causes, as menstrual troubles or rheumatic processes, etc., etc., may present precisely similar symptoms. The pathology of the disease the author is disposed to locate in the brain-tissue. For treatment, the author relies on entire abstinence from the use of alcohol and tobacco, and a general hygienic method. Local depletion does little good. Attention to digestion, exercise, and fresh air, are most essential.

27.—*The Halo around the Macula Lutea.* By Dr. E. G. Loring, of New York. [Transactions American Ophthal. Society, 1871, p. 73.]

Following upon his interesting inquiries published in the Transactions a year ago, as to the cause of the light streak seen along the axis of the retinal vessels, Dr. Loring now offers some suggestions as to the cause of the halo at the macula lutea, which is sometimes, but by no means always, seen. He thinks, and very plausibly, that this is merely another case of reflection from surfaces which are not on the same level; because at the macula the retina begins to grow thin until it becomes a decidedly depressed surface at the fovea centralis. This change takes place gradually, giving a curved form, and from this curvature arises a halo or reflex. This is best seen with the inverted image, but it is seen too with the upright image in some cases, as Dr. Loring asserts, contrary to Mauthner's statement, and in this we agree with him.

On the appearance of the reflex from the fovea, Dr. Loring says: "Even when present, it is not, however, always of a crescentic shape, for it sometimes has the appearance as if it were only the segment of a small circle which was illuminated, or as if the fossa were a triangular one, and light were reflected from only one side of it, in which case the reflection streams out something like the tail of a very minute comet; again, it has the appearance of a delicate phantom-like veil, stretched in part or entirely across the fovea; and then, again, there is no reflex at all, and the fovea looks like a small, yellow dot, varying in size and shape, and has the appearance as if it had been flecked directly on to the surface of the retina with a brush.

"May not these differences in effect of the reflection, when present, be due to variations in anatomical construction of the part, and principally to differences in shape and depth of the excavation; and, when the reflex is absent, may not its absence be due to the want of any difference in level?"

28.—*Cases of Choroidal Melano-sarcoma, with Phthisis Bulbi.* By Dr. Berthold. [Archiv für Ophthal., Jahr. xvii., Abth. 1, S. 185.]

The cases were chiefly remarkable for the age of the subjects, one thirteen, the other twenty. In both the eyeballs had become atrophied in early childhood, in one even congenitally. It was assumed that the cause was panophthalmitis from the growth of the tumor. These cases, with some published by Knapp, and others by Forster, are presented as an offset to the prevailing belief in the great malignancy of choroidal sarcomata. They are extremely prone to recurrence; and Graefe, in 1864, could recall no instance in which there had been exemption for more than four years. To have even a few cases with a less gloomy prognosis is satisfactory. On the other hand, it is shown that the disease may make its inroad at a much earlier period than is usually assigned to it.

(A case of choroidal sarcoma in my private collection was taken from a boy twelve years old. I have had no information about its recurrence.—H. D. N.)

29.—*The Origin of the so-called Glassy Excrescences on the Choroid of the Human Eye, and the Nature of Hyaline Degeneration of its Vessels.* By Dr. Alex. Rudnew. [Virchow's Archiv., Band liii., Heft 4, pp. 455–465.]

The little masses in question were examined by Donders and Müller many years ago, and called by the former "colloid globules." Others have since written upon them. They appear beneath the epithelium on the glassy lamella of the choroid, and vary from $\frac{1}{80}$ to $\frac{1}{2}$ millimetre in diameter. They are extremely indifferent to chemical reagents.

Dr. Rudnew regards these masses as due to transformation of emigrated white blood-corpuscles; he finds them at all ages, and thinks their more frequent occurrence in the old is because in them vascular obstructions are the most common. In this opinion Dr. Rudnew differs from Donders, who thought they came from the nuclei of the epithelium.

In a case of purulent choroiditis accompanying purulent meningitis, Dr. Rudnew found extensive hyaline metamorphosis of the walls of the vessels. Some vessels were thus completely occluded. There was also great abundance of the above-mentioned hyaline globules. It was possible to see that the transparent substance in the vessels was made by fusion of small masses, and Dr. Rudnew concludes that the glassy state of the vessels originates like the glassy bodies of the hyaline lamella, from changes of the white blood-corpuscles.

30.—*Formation of Bone in the Eye.* By Dr. H. KNAPP. [Archives for Oph. and Otol., vol. ii., No. 1, p. 1.]

A careful description is given of the minute appearances in five globes in which ossific deposit had taken place. It is clearly shown that there was actual bony production as distinguished from calcification. Dr. Knapp attempts to show in what particular structure, and under what conditions, osteoid growth occurs. His conclusions are, that true ossification has only been found in exudations from the choroid. Calcareous deposits, on the other hand, have been observed in all the structures of the globe. At its beginning the bony formation appears in the chorio-capillaris in small plates, which are covered by the hyaline membrane and epithelium. Where there is more abundant exudation of plastic lymph, the bone-tissue becomes correspondingly thicker, and it may in some cases extend in a transverse septum across the anterior part of the vitreous, behind the ciliary body and lens, without involving those parts.

The diseases leading to this growth are chronic inflammations of the iris and choroid. Both ossification and calcification may occur at once. The diagnosis of either is made by feeling a hard and resisting mass in the deeper part of the globe, beginning about two lines behind the cornea, while the anterior parts are soft, or at least impressible.

The paper concludes with these remarks: "As ossification does not involve the outer choroidal layers, nor the ciliary muscle and iris, it in itself is not to be dreaded as a cause of sympathetic ophthalmia. The latter can only result from irido-cyelitis supervening as a complication to ossific choroiditis.

"As long as an eye in which ossification is diagnosticated remains free from irritation, and its fellow also, the removal of the former for fear of sympathetic inflammation is not indicated. To corroborate this, I may state that I have not unfrequently observed cases in which eyes bearing the signs of ossification, and even being tender to the touch, in the ciliary region, were tolerated without annoyance for twenty, thirty, and forty years."

While all deliberate statements of Dr. Knapp are to be received with the greatest respect, there may be danger of falling into a treacherous security as regards the influence of ossified eyeballs for mischief. His careful discriminations will certainly call attention to what are and what are not dangerous cases; but all surgeons know that one of the most fruitful sources of sympathetic irritation of a healthy eye is ossification or calcification in the other; and while the middle term undoubtedly is irido-cyelitis, the peril is so urgent that a needless operation may well be performed rather than risk a patient's only eye when an atrophied and ossified bulb may be setting up trouble, which at its beginning is not very well defined.

31.—*Iridectomia without Division of the Sphincter Pupillæ.* By B. A. POPE, M. D., of New Orleans. [Archives for Oph. and Otol., ii., 1, 87.]

The operation suggested is designed for cases where only a peripheral pupil is available, and to be most useful must be small. When there are central corneal opacity, without or with iritic adhesions; prolapsus iridis, especially with partial staphyloma corneæ; diffused corneal opacity, most dense at the centre; in case an eccentric pupil is needful, and the sphincter is hard to dilate or adherent—in all these cases the operation is commended as meeting important optical indications. The author has operated six times, and says the existence of a double pupil is not annoying, and, if the new one is below, a habit of drooping the upper lid is soon acquired, and covers the upper pupil. The mode of operating is by a very small wound, and with small forceps seizing with their point the iris near its periphery, and, dragging out only a little of it, cutting it closely with finely-pointed scissors. A very small opening can thus be secured, which cannot be done if the pupillary edge is severed.

It seems likely that this suggestion may in some cases be worth remembering.

32.—*The Treatment of Amaurosis and Amblyopia by Strychnia.* By Dr. ALBRECHT NAGEL. [Tübingen, 1871, pp. 141.]

Our attention is called in this pamphlet to the hypodermic use of strychnia. The method has been already known for a sufficiently long time to permit of comparative trials by other observers, and their results in the main agree with the statements of Prof. Nagel. It is understood that a large class of cases of amaurosis and amblyopia remain incurable, viz., such as depend on extreme textural change; but for a considerable contingent, which, by other methods are scarcely at all relieved, the hypodermic use of strychnia has been greatly efficacious. Our present methods of diagnosis do not show us the minute structure of the optic nerve and retina, and we are therefore unable to tell, in a case of atrophy, just how far the fibres or ultimate elements are impaired; hence, the trial of any remedy must be attended in all cases with a degree of uncertainty.

But what the method of Prof. Nagel has done is a positive gain. It is as follows: He injects into the temple a moderate dose of nitrate of strychnia, say from $\frac{1}{36}$ to $\frac{1}{16}$ grain every day for a week. He expects the effect on vision to be produced within an hour. This may be either in extension of the limits of a contracted field, or in the increase of direct vision. If this result is not obtained within a few days, the dose is increased, but not above $\frac{1}{12}$ of a grain. If no good effect, the case is regarded as unsuited for this treatment. The diseases for which this treatment has been greatly useful have been, a boy with imperfect amaurosis of one eye, with concentric limitation of the field, and no ophthalmoscopic appearances; after one injection of $\frac{1}{32}$ grain nitrate strychnia, vision rose in half an hour from $\frac{15}{\text{LXX}}$ to $\frac{15}{\text{XV}}$ and the field was a little enlarged. The improvement was maintained for two days, when another injection of $\frac{1}{27}$ grain added a little more to the area of the field, and improvement afterward went on spontaneously to perfect cure.

Another case of amaurosis, following measles, complicated with head-symptoms, was treated with perfect success. The interior of the eyes exhibited no lesions. Slow improvement of sight was taking place under general tonics and hygiene; but, to hasten the progress, strychnia was tried. The first injection, of $\frac{1}{53}$ grain had no effect; a second, of $\frac{1}{40}$ grain, brought vision from less than $\frac{1}{\text{CC}}$ to $\frac{1}{\text{XX}}$. In a month after, four injections, v. = $\frac{15}{\text{XV}}$.

Another case, a child of three years, in whom the state of the optic nerve was almost perfectly normal, although blindness was complete, proved obdurate to strychnia. No treatment did any good.

Other classes of cases successfully handled were asthenopia of the accommodative kind, amblyopia from disuse, traumatic amaurosis, etc. A case of embolus of the central artery of the retina is contributed by Prof. Becker, in which the striking gradations of improvement in the field are shown by diagrams. The case looks more like one of neuritis, with consecutive thrombosis, than pure embolus. At the beginning, the arteries were extremely small, and the nerve a little large, the region of the macula infiltrated. At the close of a month vision was $\frac{20}{\text{XX}}$, but the field was still limited upward and outward. The infiltration about the macula had disappeared, but the nerve showed appearances of atrophy. Such are some of the important cases discussed in this pamphlet, and the treatment is well worth imitating. The use of strychnia by the stomach, and sprinkled on a blistered surface, does not have the same effect as when injected. This fact, moreover, has been substantiated in treating other diseases of the nervous system, so that the recommendations of Prof. Nagel are well supported.

It is proper to say that Prof. Nagel gives a caution against the excessive use of the remedy, as liable to defeat the object of treatment by irritating too severely the nerve-tissue, even though there may be no evident symptoms of poisoning.

33.—*The Mode by which Inflammation of the Eye is propagated in Meningitis.* By Dr. Berthold. [Archiv. f. Ophth., xvii., 1, 178.]

A child of ten years died of acute meningitis, and, eight days before death, acute inflammation occurred in one eye, with conjunctival chemosis. This symptom soon subsided, but the deep tissues of the globe were infiltrated with pus, giving the appearance of purulent choroiditis. The other eye seemed to be well, but could not be ophthalmoscopically examined. At the section, both eyes were found inflamed: in the one, the disease being a neuro-retinitis, with swelling of the head of the nerve; in the other, the choroid, retina, and vitreous were disorganized. There was purulent meningitis. The communication from the brain-membrane to the eye was, doubtless, by way of the canalis opticus from the arachnoid cavity, along the supra-vaginal lymph-space (Schwalbe), and the communicating space beneath Tenori's capsule, to the subconjunctival space—hence, in one eye the chemosis. Further, in the same way the sub-vaginal lymph-space gives passage to exudation to the head of the optic nerve and supra-choroidal lymph-space. These, rather than the nerve proper, are the probable channels by which meningeal inflammation reaches the eye. The same conclusions have been set forth by others, but the fact is, probably, not so widely recognized among general pathologists as it deserves.

34.—*A Contribution to the History of the Development of Myopia, based upon Examination of* 4,358 *School-Children.* By Dr. Friedrich Erismann, of St. Petersburg. [Archiv. für Oph., xvii., 1, 1–79.]

By this patient inquiry some valuable facts are elicited, and too much praise can hardly be awarded to such workers as Dr. Erismann and Dr. Cohn, for their industry. The chief results obtained by the examination are as follows:

Of the whole number, viz., 4,358, he found emmetropes, 26 per cent.
Myopes, 30.2 per cent.
Hypermetropes, 43.3 per cent.
Amblyopes, 0.5 per cent.

The youngest children were ten years old. The unexpected fact above shown is the small number of normal eyes, only ¼, and the high percentage of hypermetropes. In the youngest classes ⅘ were hypermetropic. It would appear that, for early life, hypermetropia, and not emmetropia, is the normal condition. But, as the child grows older, the eyeball becomes elongated until myopia may be developed—as

happens	at 11	years of age in	20.6	per cent.
"	at 13	"	28.6	"
"	at 15	"	39.3	"
"	at 19	"	47.2	"

Acuity of sight, among all the children, was:

		1 in	85.6	per cent.
Between	⅔ and	1 in	6.8	"
Less than	⅔	in	7.6	"

Among the myopics, who were 1,317, the vision was:

	1 in 77.7	per cent.
Between $\frac{2}{3}$ and	1 in 12.5	"
Less than $\frac{2}{3}$	in 9.8	"
Choroidal atrophy around the nerve was absent	in 5	per cent.
Of moderate extent	in 71.2	"
Of large extent	in 23.8	"

This table, and the one above, explain each other, and they together show how the acuity of vision suffers loss by the development of myopia. For among these v. = 1 is less by 8 per cent. than among the whole number taken together, including the myopes. The most serious cases of choroidal atrophy were naturally found among the higher grades of myopia.

Insufficience of the recti interni was studied in all the myopes. For the weaker degrees, the distance taken was 8 to 10 inches; for the higher grades, the distance was shorter, as they would choose for easy reading. This lesion is not considered, properly, the cause of myopia, but both it and myopia are regarded as resulting from the same cause. Out of 1,245 moypes, were 6 cases of strasbismus convergens. Taking the remainder, there was found no insufficiency in 67.4 per cent.

Up to 6° " "	in 12.9	"
" 10° " "	in 13.3	"
Above 10° " "	in 2.6	"
Relative strabismus divergens	in 2.9	"
Absolute " "	in .9	"

That is, 32.6 per cent. of all myopes suffer from disturbance of the recti interni. Where myopia is more than $\frac{1}{6}$, only 23.1 per cent. retain normal muscular power. But, even with the lowest grades of myopia, 23.8 per cent. have muscular troubles. It also appears that choroidal atrophy does not depend on muscular insufficiency.

Concerning hereditary tendency to myopia, it appears that 30.6 per cent. come from parents of whom one or both are myopic, and a larger proportion of them than of others have choroidal atrophy.

Another important inquiry is, What influence do spectacles have upon the myopia of children? Out of 1,245, 122 used glasses, and the degree of myopia was more than $\frac{1}{12}$ in 88 of these. It appears that, out of the 122, only 15 had neutralizing glasses; 84 had them too weak, and 23 had them too strong. It also appears that choroidal atrophy becomes greater and more frequent, vision less acute, and muscular trouble more common.

"It is, in a word, a simple fact that the use of concave glasses has a positively bad effect on eyes which are undergoing a change in their state of refraction, and it is a misfortune to be obliged to order glasses for a youthful myopic person." If they must be used, as they frequently must, for music, drawing, writing, etc., they should be laid aside when not required. The more constant use of glasses should be deferred to a more adult age, when the tissues are stronger, and able better to resist distention.

The causes which lead to production of myopia are insufficient light, badly-constructed tables and seats, as predisposing to the one most effective agent, namely, persistent tension of accommodation. The attachment of the ciliary muscle to the cornea in front, and the choroid behind, readily explains how the choroid, in efforts of accommodation, must be dragged forward. It has been shown that the ciliary processes are also pulled forward, while the zumla is relaxed, and the vitreous pressed.

To this must be added the influence of the external muscles, and on this point Graefe has laid great stress, especially in cases of insufficient interni. Both these factors, the internal and the external muscular apparatus, can excite congestion of the deep vessels of the globe, and this again tends to the increase of myopia. The great agent of mischief is forced and spasmodic effort of accommodation. The lens does not regain its normal curvature; and thus an eye, at first hypermetropic, passes over its myopia. The injurious effect of badly-chosen glasses is thus fully explained.

The importance of securing to children, in their studies, suitable light and seats, and of watching their habits of reading, is, by the above investigations, most urgently set forth. Suitable care may arrest or prevent a serious mischief to sight, while the miseries of myopes are largely due to the culpable neglect of parents and teachers.

35.—*On the Use of the Ophthalmoscope.* By Dr. ALBUTT. [London, 1871, pp. 405, 8vo.]

It hardly comes within the sphere of this report to review a book like the above. But a few words may not be amiss. Its title in full is, "On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys, also in certain other General Disorders." The great value of the book is in its consideration of diseases of the nervous system, and this constitutes its bulk. In this department it is decidedly the best treatise in English; it is based upon actual observation. It is written in a candid spirit, while there is not the fulness of detail which would be proper in an ophthalmic treatise; the author shows accuracy in his acquaintance with the eye, which entitles his opinions and observations to confidence. The book is written in the interests of general medicine and pathology, and is deserving the study of every practitioner. It is true that errors have crept into it, as, on page 38, he expresses the opinion that arterial pulsation is never visible under any circumstances, natural or artificial. Perhaps the intent in this sentence is to exclude glaucoma, in which spontaneous pulse does occu rin the arteries, but it is true inanother class of cases also, viz., certain heart-diseases, that the arteries pulsate. We cannot attempt to analyze the book, but mention it rather to commend it to general perusal, and congratulate the author upon having written what is well worth reading. This remark may be thought more pointed if the reader will turn to the half-humorous and modest sentences with which Dr. A. closes his book, on page 297.

Another valuable contribution to our resources is the fifth part of Stricker's "Handbuch der Lehre von den Gemeben," in which the anatomy of the ear and the eye are treated in a masterly way by master-hands. Special chapters are written by different authors: for instance, on "The Retina," by Max Shultze; on "The Iris and Choroid," by Iwanoff; on "The Circulation," by Leber; on "The Lymphatics," by Schwalbe; on "The Cornea," by Rollett, etc.

We are happy to say that a translation is being made by competent persons.

Miscellaneous and Scientific Notes.

Appointments, Honors, etc.—Dr. William A. Hammond has been elected an honorary member of the St. Andrews Medical Graduates' Association. Dr. S. Weir Mitchell, of Philadelphia, is the only other American member of the association, which embraces some of the most eminent medical gentlemen of Great Britain. There are two candidates for the vacant chair of Physiology in the Paris Faculty of Medicine—M. Béclard, author of the well-known treatise on physiology, and M. Vulpian, present Professor of Pathological Anatomy. Dr. John Rose Cormack has received the honor of knighthood from Queen Victoria, in acknowledgment of his services to English subjects during the two sieges of Paris. He had previously received the decoration of the Legion of Honor from the French Government. Dr. Priestley has resigned his office of Professor of Obstetrics in King's College, and of physician for the Diseases of Women and Children to King's College Hospital, both of which positions he has filled with distinction for nearly ten years. He assigns, as a reason, that he cannot spare the necessary time from his private practice. Dr. Paget has been appointed to the Regius Professorship of Physic in the University of Cambridge. Mr. Darwin's name has been placed first on the list for the forthcoming election, at the French Academy of Sciences, of a corresponding member in Zoology. Prof. Bennet, of Edinburgh, has been obliged to abandon his labors on account of ill-health, and will pass the remainder of the winter at Mentone, under the care of his namesake, J. H. Bennet, of London. The following changes have just taken place among the medical officers of the Paris hospitals: M. Empis goes to La Charité, Lorain and Desnos to La Pitié, Guyot and Siredey to Lariboisière, Raynaud, Féréal, and Cadet de Gassicourt to Saint-Antoine, Dumontpallier to Lourcine, and Constantin Paul to Bicêtre. Profs. Stanislaus, Cannizzaro and Charles Maggiorani, of the University of Rome, and Prof. Louis Porta, of the University of Pavia, have been raised to the dignity of senators. Sir William Jenner is to receive the title of Knight Commander of the Bath, and Dr. Gull a baronetcy, for services rendered to the Prince of Wales in his recent illness. M. Barth, who, since Trousseau's death, has had a large share of the consulting practice of Paris, is President of the Academy of Medicine of Paris for 1872. At the meeting of January 2d, he announced the death of Dr. Jaeger, of Vienna. At the Academy of Sciences, M. Faye takes the presidential chair for the current year.

The Everlasting Priority Question.—We regret that a paragraph in the "Review of Gant's Surgery," which appeared in our last issue, threatens to reopen a question which has already been so often before the profession that no possible good can come from a renewal of the by-gone controversy.

Prof. Mason states his pretensions as follows: "The splint now in use, and sold by instrument-makers as 'Sayre's Improved Splint,' is none other than one first suggested and devised by me while House-Surgeon at Bellevue Hospital in September, 1860." This statement he subsequently modifies so as to except the cross-bar to protect the femoral artery, an improvement to which he lays no claim.

We have been shown what purports to be the instrument originally suggested by Prof. Mason, and the present one in use by Dr. Sayre. Whatever community there may be in the fundamental principles underlying the two instruments, the resemblance is not very striking to the uninitiated eye.

Qu'importe? Servetus does not dim the lustre of Harvey's fame; Sir William Jenner's reputation survives that of Jesty; Marshall Hall's name is a household word to many who never heard of Legallois:

"Raum für alle, hat die Erde;
Was verfolgst du meine Heerde?" W. T. L.

The Pending Medical Bill.—The bill now before the Legislature, entitled "An act to protect the people against quackery and crime," provides that the County Medical Societies shall, at their annual meetings, each elect five censors, to act in the county they represent. The censors shall have power to summon all practitioners, regular or irregular, for examination as to their professional qualifications. It is declared to be a misdemeanor to practise without the sanction of the censors, and the penalty is imprisonment, or a fine of five hundred dollars.

Charts for recording Temperature.—Dr. E. Seguin has arranged a very simple and efficient table for recording the temperature and vital signs. The difficulty of securing accurate observations by inexperienced attendants is much lessened by the system adopted by Dr. Seguin; of making the normal temperature zero, and counting degrees above and below that starting-point. Each table is adapted to the complete registration, in very small compass, of the observations for a period of four weeks.

The Medical Congress of Lyons.—This Congress is to take place in September next. The different societies, hospital staffs, and learned bodies, have already elected representatives, who will form themselves into a committee for the purpose of organizing this scientific gathering. This is a praiseworthy effort on the part of our French brethren, who seem nothing daunted by the fearful disasters which have afflicted their country.—*Lancet.*

The Dublin Quarterly Journal of Medicine.—The proprietors of this excellent journal have converted it into a monthly publication, of which the title is changed only by the omission of the word "quarterly." The editorship remains in the hands of Dr. James Little. The old journal had been in existence forty years, and we hope that in its new form it will have a full measure of success and popularity.

Women Doctors in India.—The Mohammedan Nawab of Rampoor has presented to the Bareilly Mission a large building, for the purpose of a medical school for women. Several women are now going through a scientific course of instruction.

The Controversy on Alcohol.—We published in full, in our last number, the medical declaration respecting alcohol, which has caused so much excitement in England, stating at the same time that Dr. Anstie, certainly a high authority on the subject, withheld his signature. The following is what Dr. Anstie says (*Practitioner*) in explanation:

"I shall not dwell on certain obvious literary defects of the above paper, which make it not very worthy to be the manifesto of a liberal profession. Moreover, I fully sympathize with the desire expressed in the concluding paragraph, for reasonable legislation with a view to diminish the needless temptations to drink that are offered to the poor; and, had the declaration confined itself to this topic, I could have found no fault with it; but with the first and second paragraphs I entirely disagree. It is not that they make any statement which, abstractedly taken, is altogether incorrect; but I entertain no doubt whatever that their general sense, as it will be read by the public, conveys a false impression, and one which is calculated to do more harm than good.

"It is, in the first place, both a false and a mischievous idea that any considerable percentage of the drinking-habits of any class of English society springs from the improper prescription of alcohol by medical men. That such improper prescriptions *may*, and occasionally do, cause drunken habits, I have not merely admitted, but elaborately proved on more than one occasion. But I protest now, as I have protested before, that, in nine-tenths of the cases in which doctors have been blamed as the occasion of the evil habit, the accusation has been the mere invention of a lying drunkard anxious for a scape-goat to bear a portion of his disgrace. But the first paragraph of the declaration will undoubtedly be read by the public as a distinct admission that the medical profession has had a relatively considerable share in the spread of intemperance. I believe such a statement to be ridiculously false; and I would further suggest that, were it true, the eminent physicians signing this document, among whom are included a large majority of the teachers who have educated the recent generations of medical practitioners, ought to cast dust upon their own heads for having reared their pupils in such scandalous and mischievous ignorance of their duties.

"In regard to the second paragraph, I insist that it will convey to the public an entirely false impression as to the state of competent scientific opinion. It will undoubtedly be taken as a confirmation of the now obsolete doctrine of Lallemand, that alcohol is treated by the organism as a merely foreign stimulant, and is not decomposed therein. Now, I appeal fearlessly to Dr. Parkes (the only one of the signers of the declaration who has done recent experimental work regarding alcohol) to say whether his latest experiments have not strongly confirmed the refutation, by Schulinus, Dupré, and myself, of Lallemand's doctrines regarding elimination of unchanged alcohol. In a letter which Dr. Parkes was kind enough to send me some few months since, he confessed, with that candor and high-mindedness which have ever distinguished him, that such was the case. I therefore believe that Dr. Parkes must have neglected to consider carefully the sense in which the declaration will inevitably be read by ignorant persons.

"But, finally—and this is really the most important point of all—I most distinctly object to the kind of dictation by which the signature of this paper has been obtained. The secretary of the Temperance League puts pressure on the editor of the *British Medical Journal*, and the latter gentleman proceeds to put pressure (exceedingly difficult to resist) on a number of eminent professional men. To my own personal knowledge this declaration has been signed by numbers of gentlemen who have scarcely given a thought to the principles which it affirms, and who have confessed to me that their only reason for signing was the fear of seeming to oppose so good a work as the mitigation of intemperance. This is a mere terrorism, and ought to be resisted. It is a repetition of the very useless and mischievous farce which was gone through some twelve years ago, when crowds of medical men, who in their own private opinions and practice were wholly opposed to teetotalism, were induced to sign a paper advocating views the only logical interpretation of which was that alcohol was always and everywhere an unmitigated poison. The climax of that most indecorous exhibition was the discovery that several of the most eminent signers had also certified warmly and publicly to the great virtues of certain special wines and beers! I say that all that kind of insincere and skin-deep philanthropy is degrading to the profession; and, although I will coöperate with any one to get a reasonable licensing bill through Parliament, I can have no part in a declaration which is in no sense the accurate and spontaneous expression of the medical opinion of this country."

An English View of the Profession in America.—In the *Medical Times and Gazette* of January 6th we find the following liberal and appreciative remarks on the progress of medicine in this country:

We take the opportunity of saying something on a subject which has long engaged our attention, and which recently has been rapidly assuming a greater and greater importance. It is at once our duty and our pleasure to watch the changes of medical doctrine and the improvements in practice, as they are gradually evolved at home and abroad, and nothing has struck us so much of late as the marvellous strides American medicine has made within a few short years. This was most forcibly brought home to us a little while ago, when we were asked to recommend a book as the best on each of a certain number of subjects. The list was not a long one, and yet about one-half of those we had it on our conscience to recommend were American. Now, if this is not saying much for ourselves, it is saying much for our American brethren. There has always been a number of physicians and surgeons in America quite on a par with the best in this country, but that number used to be very limited. Furthermore, the

American school was marked by certain characters of its own—the curriculum was exceedingly short, only the merest smatterings of medicine and surgery could be picked up in the period; and, with the exception of those educated in the older schools and abroad, few of its representatives could boast of great scientific attainments. Nevertheless, the men so educated had to encounter the exigencies of professional life as best they might; and here the vigor of the national character came prominently into play—so that, though rough, the medicine and surgery of the States had an exceedingly practical character. We have used the past tense here; for all this, although within the memory of most men now in the profession, is already a matter of history. American medicine has fairly passed into the scientific stage of development. It is well for us to look this matter fairly in the face, and to ask how it has been that, in America, medicine has made such mighty strides in such a short space of time. As to the reality of the advance, there can, we think, be no question; it is evidenced in many ways, but in none more powerfully than in the modern medical literature, periodical and permanent, of the United States. With the abandonment of their dirty calf binding and bad paper, the Americans seem to have made other changes important in every respect; and, though we have not yet done them the compliment of pirating, as it is called, their works on this side the Atlantic, it is not because they are not used, for they are daily to be seen in the hands of students and practitioners alike.

We have heard it said that most of the American literature, as it comes to us, is not new—only reproduced—and undoubtedly the criticism is in some sense true; but the existence of such a literature implies a demand for something more than the scanty facts of pocket text-books, and is itself an evidence of the advance of which we speak.

But this eager desire for the higher kinds of knowledge is made still more clear, to those of us connected with certain London institutions, by the crowds of men from America who flock there and elsewhere, seeking to prepare themselves to take a position in their own country. We are constantly meeting Americans—young and old—who have left America for one or two years, which they are spending in travelling from school to school, here and on the Continent, picking up every thing that is to be learned, and storing it up for future use. Go to Vienna, and the great majority of the English speakers there to be encountered are Americans; so, too, though in a less degree, in Berlin; and so, too, it used to be in Paris. These men have left their homes in search of knowledge, and we may be sure that, having acquired it, they will reproduce it with interest when the proper time arrives. These are the signs of American advance, and true indications of future American greatness.

Diagnosis of Syphilis by Examination of the Blood.—The *Boston Medical and Surgical Journal*, of February 8th, publishes a letter from Vienna, by Dr. James R. Chadwick, dated January 12th, from which the following is taken:

"I have just returned from a meeting of the 'Gesellschaft der Aerzte,' at which was announced a discovery of the most stupendous importance. I am eager to be the first to herald to the New World, through your pages, the greatest success—I may venture to say—as yet achieved by the microscope, to wit, the certain diagnosis of syphilis. I will give briefly, from memory, the course of procedure which led to this grand result, having first recorded the name of the propounder of the new theory as Dr. Lostorfer. Wishing to make a series of examinations of blood from syphilitic patients, Dr. Lostorfer applied to Prof. Zeissl, in August last, who at once placed his wards at the service of the investigator. The manner of conducting the researches consisted in taking a drop of blood, placing it immediately between a slide and covering glass, then setting the specimen under cover. A large number of slides were thus prepared, and daily examined microscopically with a No. 10 immersion-lens, Hartnack. The first two days nothing abnormal presented itself; but, on the third, minute shining bodies were discerned, which grew from day to day, until they attained the size of red blood-corpuscles. The shape was, in most instances, round, but modifications of that form were here and there noticed; they were all, however, provided with tapering processes or 'sprouts.' The number of these corpuscles to be seen at one time within the field of the microscope was variable, amounting in only one specimen to fifty. A month later these objects were still visible. A solution of sugar, as well as several other solutions, caused them to shrivel up, become, as it were, folded upon themselves, lose their shining aspect, and cease to grow. Cold was also found to affect their development, inasmuch as a temperature below 10° Réaumur (54° Fahr.) put a stop to all growth.

"To prove the connection between these bodies and syphilis, very many experiments were made with healthy and diseased blood, and the diagnosis confirmed beyond the possibility of doubt.

"Blood was then daily examined while the patients were undergoing treatment with mercury (Einreibungen), and, after a certain time (in one case twenty-five days), during which the number of corpuscles was steadily decreasing, they had wholly vanished from the field. After these experiments, Dr. Lostorfer submitted himself to Prof. Stricker, and later to Prof. Hebra, to have his power of diagnosticating syphilis, by the microscopic appearances of the blood, put to the test. On various occasions he received from them numbered slides with blood of syphilitic and non-syphilitic individuals. To their astonishment, he unerringly selected from the preparations those where the blood was tainted with syphilis, with the exception that on certain slides the blood was pronounced to be in a condition allowing of no conclusion being arrived at.

"The discoverer gives the name of syphilitic corpuscles to these bodies, which, he said, must originate in one of two ways: either from germs always present in the blood, but stimulated to growth by the syphilitic virus alone; or from such introduced bodily into the blood at the time of infection.

"In conclusion, he read the records of a number of cases with his microscopic observations of the blood from day to day."

Ovariotomy during Pregnancy.—At a meeting of the Obstetrical Society of London, held December 6th, Dr. Eugene Goddard read the following details of a case of ovariotomy performed during pregnancy:

The patient was twenty-nine years of age, and in 1870 was found to be the subject of an ovarian cyst, but, as there were no urgent symptoms, the consideration of any surgical treatment was deferred. She then became pregnant, and, about the end of the second month of utero-gestation, Mr. Spencer Wells removed the ovarian cyst. Eleven and a half pints of fluid were withdrawn. The clamp was removed, and the bowels acted on the eighth day. Pregnancy went on uninterruptedly, and a living child was born at the full period. Dr. Goddard said that the compound nature of the cyst precluded the idea of tapping, as also did the risk of peritonitis, suppuration of the cyst, and the formation of adhesions. Premature labor was not induced, because the patient was already beginning to suffer constitutional disturbance from the double burden, and it was doubtful whether, by the time a viable child could be born, they would not have assumed such magnitude as to imperil the patient's safety; whereas, if abortion were induced, the child would be lost, and the tumor would remain.

Dr. Ross related a case in which Mr. Wells had operated under more adverse circumstances, as the lady was much broken down in health at the time of the operation. A small ovarian tumor was diagnosed eighteen years ago. The patient subsequently got married, and Dr. Ross had attended her in four labors. In no instance was parturition attended with any serious difficulty. It was observed that during gestation the tumor appeared to become smaller. The tumor rapidly increased about a year ago, and Mr. Wells removed it successfully, the patient being about two months pregnant. Her labor is now daily expected.

Mr. Spencer Wells said that the existence of the cyst for eighteen years, and the presence in its walls of hard, bone-like masses, had led to the diagnosis of a dermoid tumor. Mr. Wells had performed ovariotomy four times during pregnancy, and all the patients had recovered.

Action of Quinine on the Uterus.—M. Monteverdi communicates to the *Nuova Liguria Medica* the results of a series of experiments he has made on this subject. He has invariably employed the sulphate of the alkaloid. He finds that quinine exerts a general tonic influence on all the organs of the body, but especially upon the uterus. In the course of half an hour after it has been administered, short contractions occur in the uterus, unaccompanied by pain; and these gradually become longer and stronger, with distinct intermissions, so as to resemble closely ordinary pains of labor, the effect lasting for about two hours. In order to effect the expulsion of the fœtus and of the placenta, he believes that doses of about four grains will be found the most appropriate. Quinine appears to be preferable to ergot, because it exercises no injurious influence either on the mother or child, because it is very certain in its action, because the contractions it induces are very regular and natural in their character, and because it is free from danger at whatever period of pregnancy it is administered; or in cases of contracted pelvis, incomplete dilatation of the os uteri, and antecedent to the escape of the waters. He finds that it is of service in the metrorrhagia of pregnancy, in amenorrhœa in consequence of a torpid condition of the uterus, and in puerperal fever, as a consequence of its tonic action. He considers quinine to be indicated in all diseases of the digestive organs, and of the urino-genital system dependent upon atony of the various organic constituents. M. Monteverdi gives a caution in regard to the use of quinine in pregnancy complicated with any disease requiring its administration, lest abortion or premature delivery be induced. In cases where quinine proves too energetic in its action, he recommends opiates to diminish its effect. He considers quinine to be contraindicated, as a general rule, in hysteria.—*Lancet.*

A Curious Memento of Jenner.—A very interesting memento of the discoverer of vaccination has recently been presented to the Royal College of Physicians by Sir John William Fisher. It consists of a cow's horn, beautifully polished, presented to Sir J. W. Fisher, in the year 1813, by Dr. Jenner, and polished by himself. The gift was made in grateful acknowledgment of services rendered to Jenner's sick children by Mr. Fisher, then a medical assistant in Soho. The horn is now mounted in silver, and bears an appropriate inscription, stating the circumstances under which it was presented to the college. Dr. Burrows, the President, in asking the acceptance of the horn, stated that it was probable—though there was no record of the fact—that the horn had been taken from one of Dr. Jenner's favorite cows on which he made his experiments on vaccination.

Obituary.

George Edward Day, M. D., F. R. S.—We learn, as we are going to press, that this gentleman, an article from whose pen appears in the present issue, died at Torquay, England, on January 31st. Dr. Day was born in 1815, and from the beginning of his professional career distinguished himself by his extraordinary talents. Fifteen years ago, while at the height of his fame and success, he met with a serious accident, which resulted in complete loss of power in the right arm and in one leg. The *Medical Times and Gazette* says of him: "Both before and after this lamentable accident his industry was untiring. He wrote a work on the 'Diseases of Advanced Life,' and on 'Chemistry in its Relation to Physiology and Medicine;' he translated Simon's 'Animal Chemistry' and part of Rokitansky's 'Pathological Anatomy' for the Sydenham Society, Lehmann's 'Physiological Chemistry' for the Cavendish Society, and most of the medical and scientific articles in 'Chambers's Encyclopædia.' His scientific and professional knowledge was vast and exact, and his judgment sound and temperate. Almost up to the last he retained his habit of literary work, though struggling against increasing debility and exhaustion, which, however, were not to be traced in his writings. Few who read the reviews and scientific notices in our own columns could have surmised that they were written by an invalid disabled by ununited fracture of one arm, helpless upon the legs, and distracted by a host of minor sufferings arising out of his state of rheumatism and prostration. His handwriting was most peculiar, done as it was with an arm that had to be lifted on to the writing-table."

Paul Dubois, the eminent lecturer and obstetric practitioner, who died recently in Paris, was born in the year 1795. He was the son of an illustrious father, Antoine Dubois, Professor and Surgeon-in-chief of the Maternité Hospital in Paris, and to his instruction and influence is due the early prominence of the former, his father having obtained for him, when quite a young man, the position of Adjunct Surgeon to that institution. At the time of his appointment, four thousand women were annually admitted into its lying-in wards. Young Dubois was first appointed Assistant-Professor, but in 1834 he became, after a warm contest, Professor of Obstetrics. This position gave him a new and ample field for the display of his abilities; his lectures became very popular, and for many years his eloquence attracted large numbers of medical men and students to the hospital. He had the opportunities which so many busy practitioners neglect, to collect the results of his observations and practice in a valuable work on the special subject to which he had devoted his life; but he was no exception to the rule, and his vast experience remains comparatively unrecorded. For many years past he had been suffering from mental derangement, and virtually lost to the profession and the world.

It becomes our duty to announce the sudden death, on the 14th ult., of Dr. Charles Allen Lee, at Peekskill, N. Y. Dr. Lee had attained his seventy-second year, and, although he had retired from active practice, he took a lively interest to the last in all that concerned the honor and welfare of the profession. We shall furnish in our next issue a brief sketch of his life and career.

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Original Communications.

ART. I.—*Considerations on the Circulation of the Blood, with Special Reference to Counter-Iritation, and to the Effect of Increased Atmospheric Pressure.* By ANDREW H. SMITH, M. D.

WHEN we consider the extreme intricacy of the vascular system, its infinite subdivision, and the immense variety of organs with their differing functions which are dependent upon the supply which it brings to them, and then reflect that the blood is propelled through this system by the action of one central organ, any change in which must affect all parts alike, we must be struck with the fact that each organ or tissue receives precisely its due share of blood—a supply varying exactly with the varying function of the part, and adapted with absolute precision to its physiological demands. And we cannot fail to perceive that the mechanism by which the quantity of blood thrown into the aorta with each cardiac systole is thus duly apportioned to all the parts of the organism must be one of extreme delicacy. Considering the blood-vessels from an anatomical stand-point, we find that this regulating power is confined to the arteries and the veins, the capillaries being destitute of contractile tissue. The arteries, and with some exceptions the veins, are supplied with muscular fibres, and are capable of contracting under stimulus conveyed to them through a set of nerves distributed to them, and named from their function the vaso-motor nerves. The contraction of an artery diminishes the supply of blood to the part to which the artery is distributed; while the contraction of a vein augments the resistance to the circulation and causes distention of the capillaries, and a prolonged sojourn of the blood in contact with the tissues. These varying actions are made to subserve the purposes of nutrition and function, and are presided over by the nervous system with a delicacy and accuracy so subtle and unerring as to seem like a localized intelligence, defying all attempts at analysis or explanation.

This regulating power is of extreme importance, since the amount of blood required in any given part is constantly changing, and depends upon the activity of the part for the time being. Whenever an organ, before at rest, is brought into action, a demand is occasioned for an increased supply of blood, and the vessels leading to the part dilate to the extent required.

I regard all function, with the exception of that of the organs of special sense, as the result of the chemico-vital reaction between the tissue of the acting organ and the blood which courses through its capillaries; and the nervous system as having no other office in this regard than that of regulating the supply of the nutritive fluid. Hence, blood being an essential factor, and the quantity in the system being pretty nearly constant, it follows that a number of activities cannot be carried on simultaneously for want of a sufficient supply of this necessary fluid. This conclusion, arrived at theoretically, is sustained by observation. We find that a proper performance of the digestive function is incompatible with great muscular activity; that the brain and the muscles cannot put forth at the same time the maximum of effort; and, when a full meal has created a demand for a largely-increased circulation through the capillaries of the stomach and intestines, the brain is rendered anæmic, and drowsiness results. No two of the great emunctories of the system can be stimulated to excessive action at the same time. A diuretic and a diaphoretic will not act well together, nor will either act during the operation of a cathartic. These well-known facts are usually explained by assuming that the nerve-force is the primary element in all these activities, and that it is a constant quantity, and cannot be expended in one direction without being proportionally deficient in others. This explanation, however, appears to me to involve difficulties which do not pertain to the one already given.

But fortunately there is seldom necessity for several activities at the same moment, and thus the organs which are at rest can spare the blood demanded by those in action. There is scarcely a moment during our waking hours that the equilibrium of the circulation is perfect. It is doubtless one of the objects of sleep that the circulation may be relieved for a time from the perturbating influence of the contending demands of the different functions, and the blood thus allowed to distribute itself simply according to mechanical laws, and to provide for a harmonious nutrition of all parts of the body.

Under certain conditions of disease it may become desirable to attempt to modify, artificially, the nutrition or the function of some given organ or part.

Among the means resorted to for this purpose *counter-irritation* holds a prominent place. This consists in creating an irritation in a healthy part in order to relieve a morbid condition in another locality more or less distant. This morbid condition is for the most part either inflammation or pain without inflammatory symptoms, and it is a matter of daily observation that these conditions may be relieved in a great many cases by the practice in question.

The *modus operandi* has been the subject of endless and acrimonious discussion, which still leaves us so much in the dark that Headland sums up our knowledge upon the subject in these words: "It appears that, as a consequence of the action of counter-irritants, the attention of the nervous system may be drawn off from the morbid process going on at some other part of the body;" and again, "The term '*counter-irritation*' is employed to express this action, the nature of it being but ill understood. A powerful impression on any surface of the body, external or internal, seems to be capable of arresting and diverting, as it were, the attention of the system, and thus, for a time, of checking a morbid process."—(On the "Action of Medicines," pp. 67, 97.) These statements are scarcely more intelligible and scientific than the famous aphorism that "Nature abhors a vacuum." They express, however, as clearly as it is possible to express any thing so intangible, the ideas intended to be conveyed by the term "*substitution*," the substitution of one morbid process for another by a sort of hoodwinking of the system. But "substitution" has to share the honors with "*revulsion*" or "*derivation*," terms employed to express the accumulation of an excess of blood in the place irritated, which excess is supposed to be drawn, wholly or in part, from the diseased locality. This idea has at least the merit of being intelligible, and expresses, I think, a partial truth. But it is met by the objection that the extra quantity of blood under a sinapism, for example, is so insignificant that its abstraction could have no appreciable effect upon a distant organ, with which there may be no direct vascular connection. This objection is valid from the stand-point from which it is taken.

Rejecting the idea that either the nerves or the blood-vessels play any considerable part in the action of counter-irritants, Mr. Ross[1] has put forward the theory that the effect is produced by a modification of nutrition, communicated from cell to cell of the intervening parenchyma, without regard to whether the tissues be continuous, or, as in the case of internal organs, merely contiguous. The absurdity of his theory is shown by one of his illustrations, that of flatulent colic, relieved by a sinapism to the abdomen. He affirms that the irritation causes a change of nutrition which is propagated from cell to cell through the integument, fasciæ, and muscles, until it reaches the inner surface of the parietal peritonæum, whence it is transferred to the visceral serous surface, and penetrates to the muscular coat. Here it produces an *improved nutrition*, which gives the muscular fibre sufficient power to expel the flatus. (!)

Except in cases resembling the above, in which the pain results from a mechanical cause capable of being removed by muscular action excited by reflex irritation, I hold that all the benefits resulting from counter-irritation are obtained di-

[1] "On Counter-Irritation: A Theory constructed by the Deductive Method of Investigation." By James Ross, M. D. London, 1869.

rectly or indirectly through the circulation. In the first place, I consider all pain (excluding that from extraneous irritation) as proceeding from imperfect nutrition, even though there be no evidence of inflammation. This is only in accordance with the proposition that there can be no derangement of function without change of structure. Now, if the morbid condition be one dependent upon the *quantity* (not quality) of the blood supplied to the part which is the seat of pain, then, in my view, counter-irritation may be of service, but not otherwise.

In the case of inflammatory action, the agency of the vessels will be admitted with less argument. But the difficulty in either case has been that already stated—that the *apparent* change in the circulation is too trivial to be credited with the results observed. I reserve the word "apparent," and on this reservation my entire argument will rest. With the exception mentioned in the foot-note,[1] in all the discussions upon this subject, up to the present time, so far as my knowledge extends, attention has been confined to the excess of blood contained in the irritated part. If, for example, the entire mass of tissue to which the irritation extends could be cut out at one stroke, and the blood expressed from it, the excess of this blood over what would naturally be contained in the same quantity of tissue, would represent what has been considered as the sum total of the change supposed to have been effected in the circulation at that point. Or, if the irritation was supposed to be reflected upon some other point, the result there was regarded in the same light.

It is here that I think a mistake has been made. *The question is not, how much blood the vessels of the irritated part will hold, but how much they will transmit in a given time.* This becomes evident when we consider that a given amount of blood passes through the capillaries of the body in each unit of time, and is transferred from the arterial to the

[1] The germ, from which the views here given are developed, is derived from a lecture by Prof. Virchow, which I heard in 1860, but which I have never seen in print. His reference to the subject was limited to the suggestion that increasing the capillary circulation in the part supplied by an arterial twig implied a diminution of the circulation in the capillaries derived from collateral branches.

venous side of the circulation, and that the quantity passing through any one part must affect that passing through the remainder of the body, since the latter must be the exact complement of the former. Thus, if in a given time four pounds of blood pass through the capillaries of the entire body, and one pound passes through the capillaries of the arms, it follows that three pounds must pass through the remainder of the capillary system. Now, if we plunge the arms into hot water, and dilate the vessels so that an additional half pound passes through them, the remaining vessels will transmit but two and a half pounds, and the tissues which they supply will be deprived for the time of one-sixth of their nourishment. It will be perceived that this is a matter entirely apart from the quantity of blood which might be contained in the arms if severed from the body.

The consideration of the increase of the carrying power of tubes, in comparison with the increase in their diameter, involves some of the most interesting points in the mechanics of fluids. The resistance to the passage of the fluid being derived chiefly from the friction against the sides of the tube, will increase in proportion to the ratio of the circumference to the area of the section. Now, the circumference of a circle increases directly as the diameter, while the area increases as the square of the diameter. The friction is obtained by dividing the circumference by the area, and therefore decreases directly as the diameter increases, as is shown by the following formula:

Diam.	Circum.	Area.	Friction.
a.	b.	c.	$\frac{b.}{c.}$
2 a.	2 b.	4 c.	$\frac{2\ b.}{4\ c.} = \frac{b.}{2\ c.}$

From which it appears that doubling the diameter of a tube quadruples its area, and at the same time divides the friction by two.

But, great as is this disparity, it is immensely increased in practice, especially when the tube is of very small calibre and tortuous or branching. The following experiments serve to show how slight an increase in the diameter of a tube will suffice to augment its carrying power enormously:

Experiment I.—A caoutchouc tube of 5 mm. bore, and 3 feet long, perfectly straight and of uniform diameter, was found to transmit a given quantity of water in 115 seconds. Another tube, of 6 mm. diameter, but perfectly similar in other respects, transmitted the same quantity in 65 seconds.

In this case an increase in the diameter of 20 per cent., and in the area of less than 40 per cent., gives an increase in the carrying power of nearly 100 per cent.

Experiment II.—A glass tube 10 inches in length, and having an inside diameter of .052 inch, gave passage to 6 drachms of water in 122 seconds. Another tube, of the same length, and under the same conditions, but having a diameter of .08 inch transmitted the same quantity of water in 20 seconds.

In this case the addition of one-half to the diameter of the tube allowed the passage of six times the quantity of fluid.

Experiment III.—The same tubes were used as in the last experiment, and all the conditions were the same, except that defibrinated bullock's blood was employed instead of water. The blood was previously strained through very fine linen. The smaller tube required 1,440 seconds to transmit 6 drachms; while the larger tube gave passage to the same quantity in 142 seconds.

The ratio here is one-half to ten, instead of one-half to six, as when water was employed.

In applying the results obtained from these experiments to the question of counter-irritation, we find that certain stimuli applied to the skin act in such a way upon the vaso-motor nerves as to cause a relaxation of the terminal arteries, and a dilatation of the capillaries. If the irritation be considerable, the surface assumes a bright-scarlet hue, in the place of its previous flesh-color. Such a change in the color implies a very considerable increase in the diameter of the capillaries.

Experiment IV.—A tall narrow vessel was partly filled with water, at the temperature of 120° Fahr., and the naked forearm thrust into it to near the elbow, the arm resting upon a support. Enough water was then added to exactly fill the vessel. Within half a minute the vessel began slowly to over flow and continued to do so for several minutes. At the conclusion of the experiment, it was found that half an inch of water had been displaced. This, of course, represents a corresponding increase of volume in the arm, and, as this could take place only by an increase of the quantity of blood in it, it follows that half of a cubic inch of blood was added to the amount present before the observation. This represents the increase in the *area* of the capillaries, and measures what has heretofore been considered the extent of the effect produced upon the circulation.

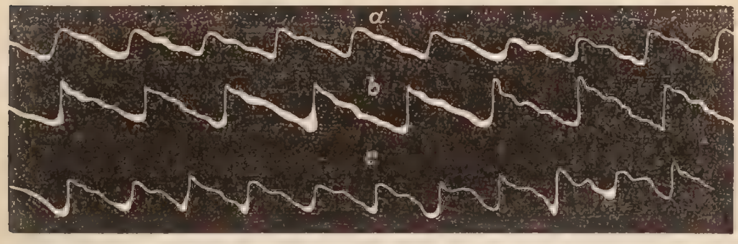
But, in view of the results observed in Experiment III., it will be seen that this can by no means be taken as a criterion of the effect obtained, the increase in the carrying power of the vessels being so greatly in excess of the increase in their area.

This increased carrying capacity is illustrated in an extreme degree by an observation of Bernard. He found that, by galvanizing the branch of the lingual nerve which is distributed to the sublingual gland, the circulation through the capillaries of the gland was increased to such an extent that the pulsation of the arteries was transmitted to the veins, and the blood escaped from the latter in jets.

Of course this excess in the circulation in one part implies a corresponding deficiency in some other part. Were the blood-vessels passive tubes, the deficiency would at once be distributed over the whole body; but the larger arterial trunks, at a distance from the point of irritation, but between it and the heart, contract in proportion as the pressure upon them is lessened, and thus the effect is confined within a limited area, and the excess is at the expense of neighboring vessels.

When, however, a large portion of the surface of the body is exposed to an action affecting the area of the capillaries, the entire circulation is changed as the result. This is shown by the following experiment:

Experiment V.—By means of the sphygmograph, a tracing was obtained of the pulse of a young man in full health and under ordinary conditions, the temperature of the air being about 82° Fahr. The pulse was found to be of fair volume as represented at *a* in the annexed cut. Without disturbing the instrument, the lower half of the body was then immersed in water at 70° Fahr. After the feeling of chilliness had passed off, a second tracing was taken. The pulse was much larger and stronger, as is shown at *b*. Hot water was then added to the bath, until a temperature of 120° Fahr. was obtained. A third tracing then showed the pulse to have less volume than before, and less tension than in the normal state.



The effect of the cold bath in this case was to contract the vessels in that portion of the body to which it was applied. The result was, that the blood, meeting with increased resistance in that half of the body, was distributed in greater proportion to the part where the resistance was normal, and hence a larger pulse at the wrist. When the temperature of the bath was raised to 120°, the vessels of the lower half of the body relaxed, and again presented an easy passage for the blood, restoring the equilibrium, and hence the radial pulse became smaller.

But, aside from this direct action in the immediate vicinity of the part irritated, there may be in some cases another action in parts more remote. A reflex impression may affect the vaso-motor nerves in parts having the requisite nervous or "sympathetic" relation to the one irritated, and this transferred action would then be the same in kind as that already described, and a similar change in the circulation would result. Bernard found that applying acetic acid to the tongue produced a very marked change in the circulation of the sublingual glands, the amount of blood coursing through their capillaries being immensely increased, and the blood in the veins of the glands retaining its arterial hue.[1] Numerous other examples might be cited, showing that the circulation in one

[1] Leçons sur la Physiologie et la Pathologie du Système nerveux.

part may be modified by irritation in another, but they are so familiar that they will immediately suggest themselves to the reader.

There are, then, as I conceive, four conditions in which counter-irritation may act to relieve pain, or abate inflammation:

1. When there is a direct vascular connection between the irritated surface and the diseased part. In this case, counter-irritation acts by opening a larger passage for the blood through the sound tissue, and thus diverting the circulation in part from that which is diseased. An example of this is afforded by the application of blisters to the neighborhood of inflamed joints.

2. When a large surface is exposed to the irritating agent, in which case the entire circulation of the body, including that of the diseased part, may be modified. An example of this may be found in the use of hot foot-baths, to relieve congestions of the thoracic or abdominal organs, or of hot applications to the chest and abdomen to relieve wakefulness.

3. When the irritation is transferred to a distance by reflex action, and there simulates the first condition. As an example of this, may be cited the employment of blisters to the breast, to act upon the lungs, there being no direct communication between the vessels of the skin and those of the diseased organs.

4. When pain is the result of a mechanical cause, which cause may be removed by exciting muscular action through the reflex function; as when, in flatulent colic, the muscular coat of the intestines is brought into action by irritation applied to the abdomen, and expulsion of the flatus results. Of course, two or more of these conditions may coexist, and probably in most cases such is actually the fact, and the result is thus rendered more or less complex.

There is a secondary consequence of the dilatation of the vessels which I have described, which may be mentioned, though its practical importance is probably not great, except in rare instances. I refer to the fact that, when the capillaries of a part are widely dilated, the blood, being in excess of the physiological demand, does not acquire the full venous character, but preserves to a greater or less extent the properties of arterial blood, even after it reaches the veins, as in Bernard's experiment already referred to. It is conceivable that some of the relief afforded in cases of dyspnœa by enveloping the chest in a sinapism may be owing to this cause, the blood reaching the lungs in a condition to require less than the usual amount of oxygen.

If the views which I have advanced respecting the effect of counter-irritation upon the circulation be correct, it follows that the aim should be so to manage the irritation as to cause the greatest possible flow of blood through the part. We should, therefore, stop short of inducing inflammation, as this tends rather to impede or arrest the capillary circulation than to promote it. More effect will be produced by irritating a large surface to a moderate degree, than by exciting an intense action within narrow limits. This is illustrated in the advantage which is derived from "flying blisters" in subacute inflammation of the joints or in sciatica. These blisters are applied for a short time only, so as not to produce complete vesication, and are repeated at short intervals. They are found to be much more efficacious than when the full action of a blister is produced.

It is not intended by this suggestion to ignore the effect which may result from the evacuation of serum by means of vesicants, or of the drain which may be established by maintaining suppuration from a blistered surface. This comes under the head of depletion rather than of counter-irritation, the latter being merely incidental to the former.

In examining the capillary system, we shall find that the skin on the one hand, and the mucous membranes and the solid internal organs on the other, are very richly supplied with these vessels, while the muscular and cellular tissues have much fewer in proportion. Excluding the latter, we may, for the sake of convenience, speak of the cutaneous system and the visceral system of capillaries. These two systems, comprising so great a proportion of all the capillaries in the body, stand in a certain antagonism to each other. The health of the body requires that neither of these should receive for any considerable time an undue proportion of blood.

Such a condition involves a double departure from the normal state, as the excess in one system necessitates a corresponding deficiency in the other. Let us suppose, then, that the cutaneous capillaries are by any means increased in diameter to a considerable degree, as they may be, for instance, by the hot bath: we shall find as a result that, the resistance to the flow of the blood through them being diminished, a much larger proportion will seek that channel to reach the venous system than will pass through the capillaries in other portions of the body. As a consequence, the remainder of the body will be left with an insufficient supply. We may sometimes observe the results of this in phenomena appreciable to the individual. Thus, the lassitude felt after the warm bath may be attributed to imperfect nutrition of the muscles from the insufficient supply of blood to them resulting from this action. The tendency to syncope produced by the protracted use of the warm bath is probably owing to the diminished supply of blood to the brain, caused by the increased facility for its passage through the capillaries of the skin. On the same principle, the wakefulness of insanity may sometimes be overcome by hot applications to the chest and abdomen.

The converse of this is observed in the vigor which results from the use of the cold bath, or from exposure to a cold and bracing atmosphere. Here we have the vessels of the skin reduced in calibre, and as a consequence a greater proportion of the blood sent out by each contraction of the heart is compelled to find its way to the veins through the capillaries of the muscular and nervous systems.

But, under certain conditions not well understood, an impression of cold upon the surface leads to such a diminution in the cutaneous circulation, and consequently to such a distention of the visceral capillaries, as to induce a lesion of some one or more of the viscera. The pneumonia, bronchitis, nephritis, or diarrhœa, as the case may be, is then said to be the result of "taking cold." If in the very earliest stage the warm bath or some internal sudorific be resorted to, and a free channel thus opened in the skin for the passage of blood, the internal organ may be relieved, and the threatened disease averted.

But an impression of cold upon the surface may, under some circumstances, prove salutary. Thus, in syncope, we dash cold water into the face and upon the chest, in order, as we say, to rouse the system by the sudden shock. The shock may perhaps have something to do with the result, but we do not find other means of rousing the nervous system to be equally efficacious. Shaking, flagellation, etc., so useful in opium-poisoning, do not approve themselves to us in this case. But cold water, combining with the shock a sudden contraction of the cutaneous vessels, and thus forcing more blood into the internal organs, including the nerve-centres, is an agent which has been used from time immemorial with success.

Medicines which are supposed to have a certain degree of control over the diameter of the capillaries are described in works on materia medica, and not unfrequently prescribed in cases in which the design is to affect the circulation in some diseased locality through the general action of the drug. The reasoning is as follows: "This medicine acts upon the capillaries—the capillaries in the diseased organ require such action —*ergo*, the medicine is indicated." Now, if the diseased part is supposed to be acted upon only as a portion of the general organism, and not from any special relation to the medicine, the employment of the latter must result in disappointment, for the simple reason that, if all the capillaries of the body are acted upon alike, no change in the distribution of the blood will ensue. The most that can happen is, that the labor of the heart will be increased or diminished. The same result would be attained by employing a cardiac sedative or a cardiac stimulant.

These remarks will apply to the use of opium and of ergot as antiphlogistics. I do not intend to raise a question as to their efficacy for this purpose, but merely to attack the theory of their supposed mode of action, which I believe to be untenable; unless, indeed, it can be shown that the capillaries of inflamed parts are peculiarly sensitive to their influence.

Closely allied to this subject is one which has recently assumed especial importance in connection with a new method of laying the foundations of piers for bridges. I refer to the physiological and pathological effects of high atmospheric pressure.

In constructing the piers for the bridge across the Mississippi at St. Louis, in 1869, a novel plan was adopted for reaching the rock below the bed of the river. An immense diving-bell was constructed, having the horizontal dimensions of the proposed pier. This was sunk down to the bed of the stream, and upon its top was built the masonry of the pier. The interior of the bell—or *caisson*, as it was termed—was kept clear of water in the usual manner, by forcing in air through pipes leading from pumps located on the shore. Excavation was carried on within the caisson, the earth being removed through a shaft in such a way as not to permit the escape of air. As the work progressed, the caisson was constantly sinking into the earth, while the masonry above was being built up in the same proportion, so as always to keep the top of the pier above the surface of the water. In this way, the earth being removed from beneath and the stone being added above, the vast structure, having in the case of one of the piers an area of thirty-six hundred square feet, was carried down to a depth of one hundred and ten feet, when it rested upon solid rock. The interior of the caisson was then filled up with concrete, and a perfectly solid foundation for the pier was secured.

Of course, in proportion as the caisson descended, the pressure of the air required to keep out the water constantly increased, until at last it attained the enormous figure of fifty-five pounds to the square inch in excess of the normal rate, a pressure nearly equal to the maximum capacity for resistance of an ordinary steam-boiler, and twice as great as the working pressure usually employed.

It was to be expected that such a pressure would exercise a very decided influence upon those exposed to it, but experience proved that the effect was much less than was anticipated. Beyond an unpleasant sensation in the ears while passing through the air-lock on entering or leaving the caisson, there was no uncomfortable feeling whatever. It was observed, however, that the pulse was accelerated to the extent of about 20 beats per minute, during the first hour of the sojourn in the caisson, after which it fell to about 20 beats below the normal standard.

The phenomena resulting from the increased pressure were minutely studied by Dr. A. Jaminet, of St. Louis, who has published a very interesting monograph upon the subject,[1] to which I am indebted for the facts referred to in this connection, so far as they relate to observations at St. Louis.

It was not until the pressure had reached twenty-four pounds to the square inch, that the workmen began to suffer. The symptoms, which were almost constant, were violent epigastric pain, shooting pains in the back and extremities, and coldness and pallor, or, in some cases, lividity of the skin. In a large proportion of cases there was associated with these symptoms a degree of paralysis, most frequently assuming the form of paraplegia, but sometimes involving at the same time one or both of the upper extremities. In a few instances the paralysis remained permanent, but in most cases it passed off in the course of a few days. Some of the patients in whom there was no paralysis were able to resume work on the following day.

Ninety-seven cases occurred, with eight deaths. The *post-mortem* appearances indicated an intense degree of congestion of the brain and spinal cord, and, in some cases, softening. The abdominal viscera were also more or less congested.

But what was very remarkable, and contrary to all expectation, was, that at no time during the progress of the work, even when the greatest pressure was attained, was any inconvenience experienced *while in the caisson.* It was invariably while the pressure was being lessened in the air-lock, or after emerging into the open air. Hence it would appear that it was not the pressure, but the removal of the pressure, which produced the pathological result. Dr. Jaminet rejects this obvious inference, and considers that the symptoms observed were due entirely to exhaustion caused by excessive tissue-change resulting from increased consumption of oxygen.

As this method for submarine operations has been adopted by the Brooklyn Bridge Company, and is likely hereafter to be frequently resorted to, it becomes important to determine, if possible, what is the true explanation of the pathological phenomena in question.

It appears to me that all the facts of the case point plainly

[1] "Physical Effects of Compressed Air." By A. Jaminet, M. D.

to a derangement of the circulation as the chief cause of the symptoms observed.

In considering the effect of increased atmospheric pressure upon the circulation, it is plain that it will vary according as the vessels interested are located in a part which is soft and compressible, or in a cavity the walls of which are firm and unyielding. In fact, the result in these two cases will be directly opposite. In the first the tendency will be to press the sides of the vessels together and expel the blood they contain, provided it can find any place to escape into where the pressure is less. In the second case the vessels will be protected from pressure from without, and would be entirely unaffected were they not in communication with other vessels exposed to such pressure. Such being the case, however, they will necessarily receive blood from these latter vessels until an equilibrium of pressure is produced.

Now, the vessels of the body generally, exclusive of the bones, fall into the first category, while those within the cranio-spinal cavity belong to the second. It follows, therefore, that in a condensed atmosphere a man will have more blood in his brain and spinal cord than in the normal state. This accords precisely with the results of the autopsies in the fatal cases recorded by Dr. Jaminet. In every instance the brain and spinal cord were found to be intensely congested.

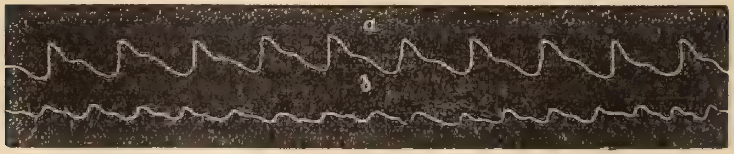
But the pathological results cannot be attributed to this congestion, else they would be developed while in the caisson instead of after leaving it. In fact, the congestion seems to exert no unfavorable influence upon the function of the cerebro-spinal axis, probably because the pressure is so equably distributed. But how does the matter stand when, after an equilibrium has been established between the vessels of the general system and those of the cranio-spinal cavity, the pressure is suddenly removed from the former? Obviously there must be an action the reverse of that we have just considered, and there must result a reflux tendency from the brain and spinal cord toward the general circulation, in order to re-establish the equilibrium. This would oppose the current of blood going from the heart to those parts, and a more or less perfect arrest of the circulation in the smaller vessels would result. This would be aided by the ready yielding of the vessels of the general system, whose tone would be lost in proportion as it had been superseded by the external pressure. In this way a channel would be opened for the blood, which it could take much more readily than to overcome the current from the brain and cord. Hence, we should have these latter organs congested with effete stagnant blood, which not only would afford them insufficient nourishment, but would expose them to the ulterior danger of embolism. The fact that, in every case narrated by Dr. Jaminet, in which death was delayed for a sufficient time, softening of portions of the brain and spinal cord was found, points strongly to the probability of the occlusion of some of the vessels by thrombi, although in only one instance the finding of an embolus is mentioned.

This reasoning derives additional support from the fact already stated, that, while the pulse was accelerated during the first hour or two passed in the caisson, it afterward fell about 40 beats, being 20 less than the normal rate. This shows a process of gradual adaptation to the new conditions, effected probably chiefly through an excessive evacuation of fluid from the system. The quantity of the urine was greatly increased, and the perspiration was also augmented to a very great degree. It is not possible that a result, thus gradually reached, could give place at once to the normal state, on removal into an atmosphere of normal density. A somewhat gradual process of readaptation would be necessary, pending which grave accidents might well occur. The experience of Dr. Jaminet shows that the longer the time passed in the caisson, and consequently the more perfect the adaptation to the increased pressure, the greater will be the danger on coming out.

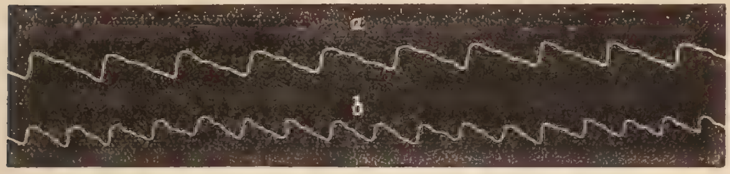
That bad effects do not follow in every case, is only in accordance with what is observed in regard to exposure to cold, or to malaria, contagion, etc.

The method already described has been adopted in sinking the piers of the Brooklyn Bridge. During the past year I have had several opportunities of studying the circulation in the workmen in the caisson on the Brooklyn side, for which I am indebted to the courtesy of Mr. Collingwood, one of the engineers. I invariably found the pulse accelerated, ranging in different persons between 102 and 132. At the same time the volume was sensibly diminished. On one occasion I took sphygmographic tracings of the pulse of several of the men before they entered, and again while in the caisson, after being exposed to a pressure of thirty-two pounds to the inch for about an hour. In order to insure the reapplication of the instrument in precisely the same manner, the following precautions were observed: To the end of the tape which secured the instrument in position, I attached a light weight which was allowed to hang down, and exert an even and constant traction upon the tape, while this was being passed backward and forward across the back of the wrist, and over the hooks attached to the instrument. This weight being the only traction applied to the tape, there was insured a uniform degree of pressure. After the first tracing was taken, and before the instrument was removed, the outline of the pad was traced on the wrist with ink. At the second adjustment the pad was so placed as to exactly coincide with this outline. These precautions rendered it easy to adjust the instrument the second time precisely as it was when the first tracing was made.

The annexed figures show the effect produced by the increased atmospheric pressure. It will be observed that the pulse is accelerated in every case, while its volume is lessened, and the dicrotism generally somewhat increased. This is due to



the difficulty which the heart experienced in driving the blood through the vessels, narrowed by the extraordinary atmospheric pressure. These tracings, which I believe to be the only ones ever made under like conditions, afford abundant evidence of the profound disturbance of the circulation which



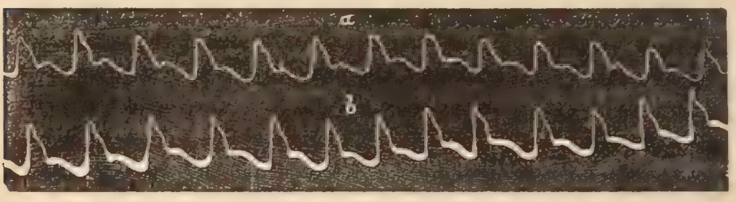
takes place, and are strongly suggestive of the consequences which might result from a sudden reversal of the conditions in persons whose circulatory apparatus was not in perfect vigor.

A few cases of paralysis occurred while sinking the Brooklyn pier, but all speedily recovered.

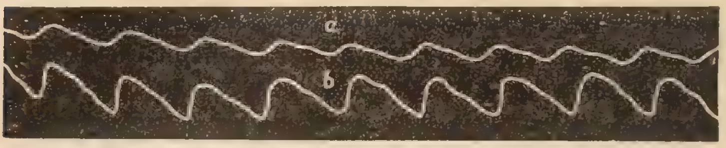
The treatment employed by Dr. Jaminet consisted almost entirely in the administration of alcoholic stimulants and beef-tea. This was in accordance with his theory of exhaustion, and was fairly successful. But, if the views which I have advanced as to the etiology of the affection be correct, the treatment should aim to diminish in some way the area of the vascular system. The most efficient plan would be to have a chamber constructed in which air might be condensed to nearly the extent of the pressure in the caisson. The patient being placed in this, the pressure should be raised by degrees to a point approximating that under which he had been working, and then be very gradually reduced until it reached the normal standard. Or, a like effect in a less degree might be produced by bandaging firmly the extremities, and, as far as practicable, the trunk. At the same time, those medicines should be administered which tend to give tone to the vessels and diminish their calibre. This effect is attributed to opium, and the cold and clammy state of the surface would seem to call for its use in stimulating doses. But, theoretically, ergot, which stimulates unstriped muscular fibre everywhere to contraction, should be specially efficacious. It is contended that, when gangrene is produced by this substance, it is by its inducing spasm of the arteries, and thus shutting off the supply of blood from the extremities where the circulation is at all times relatively feeble. This action in a proper degree is precisely what is required in this case to stimulate the lax and yielding vessels to contract upon their contents, and thus to give increased force and velocity to the circulation.

This action of ergot may be demonstrated by the sphygmograph, as in the following experiment:

EXPERIMENT VI.—A tracing was taken of my own pulse under normal conditions, and again twenty minutes after taking a drachm of the fluid-extract of ergot. The result is shown in the annexed cut, in which the upper line represents



the normal pulse, and the lower one the pulse after taking the ergot. The increase in the volume expresses the increased capillary resistance. Though, according to the theory of Marey, the excursion should diminish in proportion as the capillary resistance increases the arterial tension, yet experiment shows that obstructing the venous circulation, which likewise causes increased tension in the artery, has the effect of augmenting the excursion. This is shown in the tracing below, in which the increased volume is the result of placing



a ligature above the elbow, as if for a venesection. A like effect is produced by holding the breath, the unaërated blood passing with difficulty through the capillaries.

Alcoholic stimulants, by imparting increased energy to the heart, will be useful, but their effect will be much greater when the vessels have regained their tone, on the principle that a pump of a given power will deliver much more fluid through a rigid than through an elastic tube.

Dr. Jaminet was led to prohibit the use of the warm bath as a remedial measure, from observing that it aggravated the tendency to paralysis. In fact, in several instances paralysis came on while the patient was in the bath. The train of reasoning which I have adopted would lead us *a priori* to expect this result, as the warm bath would relax still more, if possible, the already passive, toneless vessels.

Dr. Jaminet recommends that the time passed in the air-lock when going into the caisson should be twice as long as when coming out, he considering that the system requires more time to adapt itself to an increased than to a diminished pressure. But the facts point to a directly opposite conclusion, and I cannot see that their force is lessened by the view which Dr. Jaminet defends. Inasmuch as the ill effects occur in every case while the pressure is being restored to the normal standard, or within half an hour afterward, I should make this restoration as gradual as possible.

As a prophylactic measure, the use of a moderate quantity of an alcoholic stimulant immediately before leaving the caisson would no doubt be advantageous. This would give the heart greater power to cope with the difficulties about to be encountered in the changed conditions of the circulation.

But, above all else, the duration of the sojourn in the caisson should be regulated to correspond with the degree of pressure. The greater the pressure, the more rapidly will the circulation go through the process of adaptation, and the sooner it will reach a given degree of departure from the normal state. In proportion to this departure is the danger when the pressure is removed. Hence, the hours of labor should be diminished as the caisson descends. Dr. Jaminet found that, with a pressure of fifty pounds, the men could not work longer than two hours at a time without serious danger.

My object in writing this paper has been to present certain mechanical points in reference to the circulation, which I believe to be of practical importance. The tendency of the present day is to confine our investigations too exclusively to the domain of the microscope and the test-tube, to the neglect of other studies equally important. The mechanics of medicine deserves to be elevated into a distinct branch of medical science, and offers a field for the display of the highest qualifications for scientific investigation.

ART. II.—*The Origin of Cæsarean Section; an Historical Sketch.* By C. F. RODENSTEIN, M. D., Westchester, N. Y.

SURGERY is an art which gives no scope to the exercise of the imaginative faculty. Within its domain romance and poetry find no material for idealization. The operation for cataract and the Cæsarean section form, perhaps, the only exceptions to this remark. The exact investigations of modern ophthalmology have robbed the first of the awe and mystery which attended its performance, when learned Arabians and less learned mountebanks travelled through Europe and restored sight to the rayless orbs of the blind; but to the second cling still some of the vague terrors inspired by surgical procedures not perfectly understood, some of the mythological mists of the ages in which it is said to have originated.

In fact, there is something in the very circumstances under which this operation is performed, which excites an undefined dread. The very process of parturition is shrouded, to the unprofessional mind, I know not in what halo of sacredness and mystery. When labor is difficult or obstructed, the interest is heightened; and that mingled sentiment of dread and anxiety, of alternate doubt and hope, culminates when, in the most desperate crisis of dystocia, Cæsarean section is performed, as the last resource of obstetric art.

Sometimes this heroic intervention is adopted to save two lives from impending destruction; sometimes to save a mother after her child has already been sacrificed to her safety; sometimes the dead body of a woman is opened to deliver a living child, thus literally rescuing life from the embrace of death.

An operation so grand in its simplicity, so brilliant in its success, so mysterious in its disasters, is well calculated to impress the imagination of mankind. Its very name has an imposing sound, and carries the mind back to the imperial grandeur of Rome. Its origin has been placed amid the twilight ages of the gods. The immortal verses of classic bards celebrate the birth of Olympians, demi-gods, and heroes, who are said to have entered the realms of light through its agency. The enactments of the most ancient code of Roman legislation and the sacred traditions of the Jewish Talmud have been invoked in sanction of its performance. And I might easily increase the store of ancient lore which adorns our text-books, by adding quotations from the sacred literature of the Hindoos, from the Zend-Avesta of the Parsees, and the hieroglyphic legends of Egyptian monuments and papyri, as appropriately illustrative of this operation as those adduced from the classics.

While reading the accounts of the earliest reputed cases of Cæsarean section, I was led to form a different opinion with regard to its antiquity from that which is generally entertained. The first thing that roused my suspicion was the silence of ancient medical authors on this subject. Neither the genuine nor the apocryphal books of Hippocrates contain a line about it. Aretæus, Galen, Celsus, Oribasus, Ætius, Paul of Ægina, have each written on obstetrics ; but not one has mentioned this operation, of which they could not be ignorant had it been practised in or before their day.

Instead of quotations from these or other medical authors, we are treated with citations from Pliny and the poets in proof of its antiquity. The passage so frequently quoted from Pliny in connection with this subject occurs in a description of wonderful births. The manner in which a man enters into life exerts an occult influence upon his destiny. Difficult labors are of bad omen. Footling presentations are peculiarly unlucky. But those children, he continues, whose birth has cost the mother her life, are evidently born under favorable auspices. For such was the case with the first Scipio Africanus ; "Primusque Cæsarum a cæso matris utero dictus ; qua de causa et Cæsones appellati." In the preceding chapter he had stated that difficult labors are disastrous to the child and to mankind. He derives the name of Agrippa from " Ægre partus," and refers to the sufferings of Agrippa from the infidelities of his wife, and to the cruelty of Nero, who was born of Agrippina, as the proof of his statement; and, by a pun almost as execrable, he illustrates his opinion that hysterotomy is a happy augury to the child, because Cæsar was cut from his mother's womb. Other writers, however, tell us that the name of this illustrious Roman had nothing to do with his birth. Festus derives it from Cæsaries, and says he was surnamed Cæsar because he had a very heavy head of hair. Others say that the name of Cæsar is derived from a Punic word, which signifies elephant; and that the first of the Cæsars was so called because he kept an elephant.

Whatever may be the meaning of the word, the historical accuracy of Pliny cannot be questioned. He assures us that at Saguntum a child returned to the womb of its mother after its birth; that Alcippe was delivered of an elephant; that in Thessaly a hippocentaur was born of a woman; that he himself saw one brought from Egypt preserved in honey. After this, we are quite prepared to believe that Cæsar was cut from the womb of his mother.

The operation under consideration, which now bears the name of Cæsar, is, however, older than Cæsar, if the statements of the poets may be accepted. Virgil sings of one of the Trojan heroes:

> . . . . "Licham ferit exsectum jam matre peremptâ
> Et tibi, Phœbe, sacrum casus evadere ferri,
> Cui licuit parvo."

Those who were brought into the world in this preternatural manner were dedicated to Apollo, probably because he first performed the operation at the birth of Æsculapius, after he had slain his mother for her infidelities:

> "Ut tamen ingratos in pectore fudit odores,
> Et dedit amplexus, injustaque justa peregit:
> Non tulit in cineres labi sua Phœbus eosdem
> Semina: sed natum flammis uteroque parentis
> Eripuit, geminisque tulit Chironis in antrum."

The same poet informs us that Bacchus also was cut from his mother's womb. But, according to the oldest traditions, it was not Bacchus, but Apollo, the god of Light and Science himself, who, like the god of Medicine, was taken out of the womb of his mother. However, Ovid sings:

> "Imperfectus adhunc infans genetricis ab alvo
> Eripitur, patrioque tener (si credere dignum),
> Insuitur femori; maternaque tempora complet."

But it has been said that although Cæsar may not have been cut from the womb of his mother, and although the gods and heroes whose unnatural births the poets have celebrated may have had no existence except in fabled stories of mythology, Cæsarean section must have been known to the ancients, or these writers could not have ascribed the birth of their heroes to this extraordinary intervention of surgery. But might we not—resting upon the same authority, and appealing to the same quotations—claim with the same propriety, that the ancients must have been in possession of a method of preserving the life of a prematurely-expelled fœtus, by placing it in the incised thigh of its father? Or, referring to an older but more familiar incident, that an operation for disarticulating the ribs under anæsthesia must have been known in pre-Mosaic times, for we read in the Bible: "And the Lord God caused a deep sleep to fall upon Adam, and he slept; and he took one of his ribs, and closed up the flesh instead thereof."

Myths have no actual facts for their foundations—they are the unconscious products of a psychological process. Only to modern physicians, ignorant of the manner in which mythology originated, could the thought occur that such theogonic dreams as the birth of Apollo and Æsculapius were constructed after the pattern of an obstetric operation known and practised in antiquity.

To prove the antiquity of Cæsarean section, not only the myths of the classics, but also the traditions of the Jews have been cited. The Talmud contains several passages which are supposed to refer to this operation. English authors generally accept them as conclusive evidence; but some of the German writers maintain that they refer only to Cæsarean section *post-mortem matris.* The passages in question are translated into English as follows: "In case of twins, neither the first child which shall be brought into the world *by cutting into the abdomen*, nor the second, can receive the rights of primogeni ture, either as regards the office of priest or the succession of property." "It is not necessary for a woman to observe the days of purification after the removal of the child through the parietes of the abdomen." These are the quotations of the Talmud itself, which was composed between the beginning of third and the end of the fifth century after Christ. The other authorities adduced are taken from commentaries on these passages, and the oldest of these are Rashi 1029–1097 A. D., and Maimonides 1135–1204 A. D.

Now, as far as the teachings of the Talmud itself are concerned, the evidence of a birth being accomplished through abdominal section is positive, in the English translation. In the original text it is not quite so plain. In the first place, there is not a word said about cutting; and, in the second, the word translated abdomen and abdominal parietes means something else. A Latin translation, published in Amsterdam, 1702, renders the first passage as follows: "Si quis e latere prodierit et quis post illum venerit, neuter primogenitus est, neque pro hereditate, neque pro sacerdotio." The translator selected "latere" for *Joze dofan*, because it has the same wide range of meaning, and may be rendered either by side or wall, as the Germans translate it. The primary meaning of the Latin, as well as of the Hebrew root, is to surround, to enclose, to conceal. In a certain connection it might be translated abdomen; but it may certainly be used for any thing that encloses, hence for a membrane. In commenting on these passages, Rabbi Rashi says, according to his English translators: "The abdomen must be opened by samm, the child extracted, and the parts healed." Samm, it is said, means a sharp cutting instrument. It is a word derived from the same root which, in the Bible, is translated aromatics; and Reich renders it by "a medicament," and presumes it must be a powerful caustic (Vienna paste?) by which the abdomen was opened. In the Nida, samm is spoken of as a means to produce abortion; while the word translated abdomen is in the plural, and means bowels. Again, Maimonides is quoted as saying: "A woman who cannot bear a child in the natural way shall be opened in the side, and in this way delivered of her offspring." Here the word translated side is dual, and means thighs; and the Hebrew root from which it is derived means, to be tender, soft. It not only includes the thighs but the region between the thighs, the region where the hand was placed in the rite of circumcision, and those parts which became rotten when a woman became unclean in adultery. (*Vide* N. v., 21.)

I propose, therefore, the following paraphrase of the passages in question:

If a woman has a difficult labor, some medicine is first given her to open the bowels. This is a literal translation of Rabbi Rashi. That the Germans should not catch the sense of the original by a literal translation is easily conceived; but English physicians ought certainly to know what is meant by opening the bowels. Then her thighs are to be abducted, and the membrane ruptured, and the child extracted. But if under these circumstances she bear twins, neither the first thus extracted, nor the second that comes out without help, is entitled to the right of primogeniture. The reason for this is evident from the stand-point of the Jewish faith, for "it is Jehovah that opens the womb;" but in artificial delivery the midwife determines the precedence in twin-birth, and would thus confer upon the child the right of primogeniture. Why a woman should be exempted from the rites of purification under the circumstances is not so clear to my mind.

That I am right in my interpretation may be seen from a passage in the Talmud, where it is said that firstlings of animals are not fit for sacrifice if born by the intervention of art. Were I to translate this passage as the others have been translated, it would read: "When one-third of a young is born through the abdominal incision, and two-thirds through the natural way, the animal is unfit for holy purposes." This was done first, adds Rashi, by medicaments and the knife, and then the uterus expelled the other two-thirds spontaneously, or the young is torn out sometimes alive, sometimes dead. What possible sense would there be in these sentences? but read: "When one-third has passed through the thighs, i. e., the external organs of generation, and two-thirds are retained in the uterus, and there is an impossibility of giving birth, the symphisis pubis is cut through, as is done whenever an animal is butchered, and then the uterus expels the rest of the body, or it is pulled through by force; still, such a young is not fit for sacrifice, whether living or dead." And if Maimonides, who was a celebrated physician as well as a learned rabbi, suggests, "Hoc etiam fieri solet in mulieri quæ difficulter parit," he may have foreshadowed the Sigaultean operation, or, living in the thirteenth century, he may have heard reports of Cæsarean sections *post mortem matris*, and may have thought of its application to living women; but that the Talmud bears witness to the performance of that operation either upon the living or the dead, during or before its composition, I think I have sufficiently disproved.

Another authority, more venerable in its antiquity than those I have already noticed, is constantly quoted in favor of Cæsarean section. It is the so-called Royal Law, attributed to Numa Pompilius. This code, it is true, only enjoins the opening of the body of a dead woman in case she died pregnant, for the sake of delivering her child. It would be a most remarkable law if it had been enacted in the earliest period of Roman history; but its authenticity is very questionable. It is found in the Digest, but how it came there is not very well known. During the consulship of Publius Cornelius Cithegus, five hundred years after the death of Numa Pompilius, the laws of that early legislator were accidentally disinterred. They were written upon papyrus (a remarkable thing for that Roman to have had in his possession). It is not worth while to inquire whether they contained the *lex regis*; for they were immediately burned on their discovery by Q. Petilius, the prætor, because they contained things, says Livy, inimical to the prevailing religion.

Nor can we suppose that this law originated at Rome during the reign of the early emperors. In a state where every child had to be first acknowledged by its father before it acquired a legal existence; where the life of a child was always held at the option of its father; where childhood as such had no rights whatever, what motive could there be in *such* a state for enacting a law to save an unborn infant? That Rome wanted inhabitants, and a military state needed soldiers? For this purpose some states have encouraged polygamy, and Rome might have suppressed fœticide; but from Juvenal's castigations we may learn that Restelleism was practised in Rome even more freely and openly than it is in New York. To suppose that Rome would enjoin Cæsarean section to save infantile life is preposterous. For motives of such a law we must look to a state of society altogether different from that which characterized pagan Rome. If I mistake not, we shall find its origin in those ages in which so many other decretal edicts and constitutions assumed the sanction of antiquity, to give security to the possession of titles to dignities and property, or to enforce the observance of rites and usages. Toward the close of the middle ages we find the Royal Law put in force—not so much to save the life of a child, as to secure it the benefits of baptism.

After the propagation of the Christian religion, intra-uterine life assumed an importance which it never would have had in heathen lands. The two great doctrines of the faith centre in the very act of conception. It is in conception that man becomes a partaker, not only of the stain and the depreciation of Adam's transgression, as he might of tuberculosis or syphilis from a less remote ancestor, but also of the guilt of the act which first produced the moral degeneration. And the second great dogma of Christianity teaches us that, in the very act of conception in the womb of the blessed Virgin, God, by assuming human nature, delivered humanity from the stain of original sin. The Church has always taught that through baptism, and only through baptism, men become partakers of Christ's redemption. Hence during the middle ages, when these doctrines received their fullest development, especially by the labors of St. Thomas Aquinas, the attention of the Church was directed toward giving to her children the benefits of baptism at the earliest possible moment. She ordered the baptism of a prematurely-expelled fœtus, and attempted to devise means by which, if a pregnant woman died, at least the soul if not the life of her offspring might be saved.

About this time we find reports of men who were said to have been untimely ripped from their mother's womb after her death. Gorgius de Leontine is mentioned by Abraham Tiltingh, as the first who was delivered by hysterotomy after the death of his mother. Unfortunately, this author does not inform us of the sources of his information. Sancho Mayor, King of Navarre, was torn from the entrails of his mother Climine, by Gaevara, one of the nobles of the realm, after she had been slain by the Saracens. The celebrated Doge Andreas Doria is said to have entered the world after the same manner. Burkhardt, Count of Linggen, afterward Abbot of St. Gallen, was called Ingenitus, it is said, because he was not born but torn from his mother Wendilgard. Whether the name suggested the legend, or whether he actually assumed that name on entering monastic life on account of the manner of his birth, must remain uncertain. Gebhardt, Count de Bragance, and Bishop of Kostnitz, St. Lambert, St. Raymond, Nonat, and several other celebrated ecclesiastics, and among others a Pope, Gregory XIV., are reported to have been rescued from impending death by Cæsarean section *post mortem matris.* It is very difficult to ascertain whether any of these cases are authentic. The middle ages were as prolific of legends as the prehomeric ages were of myths, and these accounts would appear more credible if they were not urged to prove that an infant can live a lengthened period after the death of his mother. The Church was determined to save the souls of unborn infants. She devised Cæsarean section to secure to children of dying women, life if possible, but baptism at all events. The councils of Cologne (1280), of Sens (1514), and Langres (1404), passed canons to make this operation obligatory. Some of her most celebrated fathers, as St. Charles Borromeo, insisted on its performance in their popular teachings. They quoted such instances as I have referred to in proof that the life of a child may be preserved two, three, and even seven days after the death of its mother. Who knows but that Providence, which has so frequently suspended the laws of Nature, to establish the doctrines of Christianity by the proof and *éclat* of miracles, may also in these instances have interfered to establish and strengthen the faith and charity of the Church? But on purely physiological principles it is as difficult to believe in the prolonged continuance of fœtal life without placental circulation, as it is to believe that the Countess Matilda of Henneberg gave birth at one time to 365 children, half of which were males, and half females, and the odd one an hermaphrodite—which rests pretty much on the same authority.

It will be noticed that, so far, our accounts of this operation are derived from myths, legends, and other unprofessional sources. But toward the close of the middle ages physicians were consulted as to the best method of realizing the benevolent intentions of the Church. Versalius's theory of the respiration of the fœtus prevailed at that time, and they recommended that a reed should be placed between the teeth of the mother until the operator could arrive. And consequently the Synods of Cologne (1528) and of Cambrai (1550) did so decree. It is probable that about the same time, or somewhat earlier, the Royal Law of Numa Pompilius was first introduced into the civil code. At least, the earliest notice that we find of it, is its enactment by the Council of Venice in 1608. Afterward it was adopted and enforced by the kingdom of Naples and the Two Sicilies. These governments appointed physicians whose duty it was to perform the operation upon every pregnant woman who died.

But somewhat earlier we find well-authenticated cases of Cæsarean section *post mortem matris* recorded by medical authors. The first recorded cases which I could find were those of Cornelius Gemma, professor of Louvain; he says that he extracted six living infants after the mothers had breathed their last sighs. Horace Augenius, professor at Turin and Padua, reports that he opened the corpse of a peasant, who had died of an ulcer in the stomach, and extracted a living child, which he baptized Fortuné. Crato de Craftheim saw the great anatomist Julius Cæsar Avanzi bring a child into the world in this manner. Since that time many well-authenticated cases have been reported, but in none has the operation proved successful if it has not been performed within a few minutes of the mother's death.

There are vague reports extant in the literature on this subject to the effect that Cæsarean section had been performed upon living women during the fifteenth and sixteenth centuries. Indeed, the learned Arabians, who, by their clumsy translations of Greek authors, and the pursuits of occult arts, have acquired a factitious eminence in every branch of literature, are also quoted in this connection, as having known and practised abdominal section for the delivery of a child. But the only foundation for this assumption is a quotation in the work of the distinguished Florentine physician, Nicolaus Nicolas, who says that a case similar to that which he reports has been mentioned by Abulcassem. Now, this case has been cited as one of the earliest Cæsarean sections performed in Europe. But Nicolas himself does not relate it as an operation, but simply says that, in a pregnant woman, a diminution in the size of the abdomen was noticed, accompanied by a watery discharge from the womb, *emissis pluribus superfluitatibus*, that no fœtus was expelled, but that at last, several months afterward, fœtal bones were taken through an opening in the abdominal walls, after which the woman became well. Whatever may have been the special abnormal condition of pregnancy in this instance, it is certain that it was not relieved by Cæsarean section. On the contrary, Nicolas suggests the possibility of imitating Nature in such cases by opening the abdomen, which implies that in this instance Nature accomplished the cure by an ulcerative process.

Probably a similar condition existed in the case of Jacob Nufer, of Sigerhausen, the cattle-gelder, who is reported to have performed this operation upon his own wife at the beginning of the sixteenth century. This is the case which has been almost universally quoted as the first authentic Cæsarean section made on a living woman. But the learned Caspar Bauhin, from whom all the subsequent writers have borrowed their accounts, reports this operation nearly a hundred years after its occurrence as a mere matter of hearsay, and from his statement it is by no means clear, even if such an event had actually occurred, that it can be looked upon as a Cæsarean section; he says: "He placed his wife upon the table, opened the abdomen just as he would in a hog, and, fortunately, at *the very first cut*, the belly was opened, and a child was rapidly extracted through the wound. If this be a description of an actual occurrence, it resembles the opening of an abscess in extra-uterine pregnancy much more closely than that of hysterotomy. But, taking all the circumstances of the case together, the peculiar vocation of the operator, the opening of the abdominal cavity with one stroke of the knife, the wonderful success of the operation—for his wife lived and bore him several children without any difficulty—I am much more inclined to place Nufer and his heroic achievement among the popular legends so rife at that age than among the actual occurrences of history.

In the correspondence of Doring and Hildan on the question of the Cæsarean section, we find an operation referred to which was first reported by Nicholaus Polins, as having been performed in Weisse, Silesia, on the 9th of December, 1531, and which is generally reported among the early cases of Cæsarean section. The patient had been suffering since her first confinement from some abdominal trouble, but had, notwithstanding, survived eight subsequent deliveries. Soon afterward a swelling was noticed in the left hypochondrium, which gradually increased until it formed an enormous fluctuating tumor, in which a living child was finally diagnosticated. According to the customs of those times, a council was held between "physicians, surgeons, midwives, and other honorable women," who agreed to open the tumor. The operation was performed, and a living child was born, but the mother died a few days afterward. Further details of the proceedings are recorded, but none which would shed light either upon the nature of the abnormal condition or of the operation; and certainly, from this statement, we have no reason to suppose that this operation was a Cæsarian section.

In the same work there is another case reported which cannot be looked upon as any thing else than rupture of the uterus. The patient, Margarethe Volezer, was near her confinement in 1545, when suddenly a loud noise was heard within her, and from that time fœtal movements were no longer felt. For four years she suffered intensely; several abscesses had formed and discharged fetid matter. On the 10th of November, 1549, Paul Dirlervang, city surgeon of Vienna, made a laborotomy, and extracted by piecemeal a putrid fœtus.

A very similar case is mentioned by Achilles Gassams, as having occurred in the middle of the tenth century. The patient approached the time of her confinement, but the expected confinement did not take place. After suffering from various ills, she gradually recovered sufficiently to attend to her household duties. After some time it was found that an abscess had formed in the left lower region of the abdomen, which was opened by Clæsi, an Austrian army-surgeon; a large quantity of fetid pus was discharged, and several bones of a fœtal skeleton were subsequently extracted.

In the accounts of these operations, to which that of Primrose and perhaps one or two others might be added, we do not find any mention made of an actual incision of the uterine walls. But there is a case reported by Marcellus Donatus, which occurred in 1540, nearly fifty years before the date of his book, in which it is distinctly mentioned that, after dividing the abdominal walls and the peritonæum, an incision was made into the uterus and a dead boy extracted; after which the uterine cavity was cleansed with aromatic wine, and the womb healed, partly by the use of actual cautery and partly closed by sutures. This account might be received as true, if it were not for the personal character of the operator. This man, whose name was Christofer Bain, is described in the text as an illiterate and audacious pretender, who travelled from village to village performing miraculous cures, and who, in this instance, made the previous stipulation that he should receive ten golden ducats if he succeeded. His word seems to be the only authority for the authenticity of the operation. If he actually opened the abdomen, it is probable that what he incised, after dividing the peritonæum, was the wall of the cyst in which the product of an extra-uterine pregnancy is generally contained. This woman is also said to have conceived and borne in a normal way afterward.

In the year 1581 appeared the celebrated work of François Rousset. It bore the title "Traité Nouveau de l'Hyterotomotokie, ou Enfantement Cæsarienne." It related the histories of fourteen successful Cæsarean sections, six of which were performed upon the same individual. Most of his cases were repeated from mere hearsay, and the others are taken from letters written to him by friends. He had never made a Cæsarean section himself; but he had travelled some distance to see a woman who had been operated on, and she showed him a scar on her belly! Our faith in his accuracy or honesty will be shaken if we judge of the credibility of his histories by the account of the woman at Mesnil, who had been delivered by Cæsarean section in six successive pregnancies, and died in the seventh, because the barber Nicolaus Guillet, who had confined her before, had died, and no one else could be found to perform the operation. Regret as we may his credulity or suspect his impartiality, we cannot but admire the enthusiasm with which he defended his cause, and acknowledge the merit of his work in bringing before the profession the possibility of saving a woman who cannot otherwise be delivered of a living child.

It is true, Rousset exaggerated the facility and harmlessness of the operation which he baptized Cæsarean. He recommended it in cases where modern midwifery can see no indication for its performance. On the first page of his work he compares it to the Gordian knot, and invites the confraternity of the knife boldly to sever it. But, though his enthusiasm carried him too far, he is entitled to the honor of having first proposed it as a legitimate operation in surgery, notwithstanding that he had never performed it himself; for it is almost certain that no one had ever made it upon a living woman when he published his book.

The appearance of this work created considerable attention. It was soon translated into Latin by the learned professor of Basle, Kaspar Bauhin, and thus made accessible to the medical profession throughout the world; while Melchior Libizius made a German translation which he dedicated to the Countess Palatine Elizabeth. Rousset met with enthusiastic admirers and violent opponents. Some of his most celebrated contemporaries maintained the impossibility of performing the operation successfully. Ambrose Paré, Rollfinck, and Dyonesius, questioned the authenticity of his reported cases. The first Cæsarean sections mentioned after the publication of his work are said to have proved fatal. Guillemeau reports five such instances, and bases upon them an attack on Rousset, which called forth from the latter a reply of such forcible reasoning, says Spengler, as to secure the approbation of all his readers. A second defence was demanded in answer to a scandalous publication of Jacques Marchand, whose violence was only equalled by Sacomb, the founder of the anti-Cæsarean school.

The cases upon which the opponents of Rousset based their arguments were certainly no more authentic than those adduced by Rousset, Bauhin, and their copyists. The impression that Cæsarean section was frequently performed in France at that time is entirely erroneous. It is very remarkable, but nevertheless true, that many incidents, related and relied upon as facts in the history of this subject, were without any other foundation than the unfortunate mistakes of translators. For instance, we read, in almost every historical sketch of Cæsarean section, that the Dominican monk, Scipio Mercurio, who was also a celebrated physician, writing at the close of the sixteenth century, says that Cæsarean section was as generally performed in France as venesection in Italy, when really he only says that it was as well known. And it was well known there simply because Rousset's work had been published a short time before, and was the subject of animated discussion.

While it must, therefore, remain doubtful, to say the least, whether this operation had really been performed in France, even at the beginning of the seventeenth century, we find the record of a Cæsarean section made in Germany, of the authenticity of which there can be no doubt. And, inasmuch as it is the first well-authenticated instance in history, in which the abdomen and uterus of a living woman was cut through and a living child extracted through the wound, and as it is the type and examplar of every subsequent repetition of this operation, and furnishes illustrations of this heroic and brilliant intervention of obstetric surgery, I may be permitted to translate a full report of this remarkable event.

This operation took place at Wittenberg, and is related by Prof. Daniel Sennert, who advised and superintended its performance, as follows: "Therefore, when, on the 21st of April, 1610, the woman's labor began with the commencement of pains, the gracious help of God was first of all implored. Besides myself there were present my colleagues, Drs. Ernst

Hettenback and Tobias Yundler; furthermore the archdeacon of our parish church, M. Heinrich Silbermann, two midwives, and several other honorable women. The operation itself was performed at eight o'clock in the morning by the surgeon Trautmann, with the assistance of another surgeon Christoph Gusth. First the abdominal walls and then the peritonæum were cut through. For the last, at least as far as I could convince myself by inspection, lay uninjured beneath the skin, and could be easily distinguished from it and the edges of the wound. After this followed the opening of the uterus, which protruded beyond the normal curvature of the abdominal parietes. The hæmorrhage was not excessive, nor the pain great, as the patient herself stated afterward. After the uterus was cut through lengthwise, the child with the after-birth were taken out without any difficulty. In fact, as soon as a way was opened, the child itself, which, thanks and glory be to God! was healthy and unhurt, as it were by its own exertion sought this outlet. But, with the still existing size of the uterus, there was no prospect of bringing it back into the abdominal cavity. The blood was therefore first removed by the use of an appropriate decoction, and then the edges of the abdominal wound were drawn over the uterus and sewed together, but the wound of the latter was not secured by sutures, although the uterus was thereby saved from any notable appearance of inflammation. The womb (still protruding through the abdominal wound and exposed to the air, whose influence could not be excluded during the healing process) was covered by a layer of puriform matter, which was, however, afterward removed by proper medication. The womb assumed a clean appearance toward the fourteenth day, and regained its general appearance and shape; it also contracted more and more, and returned to its original size. Some days afterward the edges of the wound began again to assume a blackish appearance and bled at the slightest touch; in short, gangrene had so far developed itself that small pieces could be taken off with a knife without producing any pain. This untoward condition was, however, again removed by appropriate treatment. Those fleshy masses which sprouted from the edges of the wound entirely disappeared, and the latter again assumed their former color and extent, and the wound became smaller from day to day so markedly that we were authorized to expect its closure in a very short time, and that scarcely any doubt remained of complete recovery. But, on the 16th of May, at four o'clock in the afternoon, when the patient was about to lie down again, having passed a short time out of bed, she was seized with a fainting-fit and died, contrary to all expectation, in the course of about half an hour. Certainly this unfortunate woman could not have been treated properly by her attendants during the fainting-spell. But, to convince ourselves that the uterus of the deceased was all in order, and that after all no hidden suppurative process lay at the bottom of this attack of fainting and the sudden departure of the patient, we opened the womb; we found, however, no diseased condition. The child is still alive, and by God's mercy is strong and healthy."

The importance of this case and its influence upon the subsequent development of the operation cannot be overestimated. We might wish that some features of the pathological condition which necessitated the operation might be more fully described. We may regret the imperfect account given of the results of the autopsy. But we have at least here an unmistakable Cæsarean section, whose authenticity cannot be questioned. This translation is made from the "Medical Institutes" of Daniel Lemort, a celebrated professor of the school at Wittenberg, whose character and learning command implicit confidence. The facts, as related by him, have been accepted by his contemporaries. While in France the controversy raged with unabated passion as to the possibility of a successful Cæsarean section, in which one party reports only success, and the other only failure, and the want of evidence makes us doubt whether any of these cases have ever really occurred, we have at last an instance whose historical verity is beyond question. The Wittenberger Cæsarean section became the subject of many learned disquisitions in Germany. The most noted of these is the correspondence between Michael Doring, of Breslau, and Fabricius Hildanus, of Bern. But, notwithstanding that this operation received the sanction of the highest medical authorities of that age, the example of Trautmann found but few imitators during the seventeenth century.

ART. III.—*The Practical Relations of Morbid Motor and Psychical Symptoms.* By GEORGE C. S. CHOATE, M. D., late Superintendent of the Taunton Lunatic Hospital, Taunton, Mass.

THE significance of disordered motility as an indication of mental unsoundness not unfrequently becomes a most puzzling problem both to the psychologist and to the general practitioner; and a clear understanding of their true relations, and of the comparative frequency of their association, becomes eminently important to both.

A man who has had an attack of apoplexy, and who is still suffering from hemiplegia, makes a will, which very seriously concerns the welfare of many connected with him by ties of blood or friendship. Is it a valid one? Another, well known to be a confirmed epileptic, commits a deed of violence, the penalty of which may be the severest which the laws inflict. Is he a responsible agent, and a fit subject for punishment? Or, still another, whose palsied tongue, and halting gait, mark him as the victim of grave cerebral disease, enters with sanguine confidence upon extended and experimental business transactions. Should he be allowed to retain a control of his affairs, which may compromise the welfare not only of himself, but of others dependent upon him? These questions, and many others of a similar character, affecting most seriously the well-being of individuals, of families, and of the community at large, become, in the main, dependent upon medical opinion for settlement.

The fact that some, perhaps most, forms of abnormal motility are dependent upon lesion of the same central organ, which is the material agent of the mental faculties, has sometimes led to the inference that some impairment of mind must always be present in connection with them. Our great American authority, Dr. Ray, would seem to incline to this opinion, when he says that, "while a case of paralysis occasionally occurs in which no mental impairment can be detected by any practical test, it is more or less obvious in by far the greater number of cases." That the association of the two is of sufficient frequency to suggest, in all cases where important acts, affecting the rights of themselves or others, are performed by persons suffering under this class of diseases, a minute inquiry into their mental soundness, is beyond dispute.

To enter upon a consideration of these vitally important questions with a fair prospect of arriving at just and correct conclusions, a clear understanding of precisely what morbid motility and mental unsoundnesses are, and what each indicates, is an indispensable prerequisite.

Each may be, and often is, called a disease. But, it will be clearer for our purpose, and more strictly in accordance with pathological facts, to consider each a manifestation, though in totally different ways, of some lesion in the nervous system. Disordered motility may be regarded as a manifestation, through its effects upon the musclar system, of a lesion either in the motor tract of the brain itself, in the spinal cord, or in the communication between one of these organs and the parts affected. Insanity, or mental unsoundness, may fairly be looked upon as a manifestation, through impairment or derangement of the perceptive, reasoning, or affective powers, of a lesion in the psychical tract of the cerebrum. If these are true definitions, and the two so-called diseases are simply manifestations in different ways of lesions in different portions of the nervous system, not necessarily coëxistent, it follows that the diagnosis of each can be made only through proof of its existence in each particular case, and that it would be as absurd to infer that there is mental unsoundness in a given case, because hemiplegia proved cerebral lesion to exist, as it would be to infer the existence of paralysis from the presence of morbid mental symptoms. Indeed, if insanity be only a symptom, its presence can be shown only by the actual occurrence of those changes in the character, those peculiarities in the acts and conversation, which constitute it; and the attempt to make out a case by physical signs, however useful the investigation of these signs may be in reference to determining the pathology and therapeutics of the disease, must be esteemed as altogether fallacious and unscientific. But, is insanity a disease, or simply a group of morbid mental phenomena, indicating cerebral lesion? Nearly all who have attempted a definition of insanity have agreed in this, that a perfect definition is impossible. "I can describe insanity, but not define it," was the reply of a well-known American expert, now deceased, to the question, "What is insanity?" Is not the cause of this very difficulty to be looked for in the fact that insanity simply consists in an almost infinite variety of morbid symptoms, a small part of which only are present in each case. Bucknill and Tuke preface their definition of insanity as follows: "What Dr. Johnson said of any one who should attempt to define poetry, may very properly be applied to him who attempts the definition of insanity; namely, that 'such attempts at definition will only show the narrowness of the definer.' We believe it to be impracticable to propose any definition, entirely free from objection, which shall comprise every form of mental disorder. In regard to insanity in general, it may be asked, as Burton asks when speaking of melancholy: 'Who can sufficiently speak of these symptoms, or prescribe rules to comprehend them?' As echo replied to the painter in Ausonius, 'If you must needs paint me, paint a voice;' if you will describe it, describe a fantastical conceit, a corrupt imagination, vain thoughts, and different—which who can do? The four-and-twenty letters make no more variety of words in diverse languages, than it produces diversity of symptoms in several persons." Both Tuke and Bucknill define insanity as a condition in which the free action of the intellectual faculties, of the moral sentiments, of the animal propensities—one or all of them—is destroyed by disease. Is not this, in other words, a manifestation of disease through the destruction of this free action? "Mental health," says Maimon, "consists in that state in which the will is free, and in which it can exercise its empire without any obstacle; any condition different from this is a disease of the mind." Dr. Combe defines insanity to be "a prolonged departure, and without an adequate external cause, from the state of feeling and modes of thinking usual to the individual who is in health." Maudsley says, and his definition has the merit of attempting very little: "Unsoundness of mind is that degree of deviation from healthy life which it is agreed by the common consent of mankind to regard as morbid." Griesinger, the most lucid of psychological writers, and the keenest analyzer of mental phenomena, says: "Insanity itself, an anomalous condition of the faculties and of will, is only a symptom." And, again: "Insanity is only a complication of symptoms of various morbid states of the brain." Indeed, all these definitions, if fairly analyzed, agree in this, that insanity consists in an infinitely diversified group of morbid mental phenomena exhibited in the modes of thought and action. And that these morbid phenomena are due in all cases to some deviation from the normal condition of the cerebrum, by whatever title we may call it, is now the nearly unanimous belief of modern psychologists, notwithstanding the fact that the pathological change may in very many cases be impalpable to any test at present known to us. These cases, however, may be fairly claimed to be gradually becoming less and less frequent, and every advance in the means of pathological investigation has tended to confirm what may now be considered an axiom, that the brain is the source of mental life and action, and that an abnormal condition of the former can alone render morbid the latter. It is, however, no less true, and no less generally admitted, that lesion of the brain is not necessarily accompanied by both or either derangement of the motor or of the psychical manifestations, and that from proof of the existence of the former we cannot with any degree of certainty infer either insanity or paralysis. Paralysis exists only when motility is actually impaired; insanity only when perception, reason, will, or the affections, or all of them, manifest morbid irregularities. Granting, then, that there is no necessary constant connection between diseased mental and diseased motor phenomena, which are simply distinct symptoms more or less frequent in lesions of the nervous centre, there remains the consideration of the question, how often they are associated in the different forms of central disease. And at the outset may be laid down two rules, the truth of which will be fully confirmed when our investigation of the special forms of morbid motility is made. First, the higher the function and importance of the portion of the nervous system in which the lesion affecting the motility is situated, the more likely is this to be accompanied by mental unsoundness. As we descend from the cerebral hemispheres down the track of that system, which is essential to both mental and motor life, the liability of accompanying insanity steadily diminishes, while the chance of impaired motility probably even increases. The second rule is, that diffuse, not local disease, in the very great majority of cases causes mental unsoundness. On the other hand, a local disease impairs motility more thoroughly, though it may be less generally, than a diffuse diseased condition. A small hæmorrhagic effusion, even in that tract supposed to be the seat of the highest mental processes, may not be accompanied by mental impairment, while the most profound local paralysis may result. The explanation of this peculiarity would seem to be, that the symmetrical halves of the cerebrum act together mentally, but the motor tracts in each act independently, each being connected with one-half only of the motory system. The law of compensation holds good to a certain extent in mental action, and it seems highly probable that, in a strictly-limited local disease of the cerebrum, any thing more serious than a moderate diminution of power in the intellectual faculties is seldom noticed.

Dementia and imbecility are the predominant features in the insanity connected with deranged motility. All the cases, indeed, are not clearly of this type, although it may be doubted whether a certain amount of mental impairment is not always present. Still, it is often so mingled with melancholia or delusion that the latter may be the most striking characteristic of the case. Attacks of mania and violence are not uncommon, but they rarely or never constitute the whole of a case. The memory is very generally affected, and usually out of proportion to the gravity of the other symptoms. Many cases are marked by the partial character of this loss of memory, which is sometimes confined to single trains of thought, and sometimes consists only in the inability to retain in the mind the names of particular objects. Many very striking examples of this sort are given in the standard works on cerebral diseases. Another special feature of this class of cases is frequent and peculiar disturbance of the emotional nature. In very many cases the power of control over the outward expression of emotions seems to be lost, which gives the impression at first sight of a great increase in the susceptibility and strength of the emotions themselves. Such patients, for instance, laugh and cry inordinately upon occasions, which to others are simply provocative of a slight tendency to mirth or sorrow. As it is rather the expression of feeling that has become uncontrollable, than an exaggeration of the sentiments themselves, such patients often display more practical soundness in their conduct and mode of life than is expected by casual observers of them. Unnatural irritability of temper, irascibility, and intensity of love and hate are not unfrequently observed, but here too it would seem as if the power of the will to control the manifestation of these feelings, so as to make them accord with conventional usage, was often lost, rather than that the feelings were intensified. Whether this abnormal condition of the emotional nature is sufficiently marked to impair the legality of the acts of the individual, becomes often one of the most difficult points suggested to the consideration of the psychologist.

It would scarcely be within the limits of this paper to discuss the question, whether there is an emotional part of the mind distinct from the intellectual, whether there are distinct portions of the brain devoted to the emotions and to the intellect, or whether one can be so divorced from the other that the one may become insane while the other remains sane.

For the present inquiry it is enough to say that such a separation seems irreconcilable with either sound philosophy or scientific pathology. That there are cases in which disordered feelings and emotions are the prominent features of the disease is undeniable, but a strict scrutiny will invariably determine that, although there may be no hallucination, illusion, or delusion, yet that the intellectual activity and power are notably affected; and the demonstration of this in a clear manner in each case becomes the special task of the medical examiner. Dr. Maudsley says, in speaking of this class of cases: "As, however, feeling is more fundamental than cognition, the intellectual activity cannot be entirely unaffected, though there certainly may not be any positive delusion; the whole manner of thinking and reasoning is tainted by the morbid self-feeling, through which it is secondarily affected. The patient may judge correctly of the relations of external objects and events, and may reason very acutely with regard to them, but no sooner is self deeply concerned, his real nature touched to the quick, than he displays in his reasoning the vicious influence of his morbid feelings, and an answering perversion of conduct: he cannot truly realize his relations, and his whole manner of thought, feeling, and conduct in regard to himself is more or less false."

The special forms of disordered motility, and the frequency of their association with disordered mind, remain to be considered. They are of two classes. Motility may be morbidly diminished or morbidly increased. The forms of impaired motility will first be considered, and in the order of their frequency of connection with morbid mental phenomena.

General Paralysis.—This striking form of disease, now well understood and clearly defined, stands alone in the fact that the coexistence of mental and motor disorder is a constant occurrence, not an accidental or occasional circumstance, in other forms of paralysis mental aberration being either more or less frequent or always absent; and in this particular it illustrates well the rule that, where the cerebral disease causing the abnormal motility is diffuse, mental unsoundness must be expected to occur. It has been considered a matter of some interest to inquire which is the first or primary symptom in this disease, the paralysis or the insanity; and from the history of a considerable number of cases, which have been carefully watched with a view to this point, it would appear that, while the two symptoms in a majority of cases appear simultaneously, yet in the larger part of these cases, in which there is a difference of time, the mental symptoms were precedent. The relation between the two, however, is by no means constant and regular. Mental imbecility may progress with great rapidity, while the motor impairment may go on very slowly, or *vice versa*. Not unfrequently also an almost entire remission of the morbid mental phenomena occurs even when the disease has made considerable progress, without a corresponding improvement in the motor symptoms.

The kind of mental disorder manifested in general paralysis not only varies greatly in different cases, but changes not unfrequently in an unexpected way in the same case. A considerable proportion of the cases are marked very early in the disease by a peculiar joyful exaggeration of every thing connected with self, with exalted ideas of the individual's wealth, powers of mind and body, and importance in the world, and this may continue until the mental decay and imbecility of the latter stage of the disease come to take its place. This condition of exaltation was once thought to be a test of this form of disease, but longer and closer observation has shown that it does not always occur; some cases even from the outset being characterized by depression and timidity. In nearly all there is an irregularly progressive imbecility from the outset of the disease to its fatal close. In many there are occasional outbursts of maniacal fury, in many occasional attacks of deep depression. Loss of memory and loss of control over the emotions are very constant. In a few cases delusions of a terrific character, accompanied by the most fearful forebodings, and frightful spectres, are present through the greater part of the disease. It is scarcely necessary to say that, through the whole course of the general paralysis, the patient is to be regarded as irresponsible, and incompetent to transact important business. As a general rule, this class of the insane is not a dangerous one, and acts of violence are not often committed by them. But, especially during the early part of the disease, before it is fully recognized, most disastrous pecuniary transactions are often entered into, and the most absurd speculations are entered upon, by the unfortunate victim of diseased hope. Inflated with magnificent ideas of his wealth and abilities, and elated with the most extravagant expectations, he enters upon business acts which may reduce himself and his family to beggary. That cases of general paralysis, therefore, should very early be placed under proper guardianship is most important, and the diagnosis of the disease by the family physician becomes immensely important. The most sure tests in the outset are as follows:

Motor symptoms.	Mental symptoms.
Paralysis of the muscles of speech.	Exalted personal ideas — very frequent but not constant.
Tremulousness of the muscles of expression.	Loss of memory.
Loss of the coördinating power generally.	Self-neglect.
Inequality of pupils, Irregular contraction of pupils, — not constant.	Progressive imbecility.
	Change in the character, especially from religious and sedate to profane, indecent, and intemperate, — very common.

Hemiplegia.—With regard to the association of hemiplegia with mental unsoundness, there has, undoubtedly, been a greater diversity of opinion among standard authorities than with regard to any other form of paralysis. Dr. Watson, speaking of apoplexy, followed by hemiplegia, says: "The mental faculties are in some few instances quite unhurt by the attack; too frequently, however, they suffer irreparable damage." Niemeyer is of opinion that psychical disturbance is frequent, but not constant. Dr. Wood states that "the mind appears not unfrequently weakened; the patient is apt to shed tears upon slight occasions, or without any occasion; the memory is often defective, and words are miscalled, or one substituted for another of a wholly different meaning; and, in some instances, it is long before the patient recovers the power of speech after having lost it. After a time, however, which varies in different cases from a few months to several years, the apparently wrecked system is often gradually repaired by its own inherent powers, and the patient, though seldom as vigorous in mind or body as before the attack, regains a tolerable share of health, and is able to enter again into the routine of active duties." Dr. Flint, speaking of the mental condition in cases of chronic hemiplegia, evidently inclines strongly to the opinion that few, if any, cases are unaccompanied by mental unsoundness. He says: "The faculties of the mind generally, if not invariably, are more or less impaired." Dr. Benjamin W. McCready published an elaborate article in the *New York Journal of Medicine*, of September, 1857, taking the ground, which he fortified by observation in a large number of cases, that "any impairment of mind, as a direct consequence of apoplexy, after the patient has recovered from its primary effects, must be an exceptional occurrence." He also cites the well-known case of Dupuytren, who, while lecturing to his class, in 1834, was attacked with paralysis, and had the courage and mental firmness to support his face with his hand until he concluded his lecture. That mental impairment is not often the direct consequence of apoplexy is undoubtedly true, the real cause lying deeper, and being rather to be looked for in the original diffused lesion, which gave rise to the effusion, than in the effusion itself. Still, while adhering to the fundamental fact, which has been the groundwork of this paper, that the two series of morbid phenomena, the psychical and the motor, bear to each other simply the relation of different and not constant manifestations of disease in the same organ, and that the existence of each is to be independently proved—not assumed—from the fact of the presence of the other, it must yet be allowed that, in the larger part of all cases of hemiplegia, there is more or less mental impairment. The difficulty experienced by different observers in arriving at a common conclusion would seem to arise from the fact that, in very many cases, the mental unsoundness is of such a character as only to become revealed to a very scrutinizing and comprehensive search—one involving a full examination, not only of the present character and mental condition of the individual, but also his mental capacity and moral character previous to the attack. There may be no delusion, no remarkable exaltation or depression, no noticeable defect in memory, in powers of conversation, or in capacity for the performance of many of the duties of life, and yet there may have been produced a total change in ways of thinking, in tastes, and especially in the affections, and in the mode of viewing every act in which self is deeply involved. The prudent man may become a prodigal; the chaste, lewd and lascivious; the temperate, a sot; the kind, calm father and husband, an irritable and unreasonable tyrant; the religious man, profane; and he who was scrupulously regardful of the proprieties of life, low and vulgar. It is most noticeable in this connection that, while the change of character is almost invariably from better to worse, there are undoubtedly some occasional exceptions to this, in which, while the intellect is weakened, the moral tendencies become purer and more elevated. It is of the utmost importance in the examination of such cases to bear in mind, as Dr. Maudsley says, when speaking of moral insanity, that "the individual is a social element, and to have regard, therefore, to his social relations. That which would hardly be offensive or unnatural in a person belonging to the lowest strata of society, and certainly nowise inconsistent with his relations there, would be most offensive and unnatural in one holding a good position in society, and entirely inconsistent with his relations in it." Examined in this broader and more comprehensive view, many of the cases supposed to be unaffected would undoubtedly manifest unequivocal symptoms of mental impairment. Still, a sufficient number of cases remain to render it imperative that insanity should always be proved—not assumed.

Paraplegia.—As might be expected from the nature and usual cause of this form of paralysis, mental soundness is the rule, and impairment the exception. As a rule, paraplegia is generally admitted to depend upon a lesion in the spinal cord, and often in the lower portion of it. The intimate connection, however, between the brain and spinal column—so intimate that, without violence to truth, the latter might well be considered as a prolongation of the former, and the two as constituting in reality a single organ—would naturally lead to the conclusion that, not unfrequently, the existence of disease sufficient to cause paraplegia would be accompanied by such changes in the cerebral structure also as to lead to manifestations of mental unsoundness, either by an extension of the disease from the cord to the brain, or, in consequence of an original similarity in the constitution of the two in the same individual, leading them to yield to similar causes of disease. When the fundamental lesion is either spinal meningitis or myelitis, this extension to the brain, and also a corresponding condition of the cerebrum, is more certainly to be looked for. If, on the other hand, either spinal apoplexy, or some morbid growth, or parasites existing in the spinal column, produce paralysis by pressure, the probability of the brain being either concurrently or subsequently involved is very small. That a certain small percentage of the cases of paraplegia are dependent upon disease in the cerebrum solely, has also been satisfactorily shown by pathological investigation, and, in more or less of these, psychical symptoms are, of course, to be anticipated. It is evident, therefore, that although much the larger part of paraplegic patients may be entirely free from morbid mental symptoms, yet instances of the latter are likely to be frequent enough to lead to a cautious scrutiny of all extraordinary acts committed by this class of patients: and here, as well as in hemiplegia, is the necessity imperative for an investigation so broad as to take in all the elements—that of the previous character, of the social condition, and of the original intellectual power.

Partial or Local Paralysis.—As might naturally be expected, local paralyses, except of certain special nerves, have far less significance in their bearing upon the subject of mental condition. They are produced in general by causes operating outside the cranium—by injuries, by local diseases, either in the nerves themselves or in the parts adjacent to them, by cold, by long-continued pressure, or by the introduction of poisonous substances into the system. There are, however, some notable exceptions to this want of association with the mental condition. Paralysis of any of the cranial nerves ordinarily indicates some lesion within the cranium, and in not a few instances is the precursor of grave mental disorder. This is especially true of the nerves regulating the movements of the eye and mouth. A drooping of one or both eyelids, strabismus, trembling or twitching of the muscles of the mouth, and, in general, any disturbance of that nervous power which governs what is called the expression, is apt to be associated early with mental disease. Paralysis of the special senses, too, being often though not necessarily indicative of cerebral lesion, is frequently accompanied or followed by psychical disturbance or defect.

Progressive Locomotor Ataxy.—This disease, now well known to the profession through the investigations and admirable treatises of Trousseau, Duchesne, and others, has been usually thought to be unattended with morbid mental phenomena. That it may be so in the outset is exceedingly probable, but, when long continued and confirmed, we should be inclined to expect from its general pathological character that the brain would not always escape, and that mental disturbance would more or less frequently accompany it. That paralysis of the third and sixth cranial nerves is often an ear y symptom, would seem to confirm this anticipation. Indeed, a careful scrutiny of the long list of cases presented by Trousseau, as being free from any intellectual aberration, will give rise to a doubt in the minds of practical psychologists whether this freedom so certainly exists. And more recent observations would seem to show that, in not a few cases, especially when well advanced, the mind suffers to some extent. Simple dementia or a uniform impairment of the mental faculties, together with the loss of control over the emotional nature, is the form which should be looked for in this disease.

Epilepsy.—We come now to the consideration of a class of diseases of an opposite character, marked by excessive instead of deficient motility; and, of these, by far the most frequent and important is epilepsy. How far epilepsy should be considered as a valid relief from responsibility has been the subject of much debate and controversy; and at various times different rules have been laid down by those regarded as able to speak with authority. The great psychical disturbance often immediately following and sometimes preceding an actual convulsion, and the entire prostration of mental life and power during its continuance, together with the apparent soundness during most of the intervals, have led some to the belief and assertion that, for a certain length of time before and after the attack, and only during that time, a patient should be regarded as unsound and irresponsible. Such a general rule cannot for a moment be accepted by any who have had large opportunities for observing the disease. Nor can any rule be laid down for universal application. While it is undoubtedly true that, in a considerable portion of the cases, no very marked psychical derangement may be noticed, except at times near the occurrence of the attacks, and in some not even then; yet a very large part of all epileptics have more or less permanent mental impairment, and not a few are subject to periods of maniacal excitement occurring irregularly in the intervals. During the initial period of the disease no deterioration in the intervals may be discoverable; but it is exceedingly doubtful whether, in a confirmed and long-continued case, the mind ever wholly escapes damage. This has been attributed to the effects of the convulsions themselves and the repetition of the attendant cerebral congestion, and accordingly we find epilepsy figuring largely among the assigned causes of insanity in the reports of all lunatic asylums. There is some reason, however, to believe that the mental disorder should more correctly be attributed to a gradual advance in the same pathological condition which has caused the convulsions. The fact of their concurrent hereditary transmission, both being often found separately in the same family, would favor the latter theory. The substitution of one for the other has also been shown of late to be a not very unusual occurrence. Since the introduction of bromide of potassium, the control of the epileptic attacks by this remedy has in more than one reported case been attended by the development of morbid psychical symptoms before unseen in the case. The form of disease following in the train of epilepsy has some peculiarities. Its main features are those of dementia, of a gradually progressive mental imbecility. It is less uniform than ordinary dementia, however, in its effects upon the mental faculties. The memory, for instance, is sometimes retained in full force, and occasionally seems to be surprisingly developed, while other faculties are undergoing irretrievable injury. The will, too, not only is often retained in full vigor, but acquires sometimes an increase of intensity and doggedness far beyond what is ever seen in the sane. The affective nature is very often decidedly changed. Excessive morbid irritability is a very common symptom, and likewise the usual loss of control over the passions, which is expected when reason becomes weakened. Very great religious zeal and devotion to religious topics is a marked and not uncommon symptom. Intense scrupulousness and the most minute and rigid conscientiousness and adherence to some rule of life laid down by the patient is occasionally a very striking feature of the change from the normal original character. Few cases, in which mental impairment has ensued to any considerable extent, are free from occasional attacks of maniacal excitement, reaching in some cases to intense fury, and often attended with marked homicidal tendencies. Indeed, as a whole, the epileptic insane are nearly if not quite the most dangerous class, requiring to be managed with the utmost care and precaution. Outbreaks of violence are more sudden than in other patients of a similar grade of impairment, and are characterized by peculiar ferocity and determination. Occasional attacks of depression occur in a large minority of the cases. Sometimes these seem to arise from a rational appreciation of the unfortunate nature of the affliction. At other times they appear to be the product simply of the diseased cerebral action. It is, however, rather to the cases in which the deviations from the natural conduct are more trifling and less palpable that the attention of the medical expert will be more frequently drawn—to the cases where the mental impairment is more slight, and perhaps more noticeable in the affective, than in the intellectual element. When a deed of violence, or an act affecting injuriously the rights of others, has been committed by an epileptic, an investigation of the widest character is imperative, a scrutiny which shall include character, position, cultivation, former habits, in fact, all the personal elements of the case, and which shall sift all the concomitants and surroundings of the act, its motives, and its connection with healthy and natural, or with diseased and abnormal, mental impulses. Not only is it true that, while some epileptics are sane, others are insane; but it is not less true that many epileptics are at one time responsible agents, at another time wholly irresponsible. Never, perhaps, very far from the dividing line which separates sanity from unsoundness, they occasionally make sudden and deep incursions into the latter, leaving reason and responsibility behind them. To determine just where they were when a certain act was committed, is often a perplexing task; but, if all the circumstances are scrutinized, and no element of the case is neglected, a satisfactory conclusion can almost always be arrived at.

Chorea.—This disease being general in its character, and indicating some general affection of the nervous system, we should naturally expect a pretty uniform psychical disturbance associated with it. Temporary mental imbecility is a very constant symptom of the disease, and with it a considerable change in temper and affections is usually present. Attacks of maniacal excitement too are not uncommon, and may prove fatal. Ordinarily, however, the psychical and somatic symptoms keep pace, and subside together. "The mind fails in these cases," says Dr. Flint, "from the deficiency of exercise of the mental faculties." A more rational view of the matter, however, would seem to refer both sets of symptoms to disease, it may be only functional or sympathetic, of the nervous centres. Every careful observer of the disease must have noticed cases, and perhaps it would be fair to say that these are a majority of the whole, in which some change in the character, some disturbance of either the intellect or the affections, or both, precedes the somatic manifestations. Trousseau recognizes this fact, and asserts that "these prodromata consist in impairment of the intellectual faculties." Again, he says, "With very rare exceptions there is more or less impairment of the intellectual faculties." It may be doubted whether even these rare exceptions occur; and in this disease we have the uniform remarkable association of disordered intellect, increased motility, and paralysis of both motion and sensibility—for the latter undoubtedly occurs nearly in the same ratio as the intensity of involuntary movement.

Paralysis Agitans.—This disease, having a very considerable likeness in some of its features to the preceding, though occurring at a different period of life, and probably differing essentially both in its pathology and its causes, differs also from it in this, that no mental disturbances occur necessarily or early in the disease. It is, however, a progressive malady, and if the patient is not taken off, as is exceedingly common, by some intercurrent disease, sooner or later more or less mental impairment occurs in the majority of cases. This is usually of the character of simple dementia, but occasionally some degree of maniacal excitement with delusions may be manifested. The probability of complication with morbid mental phenomena would seem to depend almost entirely upon the duration of the bodily symptoms, and the progress of the central lesion, which may be deduced therefrom.

Clinical Records from Private and Hospital Practice.

ART. I.—*Extraordinary Case of Hydrocele.* By JOHN G. MEACHEM, M. D., Racine, Wisconsin.

THREE years ago I was consulted by a gentleman of Kenosha County, in this State, in relation to a greatly-enlarged scrotum. When the patient was in the erect position, the tumor reached to within four inches of the knee-joint (he was five feet ten inches in height), and it was, in circumference, twenty-three inches. It was the largest at the beginning of the lower third, though nearly uniform in size throughout most of its extent. It was natural in color, smooth, semi-elastic, but not diaphanous. The penis was entirely obliterated, having been dragged out of sight, and presented simply an umbilicated depression at about the centre of the tumor.

Fifteen years ago, while using a pickaxe in excavating for a well upon his farm, he received a severe contusion of the testicle, by the handle of the instrument, in the act of striking an unusually hard blow. Violent inflammation followed, which confined him to his bed many weeks. He recovered, but the scrotum never resumed its normal size, but was free from pain or any inconvenience, until about a year had expired, when it began slowly, but steadily, to increase, and had so continued until he presented himself to me for treatment.

All diagnostic points were carefully considered, and I very soon satisfied myself that the tumor was an immense hydrocele. I introduced a trocar, and removed *five quarts* of a very dark, almost black serum. It was on account of this dark color of its contents, rendering it so perfectly opaque, that two or three physicians, of considerable experience, had been quite undecided as to its true character, and had therefore rendered the patient timid in regard to an operation, who, no doubt, had delayed it in consequence.

Since the first operation I have repeated it six times, and have never taken less than three pints, as he defers it as long as he can, and refuses utterly to allow an injection to be thrown into the sac, or any other mode adopted with a view to a radical cure. The fluid removed from the last operations is much lighter in color.

I report this case on account of the very unusual size of the hydrocele, ranking, I think, among the largest on record.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, February 26, 1872.

Dr. Abraham Jacobi, President, in the chair.

The following physicians, recommended by the Comitia Minora, were elected to membership: George H. Humphreys, Edward C. Woodbury, William M. Bullard, Carlos P. Tucker, Alexander F. Liautard, Charles A. T. Krog, James Mitchels, J. W. S. Arnold, Edwin D. Morgan, Jr., Robert S. Prentiss, Thomas R. Pooley, Horatio Gomez, and Gilbert H. Swezey.

The President stated that the following work had been donated to the library by its author, and he was instructed to make suitable acknowledgment: "Die Geschichte der Psychologie und der Psychiatrik in Spanien, von Dr. J. B. Ullersperger, Würzburg, 1871."

Dr. P. B. Porter, from the Committee on Intelligence, read an interesting report of recent progress in Theory and Practice of Medicine.

Dr. H. Althof, from the same committee, read a portion of a long report on the Progress of Ophthalmology during the past year. The conclusion of his report, and another report expected from the committee, were postponed to the next meeting.

METEOROLOGY OF NINE MONTHS.

Dr. D. H. Goodwillie, chairman, presented the following report of the Committee on Meteorology:

May 20 to September 3, 1871.

Barometer: mean 29.87 inches; maximum, May 2d, 30.234 inches; minimum, June 12th, 29.438 inches.

Thermometer: mean, 71.91° Fahr.; maximum, July 10th, 90°; minimum, May 23d, 54°.

Rain fell on 35 of the above 105 days, making an average of rain on every third day. On 22 of the 35 rainy days there were thunder-storms. In July rain fell on 14 days, and on 10 of these was accompanied by thunder and lightning. Hail July 16th. June 18th, rain to the amount of 2.98 inches, and at 9.50 P. M. a slight shock of earthquake. Whole amount of rain during the 105 days, 21.35 inches.

September 3, 1871, *to February* 4, 1872.

Barometer: mean, 29.65 inches; maximum, September 12th, 30.324 inches; minimum, January 23d, 29.592 inches.

Thermometer: mean, 43.24°; maximum, September 9th, 68°; minimum, January 30th and 31st, 10°.

Rain on 26 days, snow on 7. Thunder-storm September 26th. Whole amount of rain, 18.06 inches.

DISEASES OF EIGHT WEEKS.

DR. CHARLES P. RUSSEL, chairman, read the report of the Committee on Diseases:

The Committee on Diseases have the honor to present the following report upon the prevalent diseases in this city from January 1st to February 24th of the present year—embracing a period of eight weeks:

During that time there were reported to the Bureau of Sanitary Inspection 596 cases of small-pox, 123 of measles, 613 of scarlatina, 119 of diphtheria, 17 of typhus fever, and 54 of typhoid fever.

The total registered mortality for the eight weeks amounted to 4,294, a weekly average of 537. During the corresponding period of the past year there were registered 3,996 deaths —a weekly average of 500. The highest weekly average for the corresponding period of either of the five preceding years was 509 in 1870. Thus far, therefore, the mortality of the present year has been much in excess over that of recent years.

The greatest mortality occasioned by any zymotic disease has been produced by small-pox, namely, 192 deaths, a weekly average of 24. It has fluctuated between 15 and 32 deaths weekly, with no apparent tendency to diminish. Its proportion of mortality, as deduced from a comparison of the number of cases reported with the number of deaths registered, has been 32 in the 100. This percentage, however, is doubtless too large, as many non-fatal cases escape the observation of the Board of Health. Forty-seven deaths have been due to measles—about six weekly—a small number. Scarlatina has carried off 190 persons, somewhat more than in the corresponding period of the past year. Both of the two latter diseases seem now to be slightly on the increase. Sixty deaths have been ascribed to diphtheria, and 118 to croup, about the usual number. Whooping-cough has continued very prevalent, its fatal cases having amounted to 165, over 20 per week. Typhus and typhoid fever have both been marked by a very small mortality, having caused but 13 and 34 deaths respectively. Thirty deaths have been occasioned by remittent fever, an excessive mortality for the season. One hundred and fifty-four deaths have resulted from diarrhœal diseases against 145 in the corresponding period of 1871. The number of deaths from phthisis pulmonalis was, until quite lately, comparatively small, much below that in the early portion of last year. But, during the past week, this disease exhibited a most extraordinary fatality, its deaths reaching 122, the greatest weekly number ever registered in this city, the next largest having been 102, in the week ending March 4, 1871. The deaths from the local respiratory affections likewise seemed to participate in the same unfavorable influences, as they occasioned 105 deaths during the week, their weekly average having been only 85 since January 1st.

Twenty-five cases of epidemic cerebro-spinal meningitis, or spotted fever, have thus far been reported, of which 12 have proved fatal. These cases have occurred, almost without exception, in houses of the very worst hygienic character and surroundings, and generally in tenements particularly defective as regards drainage and sewerage. Whether such circumstances are directly concerned in the production of this peculiar disease is certainly a question of vast interest to the profession.

CLINICAL THERMOMETRY.

Dr. Lucius D. Bulkley read a long and elaborate paper upon this subject, illustrated by numerous mural diagrams and tables. The paper was based on the essay to which was awarded the Stevens Triennial Prize two years ago, and its purpose was not to present a history of clinical thermometry, or an epitome of what it had accomplished, but rather to contribute new data for the thermometrical study of disease, in a systematized record of the experience of the New York Hospital for the three years prior to August, 1869, and to state briefly the conclusions which that experience tended to establish.

The number of cases in which a record of temperature was regularly kept amounted to 337, classified as follows: typhoid fever, 93; typhus fever, 23; pneumonia, 64; erysipelas, 24; acute rheumatism, 17; remittent fever, 12; intermittent fever, 7; scarlet fever, 7; phthisis, 19; acute meningitis, 9; tonsillitis, 7; peritonitis, 6; miscellaneous, 49. Besides the temperature, the pulse and respiration were always recorded, and the doctor had tabulated all the cases under each disease with reference to these three vital signs. Forty or fifty of those most interesting, either from their typical character or from some other circumstance, were given in detail, with diagrams showing the curves of the three signs. Each diagram was the record of an actual case, and faithfully exhibited every failure of observation; there were no typical curves made up from the generalization of many cases, as in Wunderlich's book.

The nature of the paper precludes any extended abstract, but we hope to see it published as a monograph. It is quite time that the wealth of experience, so long carefully hoarded in the case-books of the New York Hospital, should be brought out for the use of the profession. We here confine ourselves to the doctor's concluding summary of the chief points he considers established:

"1. The body heat is maintained in health, under all conditions, at the uniform standard of 98.4° Fahr.

"2. Any constant deviation from this constitutes disease.

"3. A return to and continuance at this standard marks the termination of the disease.

"4. A single high temperature is important.

"5. The changes of temperature in diseases follow definite and known courses.

"6. Variations from these typical ranges of temperature in disease are significant, as indicating a disturbing cause.

"7. An irregular course is more unfavorable than a uniformly high range of temperature.

"8. Different temperatures characterize different diseases, and various days of the same disease.

"9. Although a high temperature indicates a more severe attack, no heat under 109° can be considered surely fatal.

"10. The daily study of the pulse and respiration in connection with the temperature is of great assistance.

"11. When the temperature and general symptoms agree, but the pulse disagrees, the two former are to be relied on.

"12. When the pulse and general symptoms agree in indicating unfavorably, the temperature cannot be relied on, if contradictory, unless the improvement in respect to temperature is marked and persistent.

"13. When pulse and general symptoms agree in a favorable indication, a high or rising temperature should arrest attention.

"14. All other means of investigation should be used in connection with the temperature to obtain the greatest benefit from the latter.

"15. The continuous daily record of the three vital signs here represented, in the way exhibited, affords much aid in the diagnosis, prognosis, and treatment of disease, by the presentation to the eye of its history in these respects.

"16. The systematic record of these three points may assist in determining, at some future day, the vexed question whether the type of disease is changing, by preserving pictures which can be easily compared."

THE ALBANY MEETING.

Dr. E. Eliot, as one of the delegates to the recent meeting of the State Society, gave a summary of its proceedings.

The President wished to direct special attention to the following resolution adopted at that meeting, and hoped each member would do his part toward inducing every respectable practitioner in the county to join the County Society:

Resolved, As the old laws, requiring every physician in good standing to become a member of his county medical society, have not been repealed, it is, and always has been, the duty of every such physician to obey the law; and, as the penalty for non-compliance has been removed by law, it becomes a point of honor for every regular physician to do so. The presidents and officers of every county society in the State of New York are requested to use their best efforts to have these laws complied with.

With reference to the special meeting of February 3d, the President said that he had been obliged to call it, in accordance with the by-law providing that such a call shall be made on the written request of twenty members. Twenty-four names appeared on the request; twenty-seven members attended the meeting, and of these only five were signers of the call. A meeting so attended, after full notification, could hardly be considered to have been a public necessity; and it behooved those members whose politeness would not allow them to refuse an individual their signatures, to reflect whether they had not also some duty to the Society in a matter involving expense and the convenience of a large body of physicians.

Adjourned.

Bibliographical and Literary Notes.

Art. I.—*The Teeth, and how to save them.* By L. P. Meredith, M. D., D. D. S. Philadelphia: J. B. Lippincott & Co., 1871.

It cannot be questioned that there is a vast amount of ignorance on this subject on the part of the public; and it is to be regretted that they suffer not only from this lack of knowledge, but the charlatan takes advantage of this for his own selfish and penurious ends.

The author, in this work, desires to give information to the people in regard to their teeth, and in this will be encouraged by all intelligent practitioners. Dentistry is a most fruitful field for quacks, and it requires no little amount of knowledge, skill, perseverance, and honesty, to do professional duty. This is not fully appreciated by the people, and thus their need of instruction in it. Much is said in the chapter on food in relation to the teeth, and this is a point in which much error exists. It is an indisputable fact that one great cause of bad teeth of the youth of the present day is traced to a deficiency of phosphates. Impure milk (Nature's true food); bread, the staff of life, robbed of its phosphates; bad air, with little or no exercise—no good, durable structure can be built of poor material. The author recognizes the action of acids as the only cause of decay of the teeth, and he completely ignores the chemico-vital theory. The discussion of this point in the work, written for the information of the people, may perhaps not be so essential as many other things necessary for them to know. The prevention of caries by proper attention on the part of the patient is much and well spoken of, and also the cure of disease by removing all decay, and filling with gold, as the best material. Every intelligent practitioner will agree with him when he says that "amalgams (a paste of silver, tin, and mercury), as a material for filling teeth," is the greatest curse that has visited the dental profession. This, for two reasons: first, it makes an imperfect filling; and the effect on the general system is not good. Under the head of dental qualifications some pertinent remarks are made. Among the requirements are, a diploma from a dental college, and a skilful use in all honesty of such an education. We, however, do believe that dental colleges, as now constituted, fail to bring the proper requirements, and make the standard of education far too low. Graduates of dentistry ought first to be graduates of medicine. Dental schools should be branches of medical colleges. It would be better, in our judgment, if the author had omitted all prescription from his work. This should come through the practitioner, rather than through the medium of a popular work. As a whole, the work is a good one, and ought to be in every family.

ART. II.—*A Text-Book of Pathological Histology: an Introduction to the Study of Pathological Anatomy.* By EDWARD RINDFLEISCH, O. Ö. Professor of Pathological Anatomy in Bonn. Translated from the second German edition, with permission of the author, by William C. Kloman, M. D., assisted by F. T. Miles, M. D., etc. Philadelphia: Lindsay & Blakiston, 1872, pp. xv.–681.

SOME of us in America have been able to read, in the German journals, some of Prof. Rindfleisch's very valuable contributions to pathology. In particular, we remember a recent article upon the pathology of alopecia areata, published in the *Archiv für Dermatology und Syphilis.* A few have had leisure to read the author's treatise in the German. To those who have not had that privilege, the translation will prove most welcome, notwithstanding its numerous offences against good English.

The works of Paget and Billroth, respectively, on the subject of surgical pathology have done much to add breadth to the views of general pathology, held by the ordinary practitioner. The work now in question, studied, as it should be, side by side with them and with Stricker's "Manual of Human Histology" (which we hope to see soon in an English dress), will aid powerfully in inducing among the profession in America a diligent study of the general principles of pathology. Those who read French should by all means study it in connection the excellent manual of MM. Cornil and Ranvier.

Justice can scarcely be done to Dr. Rindfleisch's work within the limits of an ordinary review. We therefore content ourselves with little more than a few citations from the "general part" of the treatise, believing that we shall thereby best convey to the reader an idea of its scope and character.

In speaking of *necrosis*, meaning thereby local death, without special reference to the bones, our author says: "An interruption of the return of the venous blood is seldom a cause of gangrene. The interruption must be so complete, as it appears, that this condition is fulfilled in but few cases. At least, after pregnancy we frequently observe a thrombotic occlusion of the collective larger veins of the thigh without gangrene of the leg having been produced thereby. Consequently but a single case comes into question here, namely, where a part is pinched in a comparatively narrow and unyielding outlet; as, for example, a coil of intestine in the neck of a hernial sack. The flaccid walls of the veins are then compressed sooner than the arteries, and thus the return of the blood may already have ceased long before its afflux is interrupted; hence a decided hyperæmia of the necrosed portion is also to be expected here."

We find the following interesting remarks upon rigor mortis as compared with gangrene of muscle: "The striated muscular fibre presents in its necrotization somewhat more complicated relations than the cells hitherto considered. . . . In from twelve to fourteen hours after the occurrence of death, all corpses, with the exception of those [of persons?] suffocated by charcoal-vapor or sulphuretted hydrogen, those killed by lightning, or dying from putrid fevers and long-continued debilitating diseases, fall into a peculiar stiffness, which lasts, perhaps, twenty-four hours, and, observed more accurately, amounts to a pretty considerable shortening, thickening, and induration of the voluntary muscles. Even in such limbs as are suddenly robbed of their supply of blood, we observe the same behavior of the muscles; we can produce the same experimentally not only by interrupting the supply of blood, but also by heat and cold, over-exertion, mechanical injuries and chemical agents, but most rapidly by distilled water.

"Numerous investigations, of which we owe the latest and most exhaustive to W. Kühne, have left no doubt concerning this, that this stiffening of the muscular fibre depends upon the separation of a solid albuminate from the muscular juice. This coagulum (myosine, Kühne) forms a white but slightly transparent mass, and thence causes a striking opalescence of the stiffened muscular fibres, which is conjoined with a discoloration verging upon brown.

"Rigor mortis is indeed the first step toward death, but a step which can be again recalled. We can very readily produce rigor mortis of the thigh of a frog, by tying the afferent arteries, and see the normal condition return by removing the ligature."

The author needlessly apologizes for the somewhat therapeutical character of the section on the resolution of interstitial inflammation, which is so very suggestive that we here reproduce it entire:

"It is manifest that, if, by our medical skill, we could succeed in removing again the cells which have wandered into an inflamed organ, this organ would return to the same condition in which it was before the inflammation, provided we deduct the modification of the connective-tissue fibres mentioned in the previous paragraph, which meanwhile would likewise soon disappear. The question of the possibility of a resolution of inflammation, and the means of inducing the same, has therefore a highly practical interest. It might at first occur to us to send away the cells in the same way by which they came, that is to say, to let them wander farther. In this sense moist heat is applied with advantage. Exaltation of temperature also accelerates, as is known, the movements of amœboid cells. Where, therefore, the inflammatory infiltration is not great, and the inflammatory irritant does not continue to act, one may hope, by locally increasing the temperature, to diffuse the already present wandering cells over a greater space, and gradually to convey them into the lymphatics. A second mode of resolution of existing inflammatory infiltration becomes possible by the fatty degeneration of the cellular elements. We saw above, how fatty degeneration converts all kinds of cells into a milk-like detritus, of whose immediate resorption naturally no hinderances stand in the way. The presence of abundant amounts of fluid in the inflammatory focus appears to be a decided condition for the commencement of fatty degeneration. Busche has made the interesting experience [*sic*] that, under the influence of erysipelas, massy sarcomatous proliferations disintegrated, and I have most certainly convinced myself, in one of his cases, that the sarcoma-cells thereby fall into fatty degeneration. Heat would also be an appropriate remedy for keeping down a lasting hyperæmia of this kind. In spite of this double indication, the moment of time must be exactly considered in which one may pass from the cold to the warm treatment of an inflammation. The object of the cold treatment is, by an artificially-induced contraction of the vessels principally to restrain exudation, with reference to prohibiting the further emigration of colorless blood-corpuscles. We would only pass to the use of warmth, either when this indication has been fulfilled, or when it can no longer be fulfilled; for it is evident that heat is a two-edged sword. Who will be surety that, instead of a dispersion of the exudation, which indeed we would first of all desire, a stronger concentration of mobile cells shall not occur at the heated point, that is to say, suppuration and formation of abscess? A certain amount of heat evidently acts dispersing; a higher degree irritating to the process of inflammation: the former causes the already exuded colorless blood-corpuscles to wander farther; the latter causes the process of emigration to renew itself, and it increases in intensity."

In addition to the points which we have thus briefly mentioned, we would especially commend to the reader's attention the author's remarks on the *dyscrasic conditions*, embracing the consideration of the blood *as an organ;* on the pathological processes involved in the various *elementary lesions of the skin;* on *tuberculosis;* on *inflammatory affections of the kidney;* and on *cystic disease of the ovary.*

Dr. Rindfleisch's work is the result of laborious investigation and compilation. As a treatise, it appeals more directly to the professed investigator than to the general student of medicine. Pathological histology is, at best, hard reading for the majority of busy, practical men, and it is therefore to be so much the more regretted that the translators of this work should not have given us at least good English. As it is, the French text of Cornil and Ranvier is to us much more tempting than this badly-translated German. We feel under obligation to them, however, for introducing the work, even in its present shape, to the English-speaking profession.

The book is in every way creditable to the publishers, being well printed on good paper, well illustrated, and well bound.

ANNOUNCEMENTS.—Messrs. Lindsay & Blakiston, Philadelphia, have the following books in preparation: A Manual of Dental Mechanics, by Oakley Coles; Club-Foot: its Causes, Pathology, and Treatment, by William Adams,

F. R. C. S.; on Rheumatic and Strumous Diseases of the Joints, by the same author; on Winter Cough, by Horace Dobell, M. D.; Lewis on Syphilis, and its Treatment by Subcutaneous Injections of Sublimate.

Books and Pamphlets Received.—Reports of the Trustees and Superintendents of the Butler Hospital for the Insane. Providence, R. I., 1872, pp. 22.

Clinical Observations on the Dementia of Syphilis. By M. H. Henry, M. D. New York: F. W. Christern, 1872, pp. 15.

Reports on the Progress of Medicine.

THEORY AND PRACTICE.

1.—*Prophylaxis, Symptoms, and Treatment of Cholera.* By Inspector-General Dr. John Murray.

At the last meeting of the British Medical Association, Dr. Murray read a paper on this subject, of which *The Doctor* gives the following synopsis. He said his opinions are supported by a very great majority of the medical officers now in India, as shown in a recent report submitted by him to the Indian Government, in which the opinions of five hundred and five medical officers are carefully tabulated.

Cholera is a specific disease, caused by the presence of a specific poison in the system; it multiplies or is reproduced; it must be vital and amenable to the ordinary laws which regulate other specific poisons, modified by the peculiar structures of the body which are chiefly affected. It must enter the body through some of the ordinary channels. Before health can be restored, it must be eliminated either in a vital state or after being decomposed or digested.

The poison leaves the body through the same channels by which it enters, viz., the bowels, the lungs, and the skin. Its presence in the discharges from these organs is recognizable in most instances by the smell—a mawkish, sickening odor, well known to those who have seen much of the disease. In the earlier stages the poison appears to be destroyed, or digested, without exciting any active symptoms, and this is the safest way of Nature getting rid of it. The first active symptom is diarrhœa; and here we can be useful, as the system during this stage is amenable to remedies.

All experience of the course of the disease teaches us that it rages with the greatest intensity, and proves most fatal, where people are collected in great numbers—where there are crowding and filth, defective ventilation, and impure water; and that it is aggravated by want and bad food.

Sanitation is of the greatest importance in the precautionary treatment of this epidemic, in ameliorating the virulence of the attack, restraining its dissemination, or warding it off entirely.

In the stage of *malaise* the poison is thrown off, without any violent or very prominent symptoms, by the natural functions of the system. Our task here is to support the strength, avoid indigestible food and depressing causes. The only medicine that I have found useful in this stage is a little quinine every day. The subsequent indications of the treatment are to remove the abnormal symptoms as they appear, of which the most early is *diarrhœa*. The first indication is to check this and restore the case to the stage of *malaise*, then remove the cause, and restore the natural secretions. Irritating or indigestible food in the bowels is the most frequent cause of diarrhœa; and, should this not previously have been discharged in the evacuations, it should be removed, and a recurrence of the looseness guarded against, as I have always found it the most powerful exciting cause of collapse. I have found this best carried out by a combination of opium with carminatives in the form of a cholera-pill, composed of one grain of opium, two of black pepper, and three of assafœtida. It appears to check the looseness and stimulate the secretions. This pill does no harm if needlessly administered. It should be repeated should the looseness continue. It will cure most cases, and in all restrain the symptoms until regular medical advice can be procured. This is a most important point in the use of this simple remedy. It may be distributed to every house, and be available in a few minutes, whereas the delay of a few hours may allow the disease to advance beyond control. I know no better remedy for this stage. These pills have been distributed in tens of thousands in the towns and villages of India, with most satisfactory results. Some surgeons prefer red to black pepper, and others add camphor to the opium and assafœtida.

In collapse, our power is limited by the circumstance that the vital organs are insensible to the ordinary action of medicines. Experience shows that opium, astringents, and alcohol, lie inert in the collapsed stomach, though these are the ordinary remedies for pain, looseness, and debility. It is also my experience that the free use of these remedies at this stage causes death, either by preventing reaction, or by causing local complications should reaction appear.

There is another cause of death which is not generally understood, but which it is in the power of all sufferers or attendants on the sick to check or to prevent. I allude to the extreme danger of assuming the erect posture, or even of sitting up in bed, during collapse, or the earlier stage of reaction. I have seen myself, and I have heard of many cases, where fatal syncope instantly followed sitting up in bed or rising to go to stool.

The first sign of reaction is coincident with the appearance of bile in the evacuations. The dilution of the irritating contents of the bowels, and the restoration of the watery particles of the blood, are indicated and best fulfilled by frequent small quantities of cold water, to which a little soda or carbonate of ammonia may be added with advantage. In protracted cases I have seen decided benefit from the use of Liebig's extract of meat, made fresh and given frequently. I have also seen most marked benefit from the exhibition of hot saline enemata given after each motion. In some instances it has acted like magic, the symptoms subsiding after one injection, but in many others they have been powerless. I have thought that the artificial supply of Nature's own remedies in the stage of *malaise*, the secretion of which is suspended by the action of the poison as the disease advances to collapse, might be useful, and the results in a few cases were highly satisfactory; seven out of nine having recovered, and the two fatal cases having been pulseless and dying before the remedies were used: these remedies were gastric juice and bile, in the form of acidulated pepsine, fifteen grains, and inspissated bile fifteen grains, given alternately every hour. The dose of bile was followed by vomiting; but bile soon appeared in the evacuations, and mild reaction set in gradually. Shampooing with warm turpentine liniments gives relief to the cramps, and mustard-poultices on the epigastrium restrain the vomiting. I think I have used a little quinine with advantage when Nature made an effort at reaction.

Calomel is inert in collapse, both in large and small doses, and probably the benefit attributed by many to its use arises from its being employed instead of spirits or strong remedies. There is danger of its being accumulated in large quantities when reaction takes place. Sulphuric acid and acetic acid are less dangerous; but I have not seen decided benefit from their use. I have not found advantage from ammonia, except when added in small quantities to the cold water. I found, in 1833, the transfusion of saline fluids into the veins caused most hopeful reaction; but it was only temporary, and this is the general result of numerous trials made by other medical officers in India. Brandy I consider dangerous in proportion to the quantity given in the stage of collapse, and opium as decidedly poisonous in this stage. Chloroform, though it may give temporary relief, tends to induce dangerous head-symptoms on reaction. Astringents are not beneficial. Purgatives are dangerous in the earlier stages, and not useful in collapse; they are generally condemned in India. Heat has been extensively tried by warm baths, but the fatigue entailed is dangerous. It has been tried in the form of hot-air baths, but the result has not been encouraging.

When reaction takes place, rest and careful nursing will complete the cure where collapse has not lasted long; but in protracted cases, in addition to these remedies, medical treatment may be required for low fever, uræmia, or local complications, regulated by the ordinary rules.

2.—*Case of Strychnia-Poisoning, and Recovery under Treatment with Calabar Bean and Chloroform.* By John White, M. D., Glasgow. [Glasgow Medical Journal, August, 1871.]

On the 15th August last, at 10.10 P. M., I was called on by Mr. P., who said that he would like me to come quickly and see his servant-maid, who, he thought, was either mad or dying. I went at once, and found the girl in bed in a prone posture, and in state of tetanic spasm. On making examination, the smallest touch induced powerful spasmodic convulsions; in fact, a condition ending in complete emprosthotonos. Her eyes stared wildly, her pupils were dilated, jaws firmly closed, respiration difficult and laborious, pulse very quick. The paroxysms returned every thirty or forty seconds. During the paroxysms she seemed inclined to turn on her side, and the violence with which the jaws closed was suggestive of the action of a rat-trap. On the accession of each paroxysm she howled fearfully, and so loudly that neighbors above, and below on the same stair, were kept in a state of terror for several hours. Her cries seemed slightly to precede the muscular contraction. During the short intervals, I, with difficulty, elicited the information that she had taken vermin-killer, with suicidal intent, and also that she did not wish to recover. From the symptoms, I had no difficulty in concluding that the poison had been some form of strychnine. I at once mixed a tablespoonful of mustard with a tumblerful of water, and tried to force her to swallow it. From the clinched condition of her jaws, I could only succeed in forcing a small quantity down her throat, which was almost immediately rejected in the same state as when swallowed; and it failed to induce vomiting. I then put her under the influence of chloroform, and sent for my neighbor, Dr. Niven. On his arrival at 11 o'clock, seeing that the state of the jaws precluded the use of the stomach-pump, we agreed to try the effect of the Calabar bean, conjoined with chloroform. Half a grain of the extract in the form of tincture was accordingly administered at once, and the chloroform continued, remitting it every fifteen minutes or so, to examine her condition.

For an hour and a half the paroxysms continued, though by the end of that time they were much decreased in intensity. At 12.45 the paroxysms returned with their original violence. The pupils were still dilated; the pulse 130 to 140, small and irregular. Another half-grain dose of the extract was then administered, and the chloroform continued as before. While under its influence the pulse fell to 88, full, soft, regular; but, so soon as the effect of the chloroform wore off, it again mounted to 130. This was observed on each withdrawal of the anæsthetic. While under its influence she frequently made use of the expression, "O poor Bob!" suggestive of some love-affair having had something to do with her present situation. The chloroform was continued remittingly till two o'clock, when it was withdrawn for a short time. The pulse rose to 100, and, on touching her body, spasms were again excited, though not violently as before. She now complained of pains in the head and jaws, and expressed herself as anxious to recover. Cloths wrung out of cold water were now applied to the head, which she said eased the pains considerably. The pupils were now contracted, though not very much.

At 2.45 she vomited freely, and by 3.30 the spasms had almost entirely disappeared; pulse 86, small, soft. I saw her again at 9; found her much exhausted; complained of pain in almost every part of the body, particularly the muscles of neck and jaws; feels as though she had been thrashed from head to foot. I was then able to examine her more particularly, and to obtain the following history:

M. T., aged twenty, height four feet eleven inches, stout, strong, and healthy-looking, had come recently from the country, had been a very short time in her present situation. Since her arrival in town she had formed the acquaintance of a young tradesman, of whom she thought a great deal. His employment failing in Glasgow, he had left for some other part of the country. Since his departure she had been in very low spirits—so low that she had resolved on self-destruction. With this intent she went to a druggist's shop and bought fourpence worth of poison, which she said was to kill mice. On her arrival home she mixed the poison in a cup, by means of a spoon, with cold water, and drank it off. She then poured water on the "grounds," as she called it, and drank off every particle; and having burned the wrappers, "went to bed to die." She had taken no food for three hours previously, and then only a spare meal of bread and tea. As nearly as I can calculate, I saw her twenty-five minutes after she had swallowed the poison. My patient recovered rapidly, and was able to be sent home in the course of next day. I have heard of her since her return to Ayrshire, and no bad results seem to have followed. To corroborate part of her statement, I went to the shop she mentioned, and found that a woman answering to her description had, at the same time she indicated, purchased two two-penny packets of vermin-killer for the purpose of killing mice.

The powder similar to which she swallowed I now show you. It is in moderately fine powder, of a bluish color; metallic taste, not unlike that of sulphate of zinc, and very bitter. It mixes readily with water, is partially soluble in cold, and entirely so in boiling water. Under the microscope it appears starchy looking, with admixture of small crystals, and in a regular state of division. Tests for strychnia having been applied, that substance was found to be abundant. To save the troublesome process of a quantitative analysis, the person who prepared the vermin-killer was communicated with. He courteously replied that each six grains of the preparation contained exactly one grain of strychnine. Now, as the two two-penny packets weigh exactly twenty grains, this gives three and a third grains strychnine as the quantity swallowed. The question here arises, Did she swallow the whole of the quantity she bought? From the circumstantial manner in which she described her process of mixing and swallowing the dregs, along with her determination to commit suicide, I have no doubt of it.

Another question may be put: Did she reject any part along with the mustard-and-water thirty-five minutes after swallowing the poison? I am of opinion that she did not; the rejected portion seemed not to have come from the stomach at all, but simply the ounce, or two at the most, of mustard-and-water which I had tried to force her to swallow; and the subsequent persistence of symptoms goes far to confirm this.

As to the treatment, I cannot say it was strictly scientific; but the end justified the means. The urgency of symptoms was such that we used two remedies, the action of which, so far as we know, is not physiologically incompatible; but, how far each individually, or both conjointly, acted toward the end attained, I am not prepared to state. We know that strychnia destroys life by acting on the nerve-centres, and producing spasmodic contraction of the muscles of both respiration and circulation; and we are aware that chloroform abolishes reflex action. Is it not, then, likely that the chloroform, combined with the Calabar bean, had, so to speak, restrained the physiological action of the strychnine until it had exhausted itself, and been eliminated from the system?

3.—*Desperate Attempt at Poisoning by Chloroform and Morphine; Recovery.* By E. L. McTyre, M. D., Capivari, Brazil. [Nashville Journal of Medicine and Surgery, December, 1871.]

A short time since, I was called to attend a man, aged thirty years, who, for the purpose of self-destruction, had taken, at a dose, and without dilution, one ounce each of chloroform and sulphuric ether, and eight grains of morphine. This man is of a respectable family, and wealthy; but, like Naaman the Syrian, he is a leper, and, although the disease is in its incipiency, the dark shadows it casts of a hopeless future of misery, of hideous, revolting deformity, and, above all, of separation from family and friends, made him, as he said, prefer strangling and death rather than life, under such circumstances. On pretext of wanting the medicines for a violent toothache, he easily obtained the medicines from a druggist in this place, and they were from the best European houses.

For two days previous to the occurrence he had refused all food, and, consequently the stomach was in a proper condition to be readily impressed with the medicines.

He swallowed the potion at 7 o'clock in the morning. His family were attracted to the room at once from hearing him coughing spasmodically, and, on being asked what was the matter, he pointed to the empty phials, labelled with the names of the medicines, standing on a table near him. An hour passed before an attempt was made to give him any thing, when some person present suggested the idea of giving him vinegar and water, of which he swallowed a few spoonfuls. His condition appeared so hopeless to the family, that they thought he could not survive until a messenger could reach a physician, being distant eighteen miles to the nearest.

At noon, finding he did not die, I was sent for, and reached the house of the patient at 5 o'clock in the evening, ten hours after he had taken the mixture. Found him covered with a profuse perspiration; pulse quick and full; eyes insensible to light, and pupils very contracted; stertorous respiration, with mucous rattle; in short, having all the symptoms of profound narcotism.

I attempted to give him some water, for the purpose of seeing if he could be made to swallow an emetic, but, on trial, I found I could not do any thing in this way. I proceeded then to apply mustard to the spine, extremities, and chest; and, although the mustard-plasters caused vivid redness of the skin wherever applied, the patient showed no disposition to emerge from the comatose sleep.

At 7 o'clock, finding no improvement, it occurred to me to try the effects of dressing him in clothing saturated with a strong infusion of coffee, after the same manner that we use iodide of potassium, sulphur, etc.

A strong infusion of coffee was prepared, and a linen shirt and pair of drawers were allowed to remain in this for half an hour, and, being partially dried, were put on him, and then covered with two blankets. In about thirty minutes the patient moved uneasily about in the bed, and again became quiet. Ten minutes after this he turned suddenly to one side, and vomited profusely, and asked for water. During the next hour he vomited frequently and freely.

For the purpose of combating the effects of the morphine, I gave him, during the night, one drachm of bromide of potassium, in divided doses.

When once aroused from the comatose state, he evinced no disposition to return to it, but conversed naturally, and during the rest of the night slept very little. I feared he would suffer from the irritant effects of the undiluted chloroform, on the coats of the stomach, but up to 9 o'clock next day he made no complaint. I have since heard he recovered without an untoward symptom.

SURGERY.

1.—*Successful Application of the Trephine over the Right Lateral Sinus of the Brain.* By Paul F. Eve, M. D., Professor of Operative and Clinical Surgery in the University of Nashville. [Nashville Journal of Medicine and Surgery, December, 1871.]

I take leave to report the following unusual operation—the successful removal of four disks of bone from over and near the right lateral sinus; this, too, in direct opposition to our best authorities in surgery. Mr. Erichsen declares that "there are certain parts of the skull—over the venous sinuses, for instance, near the base—to which no prudent surgeon would apply the instrument." Prof. Gross, too, says: "There are certain parts of the skull where, if it is possible to avoid it, the trephine is never to be applied;" and he specifies the lateral sinuses, to which he makes an additional objection to operating over them, due to the inordinate thickness of the occipital bone.

My patient, Mr. D. P. Eubanks, a stout, healthy man, aged forty-two years, while Sheriff of White County, Illinois, was struck from behind with a bludgeon, having a knot or bend in it, and knocked insensible for sixteen hours. This occurred two years and ten months ago; in the mean time he had sought relief by visiting Evansville, Cincinnati, St. Louis, etc., and found one surgeon who ventured only to cut down on the depressed bone. Finding I had returned from the last-named city to Nashville, he arrived here on the 25th of October, and was put upon treatment preparatory to a tentative operation at trephining.

Precisely midway between the occipital protuberance and the external meatus of the right ear, there was a depression in the cranium of about three-quarters of an inch in depth, with the circumference of a silver halfdollar. This resulted from the blow already referred to; and, while the patient had not become epileptic, he yet experienced symptoms demanding operative interference. One professional gentleman, as we have mentioned, had already attempted this. Mr. Eubanks was now habitally costive; walked with difficulty, and only a short distance; complained of constant weight and oppression in his head; of a dull, annoying pain, radiating at irregular intervals from the point injured; had lost his energy; was never cheerful, and was losing flesh and strength. Nothing could provoke a smile; was almost hopeless, and said that he occasionally felt like losing his senses.

Dr. Buchanan unexpectedly coming into my office, first encouraged an operation, by stating that his father once saved a patient in whom the longitudinal sinus had been opened; thus confirming the opinion that wounds of the large venous canals of the brain were not necessarily fatal.

On the 28th of October (Dr. Buchanan being indisposed), assisted especially by Drs. Briggs, Van Lindsley, Fitts, and others, and before the class, the usual crucial incision was made over the depression in the skull; then, by detaching the occipital portion of the occipito-frontalis and the insertion of the trapezius muscles, thus getting below the superior curved line of the os occipitis, the half-inch crown of Galt's trephine was applied, and, working it very cautiously, in about fifteen minutes a button of bone was removed, fortunately, too, without wounding the dura mater. It was our design to cut through the osseous structure, found, happily, quite thin, directly above the right lateral sinus, and now, by sawing out three other circular pieces of bone, it was made evident that our object had been carried out; for, in the space made by the elevation of these three disks, lay, undisturbed, the lateral sinus, readily recognized by the deeper color of its venous blood. Nothing now remained to complete the operation, which was performed in about forty minutes, but to cut off the angles left by the trephine, round off the bony opening, and secure over it, by silver-wire sutures, the four angular flaps of the scalp. Some five ounces of blood was lost, but only one artery was tied. Coming from under the influence of ether, the patient expressed himself feeling better, and was soon after conveyed to the hospital.

He was put on the most rigid after-treatment—absolute diet and perfect repose. A slight reaction the next day was met by small doses of sulphate of magnesia; and he subsequently experienced not a serious symptom. Of course one so long and so seriously afflicted could not recover at once, and had periods of depressions, but nothing more. Locally, cold cloths were applied, then changed to emollient tepid poultices; a very blunt probe was inserted every day or two, at the most pendant cut, to prevent accumulation under the scalp, which gave exit first to sanies, for some days, when the wire sutures were removed, and gradually the secretion became lessened and converted to laudable pus. The wound was thus kept open to the thirtieth day, when it gradually closed.

The case found most similar to the one here presented is recorded by my friend Prof. F. H. Hamilton, in his "Treatise on Military Surgery," page 240, where he notices the injury sustained by a lad ten years of age, who was brought into Bellevue Hospital with fractured skull, made by the wheels of a street-car. A fragment of the occipital bone had lacerated the lateral sinus, the hæmorrhage was profuse, but completely arrested by a pledget of lint secured by a roller. The patient, however, died during the night—due, undoubtedly, to extensive fracture of the os occipitis and injuries within the cranium.

The learned Velpeau published that Warner, Marchetti, Garangeot, Sharp, Pott, Callisen, Mosque, and Lassus, had opened various sinuses of the dura mater, and without unhappy results; and moreover, that Biluger, Copland, Gooch, Abernethy, and Hutchinson, had exposed the brain in perforating the occipital bone. Over the protuberances of the cerebellum, he adds that there are no arterial branches except the occipital, and the lesion of the trapezius or complexus muscles is of not much importance.

Prescott Hewett, Esq., the author on Injuries of the Head, in "Holmes's System of Surgery," declares that the trephine has been applied, and successfully too, close to the foramen magnum occipitis.

In the French "Dictionary of Medicine," in thirty volumes, article on "The Trepan," we translate that surgeons have been forbidden to operate over the course of the venous sinuses of the brain for fear of hæmorrhage resulting. This, however, is not so dangerous as was formerly believed, as the blood does not flow from them with much force, and the slightest compression will arrest it; so, then, continues he, these operations may be performed when a fracture compresses the venous canals, or spiculæ of bone are thrust into them.

Knowing full well the responsibilities involved in the above case, the operation would not have been attempted, but for the presence of the patient's father (now aged seventy-two), and the valuable assistance of my colleague, Dr. Briggs. Its successful termination is due, in my judgment, much to the good conduct of the patient himself, to the thinness of his cranium where trephined, and particularly to the fact that the dura mater was not opened. For this all-important result we are much indebted to the instrument revived by Dr. Galt, of Virginia, in 1860—the conical trephine.

2.—*Indications for the Employment of the Catheter in Old People.* [Practitioner, January, 1872. From Lucas-Championnièr's Journal de Médecine.]

M. Guyon, in one of his clinical conferences at the Hôpital Necker, lately remarked that retention of the urine is very common in old men, depending generally on affections of the bladder, or of the neck of the bladder, or of the prostate. Many cases of supposed vesical paralysis are in reality due to prostatic disease. Retention of urine in old people displays itself by symptoms that are eminently variable. Sometimes these symptoms are strongly marked; the patients require to micturate frequently, and in doing so experience pain and burning heat which lasts for a long time; there may even be constitutional and febrile symptoms. In other instances, again, the symptoms are by no means prominent, especially in those cases where the bladder is but little contractile; the retention is then only indicated by percussion, palpation, and catheterism, the latter alone in many cases being reliable evidence of its presence. But this indication that catheterism should be adopted as an exploratory means is somewhat delicate, for the operation is not always inoffensive, and the patient suffering but little, subsequent troubles may be attributed by the patient or by his friends to the injudicious interference of the surgeon. If, however, the symptoms be well marked, then there is no room for hesitation, and M. Guyon even goes so far as to say that the catheter should be passed in the case of every old man who evacuates the contents of his bladder imperfectly. He thinks that it is not necessary that it should enter the organ on the first occasion, since, if only introduced as far as the neck, it habituates the tissues to the contact of instruments, and indicates, in part at least, the seat of the disease. Stoppage of the flow of water is always a serious symptom in old people, and the best advice that can be given to them is to be sounded either with a simple sound or with a catheter, and that frequently. Indeed, if relief be not speedily afforded to such patients, dangerous symptoms soon make their appearance in the form of rigors, purulent urine, and violent reaction.

Purely medical treatment is of no service in such cases, and he gives an instance in point. In 1869, in the month of September, M. Guyon had in his wards a man aged forty-eight, who, after having been treated by ordinary remedies and by rest, left the hospital, but returned in January, 1870. He was now suffering from orchitis and distinct enlargement of the prostate; he passed water frequently; the urine was thick, but was voided in sufficient quantities to lead to the belief that the bladder was thoroughly emptied. On the 1st of February there was some fever present, and on the catheter being introduced about four ounces of urine were drawn off, and on a second occasion about six ounces. He was sounded four times, and was then told to sound himself. No other treatment was adopted. On the 7th the urine was clear yellow, and on the 15th he was able to remain five hours without urinating. Catheterism practised in his case twice a day caused no return of the epididymitis. In another case, occurring in a dyspeptic subject, all the symptoms of cystitis were present. For a long time M. Guyon hesitated to sound him, and for two months he was treated medicinally without effect. At length he was catheterized, and the urine drawn off. The symptoms immediately diminished in intensity, and from this moment the urine, which had up to that time been troubled and imperfectly discharged, became limpid and even entirely evacuated. A third patient passed blood, and was obliged to remain in the recumbent position. After careful exploration, M. Guyon recognized the existence of retention of urine, and passed a catheter. From this time all the symptoms of stone, of which the patient complained, disappeared. Thus, not only stoppage of the flow of water occasions grave accidents, but it simulates other diseases; it causes alterations of the walls of the bladder, and provokes cystitis. When the bladder is greatly distended, however, it is imprudent to evacuate it completely. The frequency with which catheterism should be repeated is an important question. No absolute rule can be laid down, but it may be performed every five hours; but commonly the instrument should only be passed when there is intense desire to urinate. If, however, he experience but little or no inconvenience, it should be passed at regular intervals. As a rule, the permanent retention of the catheter in the bladder is to be avoided, except, perhaps, in cases when the desire to pass water is very intense and frequent, or when the introduction of a catheter is very difficult. M. Guyon cites a case where it was worn for two years. It should in general be fixed in position till the bladder is habituated to catheterism. As adjuvants to the above treatment, injections may be employed, which may be hot, cold, or medicamented, as occasion may require.

OBSTETRICS AND DISEASES OF WOMEN.

I.—*Hæmorrhoids in Pregnant and Puerperal Women.* By FORDYCE BARKER, M. D. [American Practitioner, March, 1872.]

We make the following extract from Prof. Barker's excellent paper on the above subject, giving in part his views on treatment:

When hæmorrhoids are developed during the later periods of pregnancy, the indications are obviously to counteract the constipation or the diarrhœa, and to stimulate and to restore the tonicity of the hæmorrhoidal veins. The inquiry will then naturally suggest itself, Have we any agent or combination of agents in the materia medica capable of effecting these results? I know of no article which so clearly and positively produces these two results as aloes, and on this I have mainly relied. I am well aware that the general voice of the profession is against the use of aloes where there is any tendency to hæmorrhoids. That "aloes is contra-indicated by hæmorrhoids" is not only the doctrine of the "Dispensatory of the United States" (Wood and Bache), but it is also the opinion of most writers on materia medica from ancient times down to the present day. "Fuchsius was of opinion that, of one hundred persons who should take aloes frequently as a laxative, ninety would be attacked with piles. Murray blames physicians who are induced by the gentle and certain action of this medicine to expose their patients to so serious a consequence. It was to this purgative that Fonsecha attributed the prevalence of piles among the inhabitants of Padua, and Stahl makes a similar statement in regard to the people of Hamburg. Calvin is cited as a prominent example of this mischief produced by aloes; for this celebrated reformer is said to have died ultimately from the effects of the piles which it gave rise to; but, as he was of a frail constitution, subject to quartan ague, gout, and gravel, the part which aloes bore in his demise may reasonably be judged to have been small." But these opinions have not been accepted by all; for Cullen, Sir Benjamin Brodie, Trousseau and Pidoux, and others, have doubted whether aloes is productive of piles, but attribute this infirmity not to the medicine, but to the constipation which aloes is used to remove. I will, however, parenthetically say here, from my own observation, I am convinced that aloes will, under certain conditions of the system, and in certain doses, develop piles. The special property of aloes is "to excite the muscular contractility of the colon and rectum," and "to stimulate the venous system of the abdomen, and especially of the pelvis." That these are the effects of this agent I have not only the authority of special writers on therapeutics, as Pereira, Wood and Bache, and others, but I believe the general experience of the profession also will confirm the assertion. It would seem, therefore, that the use of aloes for the cure of hæmorrhoids in pregnant women would have suggested itself from *a priori* reasoning; but I am not aware, from any thing that I have read, that it ever has. I suppose that the general impression that aloes is contraindicated where there is any tendency to hæmorrhoids, and that it possesses emmenagogue properties, has had great influence in preventing this. In my own case the use of this article for this purpose was the result of gradually accumulating observation rather than from any reasoning on the subject.

In the early days of my professional life I was engaged to attend a woman in her confinement who suffered from obstinate constipation. I prescribed for her Dewees's pills. At the time of her confinement she mentioned that in her former pregnancies she had suffered very much from piles, but that my pills had cured them. If I had known of her hæmorrhoidal tendency I should not have given these pills, and I was therefore quite surprised by her statement, as the result seemed so contrary to all that I had been taught. From this time I began to experiment as to the effect of aloes in the treatment of hæmorrhoids, associated with constipation, in the pregnant; and for many years past I have constantly made use of aloes for their cure, whether the hæmorrhoids were the result of constipation or of diarrhœa. I give it, combined with other agents, according to the special indications of each case, and in such doses as I learn, by experience of the peculiar idiosyncrasy of the individual, are necessary to secure one easy, free, daily evacuation of the rectum. Some require a grain morning and evening, while in others a half-grain is sufficient. In anæmic patients I combine the aloes with the sulphate of iron. In the last two weeks of gestation I always combine it with the extract of belladonna. The following is a frequent prescription with me:

℞. Pulv. aloes soc.,
Sapo. cast., } āā ℈j;
Ext. hyoscyami, ʒ ss;
Pulv. ipecacuan., gr. v.
M. Ft. pil. (argent) No. 20. S. One morning and evening.

When the patient is anæmic, I add to the above one scruple ferri sulphat. Some ten days or two weeks before the supposed time of labor I substitute the extract of belladonna, ten grains to one scruple, for the extract of hyoscyamus. When the hæmorrhoids are associated with an irritable rectum, and frequent, small, teasing, thin evacuations, I substitute for the hyoscyamus a small quantity of opium, giving a smaller quantity of the aloes, as in the following formula:

℞. Pulv. aloes soc.,
Ext. opii aq.,
Sapo. cast., } āā gr. x.
M. Ft. pil. No. 20. S. One morning and evening.

It is unnecessary for me to multiply formulæ, as the general principles by which I am guided will be sufficiently evident from the above.

In some cases I have not been consulted, and have not known of the hæmorrhoidal tendency of the patient, until my attendance during labor. I have seen the hæmorrhoidal tumors sometimes become very large during the labor. Dewees says: "Much may be done during labor to prevent a severe spell of piles by the accoucheur making a firm pressure upon the verge of the anus with the palm of his hand, guarded by a diaper, during the progress of the head through the external parts, and by carefully returning them after the expulsion of the placenta, as the sphincter is now fatigued, and will not oppose their descent." I have frequently tried this expedient, but I cannot say that it has been very successful, as the tumors soon come down again, and under these circumstances they are very apt to become strangulated, inflamed, and cause a great deal of suffering. When I find this condition of things, I have within a few years past adopted the plan of forcible dilatation recommended by my friend and colleague Prof. Van Buren. My method is this: The patient being fully under the influence of chloroform, I select the moment after the delivery of the child and before the placenta is brought away. I push back the tumors within the sphincter, if I can readily; if not, I leave them alone, and introduce both thumbs, back to back, well in the sphincter, and, opening them as wide as possible, I draw them through the sphincter. During this time I have firm pressure made on the uterus by an assistant, and in several instances the operation was followed by the sudden expulsion of the placenta from the vagina. I direct the following ointment to be applied twice daily to the tumors, and well up in the rectum:

℞. Ung. gallæ co., ℥j;
Ext. opii aq., ℈j;
Sol. ferri persulph., ʒj.
M. Ft. ung.

The result has been in every instance that the tumors have graduall. disappeared, and the patients have had very little suffering from the operation.

When hæmorrhoids come on after labor, the suffering is generally much greater than when it occurs during pregnancy. They are very often induced by the action of the purgative given two or three days after confinement.

It is now many years since I have been convinced that castor-oil was one of the worst agents that could be used as a laxative when there is a tendency to piles, as in many instances I have seen its action develop them. For many years I have annually spoken of this to the medical class before whom I have lectured, and I have received many letters from former students corroborating my statement by their own observation. But I have never seen this alluded to, except in one work—viz., Hardy and McClintock on Midwifery and Puerperal Diseases—who incidentally make the following remark: "We may first observe that castor-oil is ill suited for patients who have hæmorrhoids, being very apt to produce in them tenesmus and considerable irritation of the rectum." I may add the following from Quain: "Common opinion has assigned to castor-oil a character for blandness (probably because of its being an oil) to which it is not entitled. It is an efficient purgative, but, except when given in minute quantities, it usually irritates the rectum."

In those who have or are predisposed to have hæmorrhoids, I give the following on the second day after confinement:

℞. Magnesiæ sulph.,
Magnes. carb.,
Potas. sup. tart.,
Sulphur. sublim., } āā ℥ ss.

Mix thoroughly. S. One, two, or three teaspoonfuls of the powder before eating in the morning.

This powder produces a soft evacuation, without pain, even when the hæmorrhoids are inflamed.

2.—*Case of Extra-Uterine Fœtation and Superfœtation.* By Frank Argles, L. R. C. P. E., L. M., M. R. C. S. [Lancet, September 16, 1871.]

On September 3, 1870, I was called to see Mrs. S——, whom I found suffering from constant nausea. I prescribed suitable remedies, and succeeded to an extent in stopping it. The catamenia were irregular, coming on for one or two days, and then stopping for a week, and again returning. This went on to the end of December, when they stopped. In the month of January I informed her that she was pregnant, and was able to detect the child by abdominal examination. She seemed to progress favorably until April 10th, when I was sent for in consequence of her stating that she felt the child was dead. On stethoscopic examination, combined with other symptoms, I found she was correct. Three days after this I was hurriedly sent for, and on my arrival the nurse showed me something that had come away, and which proved to be an ovum of about two months. This surprised me not a little, and I kept the specimen. On examining externally, I could still detect the dead child, and by passing my finger into the vagina could feel nothing through the os uteri, which was but slightly dilated, and would not readily admit the finger. External to the os, on the right side, the child could be distinctly felt. The abdomen appeared to be of a natural shape, not larger on one side than on the other. On percussion there was dulness about the navel, but it was tympanitic on either side. The abdomen gradually increased in size until about the end of June, when it seemed to be very much distended. Her general health was bad; sickness and fainting, with bilious diarrhœa, in which there was no solid matter, frequently occurred. Not being clear as to the exact condition of the uterus, whether there was a fœtus or not within, I dilated the os with a sponge-tent, the string of which broke, and gave some trouble in its removal. However, it was clear that the uterus was empty. As her symptoms indicated peritonitic irritation, and she was becoming worse, I asked Dr. Hicks to see the case, and he was of opinion that it was one of extra-uterine fœtation.

On Sunday, July 9th, I saw her about 11 A. M., and she appeared to be more comfortable than usual. About 3 P. M. on the same day I was sent for, and found her retching a good deal, and bringing away a greenish fluid. She complained to me of having felt an acute pain, as if something had burst. Several minor pains followed. About 7 P. M. I again saw her. She was unable to retain any thing upon her stomach, and I could see she was fast sinking. Death took place at 1 A. M. I was exceedingly anxious to have a *post-mortem* examination, which unfortunately could not be performed until three days after death. At this I was assisted by my friend Dr. Hawkes. On opening the abdomen, the child (a boy) immediately came into view. It was lying obliquely from right to left; its head was pressed forward on the chest; the occiput was turned toward the liver; the hands were folded under the chin; the back was turned forward, and the legs were drawn upward. The thighs of the child pressed the womb very much downward and backward toward the rectum, in the shape of a shield; the bladder was small, and pushed a good deal forward; the intestines were chiefly lying high on the left side, and quite agglutinated, a cyst having formed around the fœtus. The liver was adherent to the abdominal muscles, and large flakes of hair were found closely attached to its under surface. The left ovary was entire, but very large. There was no trace of the right, although the Fallopian tube on that side was much dilated, readily admitting two fingers. This led me to believe it was a right tubo-ovarian pregnancy, the ovary and end of Fallopian tube being the original envelope. Decomposition was so much advanced that, on removing the child, the attachment of the placenta could not be found. The child was large, and appeared to be about seven months. It was very perfect in every particular, except the legs and feet, which were short and rickety. On examining the womb it was found to be rather large, and very adherent to the surrounding parts. After minutely inspecting it, we could find no opening into it except through the right Fallopian tube.

The peculiar feature of this case is, the length of time that elapsed between the death of the child and that of the mother, coupled with the intra-uterine ovum.

Miscellaneous and Scientific Notes.

Changes in Bellevue Hospital Medical College.—The following important changes have taken place in the Faculty of this College: Prof. B. W. McCready having resigned, Prof. W. A. Hammond will assume the chair of Materia Medica and Therapeutics, and Clinical Medicine, in addition to that of Diseases of the Mind and Nervous System. On the latter branches he will continue his lectures and clinics as heretofore. In consequence of the resignation of Prof. Stephen Smith, Dr. A. B. Crosby has been appointed Professor of Descriptive and Surgical Anatomy. Dr. E. G. Janeway has been made Professor of Pathological Anatomy, and will lecture on that subject in the regular session, while, as Professor of Practical Anatomy, he will act as Demonstrator in place of Dr. Mosely. Prof. Mott relinquishes Surgical Anatomy to the chair of Anatomy, but will hold his Surgical Clinic at the College on Wednesdays throughout the year. A new out-door medical clinic has been established under charge of Prof. Flint; and a new clinic for Diseases of Children, under charge of Prof. Lusk. Dr. E. L. Keyes has been appointed Professor of Dermatology.

The following gentlemen will lecture on special subjects during the summer session: Prof. W. A. Hammond, Public Hygiene; Dr. Leroy M. Yale, Surgical Dressings; Dr. J. D. Bryant, Surgical Anatomy; Dr. F. A. Castle, Diseases of Infancy; Dr. T. M. B. Cross, Medical Application of Electricity.

Surgery of the Kidneys.—Dr. Gustav Simon, Professor of Surgery in Heidelberg, has just published the first part of a work entitled "*Chirurgie der Nieren.*" This part is devoted to a detailed account of a case in which the author performed the "first successful operation for nephrotomy upon the human subject," together with the history of his experiments and deductions from them.

His patient, forty-six years of age, had undergone, eighteen months previously, an operation for ovariotomy, both ovaries and the corpus uteri being removed. In the operation the left ureter was severed, and, after recovery, the urine escaped through a fistulous opening in the abdominal wall when the patient assumed the upright position, but when lying down it flowed away through the canal of the cervix and vagina. There was direct communication with the vagina through the abdominal opening. The life of the patient was rendered almost unendurable by the constant discharge of urine, and the discomforts incident thereto.

Simon first proposed to heal the abdominal fistula, thus causing the urine to pass through the canal of the cervix, then to establish a vesico-vaginal fistula, and close the vagina below it. He succeeded in closing the abdominal opening, but, as soon as the patient assumed the upright position, after the wound had healed, the urine would not flow through the cervix, the cicatrix was forced to yield, and the urine escaped as before. He then assayed occlusion of the calibre of the ureter (with nitrate of silver), in the hope that atrophy of the kidney would result. Such violent constitutional disturbance followed this experiment, twice repeated, that he was compelled to abandon it. No hope of relief now remained, except from extirpation of the organ. The dangers to be feared from this operation would be uræmic intoxication and peritonitis. Simon ascertained, by experiment, that no uræmic symptoms resulted in dogs after the removal of one kidney, and, in the human subject, the kidneys being extra-peritoneal, the risk from peritonitis would not be very great. After careful pathological observations, and after many times performing the operation upon the cadaver, and, reasoning from analogy of other surgical procedures upon abdominal organs, Simon, with the approval of the consulting surgeons, decided upon nephrotomy. The operation was performed in August, 1869—the incision being made along the outer border of the sacro-lumbalis, and extending from the eleventh rib half-way down to the crest of the ilium—a distance of from nine to ten centimetres. During the first twenty-four hours after the operation, the patient passed 460 cc. urine, containing four per cent. urea. No uræmic symptoms, nor indications of peritoneal inflammation, were at any time present. Convalescence was, however, very protracted. The patient left her bed first upon the thirty-eighth day, but six months had elapsed before her health was fully regained. There was no subsequent hypertrophy of the left ventricle of the heart.

Simon affirms that the result of this operation proves that "the removal of a healthy kidney exercises no appreciable unfavorable influence upon the health of an individual," because, he says: 1. There exists in the organism a tolerance of a very considerable amount of the matters excreted by the kidneys. 2. The quantity of urine to be excreted, while the patient is undergoing recovery, is much less than in health. 3.

The remaining kidney instantly increases its excreting capacity, and in time actually increases in size. To the question whether renal disease is not more dangerous, when only one kidney exists, than when both organs are present, he replies that the increased amount of danger is so slight that it should not deter us from operating, when the proper indications are present.

Prof. Simon, of Heidelberg, reports a case of vesico-vaginal fistula, in a patient eight years of age, caused by a calculus ulcerating its way from the bladder through into the vagina. The stone, composed of urate of ammonia and phosphates, and weighing 45 grammes, was removed, leaving an L-shaped fistula, 5½ centimetres long by one centimetre wide. He was obliged to make five operations before the fistula was entirely closed — the first operation, December 10, 1866, the final one, August 23, 1869. Simon says this is the first case reported of successful operation upon so young a child. There are only two other cases published, both of which were unsuccessful. The difficulties in the operation arise from the narrowness of the vagina, and the thinness of the vesico-vaginal walls. He denuded the edges of the wound as obliquely as possible, and attributes his success in obtaining union to this circumstance.

Simon, of Heidelberg, has operated several times for the removal of malignant growths from the various cavities of the body, by scraping among the diseased tissues by means of spoon-shaped instruments, with oval blades and sharp edges, varying in size from that of a pigeon's-egg to that of a bean—the handles being adapted in their length to the cavities in which they are to be employed. He has operated for cancer, three times upon the cervix and once within the cavity of the corpus uteri, once upon the posterior wall of the vagina, once upon an epithelial cancer of the bladder in a female, introducing the instrument through the urethra, and once upon the posterior walls of the pharynx. He recommends this method of extir_pation also for tumors of the orbit ot the antrum of Highmore, and of the rectum, and claims for it "advantages superior to those possessed by any other means of operation, upon all friable tumors with large bases, situated within the cavities of the body." He asserts that the tumors can by this means be more thoroughly removed, and with the least danger of injury to the surrounding tissues; the hæmorrhage is almost insignificant. In operations upon the uterus and vagina the pain is so slight as scarcely to require the employment of an anæsthetic; relapses, when they recur, take place at a much later period than after operations with the knife.

Simon has made several series of experiments with urine, in order to ascertain accurately how it exerts its deleterious influences upon wounds. He first endeavored to satisfy himself whether or not, by means of its chemical properties, urine acts injuriously upon the tissues. He injected into the subcutaneous connective tissue of rabbits normal acid urine, urine mixed with blood, mucus, and pus, and alkaline urine, with the following results: no ill effects were produced by small quantities of the first two kinds, but injections of the alkaline urine caused the entire series of symptoms which we are accustomed to attribute to urine in general. He then experimented to find out the length of time required for acid urine in contact with animal tissues to become alkaline. No alkaline reaction was developed earlier than the fifth day, and usually not before the tenth. He then inquired if acid urine, remaining for a long time in contact with the tissues of the body, would not act injuriously, without undergoing decomposition. His experiments enabled him to answer this inquiry in the negative. Lastly, it remained to find out how to account for the sloughing which takes place in the human subject so quickly, and over so great an extent of surface, after infiltration of urine, which is without doubt acid. This sloughing is not produced by any injurious properties of the acid urine, but is the result of a purely mechanical cause, viz., the pressure exerted by the extravasated fluid upon the tissues.

From his experiments he decides that it is unnecessary, in operations for fistulæ, etc., to devote so much care to keep the surfaces of the wounds from contact with urine, and that the use of the catheter may be dispensed with in the after-treatment. In cases where, from cystitis, or other cause, the urine is alkaline, this condition should be obviated before operating, or else the greatest attention must be given to preven the urine touching the denuded surfaces. The proper treatment for "infiltration of urine" is, to leave the already extravasated fluid to take care of itself (it will be absorbed), and, in order to prevent further infiltration, to open the urethra through the perinæum, and introduce a catheter through the wound into the bladder and leave it there.

Simon is satisfied, from his experiments with saliva upon the surfaces of wounds, that no one need at any time hesitate to operate within the buccal cavity, through fear of failure to obtain union from the injurious effects of the saliva; and, further, that when the operation does not succeed, we must look for the cause of failure elsewhere than to the saliva.

Inflammation of the Vascular Walls. [Durante, *Med. Jahrb.*, 1871, 3 H.]—In order to prove the physiological relation of the nutrition of the coats of the vessels, especially of the non-vascula intima in rabbits, the vena jugularis was ligated without injuring its sheath, at first peripherically, and then, after ligation of all the lateral branches, centrally, in order to empty the venous trunk of blood; and on the carotid, the same operation was performed in an opposite manner.

The examination, forty-eight hours after, showed that, in the neighborhood of the ligatures, all the layers of the vessels were studded with pus-globules; at a little distance from the ligatures, however, no striking change had occurred. In a second series of experiments, the vena jug. et art. carot. were exposed and removed by isolation from the nutritive influence; the current of blood, however, was not interrupted. Fifty-two hours after, the examination showed that the vascular walls were more or less changed. The inner surface was opaque; the endothelium of the veins partly destroyed, partly displaced by rounded cells. Small prominences on the inner surface proved to be coagula. The muscular tissue of the artery, whose endothelium was better preserved, appeared as if macerated; the corpuscles of the muscular fibres were angular, and at times broken in pieces. In five cases the necrosis extended twice as far as the intima. From these experiments the author deduces that the intima does not receive sufficient nutritive material immediately from the circulating blood, but that especially in the veins it is nourished by diffusion of the fluids which the other coats furnish. By cauterization of the exposed veins by the actual cautery (still better when the peripheric ligation was previously undertaken) the author was able to demonstrate in twenty-four to fifty-six hours, generally by the use of the chloride of gold or the nitrate of silver, changes in the intima, which consisted, on the one hand, of an increase of the intercellular substance in the entire periphery of the endothelium, and in the other, of an increase of corpuscles in the endothelium, a granulation of the same, and a partial change into fibres. The examination of veins, altered in various ways by inflammation, presented also a reaction in the smooth muscular fibres, whose nuclei took on proliferation, and whose corpuscles even were divided into small sections, so that the author, as regards the venous walls, asserts that "the endothelium, muscular cells, and connective-tissue corpuscles take part in the proliferation."

In order to throw light upon the relations between the intima and the blood in the occluded venous sacs, the vena jugularis of rabbits was compressed by the insertion of insect-pins and a ligature, without direct injury to the coats of the vein. The examination, thirty-six to forty-eight hours after, showed that the blood in the occluded vein remained fluid as long as the endothelium had not received inflammatory changes through the mechanical irritation of the needles; and that, further, the coagulation of the blood went hand in hand with a change in the endothelium. From these observations the author assumes that the fluidity of the blood in occluded vessels is dependent upon the normal functions of the vascular walls.

On Polypus of the Rectum in Children. (*Jahrb. f. Kinderkrankh.*, 4. II., 1871.)—Prof. Bokay met with 25 cases of rectal polypus in 65,970 sick children. One of these polypi, examined by Prof. Schenthaler, presented the following: The tissue was similar to the ordinary mucous membrane of the large intestines, but was richer in extremely delicate-walled blood-vessels and cells, of which it was not quite clear whether they all lay in the meshes of a reticulum or whether they themselves entered into the structure of the latter, as it has already been plainly shown that in hypertrophic and inflammatory mucous membrane the reticulum which has returned to the embryonic condition is formed of anastomosing cells. Embedded in the tissue of the polypi were gland-follicles with long cylindrical epithelium (Lieberkühn's crypts), and these were either simple or bifurcated. Many of the cylindrical cells were goblet-shaped, chiefly, perhaps, on account of the hardening by the chromic acid, while others may have presented themselves in the living state. The reticulum was thickened around the crypts, and the blood-vessels throughout very thin-walled, so that some appeared as mere sinus-like passages in the parenchyma. The symptoms produced by the presence of a polypus of the rectum are—irregular dejections, constipation alternating with diarrhœa, and passages from the bowels consisting of mere bloody slime. The fæces, which generally produce some pain in passing, are here and there smeared with bloody slime or streaks of blood; but rarely is the polypus so consistent as to make a furrow in the fæces, and very rarely is the blood accompanying the passages of any considerable amount. At each passage of somewhat compact fæces the polypus is pressed into the anal opening, and it is at this moment that the diagnosis is most surely to be made out, while the investigation with the finger or with the rectal mirror does not always suffice; it is hence often necessary, in the diagnosis of polypi in larger children, to encourage straining, in younger ones to produce the effect by clysters. Polypus of the rectum will not be mistaken for prolapsus of the rectum, or for dysentery, with ordinary care. Hæmorrhoidal tumors do not occur in childhood. The seat of the polypi is on the posterior wall of the rectum, between the external and internal sphincters, rarely higher up; the author has never met with several polypi in one individual. The prognosis is always favorable; the treatment which the author can recommend as the simplest and most reliable is the ligature, with or without a separation of the pedicle from the tumor. All after-treatment is superfluous; the author has never met with much bleeding, or ill results after the operation. Where proper assistants are wanting at the operation, the author recommends a sort of serres-fines-like rectal polypus clamp of his own invention, which fixes the polypus without disrupting it, and permits the ligature to be placed very conveniently.

The Peristaltic Movements of the Intestinal Canal. [*Maandblad v. d. Genootsch. t. ber. v. Nat. Genees. en Heelk. t. Amsterdam*, 1871, No. 6 et 8.]—In order to observe the intestines, bladder, uterus, etc., under the most favorable circumstances, Dr. Sanders, after ligating the œsophagus and anus, and inserting a canula with a long tube in the trachea, places the animal in a solution of common salt (six per cent.) at a temperature of 38° C., and opens the abdomen under the surface of the fluid. This latter is retained at the same temperature. In this manner the intestines remain for a long time in a normal state. On opening the cavity, the intestines were always at rest. Movements soon set in, with contractions of the longitudinal as well as of the circular fibres. The former remained principally local, but increased and diminished at times. The convolutions of the intestines then took on a sort of wave-like motion; the latter more often passed onward, and this always in a direction from above downward, toward the rectal end. Mechanical irritation acted feebly only, and at times the intestine could be strongly compressed without any movement resulting. Sanguineous congestion varied from time to time without demonstrable cause, and frequently remained for a long period. If the animal was obstructed by dyspnœa the vessels became strongly contracted, and then again distended, when the breathing became normal again. The congestion of the vessels has no marked influence on the intestinal movements, and even compression of the aorta does not cause movement in the intestine when at rest, and will even weaken the movements when present. The uterus acts much like the intestines. The active movements of the intestines which occur during suffocation, are entirely absent when both vagi are previously divided in the neck. Irritation of the peripheric ends of the vagi causes their return. The movements occur principally at two points, at the lower end of the duodenum, and the upper end of the ileum. Irritation of the right vagus causes, for a time only, the movement of the duodenum, and irritation of the left vagus that of the ileum. The stomach at times remains quiet; generally, however, from time to time it projects its contents toward the duodenum. In one case of diarrhœa Dr. Sanders observed an anti-peristaltic movement in the colon; at all other points it was peristaltic. The urinary bladder at times showed movements, even when empty. These were always peristaltic. Dr. Sanders in no case observed anti-peristaltic movements in it; peristaltic and anti-peristaltic movements occur in the Fallopian tubes.

Chloral in the Cure of Venereal Ulcers. (*Gazetta Medica Ital. Lomb.*, No. 31, 1871).—Soon after the announcement of the employment of chloroform in the treatment of primitive ulcers, either soft or indurated, Dr. Mancesco Accettella instituted a series of experiments which plainly convinced him of the few indications of this preparation in the cure of venereal ulcers. He says: While busying myself with similar experiments, Liebreich published his observations on hydrate of chloral as actively hypnotic and anæsthetic. From that time I commenced to adopt that preparation as a local remedy, in concentrated solution, upon ulcers of such ancient date that neither the acid nitrate of mercury, nor the carbo-sulphuric paste, nor other efficient caustics, had been able to effect a cure. The effects greatly exceeded all my expectations. After the first application the deep parts of the ulcer became detached, healthy and normal granulations sprung up, and the ulcer was transformed into a simple sore, with the usual tendency to cicatrization. In sixty-nine cases in which I applied the chloral topically, the following results were obtained: seven ulcerated and obstinate abrasions healed in nine to sixteen days; forty-nine soft ulcers, in from eight to fourteen days; three soft ulcers, complicated with diphtheria, in eighteen to twenty-nine days; five soft ulcers, complicated with phagedæna, in twenty-four to thirty-two days; five primitive infectious ulcers, in fifteen to twenty days. Among the cases of phagedenic ulcers it is necessary to note two, which, for twelve or fifteen months had resisted all local and general treatment, although the two women affected had the most fresh and florid constitutions. The solution employed was as follows: chloral hydrate five gram., aqua destil., gram. twenty. This, applied with a pencil to the ulcerated surface, slightly cauterizes, without producing discomfort to the patient. After two or three applications all unhealthy appearance at the bottom of the ulcer disappears, and the sore presents a bright redness which soon produces the most healthy granulations. A considerably diluted solution should be adopted for the cure of abrasions and simple ulcers. Venereal therapeutics may therefore welcome the chloral as a topical remedy, *par excellence*, in the gravest form of ulceration, the phagedenic.

Tænia following the Use of Raw Beef. [Levi, *Giornal. veneto d. Scien. Mediche*, ser. iii., tome xiv.]—In an essay read before the Venice Athenæum, Dr. Levi gives his experience with the frequent occurrence of tænia after the use of raw beef. This communication is the more deserving of the notice of practical physicians, since they are daily in a position to order raw beef as a medicament in the various forms of tuberculous disease, and also since the writer suggests the means by which this untoward event may be avoided. The most important portions of this exceedingly instructive article are as follows:

Thirty years ago, Prof. Weisse, of St. Petersburg, advised the use of finely-shredded beef as a most effective remedy against the chronic diarrhœa of infants. Trousseau confirmed the beneficial results of the treatment, and after him many other physicians of repute. The remedy is now known and recognized by physicians everywhere, and indeed not only in the cases of children, but also for adults in the most varied diseases connected with a wasting of the tissues, and especially pulmonary consumption. Dr. Levi reports in detail nine cases which he had observed in the course of a year, and in which, after the use of raw beef, tænia had developed. Two species of tænia occur in the human body; the tenia solium (*armata*) whose scolex is found under the name of cysticercus suinus in the muscles of the hog; and the tænia mediocanellata (*inerims*), whose scolex resides exclusively in the muscles of the bovine race. All the tænia which Dr. Levi met with in his patients belonged to the species mediocanellata. Dr. Levi adduces another series of cases, recently observed by Venetian physicians, of tænia after the use of raw beef, which, with his own, reach the number of twenty-four, a number which appears fully sufficient to prove the occurrence beyond a doubt. Levi comes to the conclusion that raw beef, however effective its action may be, should not be ordered, since the usual remedies against tape-worm are not always effective, and the same at times produce serious digestive disorders and affections of the intestines, from which the patient recovers with difficulty. He prefers, therefore, to administer the raw flesh of chickens, in which, thus far, cysticerci have not been discovered, although in the intestines of fowls several species of tænia (*infundibuliformis proglottina*, *crassula*, *malleus*) occur.

Worm-Abscesses, so called.—A woman, fifty years old, presented on her admission into the hospital at Freysing, in the right ileo-inguinal region as far as Poupart's ligament, a somewhat resistant, sharply-defined, immovable tumor, lying directly beneath the skin, giving pain, on pressure, on movement, and also spontaneously. There were, at the same time, constipation and vomiting. This condition had continued four days. In the course of about two weeks the tumor broke of itself, and there flowed from it pus and living thread-worm. From time to time there flowed also a yellowish, slimy, stringy fluid, and a development of intestinal gases was also noticed from the wound. Hence the diagnosis was, perforation of the intestines. Finally the wound healed up, and the woman recovered. This case would formerly have been reckoned as a so-called worm-abscess, with the theory that the worm had perforated the intestinal walls. Such a theory is combated to-day for the following reasons: When worms are found in the abdominal cavity through abscess of the abdominal walls, or when they pass through the vagina or bladder, we assume to-day (after Bamberger and others), either that a passage has already been opened by ulceration or softening, which the worm has only to follow, or to enlarge by means of its moderately firm head-end. None of the known varieties of worms possess an organ by which to effect a perforation or eating through of the intestinal coats. Against the assumption of Von Siebold and others that the ascarides are able, by means of the resistant head and with its three projections around the mouth-opening, to merely press apart the intestinal walls, so that the opening closes again behind them, Bamberger asserts that this could, at the most, occur in the muscular layers, but not in the mucous and serous membranes of the intestines with their dense structure. According to Buhl and others, collections of worms may cause stretching, softening, diphtheria, and finally perforation of the intestines. This the author does not believe. When, as in the above case, after perforation of the cæcum or vermiform process, ascarides are evacuated with pus and the intestinal contents, the former is not to be regarded as the *causa proxima* of the entire process, but simply as an accidental phenomenon.—*Hug. Allg. Med. Centr. Zeitg.*, 1871.

American Medical Association.—The twenty-third Annual Session will be held in Philadelphia, Pa., May 7, 1872, at 11 A. M. Committees are expected to report—On Cultivation of the Cinchona-Tree. On the Anatomy and Diseases of the Retina. On the Comparative Pathology and the Effects which Diseases of Inferior Animals have upon the Human System. On the Structure of the White Blood-Corpuscles. On Vaccination. On Skin-Transplantation. On the Nature and Process of the Restoration of Bone. On some Diseases peculiar to Colorado. On Correspondence with State Medical Societies. On National Health Council. On Nomenclature of Diseases. On what, if any, Legislative Means are expedient and available to prevent the Spread of Contagious Diseases. On American Medical Necrology. On Medical Education. On Medical Literature. On Prize Essays. On the Climatology and Epidemics of all the States.

Physicians desiring to present papers before the Association should observe the following rule:

"Papers appropriate to the several Sections, in order to secure consideration and action, must be sent to the Secretary of the appropriate Section at least one month before the meeting which is to act upon them. It shall be the duty of the Secretary to whom such papers are sent to examine them with care, and, with the advice of the chairman of his Section, to determine the time and order of their presentation, and give due notice of the same."

Secretaries of all medical organizations are requested to forward lists of their delegates, as soon as elected, to the Permanent Secretary.

Railroad and hotel arrangements will be announced at an early date.

W. B. Atkinson, *Permanent Secretary*,
1400 Pine Street, Philadelphia.

Manassein on the Means for lowering the Temperature. [*Pflueger's Arch.*, 1871, No. iv.]—The author found, if rabbits, seated at ease in a box, were swung in a transverse direction to their length, and with a rapidity of twenty-eight to thirty double swings, at a pendent length of 117 ccm., that the temperature taken in the rectum after the swinging was, by 0.3° to 1.2° C., in the mean by 0.66°, lower than before. The depression of temperature continued from a half to two hours, and was most decided after fifteen minutes of swinging; a longer swinging did not increase the effect. The maximum of the depression of temperature occurred, at first, some time (about thirty minutes) after the cessation of the swinging. The last-named circumstances, as well as that the wrapping of the animals in cotton-batting in no wise hindered the effects, and, on the other hand, that a more rapid swinging appeared less effective, prove that the current of air which is produced by the swinging is not the cause. Swinging in the longitudinal diameter made the animal more afraid and more restless; it had, however, the same influence upon the temperature. The effects of the swinging on rabbits were greater where the eyes were blinded, and less, on the other hand, in animals in which the respiration was moderately disturbed by the tightening of a cord around the neck, and also in animals only slightly narcotized by morphine. In injections of an ichorous fluid, the feverish increase of temperature produced was lessened by the swinging, and indeed, by repeated swinging, brought to the normal standard. Like other measures which depress the temperature, the swinging was less effective during the period of an increase of temperature.

Historical Notice of Morbus Basedowii. [*Von Graefe's Arch.*, xvii., 1, p. 203.]—Emmert shows, from an extract of a work by Parry, that this author, as early as 1825, reported eight cases of morbus Basedowii, under the title of "Enlargement of the Thyroid Gland," with hypertrophy or palpitation of the heart. The description pretty well pictures the disease, although in but one of the cases was exophthalmus noticed, and no special weight given to that symptom. Besides this, Parry gives five further cases, where enlargement of the thyroid gland occurred in connection with affections of the head, epilepsy, headache, dizziness, deafness, etc. Emmert, therefore, proposes to call the disease after neither Basedow nor Graves, but after Parry. In conclusion, the author reports twenty cases of his own observation. Of these, 10 per cent. occurred in men, and 50 per cent. in women. Exophthalmus was constantly present, in one case, on one side only, while the thyroid gland was enlarged on both sides. In every case there were a diminution of sensibility in the cornea and conjunctiva, and lessened reflex action of the lids. At times there occurred a slight impairment of the sight without any apparent ophthalmoscopic cause; in two cases, atrophia nervi optici, and more often enlargement of the retinal veins, with simultaneous constriction of the arteries. In about six cases peculiar disorder of the speech occurred. When the individual attempted to speak, and the mouth was widely opened, there set in a spasmodic movement of the jaw, and only after some exertion was he master of his speech, which was somewhat hasty and often indistinct.

Annual Report of the Clinic for Laryngoscopy, at the Vienna University, for 1870.—Schrotter presents in this report a review of the doings of his laryngoscopic clinic. The author briefly describes the different variety of disease in series. In the chapter on symptomatic catarrh the author says that he has observed it four times with vitium cordis, and once with a tumor of the mediastinum. In one of these cases, which is fully described, Schrotter bases the diagnosis on an open septum ventriculorum, a diagnosis which was also confirmed by Skoda. The chapter on perichondritis deserves especial mention, as it contains a number of exceedingly instructive cases. The chapter on neoplasms is very full, and the author reports excellent results from local anæsthesia in many of these cases. His method consists in pencillings with chloroform and concentrated solution of morphia. In connection with the disorders of innervation, the author adduces a new observation, and one, as he says, not yet noticed by any other author, which consists in the fact that short spasmodic twitching movements of arytenoid cartilage of the paralyzed side occurred, whence the author supposes a partially-retained functional activity of the oblique muscle. Schrotter's results are strikingly favorable in the treatment of the tracheal stenoses caused by struma, as he succeeded in curing the majority of his patients with iodine-glycerine innunctions.

Experiments on Hæmorrhages from the Lungs. [*Centralbl. Med. Wiss.*, 43, 1871.]—Injections of blood were made by Sommerbrodt into the air-passages of five-and-twenty small and medium-sized dogs. A metal canula, with stopcock, was fixed into the carotid, and a similar one inserted through a previous simple puncture into the trachea. By means of a metal syringe, 104 cn. of blood was drawn from the carotid, and injected thereupon slowly into the trachea. In the same manner, half-fluid blood, or, just before or just after fresh blood, a solution of liq. ferri sesquichlor. was injected. The animals were killed by bleeding from the carotid, in from one to twelve hours, in respectively two to twelve days after the operation. The principal results of the experiments were as follows: 1. Extravasation in the bronchi was present, exceptionally only, twenty-four hours after the injection of fresh blood. 2. The artificially-produced extravasations disappeared with equal rapidity. 3. Injection of a solution of liq. ferri sesquichlor. (gutt. viii. ad 30.0 aq. dest.) caused croupous pneumonia; from five to ten minutes after such injections, constant vomiting was noticed, which the author regards as a reflex action from irritation of the fibres of the vagus given off to the lungs, and which seems to have an analogy to the pervious side. He further advises that the opening be made by the above section, and also suggests the use of the galvano-caustic method.

The Plague in Persia.—Dr. Castaldi, the Ottoman sanitary delegate attached to the Turkish embassy at Teheran, has recently, by order of the Ottoman sanitary department, inquired into the nature of a disease which had broken out in Persian Koordistan, reported to be the plague. Dr. Castaldi has sent in his report, and in his opinion there cannot be any doubt as to the character of the disease. The malady is characterized by a strong fever, accompanied with typhous symptoms, with the appearance of buboes under the arms, in the groin, and in the neck, of carbuncles on various parts of the body, and of spots scattered over the whole surface of the skin. It carries the sufferer off quickly, attacking several houses in one place, and several persons in the same family, and transmits itself from an infected place to a healthy one. "Such a malady," says Dr. Castaldi, "cannot be any thing but the Eastern plague of former times." According to the report, it was during the last winter that the plague appeared in the district of Mukry, in the Aderbidjau, in two villages situated at a short distance from each other, and called Djoumouchau and Arbauouz. These two small villages have been completely depopulated, only seven or eight persons having been spared by the scourge. Dr. Castaldi feels at a loss to point out the causes which have produced the epidemic, for the district is the healthiest in all Persia.—*Medical Times and Gazette, March 2d.*

On the Use of Phosphorus in Diseases of the Skin. [*Gaz. Med. Ital. Lomb.*]—Since between arsenic and phosphorus there exists a species of parallel relative to their chemical properties and physiological effects, Dr. Broadbent experimented with the latter in various forms of eczema and psoriasis. For that purpose he employed two grains of phosphorus in oil, and administered three to seven drops thrice daily in some mucilaginous vehicle after meals. In this manner six cases of eczema were successfully treated, of which one concerned a young girl of twelve years of age with eczema of the head, which, within three months, had extended upon the forehead and face. At first she took four drops, and subsequently five, of the phosphorated oil, continuing thus for three months, without any ill effect; after that time it was necessary to suspend the remedy on account of indisposition; after three weeks he resumed the medicine, and, a fortnight later, of the entire eruption there remained but a slight redness of the skin, which finally disappeared upon the use of frictions with creosote and red-precipitate ointment. The cases of psoriasis numbered six in all: of which two resisted the phosphorus, as well as all other medication; two disappeared with the employment of phosphorus alone, and two others with phosphorus and frictions with creosote.

Disturbances of Sensibility after Gunshot-Wounds. [*Berl. Klin. Woch.*, 20, 1871.]—Berger found in simple muscular shot-wounds, without injury to large nervous trunks, extensive disorders of sensation in the vicinity of the scar in the entire wounded limb; and, as regards the nervous plexus in the neighborhood of the gunshot-wound, in many cases there appeared, one-sided, a want of sensation, exactly limited by the median line on the side of the wound, in the skin, as well as the external mucous membranes, conjunctiva, mouth, throat, and nose. Generally the sense of touch and sensibility to pain were simultaneously depressed, the latter often to a high degree. A depression of the muscular sensibility was not demonstrable in these cases; motive paresis was rarely present; on the other hand, the faradaic and galvanic contractility was often lessened. The reflex excitability was depressed, corresponding to anæsthesia. The majority of the wounded had no consciousness of objectively demonstrable anæsthesia (aside from an almost constant feeling of numbness around the scar); but one individual experienced, at the moment of the wounding (bayonet-stab through both thighs and penis), a feeling of numbness in the face, and the whole right side of the body, which to a less degree still continued at the time of the report.

Resolutions on the Death of Dr. Bulkley.—At a meeting of the Medical Board of the Nursery and Child's Hospital, it was

Resolved, That, in the death of Dr. Henry D. Bulkley, the Medical Board of the Nursery and Child's Hospital have to deplore the death of one of the earliest officers of the institution—a member of its consulting staff since its organization.

Resolved, That the close of his long life of usefulness is covered with honor, and that his memory will be cherished by his former colleagues of this Board, in common with all who were associated with him, knowing the inestimable worth of his private life, and the eminent skill and spotless integrity of his professional career.

Resolved, That a copy of these resolutions be sent to the family of the deceased, to whom we tender our warmest sympathy, and that they be published in the medical journals of this city.

T. M. Markoe, M. D., }
J. J. Hull, M. D., } *Committee.*

Simultaneous Occurrence of Two Acute Exanthemata.—Herschman reports five cases of variola scarlatina; in all five cases the scarlatina appeared after variola; in fact, on the seventh, twelfth, thirteenth, fifteenth, and sixteenth days. All five cases occurred in the space between the 10th and 15th of November, in the small-pox ward of the St. Joseph's Child's Hospital. In all the cases both the eruptions were well marked and accompanied by equally characteristic concomitant symptoms. The outbreak of the scarlatina was in each case preceded by a marked raising of the temperature. Anchenthaler reports a case of simultaneous morbilli and variola in the same individual. The author rests the diagnosis of the morbilli (as that of the variola admitted of no doubt whatever) upon the characteristic prodromal phenomena of measles, upon the appearance of the patient, the nature of the eruption, the spread and subsidence of the exanthem by desquamation, upon the extent of the curves of temperature, which attained a maximum point at the florition stage of the first eruption, and a second during the development of the variola.—*Jahrb. f. Kinderkr.*

The Western Lancet.—This is the title of a new monthly journal of medicine and surgery, edited by Drs. Eustace Trenor and H. P. Babcock, and published by A. L. Bancroft &

Co., in San Francisco. From the contents and general appearance of the first numbers it is evident that the new candidate for favor is to be made worthy of success; and there is always room for good journals. Each issue will be accompanied by at least one photograph from a surgical case or pathological specimen. A more original name might have been chosen, but that is a matter of little consequence, and we hope the new enterprise will be substantially appreciated by the profession.

On Lisping. [*Æst. Zeitsch. Pract. Heilk*, 1871, 29.]—By lisping (blæsitas) are understood those affections of speech which consist in the transformation of the letter *s* and its combinations into a hissing, unintelligible sound. It is founded either on faulty using of the tongue acquired through carelessness, or in some malformation of the latter, or finally on absence of teeth. The treatment of this defect, where it does not depend upon some incurable pathological change, is usually a successful one. The author has corrected defective formation of the *s* sounds in six to eight sittings, by demonstrating the correct formation of these sounds, and compelling their energetic practice. The treatment of the faulty formation of the *sch* is more difficult and complicated. The original is referred to for a detailed account of the author's views.

Gastric Juice and Pepsin in Solution as a Healing Application. [*Stœhr. Wien. Med. Wochenschr.*, No. 16, 1871.]—The author instituted a number of experiments with the above fluids, applied as follows: the gastric juice of dogs was pencilled, at short intervals daily, fifteen to twenty times upon the wounded surface, or small pledgets of cotton were applied, and upon them a second larger layer of wadding dipped in a very dilute solution of muriatic acid. Several experiments were made, especially upon chancres, upon soft chancres in particular. After five to eleven days, commencing cicatrization followed as a rule. The remedy is chiefly indicated in soft chancre, in diphtheria, phagedæna, and nosocomial gangrene.

Treatment of the Itch in Children.—Dr. A. Monti has made some experiments with copaiba-balsam and carbolic acid. The first was pure, the second in solution, one drachm to one pound of water, or in the form of a salve, one drachm to four ounces simple cerate. These preparations Dr. Monti has rubbed twice or thrice daily into the parts affected, after a preliminary washing with ordinary soap. The use of copaiba is cheaper than that of balsam Peru; that of carbolic acid is still cheaper, and the garments are not injured by the latter. The treatment lasts with both from two to twelve days; that by copaiba is especially adapted to sucking infants. The carbolic acid also relieves any eczema that may coexist. The copaiba-balsam has no effect, good or bad, on eczema.

The Chorda Tympani. [*Kelp Erlenmayer's Arch.*, 1871, 3.] —In a case of double paralysis of the facialis, the sense of taste was entirely lost at the edges and tip of the tongue; in the posterior portion, however, supplied by the glossopharyngeus, it was uninjured. It is therefore certain that, as Steiner (*Arch. für Heilk.*, 1870) has also asserted, the sense of taste in the front of the tongue is effected by the chorda alone.

A New Hospital in Philadelphia.—We learn from the *Philadelphia Medical Times* that the new orthopedic hospital was opened on February 17th. The medical staff consists of the following gentlemen: Consulting Surgeons, Drs. S. D. Gross and Geo. W. Norris; Attending Surgeons, Drs. T. G. Martin, D. Hayes Agnew, H. Ernest Goodman, and W. Hunt; Physician, S. Weir Mitchell, M. D.; Medical Electrician, Matthew J. Grier, M. D.; Resident Physician, David Davidson, M. D.

The Jefferson Alumni.—A social reunion of the Alumni Association of Jefferson Medical College is to be held in Philadelphia during the session of the American Medical Association in that city. Those who expect to be present are requested to send their names and addresses to either of the secretaries, J. Erving Mears, M. D., 222 South Sixteenth Street, and R. J. Dunglison, M. D., 636 North Eighteenth Street.

Lectures on the Nervous System.—A course of lectures was begun on the 19th ult., at the New York State Hospital for Diseases of the Nervous System, and will be continued every Tuesday, at 1.30 P. M., by Drs. T. M. B. Cross and John J.

Mason, attending physicians to that hospital. Students of medicine and physicians generally are invited.

Congenital Syphilitic Muscular Affection.—Herring reports a case of peripheric paresis of the right arm produced by congenital syphilis. The tenderness of the paretic muscles, said to be frequent by Bednar, was not present in this case, although there was at first great pain on movement in the paretic extremities. The child in question was three weeks old. The paresis was cured by the use of sublimate-baths.

Death from Bichloride of Methylene.—A married woman (*Medical Times and Gazette*), forty-four years of age, who was about to undergo an operation for cancer of the breast, expired suddenly while under the influence of bichloride of methylene.

A Severe Form of Chicken-Pox.—It is reported that an aggravated form of chicken-pox has been prevalent in San Francisco. Some of the severer cases have been mistaken for variola.

Obituary.

Prof. Charles Alfred Lee, A. M., M. D., who died at his residence in Peekskill, N. Y., February 14, 1872, aged seventy-one, derived his blood from the Lees and Browns of Massachusetts and Connecticut. His ancestors in both branches extend back and occupy distinguished positions in America for more than two centuries. In collateral descent his paternal ancestors are allied to Governor Bradford's descendants. The earliest lineal paternal ancestors of Dr. Lee in America, that can be traced with certainty, are:

1. John Lee, who died in Massachusetts in 1690, and had for wife Mary Hart, of Farmington.

2. David Lee, of Coventry, son of the former, who married Lydia Strong, daughter of Jedediah Strong, and granddaughter of Elder John Strong.

3. Rev. Jonathan Lee, born July 4, 1718, who was a man of great force of character and influence, and the first Con-

gregational minister in Salisbury, and was twice married; first, to Elizabeth Metcalf, great-great-granddaughter of Governor Bradford, September 3, 1744; second, to Love Brinkerhoff, 1762; had eleven children, and died October 8, 1788.

4. Samuel Lee, Esq., of Salisbury, who married Elizabeth Brown, of Pittsfield, daughter of Captain Jacob Brown, of Sandisfield, an officer of the Revolutionary War, who accompanied Arnold in his fruitless expedition through the wilderness, up the Kennebec, against Canada, in 1776, and died in Quebec March 14, 1776; and niece of the gallant Colonel John Brown, who was killed in battle while fighting for American liberty, October 19, 1780, aged thirty-six.

Charles Alfred Lee, son of Samuel, was born at Salisbury, Conn., March 3, 1801. Much of his boyhood was passed in the family of his uncle, the late Elisha Lee, Esq., of Sheffield, Mass. Here he fitted for college, becoming a member of the Lenox Academy, at the age of sixteen. One year later he entered the sophomore class of Williams College, Mass. While at college he was noted for his great industry, systematic habits of study, strict performance of duties, and for irreproachable morals. On graduating at this institution A. M. in 1822, he received the honorable distinction of being chosen by the faculty to deliver the philosophical oration at the public college commencement. His studies had been prosecuted with a view to entering the Congregational ministry, but the state of his health on leaving college caused his medical advisers to urge him to study medicine, a vocation better calculated to improve his constitution, which had been seriously impaired by long confinement and close study, and from neglect of exercise and inattention to the laws of health.

Accordingly, he commenced the study of medicine with his brother-in-law, the late Luther Ticknor, M. D., of Salisbury, Conn. He attended two courses of lectures at the Berkshire Medical College, at Pittsfield, Mass., where he held the office of demonstrator of anatomy during the winter session, and instructor in botany during the summer course. After receiving the degree of M. D. at this institution, in 1825, he engaged for some time in practice with Drs. Ticknor and Asahel Humphrey, of his native town. In 1827 he removed to the city of New York, where he continued his studies, and engaged actively in practice. In this great and growing city he encountered all the difficulties that usually lie in the path of young men in their efforts to acquire professional business and position. Being governed by correct morals, sound principles of action, with close attention to business, and indomitable perseverance, he was enabled in a short time to overcome all obstacles, and to take his place where his talents and education entitled him to be—in the front rank of the profession.

As a necessity from the organization of his mind, he was a profound thinker, and an enthusiastic, ceaseless worker, enlisting actively in any new measure of a public or professional character that promised in any way to be useful to the community or to elevate the profession of medicine, and ameliorate the condition of those requiring the aid of a physician.

When the Northern Dispensary of New York City was being established, Dr. Lee and Dr. James Stewart were among its most active and most efficient promoters, and to them is largely due the successful founding of this one of the most useful of the many public charitable institutions of the city. He faithfully discharged the duties of attending physician to this dispensary for over four years, during which time he prescribed for and attended more than four thousand patients annually. On resigning this position he received from the board of directors a unanimous vote of thanks for his very acceptable services, and was chosen chief physician, with the privilege of making all the subordinate appointments, supplying medicines, and exercising a general supervision over the establishment, under the board of managers. In a few years the extension of his private practice, which began to engross most of his time, induced him to resign this responsible office, when he was elected one of the consulting physicians, a position he held for many years.

On the 28th of June, 1828, Dr. Lee was united in marriage with Hester Ann Mildeberge, daughter of John A. and Ann (De Witt) Mildeberge, of New York City, by whom he had nine children, only three of whom, all sons, survived.

In 1832, during the first visit of Asiatic cholera, Dr.

Lee was appointed physician to Greenwich Cholera Hospital, and also attending-physician to the New York Orphan Asylum. In the latter institution he gave, in the form of lectures, specific instructions to the nurses on hygiene, ventilation, and the laws of health, and how to proceed in case any of the inmates were attacked; and, although there had been two deaths in the house the day preceding his taking charge, there was not another fatal case among more than one hundred children subsequently seized with the disease. He was indefatigable in his attention to his hospital duties, abandoning for the most part his private practice, sleeping and eating in the hospital, that his services might be prompt and efficient. During the epidemic, the doctor attended upon and prescribed for nearly one thousand cases of cholera.

From the time Dr. Lee commenced his professional career in the city of New York, up to 1845, he was most assiduously engaged in the practice of his profession, scarcely leaving the city for a single day. At this time, finding his health much impaired, and his nervous system particularly enfeebled from such close and laborious attention to his studies, and the routine duties of his profession, he accepted, by the advice of professional friends, an appointment to the chair of Materia Medica and General Pathology, in the Geneva Medical College, New York. The duties of this professorship required an absence of but eight weeks annually, which, however, gave him some relaxation, and proved beneficial to his health. The remainder of the year was devoted, as usual, to his practice in the city.

The liberality and independence of the doctor's principles were signally illustrated in 1846, while Dean of the faculty, in procuring the admission of Miss Elizabeth Blackwell as a regular student of medicine in the Geneva College. Her admission was after she had applied in vain to most of the medical schools in the principal cities, and when the doctor knew that his course would be likely to provoke criticism unfriendly to the Geneva College and himself.

After the year 1850, Dr. Lee devoted himself chiefly to teaching various branches of medicine in different medical colleges, among which may be named the University of the

City of New York; Geneva Medical College; University of Buffalo, medical department; Vermont Medical College, at Woodstock; Maine Medical School, at Brunswick; Berkshire Medical College; Starling Medical College, Columbus, Ohio. The branches taught by him in these different colleges were: Therapeutics and Materia Medica; General Pathology, Obstetrics, and Diseases of Females; Hygiene and Medical Jurisprudence.

In addition to the chairs actually filled, Dr. Lee was invited to fill the chair of Theory and Practice of Medicine in the University of the City of New York at the time it was made vacant by the resignation of Meredith Clymer, M. D. The same chair was tendered him in the University of Louisville, Ky., when it became vacant by the death of Daniel Drake, M. D. Existing professional engagements compelled him to decline both of these desirable positions. He also received applications to fill chairs in other colleges.

In 1850, Dr. Lee, in connection with his colleagues, Drs. Hamilton, Flint, Hadley, and Webster, founded the Buffalo Medical School, acting under the charter of the University of Buffalo. He continued to deliver his annual course in this institute until 1870–'71, when, at his own request, he was permitted to retire. The trustees at once elected him Emeritus Professor of Materia Medica and Hygiene, and at the same time passed unanimously a vote of thanks for his past valuable services to the university. He retained this position to the time of his death. Dr. Lee gave an annual course of lectures in the Maine Medical School for about fourteen years, and in the Geneva College for ten years. He was generally recognized as one of our best American teachers, thoroughly acquainted with the latest and soundest views upon all medical subjects, and never failing to interest his class. He never attempted any rhetorical display, generally reading from his copious notes.

After his college duties became so absorbing, he confined his practice chiefly to one of office consultations, and to consultations with other physicians. His consulting practice, too, was very large in the regions of the medical colleges in other States where he lectured, patients coming to him from long distances. His experience and advice, too, were greatly sought after by medical practitioners. He was always a supporter of the ethics of the profession, and therefore popular with his brother practitioners, which did much to extend his consultation business.

Successful as Dr. Lee undoubtedly was as a teacher, it is by his pen that he has won his highest reputation. As an author and a medical writer, he is very widely and favorably known, both in this country and in Europe. He wrote extensively on a great variety of medical and scientific subjects. His "Physiology for the Use of Elementary Schools" was published by the American Common School Society about 1835. It was subsequently enlarged and published by J. Orville Taylor, of New York, and has passed through ten or more editions. This work answered very well the purpose for which it was written, has had a large sale, and has done much to popularize this important branch of knowledge with the people, and opened the way for its being taught in the common schools and seminaries of learning throughout our country. His "Manual of Geology, for Schools and Colleges," was published by the Harpers in 1835, as one of the volumes of the "Family Library." This had a very extensive sale throughout the United States and the Canadas, and has done much to create a taste for the study of this useful science. In 1843, Dr. Lee was instrumental in establishing the *New York Journal of Medicine and the Collateral Sciences*, a bi-monthly of one hundred and forty-four pages. Owing to the pressure of other engagements, the late Samuel Torry, M. D., author of a valuable work on the climate of the United States, was engaged to edit the first few volumes. On the death of Dr. Torry, Dr. Lee assumed the entire management, his name appearing on the title-page of the fourth volume. He continued the journal to the close of the tenth volume, his own pen furnishing much of the original material. It is not saying too much to state that this journal took a high rank in periodical literature, and contributed greatly to the respectability of American medical journalism.

In 1845, Dr. Lee brought out an edition of "Principles of Forensic Medicine," by William A. Guy, M. D., with extensive and valuable notes and additions, adding much to the value of the book. In this labor he had the coöperation and advice of the late Chancellor James Kent.

In 1848, Dr. Lee commenced the most important and laborious professional work of his life—the editing an American edition of Dr. James Copland's "Dictionary of Practical Medicine," issued irregularly in London, in numbers comprising 144 pages of large octavo, double columns. Several attempts, one of which was in Washington, D. C., by Duff Green, had been made to issue an American reprint of this work, but, from one cause and another, all had failed. The editor, though apparently fully occupied with his medical journal, his practice, and his five or six annual courses of lectures in different colleges, undertook the Herculean task of supplying, in the way of notes and additions, all of permanent value to be found in medical journals, monographs, formal treatises, and even manuscript lectures, a task which involved the necessity of supplying himself with complete lists of the American medical periodicals, and the careful search of all known works of native origin relating to the different subjects discussed. The completeness of the American Medical Bibliography, at the end of each article, shows the vast amount of careful and discriminating labor and research expended in this department of the work. This was at the time the heaviest and most expensive medical publication ever undertaken in the United States, and will ever remain, with the extensive notes, a grand and lasting memorial of the indefatigable industry and research of the editor. It was the wonderful completeness and time-saving value of this list that first suggested to the author of this sketch the advantages to the medical man of a comprehensive subject-index to the medical literature of our various American medical journals, a work that is now well in hand. The Dictionary was fifteen years in passing through the press of the Harpers, owing to its slow publication by its author in London. The entire work forms three immense octavo volumes. It is not an exaggeration to say that this forms the most complete and valuable work on the theory and practice of medicine, including etiology, pathology, and therapeutics, ever issued from the English or the American press. Dr. Lee received the hearty thanks of Dr. Copland himself for the able and satisfactory manner in which he had given the American edition to the public.

During the progress of this great work, Dr. Lee brought out other valuable publications, among which was an edition of a learned and practical treatise on "Food and Diet," by Jonathan Pereira, M. D. This enterprise was undertaken at the request of the distinguished author, from whom the American editor received kind acknowledgments and thanks. Besides the extensive notes, over seventy pages of original matter was added by way of appendix. As a matter of justice, it should be stated that the entire profits of the numerous American editions of the work have been generously assigned by the editor to the author and his heirs.

In 1840, Dr. Lee issued, with many valuable notes and an appendix of seventy pages of original matter, an American edition of an English work, entitled "Bacchus, an Essay on the Nature, Cause, Effects, and Cure of Intemperance," by Ralph B. Grindrod. The additions by the American editor were subsequently, in 1851, incorporated in the English edition of the work. The American profits of this work were also relinquished by Dr. Lee to the British author.

In 1843 he edited and published an edition of A. T. Thomson's "Conspectus" of the London, Edinburgh, and Dublin Colleges, and of the United States Pharmacopœia. To this work he also added many valuable articles and extensive notes.

In 1844, he supervised and revised an edition of the "Pharmacologia, or, the Theory and Art of Prescribing," by J. A. Paris, M. D., which was published by the Harpers.

About twenty-six or twenty-seven years ago, Dr. Lee wrote for the *New York Churchman*, at the request of the editor, a series of essays entitled "Medica Sacra," which were published in its weekly columns. They attracted much attention.

Besides the works already mentioned and printed, the doctor prepared, during the last years of his life, a work on the "Indigenous Materia Medica of the United States," which is in manuscript, and would form a volume of about six hundred pages, and would be a valuable contribution to this department of medicine. In addition to those noticed, a number of other useful volumes were written or edited by the subject of this memoir. He was a constant and voluminous contributor to various scientific, literary, and professional journals, at home and abroad, for more than forty years. His writings on hygiene, the laws of health, temperance, and the influence of alcohol—i. e., liquors—on the human body, were commenced in the year 1828, and continued through various channels to the time of his death.

Dr. Lee was one of the first to detect and call attention to the extensive and dangerous adulteration of malt-liquors in the United States, which, by careful analysis, he demonstrated, in 1834, from ten different samples of Albany ale.

His wide-spread reputation as a forcible writer upon the subject of temperance induced the British Temperance Reform League to invite Dr. Lee, while in England, in 1862, to deliver an address before them at their annual meeting in Exeter Hall, London. His remarks on the occasion gave great satisfaction, and he received a unanimous vote of thanks.

Dr. Lee first visited Europe in 1848, for the purpose of recruiting his health, which had become greatly impaired by his close and severe labors. During this visit he became acquainted with many of the distinguished and leading medical and scientific men of Great Britain and the Continent. He visited all the most noted hospitals and public institutions. He took a special interest in studying the management of institutions for the insane, and wrote a series of papers on this subject, of decided ability, which did much to inform the profession of our country of the improvements introduced into hospitals for the insane. An absence of nine months, with the agreeable society he met, and the tonic of travel, entirely restored him to health, and on his return he at once renewed the routine of his usual duties as teacher, editor, author, and practitioner.

The character of Dr. Lee's mind, and the range of studies that engaged his attention, entitle him to be ranked with a class of medical men, never numerous in any country, such as Rush, Mitchell, Hosack, Francis, Drake, etc. His thoughts took a philosophic range of great scope, manifesting some preference for the natural and exact sciences. This quality of his mind, with his thorough and comprehensive studies, led him to be selected and esteemed as one of the most important medical experts in our country. He was a man of genial disposition, elegant manners, and was affable and courteous to all. He was thoroughly unselfish, and ready at all times to help others; and was particularly the friend and supporter of young men entering the profession. His sympathies were active and humanitarian in their turn, and often brought him before the public.

In the spring of 1862, the second year of the war, Dr. Lee visited Europe to collect plans, models, and specifications of the best and most recent naval, civil, and military hospitals of Great Britain and the Continent, for the use of the United States Government. In the prosecution of this philanthropic enterprise he was eminently successful. The heads of the War and Navy Departments of Great Britain, with much consideration and promptitude, placed at his disposal accurate maps and drawings, with working plans and specifications of the most approved naval and military hospitals of the kingdom. These, with others, were placed in the archives of the War Department at Washington, and served, no doubt, as valuable models for the erection of similar establishments during the progress of the war. During the same year, while on an extended tour through England, France, Germany, Switzerland, Prussia, Austria, and Italy, he wrote for the *American Medical Times*, of New York, about fifty elaborate and carefully-prepared letters, designed to furnish useful and important facts and information to our army and naval surgeons, as well as to practitioners generally, in regard to military and naval hygiene hospitals, and hospital hygiene, bringing prominently forward the medical and surgical treatment of patients in the various public charitable institutions of the different countries, and their special arrangements and management. Before leaving England on this tour, Dr. Lee was elected a member of the "British Social Science Congress," then holding its annual session in London, with Lord Brougham as its president. The doctor was unanimously called to preside over the section of the health department of that useful association.

In consequence of the protracted and severe military struggle in which his native country was then unhappily involved, Dr. Lee was induced, late in the fall of 1862, while at Naples, on his way to Egypt and the East, to turn his steps homeward. He reached New York early in 1863, and immediately offered his services to the Government, in the capacity of a surgeon. He was accepted and assigned to duty in a hospital, but, very properly believing that he could be more useful in a wider field than he enjoyed in the subordinate position which had been allotted him, he soon resigned, and accepted a situation as hospital inspector and visitor, in the United States Sanitary Commission's employ. He labored efficiently in this field until the close of the war.

In the spring of 1865, soon after the surrender of General Lee's army, the doctor was engaged for several months throughout the South in collecting materials for "Memoirs of a Sanitary History of the War," particularly as relating to the Confederate armies, with hospital statistics, army diseases, etc., in which he was remarkably successful. A portion of these valuable papers will be found in the "Sanitary Records and Medical History of the War," issued by the United States Sanitary Commission.

In 1850 Dr. Lee purchased a handsome residence in the neighborhood of the Highlands near Peekskill, on the Hudson. He loved domestic retirement and quiet study; and there, with his library and his family around him, he passed much of his time in study and writing, and in the enjoyment of quietude during the closing years of his life.

For the last ten years he has taken a very active interest in some of the great humanitarian movements, especially in instituting and encouraging reform in the management and care of the chronic insane of the United States. As a large majority of this unfortunate class are quiet and harmless, and able to perform considerable bodily labor, he was opposed to shutting them up in close and crowded wards and cells, but favored their distribution under the care of suitable attendants and keepers, in simple, cheap, and comfortable cottages, after the French system, where they may enjoy open-air life, and a degree of domestic comfort, with sufficient daily exercise to preserve health. These views he has advocated at considerable length in the form of two able reports, one made to the New York State Medical Society, and published in their Transactions for the year 1865; the other as chairman of a committee appointed by the American Medical Association, and contained in their Transactions for 1868.

Dr. Lee was a member of the New York Academy of Medicine, the New York State Medical Society, the American Medical Association, the New York Historical Society, the New York Lyceum of Natural History, etc., and honorary member of the Connecticut State Medical Society, the Ohio State Medical Society, and a long list of other learned and scientific bodies, both at home and abroad.

In 1835, he became a member of the Episcopal Church, with which he retained a consistent Christian connection to the time of his death. He was warden of St. Peter's Church at Peekskill for many years, always taking an active interest in its affairs. For a layman he was remarkably well versed in theology, and liberal in his views of religion. Dr. Martyn Payne writes: "I had enjoyed an intimate acquaintance with Dr. Lee fer more than forty years, and had the highest esteem for him as a man of the most lofty virtues, and profound religious convictions. His writings attest the comprehensive nature of his scientific and literary acquirements, as well as a vigorous and logical mind."

Dr. Lee was taken ill on the 30th of January, with endocarditis, and after two weeks of suffering died, with calmness and Christian hope. His wife and three sons survive him, and were with him during his sickness. His remains have been interred in "Sleepy Hollow Cemetery," at Tarrytown, N. Y. —J. M. Toner, M. D.

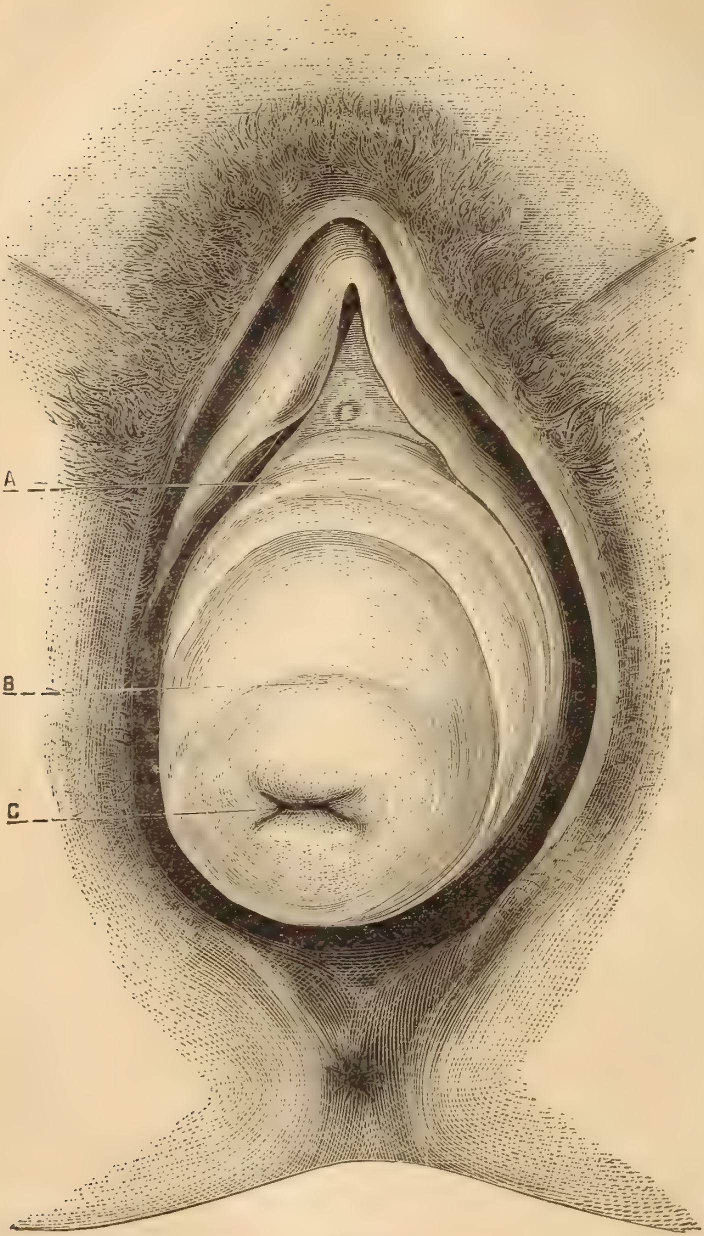
Dr. Jonathan Letterman.—In the death of Dr. Letterman, which occurred in San Francisco on the 15th of March, the profession loses a member of no common ability, and the Medical Staff of the Army a former associate, whose labors shed lustre upon its ranks.

Dr. Letterman was born near Washington, in Western Pennsylvania, and, after a thorough education at Dickinson

College, pursued his medical studies in Philadelphia, as a pupil of Prof. F. Gurney Smith, and graduated with distinction at the Jefferson Medical College, in 1849. Entering the army in the same year, he served for years upon the Pacific coast and in the Northwest; and at the commencement of the late war was placed in medical charge of the Department of Western Virginia. Here his great executive abilities first displayed themselves, and after a brief term of service he was called to Washington. As soon as he had reorganized the military hospitals of that city, he was selected by Dr. Hammond—then just appointed Surgeon-General—to fill the responsible position of Medical Director of the Army of the Potomac. He was still only an assistant-surgeon, the junior in rank of many over whom he was called to preside; and, with the army in retreat, and the commissariat and medical department equally disorganized, his position was one of peculiar difficulty. How he filled that position, and to what thorough efficiency he raised his department, are matters of history.

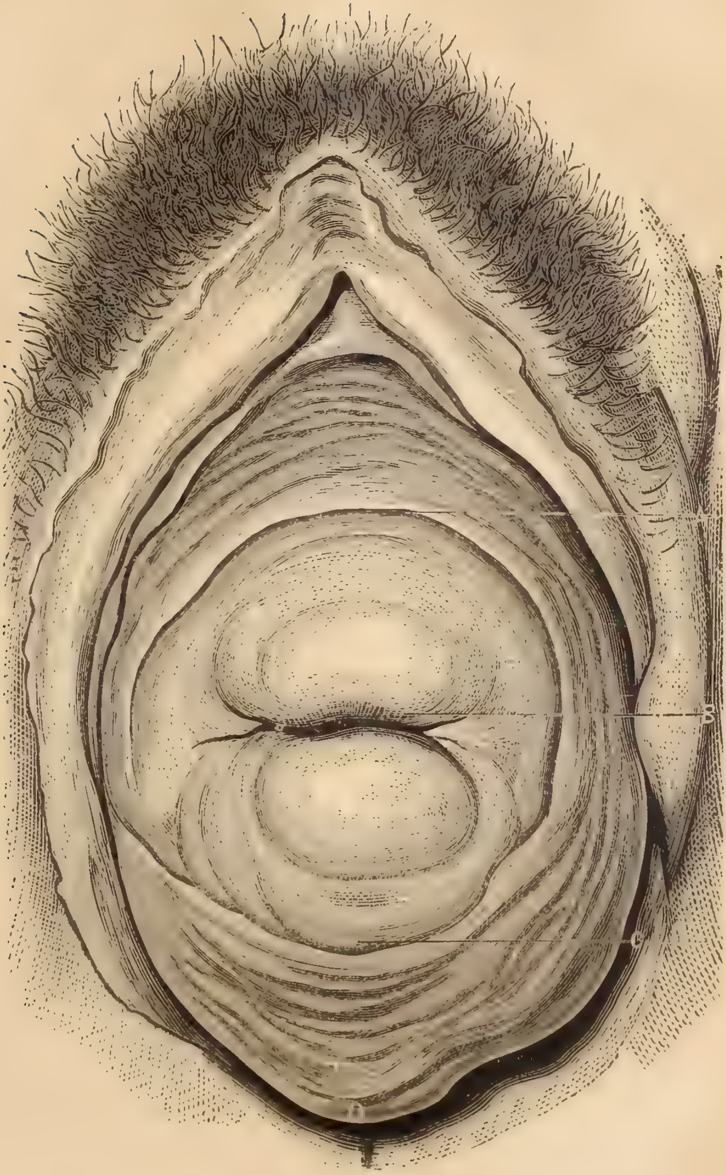
With extraordinary executive ability and energy, he created, *de novo*, an ambulance system, a method of field-purveying, and a system of field hospitals, so perfect that their adoption was, by act of Congress, made obligatory upon all the armies of the United States. In December, 1864, he resigned his commission—having been previously recalled at his own request from the field—and went to California; where, after a year's residence in the southern part of the State, he settled in San Francisco. Here he rapidly acquired a successful practice and was elected the city coroner; but, depressed by domestic misfortunes, his health had been failing for some time before his death.

In his private life Dr. Letterman was as modest and unassuming as he was distinguished in ability, and was equally beloved by his military and professional friends. He had faults and opponents, as all men of strong character have; but, when the medical history of our civil war shall be written, no name will more brightly illuminate its pages than that of Jonathan Letterman, author of "Medical Recollections of the Army of the Potomac."



INVERSION.

A. Os Tincæ. B. Extent of Cervix. C. Body of the Uterus.



EVERSION.

A. Os Tincæ. B. Internal Os.

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Original Communications.

Art. I.—*On the Mechanism of Spontaneous Active Uterine Inversion, and the Reduction of a Case of Complete Inversion by the Combined Rectal Ana-Vaginal Taxis.*[1] By Isaac E. Taylor, M. D., Corresponding Member of the Berlin Obstetrical Society, the Medical Society of Christiania (Norway), etc., etc.

Of all the physical lesions of the uterus, that which appears the most extraordinary is produced by a kind of displacement which the organ executes, that is to say, upon itself, and reversing entirely the order of Nature, partially or wholly.

I propose this evening to present some observations on the mechanism of spontaneous active inversion of the uterus in the unimpregnated state, in the recently confined, and on the reinversion of that organ. I shall also make some remarks, with a recital of a case of complete inversion of the womb, treated by a different method than is usually resorted to, and refer to some of the more recent methods of treatment adopted at the present day. I desire to be expressly understood as excluding those cases of inversion which occur through artificial or external means. I shall adduce those cases only where the

[1] Read before the Academy of Medicine, April 4, 1872.

cause is inherent in the uterus itself, the same as we notice in other viscera by their own action, whether in the human or animal creation.

I need not suggest the names of any authorities at the present time to prove that cases of spontaneous intussusception, invagination, or inversion of the uterus, have and do occur. Facts are too numerous to merit a negative reply, especially in those instances which take place at the time of labor, or after an interval of some days. The occurrence of this unfortunate accident in the female who has never had children, or been pregnant, is, in the opinion of some of the modern gynæcologists, a great mistake. The evidence, according to their experience, is, that it is impossible in the unimpregnated uterus. They believe that it is obviously essential for its occurrence that the organ should have attained a certain size, and that its walls should be comparatively ductile. West does not positively deny its taking place, provided its parietes should be yielding. Duncan, the latest writer on the production of inversion (1868), says: "I shall only say that I cannot believe that such an inversion ever occurred."

Within the last few years the attention of the medical profession has been more particularly directed to the manner of replacing the uterus, after its inversion, to its natural position, on account of the success attending personal and persevering efforts, than to an investigation into the special causes of the inversion, and the mechanism during the act. Methods and processes have been advanced and claimed as new, which are in fact older views resuscitated without conceding the credit to the just and proper authorities they were "based on." The study of the mode of the production of inversion would seem to have been in abeyance, and the truthfulness of the interpretation of its mechanism or progress has been regarded as absolutely settled.

Judging from the discordant opinions which are entertained by some standard authorities on inversion, anatomically and physiologically, I have thought, independent of the views I will offer regarding its production, it might be well to resurvey some points which have a bearing on the subject, and which would be opportune and applicable at the present moment. A resurvey of this important subject may prove beneficial. It is very difficult to appreciate all that has been done on any one subject without retracing our steps and scanning the various opinions as to its mechanism and treatment. It is possible it might be considered superfluous and useless, as the opinion has been expressed that any other view than the ancient one would simply be an inference or an impossibility. I am willing, nevertheless, to accept this charge, and will proceed to ask your attention to the subject. The views and opinions I hold I am fully aware are not in consonance with those of the profession, but this ought not to deter me from presenting them, as they are the results of long and continued thought, with no limited amount of experience in uterine and obstetrical practice, public and private. I intend presenting such cases as will clinically suggest another aspect besides the generally-received one of how the inversion takes place. The usual views are:

1. That the fundus of the uterus is always the part first indented or depressed, and the uterus in a state of partial or complete inertia.

2. That it may occur when there is active contraction of the body and fundus.

I wish to remark, on these propositions or theories, that I do not deny that the fundus is the part first depressed, as in a passive condition of the uterus when in state of inertia or functionally paralyzed, by artificial means, as the traction on the cord, or too short a cord, or by the forceps, or by small and large polypi, and when the long obstetrical forceps may be applied to deliver the polypus. I have on several occasions had the opportunity of operating by the latter means, and have noticed the manner of inversion.

In our profession there is not one who does not recognize and realize how great the difference of opinions and views is we hold on the same subject, and from the same stand-point. By a different process of investigation, and experience, and reasoning, we may present another explanation, although the same result will be obtained, as equally satisfactory.

It is usually conceded that inversion of the womb is a rare accident. It is my firm conviction that this opinion is incorrect. My impression was confirmed in looking over the large number of cases which are recorded in the different journals and transactions. Many members of the profession, it is true, have been engaged in practice for years, and have never met with a case. Prof. Simpson, it is said, had never seen a case of acute inversion. This, however, is no argument in favor of its rarity. They may have been fortunate in the management of their obstetrical cases by not making traction on the cord, or the harsh removal of the placenta, and believe that they have thus prevented its occurrence. Reference has by many authorities been made to the large number of cases of labor occurring in the Rotunda Hospital, Dublin, as a proof of its infrequency. From the statistics the inference has been drawn that it must have been from the great care, attention, and management, bestowed upon the delivery of the placenta, with little or no pulling on the cord. Dr. More Madden, in his article on "Acute Inversion," in the *Dublin Journal of Medical Sciences*, November, 1870, states that "up to the end of the year 1868, when 190,883 women had been confined in the hospital since its foundation in 1745, only one case of acute or recent inversion was observed, nor has any happened since then." Desormeaux and Dubois entertain a different opinion. Dr. Radford, of Manchester (1837), is also inclined to think so, and cites the evidence of others in favor of that view. Some members of the profession have informed me that they have met with several cases. Silence on the subject has been observed by some, who have thought censure might be attached to them for bad or careless conduct of the case of labor. Instances, therefore, of cases under such circumstances, if they had been reported, would have swollen the list of frequency.

Experience at the present day shows also that spontaneous inversion is considered as frequent as the cases from artificial sources or mismanagement. Dr. McClintock, nevertheless, writing as late as 1863, says: "I cannot help expressing my conviction that, whenever the uterus is inverted at the time of parturition, it is to be attributed to some mismanagement of the delivery of the placenta, and in confirmation of which I adduce the accumulated experience of Drs. Clarke, Labatt, Collins, Kennedy, and Johnson."

Inversion of the uterus has been divided into acute, recent, and chronic, active and passive, partial and complete.

By spontaneous active inversion I comprehend those cases which are created by the active contraction of the uterus itself, without subordinate means.

Three degrees of inversion are usually accepted or adopted:

1. A simple dimpling or depression of the fundus.
2. When the fundus passes down to the os tincæ.
3. When the organ is entirely inverted, the os tincæ remaining alone uninverted. Complete inversion, it is held by some, being more rare than the others.

Each of these forms exists in various degrees. To effect the greatest degree of inversion, the displacement must commence from the slightest cup-like indentation of the fundus, and go through all the intermediate stages. More or less time, it is known, is occupied in producing the inversion. Sometimes it is accomplished instantaneously under ordinary contraction of the uterus; at other times, and more generally, by considerable uterine and abdominal efforts, chiefly abdominal, according to Crosse. In some instances the accident is gradual from depression to introversion, or intussusception to inversion, the inversion being days, weeks, or years, before it becomes complete, agreeably to some writers.

It is held by the same authorities, Crosse for instance, that, if the uterus be well contracted after delivery, it cannot subsequently become inverted, unless again distended by blood, the sudden escape of which places the organ in circumstances somewhat similar to those attendant on delivery, and renders inversion just possible. We cannot, therefore, be far wrong in stating, as the general opinion, that inversion can only be commenced soon after labor, and that most of the recorded instances of its occurring after the lapse of days or weeks may be placed to the account of oversight or error in diagnosis. A more strongly-expressed opinion by another author (C. A. Lee) considers it self-evident that "inversion must date from a previous labor, if not depending on the weight of a polypus." It is not to be rationally supposed that such a process of involution could come without a cause, and no one can believe that, after the placenta has been separated, and the uterus normally contracted, such an event would be likely to happen. The statements of inversion occurring first at an interval of many days after delivery have been wrong, and in such cases the displacement really existed in a slight degree from the time of labor. To establish this point, Crosse says: "It is much easier to believe that an inversion was present at the termination of labor, and not detected then, than after the uterus had contracted, and hours and days elapsed, inversion commenced, and showed itself quickly, in an advanced stage of development."

Since Crosse wrote his paper on "Inversion of the Uterus," in 1844, twenty-eight years ago (published originally in the "Transactions of the Provincial Medical and Surgical Journal Association"), numerous cases of spontaneous inversion have been reported, and it is at the present day received as an admitted fact by some obstetricians. Dr. T. Radford, of Manchester, in a valuable, practical, and instructive essay on the subject, was the first who recognized and presented an explanation of the production of inversion, and the condition of the uterus at the time of the inversion, different from the authorities I have referred to, and others more ancient. (Radford's paper was published in the Dublin *Journal of Medical Sciences*, in September, 1837.) This was several years before Crosse's essay, in which he gave several instances bearing upon the active spontaneous inversion which he had seen. Ashwell, T. Smith, and some others, followed and entertained the same opinion, but differing in their explanation. Contrary to the views and opinions as given above, the unimpregnated uterus, whether in the primipara or multipara, had also been considered as undergoing inversion partially or wholly. Regarding the enunciation that the fundus must be the introcedent part first before complete inversion, it is announced that "no evidence has been presented of the inversion commencing in the opposite direction, that is, from the cervix or the contiguous part of the body of the uterus toward the fundus." One of these authorities, Crosse, suggests that with a dilated cervix it might appear very possible, in a mechanical point of view, to find the progress of the displacement to be from the cervix toward the fundus.

"No one claims," says Lee, "that such a process of involution has been observed. It is simply inferred from the circumstances under which some inversions have taken place, and it may well be doubted whether there are any circumstances which give any plausibility whatever to such an hypothesis." I have given these explicit extracts, which are but theories or hypotheses at best, from these authorities, but opinions which are indorsed by the profession, as the exemplification of their views on the subject of inversion. To these opinions I shall enter my firm and conscientious conviction as opposed to such an interpretation so strongly expressed. I will endeavor, from clinical experience and investigation, to show that "an opposite direction" of the mechanism of an inverted uterus does and will take place.

Inversion of the uterus has been compared to an intussusception or a hernia, similar to the finger of a glove turned inside out, or a bottle which is indented at its base.

Astruc's explanation of inversion has been so frequently referred to on this point to elucidate its mechanism, that I will resort to his version to aid me in confirming the view which I hold. Astruc, after showing how inversion is produced by the pulling on the cord, says: "Sometimes it is produced from contraction of the womb, which forces the bottom inside out, through the mouth of the womb, which is not yet closed, in nearly the same way as the contractions of the intestines in violent colic force one part of the gut into another. Sometimes, however, the superior part passes into the inferior, and sometimes *vice versa*, which is the case in the iliac passion. On this explanation a high authority says that many authorities have compared inversion to the turning of the finger of a glove inside out, on removing it from the hand. Now, the one is just the reverse of the other in the human frame. As to the successive changes, although the effect may be the same portion, when in both instances the inversion is rendered complete, the comparison is erroneous. It would be correct only on the condition that the inversion progresses from the cervix to the fundus (Crosse).

I have selected the word intussusception, as I believe it expresses more significantly the correct interpretation, and is more in accordance with cases which occur in the active spontaneous form of the affection. By intussusception I understand the partial inversion commencing at the cervix, and the fundus subsequently following.

By inversion, the turning inside out by either of the two methods.

In an unimpregnated healthy uterus we all recognize that the uterus is small, firm, and solid. The substance of its neck and its orifice is dense, differing from the body and the fundus in its structure and function. It is composed of a white, fibrous element, elastic and yielding, more so than the superior part, which is truly muscular. Its cavity is very small, especially in the young female, so that its intussusception, much less its inversion, would appear naturally impossible, without being diseased in some way in its structure. It is generally considered that previous dilatation must take place, either by the product of conception, or a foreign body, or by the accumulation of blood or water, so that atony should exist, without which the inversion could not occur. Boivin is not positively committed to this view of the case as to the impossibility of the unimpregnated uterus being inverted, unless abortion has existed. She says that "previous dilatation of the uterus ought not to be considered as a real predisposition to inversion, except in so much as the uterus, entirely or nearly empty, retains a certain degree of softness, flaccidity, and inactivity, such as sometimes obtains after labor." The fact of invagination or inversion of the unimpregnated uterus, though so emphatically denied by the older as well as by some recent gynæcologists, is defended by Boyer.

To sustain that opinion, he remarks that "there are men who are not wanting in instruction and experience who have observed cases in young females, where the womb has not been subjected to the action of a cause to widen its parietes and to thin its structure. We partook of the general opinion, but at the present moment we have seen the uterus inverted, without previous dilatation." Puzos made this kind of inversion, that is, of the unimpregnated state, the subject of a memoir which he read before the Academy of Surgery, in 1784. This memoir would have been forgotten or overlooked entirely if it had not been published in the *Mercury*, of France, by the editor. After having spoken of inversion which follows after confinement, he passed to that which proceeds from external causes unbeknown to him, and independent of confinement. He states that the malady had been noticed in the female without any suspicion, in females who had never had children, and in others who had not had children for fifteen or twenty years. The partisans of the opinion opposed to this view did not call in question Puzos's facts, nor other like facts reported by some authorities, but they interpreted them in a different way, and say that he had mistaken a polypus for the womb, or that there was a clandestine confinement. This is what might politely be termed a gratuitous assumption. This is precisely the manner some recent authorities have exercised their right of judgment and pronounced opinions as though it was "*ex cathedra.*" The diagnosis was wrong and incorrect, therefore totally false. To set at naught these gratuitous opinions, I have quoted the high authority of Boyer, as to the character of those who have witnessed such cases as he refers to, even if Puzos is not credited, to show that there are instances where the unimpregnated uterus may be partially or wholly turned inside out.

The cases of Le Blanc and others have been referred to by some authors, and considered as imperfect, therefore of no value, and be set aside as unworthy of credence, without any argument or proof from experience for so doing, and have been pronounced impossible, and an error. I shall therefore in this paper confine myself to the presentation of cases of recent date to elucidate the object I propose, so that the rejected cases may not be considered as bearing on the subject. References of this kind would not strengthen the position I hold, but would be brought to militate against it.

It may be recollected, by some of the gentlemen present, that Dr. Edward Rigby several years ago published a short article on what he styled the "squatty uterus." This form of uterine affection was considered by him as a relaxed or softened condition which the uterus sometimes underwent in females who had in some instances amenorrhœa or other uterine affection. My attention from the first time I read his remarks struck me as novel and singular as to the "title" given to the disease. From that time I directed my investigations, when the opportune cases came under my supervision. In several instances I can confirm the opinion as expressed by him, that it was a soft and flabby modification of the structure of the uterus, but what its exact status pathologically was I could not positively state, though I supposed and believed it was possibly only an intumescence, the uterus being larger, fuller, and softer than natural, through an increase of its vascular condition. In some of the cases when I have sounded the uterus for the purpose of restoring the menses, after a few months, or even after one month's arrest, and where other remedies had failed, the uterus in these cases would measure in the primipara as much as four to four and a half inches. If firmer pressure was made with the sound, it might be lengthened still more. The internal and external manipulation gave evidence that the uterus was softer and larger than natural, and did not present that solid, firm and movable feel to the touch that a natural uterus would.

In my monograph on "Procidentia Uteri" (published in the "Bellevue and Charity Hospital Reports" for 1869), I particularly asked the attention of the profession to the ductility of the uterus in many instances, showing the opinion Huguier had promulgated in cases of procidentia uteri was not tenable, that in the great majority of cases there was an hypertrophy of the supra-vaginal portion. On the contrary, that the supra-vaginal part was ductile, and the body also. As proof of this statement, I pointed out that it would admit of being elongated or stretched by the sound from one to two or three inches more than others, and even at the same sitting; that the organ might be compared to a piece of moist, soft, pliable India-rubber, and that in some instances after excision of the infra-vaginal part of the cervix, it would in a few hours, in some cases, recede to its normal place, and be reduced from six or seven inches to three or four, which could not have been the case if there was an hypertrophy, as mentioned and entertained by Huguier; that it was a condition of the uterus nearly allied to that which is recognized after confinement, where the organ had not undergone its perfect involution.

Independent of these cases, we presented two cases where pregnancy existed at from three to four months, where gestation could not by close investigation be made out, in which the sound was introduced, and the uterus measured ten to twelve inches. The ductility of the uterus had also been noticed in the unimpregnated state in cases of procidentia uteri, in which the depth or length of the womb measured from eight to ten inches. Cases of this character, where the sound has been introduced to this depth, have been supposed to have perforated the tissue of the uterus, and gone through the peritonæum. Another opinion advanced is, that where the sound has passed that length, it has entered the Fallopian tube. I will not deny that in both these kind of cases it may have happened. In the cases which have come under my observation no injury or pain has ever resulted. I take it for granted, therefore, that I did not pierce the uterus and the peritonæum, and from the central position of the sound, when the uterus was felt above the pubes, that I had not entered the Fallopian tube.

Dr. Alt, in his gynæcological lecture, December 10, 1870, describes two cases occurring in the recently-confined woman, in which the sound was passed seventeen to thirteen centimetres, that is, from five to seven inches. No hæmorrhage followed in either case, nor any symptom of disease. On account of the length the sound entered, he believed that a perforation of the uterus occurred, and he agrees with Dr. Hoenig, who holds the same view.

Hildebrand, and some others, think that the instrument goes into the abdomen through the Fallopian tube. Hoenig's analysis of cases on record proves this to be untenable, but more especially is the incorrectness of the opinion proved by the observations of Dr. Edward Martin, of Berlin, in his work "On Displacements and Curvations of the Uterus." On laying open the abdomen in one of his cases similar to those of Alt, he saw that the instrument had passed into the abdomen through the walls of the uterus. Cases of this nature have been witnessed in this city, but the condition of the uterus was so soft and thin that it could easily be pierced. The testimony of these authorities is opposed to that of Hildebrand and others who believe that the sound enters the tube. The testimony of Martin and Alt shows that it is easier to penetrate the tissue of the uterus than enter the tube. Dr. Tyler Smith was the first to call the attention of the profession to Fallopian-tube catheterization, in 1849, for sterility. The instrument he used was the Gairal Eustachian catheter and then introduced through it a fine whalebone bougie, to enter the tube. Dr. S. M. Cartwright, of New Orleans, has cauterized the Fallopian tube with nitrate of silver. He has also treated successfully a case of ovarian tumor in 1851 through the tube. He does not consider it easy in an unimpregnated state of the uterus to catheterize the Fallopian tube, unless the uterus is diseased. It is of course difficult to decide on the exact status of the uterus where the sound enters to such a length in the unimpregnated state. I conceive, however, that it is a demonstrated fact, and, as I have seen no unfavorable symptoms springing from that mode of testing the length of the uterus, whether in the newly-confined or unimpregnated state, I must reject the opinions which advocate the perforation of the uterus, or the entrance in a large number of cases into the Fallopian tube; and recognize the soft condition of the uterus, spoken of above as causing the ductility, which, when sounded, gives rise to the different lengths, as stated. In the *Atlanta Medical and Surgical Journal* for December, 1871, before the Atlanta Academy of Medicine, this lengthened state of the uterus for procidentia uteri was under observation and discussion; and the various opinions expressed on this subject, without any reference to the condition of the uterus, I have referred to in this article on the ductility in procidentia uteri.

Boyer claims that there must be a certain condition of the uterus in the unimpregnated state before inversion may take place. What that condition is he does not state, but it has no relation to gestation or expansion of the body of the uterus by any foreign substance. Ashwell hints at it, while he admits inversion may occur in the non-pregnant uterus. Resting as I have upon the term "squatty uterus" as significant in its characteristics of softness, flabbiness, size, and ductility, as tested in many instances by the sound, or becoming procident by the gravity of its position, my experience would authorize me to adopt that peculiar or certain condition I have referred to of the uterus. I frankly admit, that I do not profess to give its true status, unless it is simply what is referred to by some of the older gynæcologists as an intumescence. It is certainly in many cases not a true pathological change of structure such as we notice in the uterus after confinement, and before perfect and natural involution has transpired, whether of recent date or after some years, as we recognize in procidentia uteri. We must bear in mind the peculiar structure of the uterus, isolated as an organ from every other visceral structure in the human frame, springing into existence as a physiological organ at puberty, subject to important physiological and pathological changes or processes during a thirty-year's life, formed anew again and again, as a consequence of gestation, then passing into forgetfulness, and physiologically, virtually dead, but incident to other serious pathological mutations afterward. During, therefore, this living existence many changes or peculiar conditions of its structure transpire which no gynæcologist has yet recognized and explained, but the facts we notice, and the observations I have made, are founded on the facts under our cognizance.

In illustration of the cases I propose presenting for consideration, after the general remarks I have made, and to pave the way for further investigation and observation, I shall offer first the case of Mr. William Lawrence, of London, published in the London *Medical Gazette*, December 5, 1838, in a clinical lecture, and is styled "Spontaneous Partial Inversion of the Uterus."

No authorities that I have read, except Klob, have made any reference to his case. Klob mentions it only incidentally, and says: "A very remarkable class of cases of inversion are those in which, without efficient cause, an inversion of the cervix into the vagina takes place, drawing the fornix of the latter with it, and thus forming a polypus-like tumor in the cavity of the vagina, which may reach down to the vulva, at the lower part of which the internal orifice is situated." This description of Klob's would answer for Lawrence's case, the recital of which will show a more extensive inversion than he has alluded to. In my monograph, referred to above, I illustrated my views on the subject of the eversion of the cervix uteri, and gave cases which measured, when complete, from three to four inches in diameter. In the cases to be related by Lawrence and myself of partial inversion of the uterus, they are greater in length and differ in appearance entirely from eversion, as the diagram will show. The case of Lawrence appears either to have been overlooked or ignored by those who have advocated the views as maintained above. It is too much the case with members in our profession to disparage all evidence which has a practical bearing on old and preconceived opinions, and while they have no more vantage-ground, if as much, from the theories they wish to advocate and circulate, they dismiss the subject by simply considering it an error of diagnosis, or a mistake entirely. For one, I am disposed to take a more liberal and conservative view of my professional brethren of an established reputation, equal if not superior by their advantages and experience. I do not hesitate, therefore, to claim the truthfulness and honesty of their reported cases, as of equal clinical value as any other authority, as the testimony of Boyer has affirmed.

CASE I. *Spontaneous Partial Inversion of the Uterus, with Procidentia.* (By WILLIAM LAWRENCE.)

Sarah Smith, thirty-two years of age, a maid-servant, was admitted into St. Bartholomew's Hospital, June 12, 1838. She had always enjoyed good health, and felt herself quite well at the time of her admission. Three years ago she had borne a child, and the catamenia had been perfectly regular since that time. She had menstruated three weeks before she came to the hospital. She represented to me that she had a swelling in the private parts. I found on examination a tumor hanging from the external organs as large as my fist. it was largest in the middle, a little smaller above and below. Observing a transverse fissure in the middle of its inferior end, I at first supposed the case to be a complete prolapsus uteri, but could not recognize the usual appearance of the os tincæ. The surface of the swelling in its upper two-thirds was smooth, pale, and nearly dry. This was obviously the vagina completely everted. The lower third was a soft, villous, red surface, moistened by colorless mucus, and was soon recognized by the characteristic folds of the uterus inverted. A decided line marked the boundary between the vaginal and uterine portions of the tumor. She stated that she had experienced a bearing down and uneasiness for five months—that a protrusion had occurred at the external parts for three months, going up of itself, after she had laid down permanently for the last three weeks, though she had continued to perform her duties as a domestic up to the day of her coming into the hospital. The mucous membrane of the cervix was healthy, and the cavity also. The exposed membrane had secreted abundantly, for the chemise was completely saturated, and stiffened with an almost colorless discharge when the patient had gone to bed. I covered the protrusion with a soft cloth, and pressed it upward with the hand. It was necessary to exert considerable force, under which it suddenly receded. I saw her in August, when she remained perfectly well.

CASE II. *Spontaneous Partial Inversion of the Uterus, with Procidentia.* (Service of Dr. I. E. TAYLOR, reported by Dr. ST. GEORGE W. TEAKLE, House Physician, Charity Hospital.)—Susan Horton, colored, aged twenty-two years, single, born in Philadelphia, domestic, was admitted into the Charity Hospital, August 16, 1871, by Dr. Taylor. This patient was sent to Dr. Taylor by Dr. Whitehead, as a case of procidentia uteri.

Patient has always enjoyed excellent health. Her menses first appeared when she was thirteen years of age; has been regular ever since. Never had a discharge of any kind from the vagina or uterus. Never had sexual intercourse with any man. Has always worked as a chambermaid. Last June was obliged to cook, and while acting in that capacity was compelled to bring the coal from the cellar. After she had acted as cook for one month, she experienced dragging pains in the hypogastric region, and was easily tired, and had considerable soreness about the vulva. She noticed, on examining the parts, that there was a red substance about one inch broad projecting through the labia majora, about two inches in width, a thick, glairy secretion. At this time her general health became very much impaired. When admitted, all the above-named symptoms were increased. It was supposed to be a simple case of prolapsus, with elongation of the supra-vaginal portion of the cervix. On more close inspection, it was recognized as a partial inversion of the body of the uterus with prolapsus. The hymen was intact. The inverted part was covered with a glairy secretion, and the mucous membrane had a red, villous appearance.

The sound, on being introduced at the lower part of the tumor, was three inches to the fundus. From the transverse orifice to the os tincæ, one and three-fourths inches, from the orifice to the vulva, two and one-fourth. The transverse diameter was one and a half in the centre of the tumor. The sound took an upward direction and slightly backward.

Mr. Kochler, the artist, made an exact drawing of the case, by measurement. The patient was requested to keep her bed, as she suffered no special inconvenience from the tumor. Dr. Taylor preferred to do nothing with the case under these circumstances, so as to ascertain how far Nature would aid in restoring it to its normal position and condition. In this case, as will be noticed by the diagram, no operation for procidentia uteri could possibly be of any avail. The peculiar condition of the uterus continued gradually changing till October 4th, when it had receded into the vagina, although the os tincæ was perceptible on the opening of the vulva, and natural in appearance. The measurement of the uterus at that time was three inches, from the os tincæ to the fundus.

The following are the measurements taken every week from September 1st: One and three-fourth inches; September 12th, one and a half inch; September 19th, one inch; September 26th, one-half inch; October 4th, reinverted.

October 31*st.*—She was presented for the last time to the class, and, on examination, the uterus was normally placed, movable, and measured two and three-fourths inches. Her menses had ceased for four days. She had travelled from the ground-floor of the hospital to the top for the last ten days, and left the hospital in that improved and natural state. No case could have presented a better illustration of the ductility of the uterus, in an unimpregnated state of the uterus in a primipara, than did Horton's. This morbid condition was independent of any conception, or abortion, or distention by blood. Had the view of Huguier been correct, the result would have negatived such an opinion. Its spontaneous reduction, and its replacement to its normal standard and measurement—its physiological function continuing regularly, and the uterus attaining its natural position in the pelvis, are points of much interest and value for investigation and observation.

CASE III. *Complete Inversion of the Uterus in the Unimpregnated State.* (BOYER.)—I intentionally draw upon Boyer for this case, because his testimony that an unimpregnated uterus may be inverted is not to be disputed, even if others may be doubted. He is considered, as Crosse says, one of the ablest of surgeons, and most perspicuous of authors, both pretty sure guarantees for his having been an accurate observer, and not likely to be mistaken.

This woman was aged forty-four years, of large stature, and considerably fat. Has always been regular, and had had three children. She had never lost blood nor had leucorrhœa. After having felt during a long time a weighty filling and heaviness in the pelvis, and a dragging in the loins, and when she was walking or standing for some time, there presented, at the entrance of the vagina, a tumor which she felt with her finger. Finally, the tumor escaped from the vagina. She consulted two physicians, who regarded the tumor as a polypus, and proposed the ligature. On examination, Boyer perceived between the labia a tumor, which had passed about three-quarter inch out. This tumor was a little larger than natural, having a pyriform shape; its pedicle, large and short, was surrounded by a collar slightly projecting, under which the finger penetrated to the depth of a few lines. This tumor was tender to the touch—color grayish, its surface unequal and villous. Boyer says he has no doubt of the correctness of his opinion as to the nature of the case. I could cite other cases from high authority, but those related will be sufficient to show that partial and complete inversion will and does occur in the unimpregnated uterus, consequent on some particular or peculiar change or condition of the uterus in its structure other than pregnancy, which causes its parietes to be widened and thinned.

To add to the cases I have related in the unimpregnated state, I will extract from Ingleby's cases and observations (p. 227) the following case after labor, similar in character to the cases of Lawrence and my own. It was considered, however, and classed, under the general interpretation, as a case of inversion according to the older view, but the history does not verify that opinion. The relation of the case will speak for itself:

Case IV. *Partial Inversion of the Uterus, Eight Days after Confinement.* (Ingleby.)—Mrs. W. was delivered of her first child on Thursday night, the 17th. The practitioner informed me that, immediately on the removal of the placenta, syncope took place, followed by profuse hæmorrhage, which subsided a little the next morning. She was harassed by frequent pains in the hypogastrium, and an occasional increase of hæmorrhage, until the succeeding Thursday night, eighth day after delivery, at which time I was requested to see her. I found her nearly without pulse, exsanguined, comatose, delirious on being roused, as if she was moribund. On laying my hand over the pubic region, the uterus, which felt hard, presented two singular features, its form being almost conical and its circumference particularly small. I ascertained that the vagina was filled by a very round tumor, which almost reached the os externum, corresponding to the fundus uteri, and resembling a large polypus. On carrying the finger as high as I could possibly reach, I distinctly felt the os uteri encircling the tumor like a firm stricture. It was clearly a case of inversion of the uterus. The body of the organ being above the brim, and the os internum occupying the centre of the inversion, an attempt was made to reinvert it. In about five minutes the stricture gave way to the compression which we made, the left hand gained full possession of the cavity, and was felt externally over the pubes. In his remarks on this case, Ingleby says: "The circumstance of the hypogastrium containing a portion of the uterus, the body of the organ, as in this instance, may prove exceedingly deceptive. How important, then, to ascertain whether the hypogastrium is occupied by the fundus uteri, characterized by the firmness, roundness, and superior size!" Ingleby had not evidently recognized the true character of the inversion of this case. If this was the correct description, then the fundus was certainly not indented, nor do we see after complete or partial inversion the os internum in the centre of the fundus or lower part. I am further corroborated in my views by the additional remarks of Ingleby, that "there is a form of inversion in the course of its longitudinal fibres which has not, I believe, been noticed by authors."

In an important case I was compelled, in conjunction with another physician, to apply the forceps under the disadvantage of uterine inertia. After the delivery of the child, there was no tendency to expel the placenta, but, a portion of the mass having separated, a slight effort was made with the funis. The placenta descended considerably beyond the os internum, together with a quantity of the uterus, nearly the whole right side, the left not being sensibly compressed. The cases of Ingleby, as cited, certainly do not illustrate the general view as to the inversion of the uterus. On the contrary, the four instances, as recorded, give evidence of another kind of mechanism. I will now proceed to bring forward the different theories which are advocated at the present day to substantiate their view of the mechanism of inversion of the uterus by depression of the fundus.

The three principal theories existing at the present day on spontaneous active inversion of the uterus are those of Drs. T. Radford, Tyler Smith, and Duncan. It has been considered by some, at the present day, that the greatest progresses in all the sciences consist in the discovery and elucidation of mechanism, and that mechanism is thought more of now than formerly, and includes much that was supposed vital.

Dr. Radford believes that "action of the inverted part, with want of contraction of the parts below it, is the theory of active uterine inversion;" that the uterine pain, diminution of bulk, resisting feel, sudden formation, and rapid protrusion, convinced him of the deduction that the fundus and body of the uterus, instead of being in a state of collapse or relaxation, are really in a state of constant excitement or action.

But this is not the case with the os uteri; on the contrary, it is soft and yielding, as we find it offers no resistance to the coming down of the fundus, when protrusion is forthwith and rapid. The inversion is another instance of irregular contraction, in which the fundus acts powerfully while the cervix uteri is in a state of atony.

Dr. Tyler Smith ("Obstetrical Transactions," 1869, vol. x.) relates two instances of inversion of the uterus and their treatment. I cite these remarks, as they are later than those published in his work on Obstetrics. Referring to Dr. Duncan's views on the production of inversion, he says: "I have endeavored to elucidate the causes of this accident, comparing it, on the one hand, to intussusception and inversion of the intestine, and on the other showing its relation to spasmodic rigidity of the os uteri during labor, to encysted placenta, the hour-glass contraction. I have demonstrated that inversion of the uterus is often the result of an active and not passive condition of the organ. In the first stage of the act there is a cup-like depression of the fundus uteri."

"The presence of the fundus in the middle portion of the uterus tends to hour-glass contraction, and this grasps the fundus and passes it on toward and through the os uteri, and the inversion depends on an abnormal contraction of the organ, the os itself being at this time in a state of inertia or atony just as it is in the second stage of labor."

Dr. M. Duncan holds that hour-glass contraction is a disorder nearly identical in its first stage with the first stage of spontaneous uterine active inversion. Paralysis or noncontraction or a weaker action of the other parts of the uterus, with contraction or spasm of other parts, has by many authors been pointed out as contributing to an understanding of the phenomena of encysted placenta. Hour-glass contraction cannot exist unless the parts above the contraction are in a state of inertia, unless there is a local paralysis of the uterus. "I know no one," Dr. Duncan says, "who has to an equal extent advanced the subject of the causation of active uterine inversion."

We may simplify the different views and opinions or theories respecting the mechanism of the inversion of the uterus into the following order:

1. Inertia or functional paralysis of the uterus, with depression of the fundus, preceding its complete or partial inver-

sion.—*Crosse*, *Lee*, *Dubois*, and others. This is the opinion generally accredited.

2. Contraction of the fundus and body, with inertia of the cervix. Irregular contraction of the uterus. Indented or depressed fundus first.—*Radford.*

3. Contraction of the fundus and body, with inertia of the cervix uteri. Hour-glass contraction. Irregular contraction. Depressed or cup-like indentation first.—*T. Smith.*

4. Paralysis or inertia of the fundus and body, with contraction of the cervix uteri. Hour-glass contraction. Fundus depressed first.—*Duncan.*

6. Contraction of the fundus and body. Cervix uteri at rest, or collapsed or inert. Regular or continuous action of the uterus. Cervix uteri first rolled out, or doubled up, the body following, and the fundus last.—*Taylor.*

The first proposition or theory has no relation to my subject, and I shall not therefore touch upon it.

It is the last theories I shall address myself to—Radford's, Smith's, and Duncan's.

The views and opinions of Radford and Smith, as it will be noticed, are more nearly allied in some instances to each other. The observations of Smith are also in some instances nearly the same as Duncan's, though Duncan considers the fundus as paralyzed, or in a state of inertia, while Smith considers it contracted.

To examine these views clinically, I will quote a case from Radford and Smith. Dr. Duncan gives no case.

"I was requested to meet Mr. Dick in consultation with a patient in her first labor. The patient was confined on the lap of a female friend, and was delivered in that position.

"After the child had cried, and the placental fœtal circulation had ceased, the funis was divided. It was of the ordinary length, and did not encircle the neck. The patient was now put to bed. I placed my hand on the abdomen, and found the uterus hard, and rather larger than usual. I now took the funis in the left hand, and gently stretched it, and passed the finger of the right hand into the vagina to examine for the placenta, but was not able to feel it. While I made the inquiry, Mr. Dick placed his hand rather suddenly upon the belly; a strong forcing pain came on, and the woman exclaimed: 'Oh! the after-birth is coming!' The placenta was now rapidly advancing, and in a moment it passed through the os externa. I then discovered it was not the placenta alone, but the uterus, constituting an extreme degree of partial inversion. The protrusion was sudden and forcible, and was attended by a bearing-down effort. The uterus was returned easily, and the hand carried into the cavity. The os uteri grasped the wrist, assuring me that regular action was in operation."

"I was summoned by my friend Dr. W. Bryant to assist him in a case of tedious labor for inertia. After consultation, we determined on delivery by the forceps. After chloroform, we delivered her of a living child. While she was still insensible, a few seconds only after the birth of the child, I placed my hand on the lower part of the abdomen to ascertain if the uterus contracted properly. At this time, the vulva was uncovered, and a slight amount of hæmorrhage was taking place. While my hand was on the hard and contracted uterus, I felt the rounded mass disappear; it seemed to subside into the pelvis. At the same instant, a mass presented at the ostium vagina, and was quickly expelled. Our first thought was of twins, but we soon saw that it was the inverted uterus, with the placenta attached. The placenta was carefully peeled off, and we found but little difficulty in returning the organ through the os uteri. No time had been allowed for firm contraction of the os upon the extended organ. There was no traction on the cord."

These are typical cases of the spontaneous active inversion of the uterus, and many cases could be adduced from a large number to show that active contraction existed in the body and fundus before the inversion took place. It is admitted by Radford and Smith that, after the delivery of the child, the uterus was hard and well contracted and felt as a round body above the pubes. There was no paralysis or inertia in these cases; no cup-like indentation or depression is spoken of as having shown itself, or felt by the hand. The inversion was sudden and quick, through the contraction of the uterus. In cases of this nature, we cannot but appreciate the fact that activity of the whole organ, body and cervix, would render inversion impossible. As the contraction of the fundus was evident to the hand, firm and round, the lower part of the uterus body and cervix must have been relaxed, or in a state of inertia, to admit of the inversion. On this single point, I will quote one of the prominent authors who advocates the depressed fundus in inversion first, to present his own question in answer to the cases I have cited from Radford and Smith: "Will any one therefore claim that the commencement of the process of inversion is in the spot actively contracted?" It is clear that Radford and Smith certainly do, as well as others. If so, then I would ask, Can the muscular action of the walls of the uterus indent or drive any part of its structure into the cavity? I am not now speaking or referring to any part of the uterus affected with paralysis at this time. I take it that no regular or irregular action of the fundus of the uterus could produce introcession of a part of the parietes of the body. As these points will be conceded and have been by the advocates of the indentation of the fundus first by contraction, or inertia, or paralysis of the fundus and body of the uterus, then there must be some other method or mechanism to be recognized, to account for the production of inversion in cases of active contraction of the body and fundus of the uterus.

As illustrating this point more than I have, and before I advance to an explanation for the reason I entertain an opposite opinion to the generally-received one, I will give a clinical instance of the spontaneous active reinversion or replacement of the uterus, of recent date and of unquestionable authority.

Spontaneous Active Inversion of the Uterus, and Spontaneous Active Reduction.[1]—"I was called to see a patient, and found her lying on her back, complaining of severe bearing-down pains, and a dragging sensation, especially when coughing. She appeared much exhausted. Skin cold, and pulse very small and feeble. I ordered her some brandy directly, and placed her on her left side, when I found the outlet and lower part of the vagina occupied with a large round tumor, about the

[1] "Obstetrical Transactions," vol. x., p. 35.

size of an orange, hard, tense, and perfectly immovable. I passed my finger up behind it, and found it to be of a pyriform shape, the pedicle, if it may be so called, lying against the anterior wall of the vagina. No uterine tumor could be felt above the pubes. I judged the case to be one of inversion. Efforts to reduce it were unavailing. Dr. Shaw left for a while, to see Dr. Davis, but he could not come. On his return he found the patient had had a nice sleep, after a mild anodyne, and felt easier. On examining, to his surprise and satisfaction, he found the tumor had disappeared, and that the vagina was free from contents, the os uteri being felt high up, and unoccupied by any protrusion. The fundus could now be felt distinctly above the pubes. No hæmorrhage from the beginning, of any consequence."

Almost every one who is acquainted with the literature of inversion of the uterus will recall the case of Barre, presented to the French Academy of Surgery, which had been inverted by traction on the placenta. He attempted on the evening of the twelfth day to reduce it, but failed; and, one day the patient, desiring to take a lavement, in making the effort, fell upon the floor; at the same moment she experienced an extraordinary movement in the abdomen with a very lively pain, a great loss of blood and fainting. When placed on the bed, there was recognized a complete reduction of the organ. Baudelocque, who had been charged with the examination of the case, and to report on the same to the Academy, considered it was "totally false," and denied it *in toto.*

A few years afterward, however, a patient called on him, having an inverted uterus for eight years. It was a partial inversion. Baudelocque failed to reduce it. On the day fixed for another attempt, she fell on the floor, a peculiar movement was experienced in the abdomen, and, when placed in bed, he realized the reduction.

Those who have rejected the idea of an unimpregnated uterus becoming inverted, entertain, to be consistent, the same opinion regarding its being reinverted spontaneously. Duncan expressly says: "Spontaneous active uterine replacement is inconceivable, because the necessary conditions, being described generally the same as those for spontaneous active in version, cannot be supposed to exist." With the views as expressed by Duncan, and which I have quoted from, I do not suppose he could believe it. Duncan admits that Daïllez has recorded cases of reposition, but he conceives it might be in spontaneous passive replacement. An authority of acknowledged repute, who ignores cases of this kind, while referring to the clear and distinct cases reported by Meigs, adopts the same expression, as to the want of correctness of judgment and diagnosis as in the unimpregnated state, and remarks: "If we must admit that the uterus can spontaneously invert itself inside out, it is but right, perhaps, that we should also believe in its ability to turn itself back again." If this is admitted as a perhaps, may I ask, that if the uterus has in many instances become reinverted, as so clearly recorded in the case of Shaw, and others of recent date, and even after several years' duration, according to Baudelocque, who at first so positively denied it, why should it not be admitted that the uterus could reinvert itself by its own action, as well as invert itself under the special condition or one nearly allied to it, which I have adduced?

No two theories could be more opposite than Radford's, Smith's, and Duncan's. Smith holds that in some instances hour-glass contraction occurs, yet there is contraction of the fundus. Duncan asserts that hour-glass contraction cannot exist unless the parts above the contraction are in a state of inertia, unless there is a local paralysis of the uterus; for, as inversion consists in causing the descent of the fundus of the uterus, this cannot be effected by any uterine efforts, except those made by parts below the fundus. Both these theories cannot be correct. It is not to be believed, as I have previously said, that the uterus when in a state of contraction could produce depression of its fundus, nor could we believe that such depression could occur if it was in a condition of inertia, except from other causes than, as is alleged and believed, by impulse of the intestines or abdominal nisus, or by a local paralysis.

In natural labor it requires, according to some authorities, from thirty-five to forty pounds' weight to expel the fœtus, and from eighty to one hundred pounds where the forces are more needed. But, agreeably to others, it will require nearly half a ton. After the labor, if the half of the forces only are desired to deliver the placenta, it is evident when the uterus is in a state of active contraction, when the muscular structure is more dense and strong, it would require as much power as a natural labor. If it requires this amount of force of forty pounds to deliver the child, and it is principally through the uterine forces it is delivered, it would appear that the same force would be necessary for inversion: what amount of pressure, then, would be required to indent the uterus when in a state of contraction? Could the deltoid or gastrocnemii muscles when in powerful or strong action be depressed or indented?

The advocates who favor the indentation of the fundus, either in the active or passive inversion, consider that the impulse of the intestines and the abdominal nisus will produce inversion. It is believed that this force or power is nearly if not quite as direct as that of the uterus itself. Joulin has, however, asserted that this force, the abdominal nisus, has no direct action, and it is probable that its actual product scarcely rises above a few pounds. Klob, while addressing his remarks to the subject of that part of the uterus which is locally paralyzed in exceptional cases, and has been supposed to fall into the cavity of the uterus, says: "I take it to be absurd to propose to regard the pressure or impulse of the abdominal nisus as the cause of the sinking inward of the paralyzed part." If this is true as so emphatically asserted by these able authorities, in cases of inertia, how is it possible for a contracted organ to be indented at the fundus cup-like before inversion? To strengthen this point more positively, Dr. Robert Lee, on the impulsion or pressure of the intestines producing the indentation or depression of the fundus uteri, says: "On examining the bodies of women who have died soon after delivery, I never saw the uterus in such a state of uterine relaxation or inertia as to render it possible for the intestines, however forcibly drawn against the fundus by the action of the abdominal muscles, as in vomiting or coughing, or straining, to produce the accident." If, as I have remarked above, that activity of the fundus and body renders introcession or projecting inward of its walls an impossibility, how is it possible to believe with Smith that a contracted fundus should become an introcedent body to excite the annular contraction that is spoken of by himself and Fabre, and thus become the means by which the fundus should pass through the cervix uteri and become inverted? Reversely to this opinion, Duncan says, "There must be, therefore, a paralysis of the part before inversion can be begun." To fortify and illustrate this view, he falls back upon the paralyzed condition of that part of the uterus where the placenta is attached.

As proof of this, he refers to the testimony of Rokitansky. Now let us see how well this point is sustained: "This part, the paralyzed part, is forced into the cavity of the uterus, by the contraction of the surrounding tissue, so as to project in the shape of a conical tumor, and a slight indentation is noticed at the corresponding point of the external uterine surface." I would remark that Fabre entertained the same view, which was first clinically shown by Rokitansky as a pathological fact. Very few obstetricians but will admit, in some cases of tedious labor, inertia of the body and fundus of the uterus, but, in these cases of indented or depressed fundus, the uterus, after recovering from the laborious task it has gone through, regains its tone and natural contractility, and the inertia passes away, and no paralysis or inversion occurs. It is only a temporary functional inertia or paralysis, incident to the uterus being overtaxed. The process Rokitansky refers to is a diseased process—a true paralysis of a portion of the walls of the uterus, and has no special relation to the ordinary or functional paralysis of tedious labor or adherent placenta. If this were the case, the physiological act of labor would be frequently pathological, and Nature is not so aberrant. This morbid process is hardly ever recovered from, and I have seen the bulging in of the walls of the uterus, from this condition of the uterus, mistaken for polypus, and operated upon for that disease. Klob insists, and would have it specially attended to, that he particularly cannot understand wherefore the paralyzed portion of the uterine walls always sinks inward into the uterus, while at the same time, in consequence of the energetic contraction of the other parts of the walls of the uterus, the cavity is reduced to small dimensions. On the contrary, regarding the pathological condition of the uterus where the placenta is attached, an author of Great Britain holds that the placental site is thicker than the rest of the walls of the organ, and naturally projects inward—whether the placenta be still adherent or detached; therefore, the first condition is the falling in or depression of the placental site of the fundus, the first degree of inversion. If the arguments of Klob and others show that, even in atony by paralysis of the placental site, it could not be projected inward, by the abdominal nisus or intestines; how could it be, then, when the paralyzed part was thicker, except by its gravity? Statistics, however, show that the fundus is rarely occupied by the placenta, for, out of seven hundred and thirty-seven cases as recorded by Reid, only sixteen cases were at the fundus, while there were five hundred and sixty-five at the sides—one hundred and forty-five in the vicinity of the os, and eleven on the os. In the various *post-mortem* examinations I have seen, and from several specimens which have been sent to me after sudden death from hæmorrhage before delivery, the cervix was closed, and the parietes were found thin and flabby, and in some parts thicker than others. When held up to the light, the thinner parts were translucent, but no projecting inward of any part of the uterine structure. Had the slightest contraction taken place, the parietes would have assumed a more dense and firm condition. In the instance of Cæsarean section I performed just previous to the death of the mother, the parietes would scarcely have measured one-sixth of an inch, while afterward the uterus was fully half an inch thick, if not more. I do not perceive, with all I have advanced, how the views of Duncan can be substantiated, that "it is necessary to show that paralysis or complete inertia of the higher parts of the uterus may occur, while the lower parts (the cervix) are in a state of contraction." The three important points of the advocates for the production of inversion of the uterus, by commencing with the indentation of the fundus, viz., through contraction of that part, the impulse of the intestines, the abdominal nisus and local paralysis, have very little if any influence in effecting the object, as these observations show. Is it not possible to believe, therefore, that inversion may commence, contrary to the opinions advanced, at the neck? If so, I will now ask, in return, if there is any positive evidence of a case of spontaneous active inversion taking place, in which the fundus was recognized by the hand over the pubes and abdomen, as depressed before the inversion took place or occurred, except that based on the result? The result of the act or mechanism is certainly not the proof that the fundus was indented first. Sometimes the uterus is inverted, and the cervix is felt high above the pubes, even near the umbilicus. Depression may exist in a part of the uterus not accessible to the touch exercised through the abdominal parietes. Instances have occurred where a surgeon of eminence had given assurance of his having felt the contracted uterus above the pubes, and satisfied himself that all was right, when the organ at that time was totally inverted, the fundus being in the vagina, and the os uteri uppermost. Reversely, the case of Ingleby, as cited, shows that the fundus was felt round, firm, and globular, while the uterus was partially inverted from the cervix upward. This strong evidence, more than the mere *ipse dixit* of this or that authority, shows how mistaken the tactile examination over the pubes is, in the diagnosis of inversion of the fundus. It teaches us that there is no positive proof that the advocates of the depression theory have to present, except the result to which they appeal, nor "that the human family are the exception to the general law of inversion of the uterus," which I think is not proven.

Independent of the pathological views of local paralysis, and the other points alluded to, we must not forget that a physiological one should not be ignored, of how the uterus may be prevented from inversion except by its own contraction.

John Bell, on the nature of wounds of the abdomen, holds that the abdomen should be considered not as a mere closed cavity, but that it has other and more important objects to perform than the mechanical agency, which may be less or greater according to circumstances. We all know that it has been demonstrated with much simplicity of thought and much power, and which we must admit as true, that all the viscera are moved, by the diaphragm and the abdominal muscles, upward and downward with an equable, continued pressure which has no interval. When the bowels are forced down by the diaphragm, the abdominal muscles recede; when the bowels are pushed back again, it is the reaction of the abdominal muscles that forces them back and follows them. There is never an instant of interruption of this pressure—never a moment in which the bowels do not press against the peritonæum, nor is there the smallest reason to doubt that the same points in each are essentially opposed. How, then, without this equable, universal, and constant pressure, could the viscera be supported? How, without this uniform pressure, could the viscera fail to give way and burst? There is, then, not only a uniform pressure, by which the intestines are retained in their place, and have their inherent mechanism, but there is implied also a conservative principle which the abdomen has, and without which important principle or power in the abdomen the uterus would prolapse.

Two different anatomical views appear to pervade the minds of the profession—those of Charles Bell, and those of Deville, Kölliker, and other anatomists. In the anatomical as well as in the mechanism of parturition they are entirely different.

Anticipating the explanation I have to advance on the mechanism of inversion, it will be necessary for me to make some reference to these views. Bell is of opinion that there are no circular muscular fibres in the cervix uteri or os tincæ, corresponding in place or office with the sphincter of other hollow organs. He has "no hesitation in saying that toward the fundus the circular fibres prevail, that toward the cervix the longitudinal fibres are most apparent, and that, on the whole, the most general course of the fibres is from the fundus toward the orifice. This prevalence of the longitudinal fibres is undoubtedly a provision for diminishing the length of the uterus, or drawing the fundus toward the orifice, at the same time. The longitudinal fibres must dilate the orifice, and draw the lower part of the womb over the head of the child."

By this muscular action it must be apparent, according to Bell's description, that the cervix uteri is pulled open, by the active contraction of the longitudinal fibres, and the cervix is not in a state of atony during the act of parturition. Bell does not think that the *ramollissement* incident to pregnancy takes place in the muscular fibres of the cervix.

Many of the advocates of the older view of inversion, i. e., of the passive order, and some of the more recent gynæcologists who believe in the active form, cling still to the view of Bell. Since his day more minute investigations regarding the muscular structure of the uterus have been made by high and competent authority. They have demonstrated that circular fibres do exist in the cervix uteri. Kölliker particularly speaks of the fine circular band of muscular fibre, which he calls the sphincter of the os tincæ, and says that there are only isolated longitudinal fibres in it; that the longitudinal fibres are made up principally by the intercrossing of the circular ones, and produce the strong anterior and posterior longitudinal muscular layers; that they only extend to the internal orifice, or lower part of the body, and then debouch from right to left, and left to right, and thus form the strong circular fibres at the lower part of the body of the uterus.

It is at this portion of the uterus, I believe, after inversion has occurred, that the history of many cases related will show where the difficulty of reduction exists, and not in the cervix proper.

The cervix uteri is, contrary to the opinion of Bell, at rest —collapsed—or inert, during labor; while the body and fundus are in steady, regular uterine action. There is, therefore, only the natural contractility in the cervix uteri, incident to a sphincter-muscle during the birth of a child, or the delivery of the placenta.

At this stage of my remarks, I shall be pardoned for digressing awhile, if, in justice to myself, I tender a word or two in explanation of the observations I published in 1860 (June), on the non-shortening and mechanism of the cervix uteri during gestation, and the first stage of labor. These observations were more especially enlarged upon in my monograph on Placenta Prævia in 1864. Correspondence has been held with professional gentlemen, from abroad, as well as at home, on the subject, inquiring whether I held the same opinion as Caseaux, Stoltz, and Duncan. The response returned was that we differed entirely—Stoltz, Caseaux, and Duncan, entertaining the view that the cervix uteri was "effaced" at the end of pregnancy, while my own opinion was that there was no change whatever, and that sometimes it was lengthened, though physiologically softened. Stoltz originated the theory in 1826, Caseaux confirmed it in 1839, and Duncan approved of it. Caseaux was my preceptor in 1840.

In 1842 I promulgated his views on the changes incident to pregnancy in the cervix uteri, when I edited Dr. Evory Kennedy's work on Obstetric Auscultation, accompanied with diagrams of the same. This was seventeen years before Duncan's first publication in the March number of the *Edinburgh Medical and Surgical Journal*, 1859. At that time, Duncan held that it was imperative for obstetricians to accept his views (which were those of Caseaux and Stoltz), or disprove them. In 1863, in an article on the same subject, Duncan changed his experienced views, in strong and emphatic language, as to the merits of Caseaux and Stoltz, and said that "it would be a grave error to consider Stoltz's and Caseaux's views, and others, as modern;" and takes the credit from them, and gives it to Weitbrecht as the discoverer. From this *mémoire* of Weitbrecht, an anatomist of Russia, in 1750, he selects passages in proof, and states that the diagram which Weitbrecht gives of his views is from a dissection of the uterus of a woman at seven months' pregnancy. Duncan himself gives one, a month later. Neither Weitbrecht nor Duncan, who is so strenuous for the anatomist's views, gives any representation at the end of pregnancy, nor do they say what the appearance of the cervix is at that time. Eight months is as far as Duncan has advanced in presenting the mechanism of the cervix uteri during gestation. Now, how Weitbrecht, as he is supposed to be the discoverer, could decide, from the behavior of the cervix uteri during the full term of pregnancy, by one dissection of a uterus at seven months, I am unable to comprehend or appreciate. If the last one or two months are left to theory to be supplied, and not ocular demonstration, or *post-mortem* specimens at the end of gestation, then Weitbrecht has no claims as a discoverer, nor has Duncan, who only indorses the anatomist's incomplete investigations. Duncan has receded from his acceptance of Stoltz's views, and now rests on those of Weitbrecht. My own investigations and observations are supported by ocular demonstration of several thousand cases, at the termination of pregnancy, at the commencement of labor, by more than twenty-five morbid specimens at full term, which exhibit the cervix uteri of the same length and sometimes longer than at the time of conception. I am not aware, from an extended research of the literature of the question, of any authority who has advanced such an opinion before myself, as far back as 1852, though first published in 1860. Depaul, of Paris, now teaches this view, as I learn, by the touch. And from the German journals it has been accredited to myself. My clinical experience, upon which is founded the opinion I entertain on this subject, negative the views of Bell on the mechanism of the uterus during labor. They show the physiological function of the cervix, that, in the delivery of the child, the placenta, or coagula, the cervix uteri is passive, at rest, and pushed or lengthened out when the child's head is passing through, as we notice is the case with the perinæum, and not pulled back over its head, as Bell conceives. Should the labor be a tedious one, and the child's head remain some time before the cervix is fully expanded by the constant and uninterrupted pressure, the cervix may and does pass into a state of atony, or become functionally paralyzed. In either case the body and fundus are the active motor power, while the cervix is dormant, except so far as it retains its natural contractile element, after the child has been delivered.

Instead, therefore, of inversion occurring through an irregular or hour-glass contraction, agreeably to Radford, or Smith, or Duncan, it is the continuance of a regular and natural contraction, the same as we perceive if there were twins. We all know that, soon after the birth of the first child, the uterus commences anew its efforts to expel the second child, and it is easily accomplished. Should, however, the cervix take on an excess of contraction, the cervix will be closed sufficiently to delay the delivery of the second child, and it may be some time. If this were not so, then by an excess of irritability of the circular fibres of the cervix, or of the lower part of the body of the uterus, the hour-glass contraction would ensue, the after-birth would be detained. Now, if contraction of the fundus did not take place, then inertia would follow, with a strong probability of the consequence, in cases of that kind, of hæmorrhage, and the unfortunate results belonging to it. Hour-glass or irregular contraction may also occur through the anterior and posterior longitudinal fibres, thus dividing the uterus lengthwise, or it may occur by the orbicular muscles, or through the whole of the circular fibres of the body. Duncan, however, rests upon the contraction of the cervix solely for his explanation of hour-glass contraction, while the fundus is in a state of inertia, or locally paralyzed.

I accept the incidental remarks which have been made by some authorities, that inversion is comparable to the prolapsus of the rectum and bladder, and I may add the uterus. I believe it is as natural a course of action, as well in the animal as in the human subject. It is more clearly and more distinctly illustrated as a clinical fact, in what we notice when the horse dungs. First the gradual elongation of the anus, when filled with its contents; then, after the dung has escaped, the anus rolled out or everted, when trying to cast off the small particles which remain, and through the slight *vis a tergo*, with the relaxed condition of the sphincter, the intestine follows, and the intestine and rectum are fully inverted. We not only have this illustration through the horse's anus which it resembles perfectly in contour the cervix uteri, but it is admitted that the uterus in the mare, cow, and sheep, are inverted in the same manner from the cervix upward. Le Blanc, the celebrated surgeon, who was first a veterinary surgeon, had seen numerous cases of inverted uterus in the cow. Trousseau, Reinard, and others whom I have consulted, attest to the frequency of inversion in these animals. Why, with all these illustrations, should we consider "the human uterus as the only exception," as so positively asserted, in its mechanism for inversion by the indentation or depression of the fundus first in active spontaneous inversion? How shall we account also for the mechanism of the spontaneous, and sometimes instantaneous reinversion or reduction of the organ, whether of recent date or after many years?

It is my conviction that the mechanism is the same, in the return of the organ to its former natural state, as it is in the creating of the inversion. In the spontaneous reduction, however, the body and fundus of the uterus, when in a state of contraction, are aided by the elevating action and forces of the abdomen, and the fibro-elastic structure of the pelvis. We are not to forget that this peculiar uterine tissue has properties eminently fibro-elastic, muscular also in some parts; that the vagina, uterus, and appendages, are enclosed in this tissue. The remark of Joulin may be applicable at this time, as showing and demonstrating this elevating principle of the abdomen, and the forces of the same during pregnancy. Under this influence the uterus rises more and more according to its increase, as its weight becomes greater. It rises in spite of the laws of gravity, repels the intestinal mass, the diaphragm, mechanically distending the abdominal walls, and at last it is elevated above the superior strait.

It is possible this might be explained by another process. But what forces can make out a point of support? What is the lever which causes this large mass to move? The textures which surround the organ are not sufficient of themselves. The elevating principle or forces of the abdomen should, therefore, claim our attention in the mechanism for the reduction of the organ, independent of the contractile element of the uterus. When lifted up it is retained in its place by the conservative principle of the abdomen so admirably suggested by John Bell, and indorsed by Duncan. If this conservative principle is deficient, then the uterus in the unimpregnated state, or in gestation, or after its reinversion, would have no support but from the subjacent parts.

After resurveying the principal points I have referred to, as tending, besides the contraction of the uterus itself, to establish an inversion, viz., the abdominal nisus, the impulse or pressure of the intestines, the functional or true paralysis of the fundus uteri, I think I have opened the subject to show that these have but little influence in producing inversion in the spontaneous active form independent of the muscular action of the organ itself.

If we consider the physiological mechanism of labor, and the natural physiological condition the uterus passes through to accomplish the delivery of the infant and the placenta, it is my decided conviction that the spontaneous active inversion of the uterus rests upon the prolonged natural and energetic action of the body and fundus; the cervix, the lower part, yielding first, is then rolled out, or everted, or doubled up, as there is no obstruction from the contractility of the cervix, which is at rest, or functionally paralyzed; the body is gradually, sometimes instantaneously, forced lower and lower, or inverted, and, as the fundus continues its action, and the contraction yields, the fundus is flapped out, or falls through, and the uterus is then either partially or wholly inverted, the fundus presenting.

Should the conservative principle and the elevating forces of the abdomen be rejected as having no important bearing, there is no reasonable way to account for the replacement of the uterus in cases of inversion or procidentia, or after they have been returned. The usual expressions which are so frequently made use of by the operator for reinversion of the uterus, when the displacement is about being accomplished, is, that the organ went back with a bound—a snap—a spring into its place, testify to some inherent principle existing physiologically in the abdomen, which could not well be explained in any other way.

June 6, 1869.—I was requested to visit Mrs. W., in consultation with Drs. N. C. Husted and Bliss, for an inversion of the uterus. Primipara, aged twenty-five years. After the delivery of the child, in a few moments, the physician in attendance introduced his hand to remove the placenta. Through strong efforts, he succeeded in inverting the uterus, the placenta still attached. He peeled off the placenta and tried to replace the uterus, but could not. Dr. N. C. Husted was sent for. I saw the patient a few hours afterward. The opinion of Dr. Husted was confirmed. The appearance of the tumor was large, soft, and flabby, and, when pressed, would contract. There was no hæmorrhage at that time. The tumor was sensitive to the touch. Patient was anæsthetized. Dorsal position observed. I attempted to reinvert the uterus, after returning it into the vagina. Grasping the fundus and body in my right hand, with the left placed directly above the pubes, and expanding the upper part of the vagina with the fingers, I proceeded to make direct pressure on the cervix, as well as I could by the fingers of the left hand, and made direct and steady pressure upward in the axis of the superior strait, but without success. I then withdrew the uterus from the vagina, and, placing my index and middle fingers in the rectum, introduced them into the cervix uteri, and raising the body of the uterus, thus forming a *point d'appui*, instead of the abdomen; then, grasping the fundus and body of the uterus, I pressed firmly with the right hand against the fingers near the cervix. Having introduced the uterus into the vagina, I made steady and firm pressure downward and backward on the two fingers, which also assisted the stretching of the cervix. By perseverance, I could feel the organ begin to unfold at the cervix, and diminishing in length, when, shortly afterward, continuing efforts on the remaining part of the fundus, and removing the fingers from the rectum, the fundus passed through, and the uterus was restored to its normal position. On examination by Drs. Husted and Bliss, the fundus was felt firm and well contracted above the pubes.

In this case I had the opportunity of noticing and feeling the contraction and relaxation of the uterus, the same as we realize after delivery through after-pains. Meigs was the first, I think, if not the only authority at that time, to refer to this circumstance, and he counsels that no effort should be made during the contraction, but to wait till the spasmodic action has ceased, and then resume the pressure when relaxation takes place. Adopting the suggestion in this instance, I must say I did not feel any contraction of the cervix when the fundus and body were in action, verifying the natural action of the uterus during labor. In a short time the uterus was restored perfectly.

I was not aware, when I had accomplished the object of restoration by this method, that I had been anticipated by Prof. Courty, of Montpellier. He is therefore entitled to the credit. I had frequently thought and spoken of it as possibly useful expedient to adopt in some cases. In chronic cases it is also applicable. Courty's case was of ten months' duration, the patient having incessant and free hæmorrhages, with extreme debility. His success was rapid, complete, and permanent. The advantage of this method, it is my impression, is greater than the former and usually-adopted one I first attempted.

The method I essayed at first is the one which has been advocated by all the older authorities, as the most natural, according to the principle that the part which comes down last should be the first returned. Agreeably to my own views, as it is the first to be inverted, so it is the first to be restored. Some recent authorities have claimed this course as of their own suggestion.

In contrast with this method, as the fundus is the part first born, that part must be indented first, as the indentation began there, and the attempt must be made to overcome the accident through that portion. By many authorities it is repudiated entirely, although we have records of some instances which have been successful. If the fingers, or hand, or fist, have not succeeded, then we must use the bougie, or stick, or firm pressure on the fundus by a bougie, resting against the abdomen, while the pedicle or neck is manipulated, or a stem-pessary of a peculiar shape. It is more especially applicable in those cases where the uterus does not undergo much if any contraction during the efforts which are made through the fundus, and the uterus is softer and more yielding. One of the strongest objections urged against this procedure is that, in dimpling or indenting the fundus, if we could press the fundus upward, we should find ourselves opposed by a double inflexion, for the body would be grasped by the os uteri, and the fundus would be within the body. It is evident, therefore, that our forces should be directed so as to act upon the angle of inflection.

The next method of taxis is where one of the cornua of the uterus is first indented gradually, and by degrees passed through the cervix, the other side, and the fundus following. This procedure has been termed the mixed method. It is the same which is carried out by the veterinary surgeon in the mare and the cow. It is in imitation of some of the cases of inversion which have occurred, and especially of the one I have quoted from Ingleby. I take it that the principle of reduction of the inversion will depend upon the condition of the uterus and the circumstances at the time of reduction, which of the four methods of taxis are to be adopted.

The elastic pressure, which has been so urgently impressed upon the mind of the profession the last few years, acts, as I believe, on the same principle as the rectal and vaginal taxis, and the first method spoken of. It tends to expand the superior part of the vagina, thus enlarging the cervix at its base, exciting or stimulating the fibro-elastic tissue as well as the contractility of the uterus itself, and, aided by the inherent action incident to the elevating forces of the abdomen, bounds or is lifted up into its natural position, the same as we recognize in spontaneous active reduction. The elastic pressure, like all other means to remedy this accident, has its instances of success and unsuccess. In some cases it has acted promptly in a short time. In others, as the case of Dr. F. A. Ramsey, of Knoxville, Tenn., it was seventeen or eighteen days continued, when success was obtained. I believe the length of time in the case of Dr. Ramsey is the longest period on record by this treatment. It teaches us, what we have been the last few years realizing in cases of inversion of the uterus, that steady, persistent, gentle effort, continued, will be crowned with success, either by the taxis or elastic pressure, which is only an internal taxis. This treatment has become, since the cases of Coley, of Bridgewater, in 1837, and T. Smith, of London, one of the established means to adopt when the different procedures of taxis have not succeeded.

The ancient method of incising the cervix, when the cervix does not yield under taxis, was advocated by Millot, a century ago, in 1773. It has lately been revived by Mr. Barnes, of Great Britain, and presented as original and new.

Millot recommended but one incision. Columbat, imitating Millot, advises several small incisions from the centre to the circumference. These incisions were one-sixth of an inch deep, as he considered they would endure greater dilatation than one, because the stricture would not be as well liberated as by the multiple incisions. Barnes made three long ones into the cervix, one on each side and one behind. The incisions were about one-third of an inch deep and one-third of an inch long. "I passed," he says (after making the incisions), "the uterus back into the vagina, followed it with my hand, and proceeded to reinvert. I was surprised to find how readily the uterus went through the os." Barnes admits he was very timid respecting the operation, and felt uneasy at feeling the uterine tissue give way, as though the incisions were extended by laceration. There was no bad symptom or hæmorrhage in his case. In Dr. Thomas's case it was not so favorable or successful, although he was more decided in the depth of his incisions. They extended down to the subjacent parts, through the tissue of the neck. After the operation, a free jet of blood was projected from an artery, which appeared nearly equal in size to the radial. In this instance he probably cut the circular artery. It was with difficulty the artery could be ligated for half an hour. We must not forget that the difficulty consists in this part of the uterus being very hard and dense, and the retraction of the artery so great that these vessels cannot be easily seized and tied. It is for this reason that the operation of Huguier for procidentia uteri is so much feared.

From the case of Thomas we learn that by the operation of incision, agreeably to the views of Millot and Aran, we should be careful not only how deep we make our incisions, and exactly where, even if the measure to relieve the constriction promises to be of value in certain cases.

The mind is ever fertile in reasoning out and suggesting from experience, and in the medical profession it should be more so than in many other avocations, when it is for the relief of such unfortunate accidents as the present one under consideration in the female.

Besides the recently-revived method of Millot and Columbat, it has been proposed, though not yet carried out, by Tait, of London, and Dr. Thomas, to pass, according to Tait, a tenotomy-knife through the inverted fundus, and then incise the neck of the peritoneal flask on both sides, and thus reduce the dislocation.

Another suggestion of Dr. Thomas's and Tait's was, to perform the operation of opening the peritonæum, as for ovariotomy, and then dilate the cervix by dilating instruments. Dr. Thomas was the first to essay this treatment, which proved, fortunately for the patient, successful under several adverse circumstances.

With these various resources the gynæcologist, in cases of inversion of the uterus, recent or chronic, it is evident (although the cases may be of long duration, and if adhesions have not formed with the surrounding structures, which is rare, as it is seldom the intestines have been found in the uterine peritoneal cavity) that one or the other devices are capable of effecting the desirable object of the replacement of the uterus. Under the conservative principle of action based on experience of the great success attending the various methods, whether by the usual or the combined rectal and vaginal taxis, or by the elastic pressure, or incisions of the neck, external or internal, or opening the peritonæum. The idea of ablation of the uterus should be deferred as the very last resource. Much as this operation has been lauded and adopted by some gynæcologists, it has gradually been laid aside, and probably may pass into the "idea of a day."

The experience, in a clinical point of view, as to treatment, proves the great advance which has been made by careful and patient study, faithfully wrought out. Correct observation shows the conservative tendency to save that important organ from mutilation for inversion, and not, as some authorities would advocate in this enlightened day, the *maiming another* organ, for a procidentia uteri. We should advance with the great advantages we possess through experience and observation, and not retrograde.

Art. II.—*Respiratory Murmurs.*[1] By James R. Leaming, M. D.

Since the time of Laennec, those engaged in investigating physical conditions of the chest have ever united in looking to the breath-sounds for the elementary key.

Able and distinguished men have given much of their lives to the consideration and practice of auscultation, but certainty in diagnosis in incipient disease is yet vainly desired. It must be that the method of study has been faulty, or that attention has been wrongly directed. Under these

[1] Read before the Academy of Medicine, January 4, 1872.

circumstances, presumption may be pardoned, even if it should fail in the attempt to show a better way.

Laennec recognized both bronchial and pulmonary breath-sounds, and explained them as being caused by air-friction. In describing pulmonary respiration, he says: "On applying the cylinder, with its funnel-shaped cavity open, to the breast of a healthy person, we hear, during inspiration and expiration, a slight but extremely distinct murmur, answering to the entrance of the air into and expulsion from the air-cells of the lungs. This murmur may be compared to that produced by a pair of bellows whose valve makes no noise, or, still better, to that emitted by a person in a deep and placid sleep, who takes now and then a profound inspiration" (Forbes's Laennec, p. 29); and the translator adds in a foot-note: "It will be most easily and distinctly perceived by applying the naked ear to the chest of a child." Laennec's view is theoretical, not based on a careful study of all the facts. Indeed, at that time the minute anatomy of the lung, and the constitution of the residual air, were not known. Subsequent opinions have been influenced more or less by Laennec's, especially in this, that all respiratory murmurs are considered to be air- and tube-friction sounds. Many differ from him as to the seat, but all agree with him as to the mechanism. M. Beau, of Paris, placed its seat in the pharynx; Dr. Sanderson, of Edinburgh, in the rima glottidis. Skoda, of Vienna, considered vesicular murmur as occurring only in inspiration, and being caused by air-friction, and he likened it to the noise one makes in forcing the air through the nearly-closed lips. He denies that the respiratory murmur has any thing to do with the vesicular breathing, which, he says, is a purely bronchial sound. Andral called it a sound of pulmonary expansion or vesicular respiration, thus designating its seat, and giving it name.

Many speak of vesicular and respiratory murmurs as interchangeable terms. The late Dr. Hyde Salter placed the seat of the respiratory or vesicular murmur in the convective system, and mostly in the sub-pleural, minute bronchioles (*British and Foreign Med.-Chir. Rev.*, July, 1861). Dr. Waters, of Liverpool, whose prize essay on the minute anatomy of the human lung has done so much to increase our knowledge on this subject, describes the mode of connection of the bronchioli with the air-sacs. The opening sometimes is, as it were, a hole punched out, clean and round, and the air, passing in and out, must make a sound much in the same way as is done in a toy tin whistle. The late Dr. Cammann, of this city, believed the cause of the murmur to be the passage of air into the air-sacs and out again. Dr. Williams, after speaking of portions of the chest where blowing sounds are heard, goes on to say: "Then there is the vesicular respiration, which is heard in most other parts of the chest; it is a diffused murmur caused by the air penetrating through the minutest tubes, and into their numerous vesicles or cells." Dr. Gerhard, of Philadelphia ("Lectures on the Diagnosis, Pathology, and Treatment of Diseases of the Chest"), says: "The sound of air entering the vesicles is different from that caused by its passage through the tubes, and the former is, therefore, known as the vesicular sound, the latter as the tubal or blowing sound. The vesicular sound is often called a murmur, from its softness and diffusion over a large space, and cannot be produced unless the vesicles are healthy or nearly so." And again he says the cause of difference "seems to be the different manner in which the air impinges upon the vesicles and tubes. But the vesicular sound is in part owing to the vibration of the air, and in part to the noise produced by the dilating of the vesicles themselves."

Dr. Walshe represents the natural respiratory murmurs as caused by inspiration and expiration, for which there is usually a healthy type, "commonly termed—*a*, pulmonary or vesicular; *b*, bronchial; *c*, tracheal; *d*, laryngeal; *e*, pharyngeal, according to the part of the respiratory apparatus from which the sounds audible externally are transmitted." Dr. Corrigan divides the sounds heard in auscultation into "simple sounds or murmurs, and compound sounds or rattles. . . . All the sounds heard in the chest belong to one or the other of those two kinds; and, if, when you hear a sound, the exact nature of which you may be in doubt, you will first refer it to its class, your labor in determining what it is will be very much diminished." The American editor of "Stokes on the Chest" describes vesicular murmur as that "of a soft and gentle, or, as it has been otherwise described, a mellow, continuous, gradually-developed, breezy murmur, unattended with a sensation either of dryness or humidity; and we are properly cautioned by M. Fournet and his reviewer not to expect a character of sound which conveys the notion of a successive dilatation of separate vesicles, or, as it is sometimes called, pure and vesicular." Dr. Hyde Salter says: "There is another reason, to which I have not referred, which makes me think that the respiratory murmur must have a tubular or *quasi* tubular seat, and cannot be formed in the air-cells; it is, that fine crepitation, such as that of pneumonia, *supplants* it; it does not merely drown it, it supplants it, the two do not coexist;" and farther on: "If, then, pneumonic crepitation is a veritable tube-sound, and its seat the microscopical tubes immediately subtending the air-cells, the supplanting and destruction of the respiratory murmur by it would show that this latter has an identical seat, and is therefore a tube-sound." This explains Dr. Salter's views as to the seat and cause of the murmur. He believes it to be caused by the passage of air through these microscopic air-tubes, just before they reach the vesicles; and, as he is one of the latest and most brilliant writers on this subject, perhaps he represents the more advanced views of the profession. He does not deny that sounds formed anywhere in the convective system, from the mouth or nose to the smaller bronchiæ, mingle with and enter into the composition of the respiratory murmur, but he denies that the air-vesicles or alveoli have any thing to do in forming the sound. He believes the sound is formed in the bronchioles, immediately subtending the pulmonary pleura.

Carefully examining the opinions of different writers, it is evident that some consider the respiratory murmur as having a single seat and cause, while others recognize its composite character. Yet, I am not aware that any one has ever attempted to analyze the murmur, and study its constituents separately as well as together. They speak of the vesicular character, the pulmonary quality of the respiration, but they attempt no analysis. To show that this may, and ought to be done, in order to attain unto a higher grade of excellence in diagnosis, is the main object of this paper. A clear understanding of this whole matter will make it necessary, as preliminary, to look at the minute anatomy of the tissue of the lungs, and of the bronchial system; secondly, the circulation of the lungs and of the bronchial system; and, thirdly, the characteristics and constitution of the residual air, its object and office. The bronchial system may be, and is frequently, called the convective or the broncho-respiratory system, and the pulmonary is called the true respiratory system. They differ in almost every respect. The office of the broncho-respiratory is to convey air into the true respiratory system, while the true respiratory system is where the great function of vitalizing the blood is perfected. The bronchial system is characterized by cartilage in its fibrous sheath. In the upper part, where it is necessary to prevent collapse of the tubes, the cartilage is in nearly perfect rings, but as the tubes pass into the lung-structure, where they are occupied by the residual air, the cartilage gradually loses the character of rings, and appears merely as deposits occurring at irregular intervals, down so far as the bronchial arteries extend, to where the bronchial veins commence to carry back the blood that has passed through the capillaries of the bronchial mucous membrane. The mucous membrane also, of the broncho-respiratory system, is different from that of the true respiratory system, in this, that it is ciliated epithelial mucous membrane, while the other is of tesselated basement epithelium. The circulation also is entirely different. The convective system is supplied by the bronchial arteries; the pulmonary substance by the pulmonary artery, and by the nutrient arteries of the lungs, which are the connecting link between the two systems. The nutritive arteries arise from the bronchial arteries, but have no accompanying veins. Thus, blood, after performing the proper office of nutrition in the pulmonary tissue, is at once reaërated, and passes into the venous radicles of the pulmonary vein prepared for systemic circulation.

The bronchial arteries have been called the nutritive arteries by anatomists, but they have not dwelt upon the fact that the *venæ comites* do not attend these arteries into the pulmonary structure, and that, consequently, this gives them a peculiar character. The bronchial veins return all the blood of the bronchial arteries; the nutritive arteries have no veins. Their blood is reaërated where they do their work, and it finds its way into the venous radicles of the pulmonary vein as arterial blood. This anomaly in the circulation is of great interest in explaining physiological causes and pathological effects. In pneumonia, it is the nutrient artery, accompanied with its plexes of ganglia of the organic nerve, lymphatics, etc., that preserves the life of the part, and governs the whole process of resolution. We can all remember the anxiety of practitioners, in the past, to prevent abscess and gangrene of the lung after inflammation. But time, and a more careful study of the natural history of the disease, have proved to us that gangrene and abscess are rare accidents, even when no treatment at all is had. This peculiar arrangement of the nutrient artery gives us an early knowledge, in many cases, of commencing phthisis. Occupation of the air-sacs by tubercle interferes with the circulation, and blood is thrown back upon the bronchial artery, and the result is bronchorrhagia, a conservative act; for, like the application of leeches, it sets the absorbents actively at work to remove the cause—the new tubercle. And, in this way, cases of early phthisis are self-cured, or, at all events, ameliorated, and the physician is guided in his treatment.

This singular fact in the circulation was discovered by the late Dr. Cammann, in making his experiments to prove the non-anastomosis of the arteries of the lung. Using a colored fluid suitable for fine injections, he found that, when he injected the pulmonary artery, the fluid returned easily by the pulmonary vein; but, injecting the pulmonary vein, the fluid not only passed into the pulmonary artery, but, if the injection was carefully continued, it would also find its way into the bronchial arteries. Then, again injecting the bronchial arteries, he found that the fluid after a little time passed into the pulmonary vein; this proved that there was communication between the bronchial arteries and the pulmonary vein, but not with the pulmonary artery. This was shortly after 1840, and before, I believe, any experiments had been made in Europe, in regard to this circulation. Since then, several observers have come to nearly the same conclusion. Drs.

Williams and Adriani believe "the vessels of the bronchial mucous membrane terminate in the pulmonary veins, and those of the deeper plexus in the bronchial veins." Dr. Waters says, after explaining his experiments, which were very full and minute: "That a distinct and free communication exists between the bronchial vessels and the pulmonary veins, admits of ocular proof. I have seen, with the aid of the dissecting microscope, the small vessels passing from the outer surface of the bronchial tubes, and forming a small trunk, which terminated in a pulmonary vein." Dr. Waters also says: "It may be said that such a view militates against the generally-received opinion of the purity of the blood returned to the left side of the heart, for, if the bronchial blood is poured into the pulmonary veins, it is returned to the left auricle without undergoing the process of aëration. I would answer that the view I have taken is supported by anatomical facts, a basis on which all physiological theories should be founded." I remember that Dr. Cammann, also, could not reconcile the incongruity of the apparent fact that venous blood passed directly into the aërated blood of the pulmonary vein, and then to the left heart. Both of these gentlemen overlooked the truth that the blood from the nutrient artery passes through capillaries in the true respiratory system on its way to the radicles of the pulmonary vein, and, of course, is reaërated. Dr. Robert Lee, if my memory serves me (for I have not the paper at hand), says that the extension of the bronchial artery, after it has quit company with the vein, receives additions from the mammary and intercostal arteries, and has the proper title of nutrient artery. I do not quote his words, but the substance as I remember it.

I believe, then, I am warranted in holding that there is a complete difference in the blood-vessels of the convective and of the pulmonary systems. The nutrient arteries of the bronchial system have their venæ comites; the nutrient arteries of the true respiratory system have no accompanying veins, but pass their blood reaërated directly into the pulmonary vein, prepared for systemic circulation. The nutrient artery is no exception to the rule of complete difference in the two systems, for in its office it belongs wholly to the true respiratory.

The vessels of the bronchial system are the bronchial arteries and veins; the vessels of the true respiratory are the pulmonary artery and vein, and the nutrient artery of the lungs.

Where the bronchial system ends the pulmonary begins, and the division is sufficiently marked—it is where cartilage ceases and alveoli commence. The structure of the true respiratory system is composed of terminal bronchii, in which are developed alveoli and the air-sacs, that is, wherever alveoli are found. Its whole object or office is aëration of the blood of the body. It is greatly distensible, and in this differs from the convective system, which is but little so, and its formation evidences design in the economy. of space and for its especial purpose. The bronchioles have alveoli developed in their sides, but not to the same extent as in the air-sacs, which are but a skeleton net-work for the convenient spreading out of alveoli, with their rete mirabile of capillaries, for the aëration of the blood. The terminal bronchus enlarges at its end, and the air-sacs are developed from this enlargement, according to Dr. Waters, as a cluster of leaves are, sometimes from the end of a twig. From six to thirteen of these air-sacs are in connection with the enlarged end of a terminal bronchus, and this little cluster forms a lobulette—a complete type of the whole lung. Each lobulette has its terminal bronchus and air-sacs for the development of alveoli, its twig of pulmonary artery and vein, its branch of nutrient artery, with the accompanying gangliæ of organic nerve, lacteals, absorbents, etc. A collection of lobulettes form a lobule, and a number of these constitute a lobe. The fibrous bands of the bronchial sheath are continued, though with great tenuity, through the terminal bronchi into the air-sacs, both of the white and yellow variety. They surround the mouth of each air-sac, and give firmness to the frame of each alveolus. Muscular fibres also accompany these bands, though their presence is doubted on account of their extreme tenuity. Niemeyer speaks of muscular fibres as present in the true respiratory system. In emphysema, the air-sacs lose their power of contraction, and become dilated, causing great suffering and disability to the patient. Time and freedom from catarrh allow the function of contraction, which is a muscular habit, to return.

Physiologists describe residual air as filling the respiratory system as high up as the third or fourth divisions of the bronchiæ. It not only fills the true respiratory system, but distends it. The elements of the distending force are: atmospheric pressure, muscular contraction, rarefaction, and the laws of diffusion of gases, and that of affinitive attraction between oxygen and venous blood. The residual air occupies its position with such persistence as to be with difficulty dislodged after death, even with much pressure. It keeps its place with vastly greater tenacity, during life, when each element of force is in active operation.

During inspiration, the contraction of the diaphragm increases the capacity of the chest, and at the same time the epiglottis is raised, and the weight of the atmosphere operates actively in dilating the lungs. Rarefaction of the newly-inspired air takes place upon inspiration, owing to its immediate and intimate admixture with the residual air, and is the third element of dilating force. The residual air is estimated to be 170 cubic inches, and the inspired air at 20. At each inspiration, therefore, the residual air will be increased about one-tenth part in dilating power, *plus* the rarefaction of the inspired air. But the peculiar elements of this expanding force are, the laws of the diffusion of gases, and that of the affinitive attraction between the unaërated blood-globules, in the capillaries of the rete mirabile of the alveoli, and the oxygen, which is equally distributed throughout the residual air. Chemistry demonstrates that gases differently constituted in certain relations instantly intermix when brought together. The inspired air and the residual air present these differences. Air entering the convective system moves in a body through the bronchial tubes, till it meets the residual air, when, the law of the diffusion of gases operating, immediate admixture takes place. The residual air is instantly renewed with oxygen, in accordance with this law. The inspired atmospheric air moves through the convective system, as far as the fourth division of the bronchiæ, with no other resistance than the friction of the tubes. When it meets the residual air, it is immediately consumed, as it were, and does not accumulate, causing resistance. On this account the inspired air moves with increasing velocity, producing air- and tube-friction murmur. Tidal air in health is only heard in inspiration. Velocity of the moving air in the tube is the cause of murmur. Any one may demonstrate this fact by breathing through a tube gently, when there will be no murmur, but, if he increase the velocity of the moving air, he will get sound, which will be increased in sonority and raised in pitch just in accordance with the rate of motion. In health, in unconscious breathing, expiration is not heard, and we know by experience that, when it is heard in unconscious breathing, there is disease; it may be phthisis, or it may be emphysema—other conditions must determine which. A murmur may be produced at will, by hurrying the respiration. It is heard in systemic diseases like cholera, or in diseases of particular organs, as in cardiac apnœa, or Bright's small kidney. The cause of murmur, in air moving in a tube, no matter what are the other conditions, or the disease, is the *velocity*, increasing the air- and tube-friction.

Prof. John W. Draper has given a convincing explanation, based on accurate experimentation of affinitive attraction in the systemic capillaries, as one of the efficient causes of the circulation. The same power operates in the pulmonic circulation, but with this important addition, that the attraction is not alone in the pulmonic tissues and the blood, but principally in the venous blood and the oxygen of the residual air. This is the cause that brings the venous blood and oxygen together, in order that the blood may be purified and fitted to continue the life of the body. Let us endeavor to comprehend the intricate mechanism of the respiratory act. Inspiration has taken place—twenty cubic inches have been added to the residual air, evenly and equally admixed—dilatation has taken place with force, and is continued and increased by the rarefaction of heat. The true respiratory system, by its muscular power, contracts forcibly, antagonizing the dilating residual air. Each particle of pure air, acknowledging its attraction for the venous blood, presses up to the alveolus, through the struggling mass, and rushes to the blood-globule in the capillary—makes the interchange—gives up its oxygen, and receives in return detritus and carbon materials, loses its attraction, becomes passive, but is crowded back by other eager particles pressing forward, until finally it finds itself well up in the bronchus, with its filthy load, whence it is expired. The blood-globule from the pulmonary artery, entering the capillary of the alveolus, hurries along through the rete mirabile, drawn by its affinity for oxygen, till it meets a particle of pure air, makes the interchange, loses its activity, but is pushed onward by other globules pressing forward from behind, till it finds itself in the venous radicle of the pulmonary vein, fitted for systemic circulation. The movement of the blood-globules is much assisted by the contraction and relaxation of the muscular fibres of the true respiratory system. Different bundles of these fibres, contracting and relaxing in succession, give not only a living vibratory motion, which assists in hurrying the globules along, but produce a susurrus, which, being heard at the chest-wall in multitudinous concert, is true respiratory murmur. These facts in minute anatomy and physiology (and they hardly admit of any dispute) prove that the residual air, as a body, has no more motion than has the bottom of the deep sea. No change can occur except molecular, and none other is necessary. The law of diffusion of gases assures the comparative purity of the residual air, as well as its constant and guarded impurity, which is so necessary for the accomplishment of the vital act. The circulation would not go on, if each blood-globule should immediately come in contact with pure air, for then it would lose its impelling force, and, all of the globules alike losing their attraction, there would be stasis. Instead of this, both in the blood and the residual air each globule and each air-particle moves in perfect order, never in each other's way. This shows how the individual may live in bad air for a time, resisting its evil tendencies, and even that of poisonous gases. It shows also why medical inhalations fail in their object. Medicated vapors have little or no admission into the residual air. Even oxygen gas, which is sometimes serviceable, can only supply atmospherical deficiencies. It can neither do the harm nor the good that has been predicated for it. An animal may even live for a time in pure oxygen gas, the active interchange taking place between the gas and the blood restoring the necessary grade of impurity in the residual air.

If, then, the only change or motion that is possible in the residual air be molecular, what becomes of the theories of air- and tube-friction murmurs, whether in the smaller bronchiæ or the air-sacs and alveoli, as cause of the so-called vesicular murmur? They are physical impossibilities. And, too, what becomes of the theories of the mechanism of crepitant *râle?* If there is no motion but the molecular, there can be no bursting of bubbles in the microscopic tubes, and that theory falls. If the residual air constantly and forcibly distends the true respiratory system, how can the bronchioles and air-sacs come together, to be separated by each inspiration of fresh air, so as to produce fine crepitant râle? This theory, likewise, supposes a physical impossibility. All theories, whether of vesicular murmur or crepitant râle, which ignore the presence of the residual air, are of necessity incompetent. The fact that residual air has none but molecular motion may be demonstrated by a distensible bag, as of India-rubber. While it is being forcibly filled with air, there will be air- and tube-friction murmur at the mouth only, where the air moves in a body with velocity. The body of air in the bag will be increased by particles of air sliding in among each other and without sound. But there will be resisting vibratory sound in the walls of the tense dilating bag; different, however, from that of the contracting true respiratory murmur in this, that it is only heard during dilatation, while the other is continuous, because owing to active muscular contraction. Dr. Hyde Salter says, after speaking of the occupancy of the true respiratory system by residual air, and that about twenty cubic inches of atmospheric air are added at each inspiration: "Each air-cell is, therefore, a tenth larger at inspiration than at expiration. Now, it is inconceivable that this slight variation in the capacity of these shallow open concavities should be attended with any sound. I cannot conceive it possible. For, be it remembered that the air-cells are not nearly-closed cavities communicating by constricted orifices with the general cavity of the lobular passage, but wide-mouthed and patulous like a teacup. And be it remembered, too, that in respiration the air is not pumped out of and into the cells, but, as they undergo this slight change of volume, a small part of their contents passes just without them, and then again, on their recovering their capacity, from without just within them, if one can speak of 'within' and 'without,' in reference to such slight interchange of situation. For, really, the renovation of the air in the tissues of the lung does not depend on its actual removal, but upon the law of the diffusion of gases."

This reasoning is cogent and unanswerable. It proves beyond cavil that there is no motion in the air-sacs and alveoli to produce air- and tube-friction sound, and yet he attempts to show that there is such motion in the smaller bronchiæ and intralobular passages. He says: "But while the movement of the air at each alveolus would be so slight, so almost inappreciable, the collective expansion of all the alveoli common to a lobular passage, and the consequent abstraction of air from the general cavity, would be considerable, and would create a considerable rush of air into the lobular passage to supply its place, for the modicum of air, however small, appropriated by each dilating air-cell, would of course be multiplied by the number of cells communicating with the common axial cavity of the lobular passage."

Dr. Salter's able reasoning shows that there is not enough motion in the alveoli or air-sacs to cause sound, and the same reasoning applies with equal force to the air in the bronchioles and intralobular passages. The residual air occupies these passages just as well as it does the air-sacs; one-tenth is added at each inspiration to the whole body of residual air, and Dr. Salter himself has said that these small bronchial tubes were largely distensible, consequently the velocity of motion in these passages where alveoli are developed must be too little, if there be any at all, to produce any sound. There certainly can be no rush; indeed, I have already shown that there can be no motion, except the molecular. But, for argument's sake, if there should be motion in these minute tubes, as Dr. Salter claims, it could not possibly have the velocity necessary to cause sound. Dr. Salter's argument to prove that the seat of crepitant râle and the seat of respiratory murmur are the same—"The râle supplants the true respiratory murmur, the two do not coëxist"—heretofore quoted, is convincing. Had he placed the seat in the air-sacs and alveoli as well as in the terminal bronchioles, he would have been correct, for then he must have acknowledged that it could not be by tube- and air friction, and he would have been forced to accept the true explanation, that of dilatation and contraction. Crepitant râle indicates the commencement of the process of inflammation, and it supplants the true respiratory murmur. Let us study the evidence in the light of the true respiratory murmur.

If you have lately examined the chest of a person in health, and have noted the murmur in its fulness and perfection, and should be called to see him suffering from a chill, with pain in the head, back, limbs, etc., and should again examine the respiration carefully, you will still hear the true respiratory murmur, but it will be obscured or muffled. All the capillaries of the lung are crowded with blood, and this is the explanation of the muffled murmur. If you wait a few hours, and again examine him, you find the true respiratory murmur absent, and, in place of it, the fine crepitant râle. The congestion of the capillaries of the lung still remains; there is scarcely a perceptible difference in the percussion-note; the residual air still occupies its seat in the true respiratory system, and it still continues to dilate the air-sacs, alveoli, and terminal tubes. Whatever change has taken place, must have been at the seat of the true respiratory murmur.

In tissues that may be seen, what is the first result of inflammation? Is it not that plastic material is thrown out into the connective tissue? This, also, must take place in the lungs. The connective tissue of the lungs, delicate as it is, has been filled with plastic material. It has become thickened and stiffened, it cannot contract, and the true respiratory murmur is gone, but it must yield, though unwillingly, to the dilating force of the residual air, increased one-tenth at each inspiration, separating newly-formed plastic exudations, causing sound, which we hear as fine crackling, and call it crepitant râle. If we wait a few hours more, and examine again, we will find that crepitant râle as well as true respiratory murmur has gone, and all is silent, or there may be bronchial or tubular breathing. Exudation has been poured into the true respiratory system, and consolidation is the result. The seat of crepitant râle is now become the seat of exudation.

If I have studied this matter as correctly as I have carefully, this is the process gone through with, and is the true mechanism of crepitant râle. In this paper I have endeavored to show that the bronchial respiratory system is entirely different from the true respiratory system in anatomy, physiology, object, and use, and that the physical signs of pathological change are equally distinct and different. That the residual air, occupying, as it does, the true respiratory system with force, precludes the idea of currents of air within the lungs, and consequently the accepted theories of the vesicular or respiratory murmurs and of the formation of crepitant râle are necessarily incompetent. If my points are well taken, and the proof convincing, the profession will eventually sustain the truth, and much that has been received as settled literature will be swept away as rubbish, to give room for truer and better grounds of faith.

The composite character of the respiratory murmur must be made evident, analytically as well as synthetically. The two elements, different in cause, character, and seat, must be individually studied in order that we may correctly understand their significance in pathological changes. We may present their union and the result to the eye, thus:

$$\left.\begin{array}{c}\text{Broncho-respiratory murmur.}\\ \\ \text{True respiratory murmur.}\end{array}\right\}\ \text{Respiratory murmur.}$$

The reasons for introducing a new terminology are, that broncho-respiratory and true respiratory are descriptive, and indicate the seat of the murmurs. The term vesicular murmur was applied by Andral, supposing that it described the minute anatomy of the seat of the murmur.

Later investigations show that the term is misapplied, for there are no structures that may properly be called vesicles in the lungs. Again, the terms vesicular and respiratory have been applied indiscriminately, and their present use would lead to confusion and misapprehension.

In order to practically study these murmurs, it will best be done by selecting a healthy person about twenty-five years of age, with perfectly-developed chest and with muscles not hardened by manual labor.

RESPIRATORY MURMURS.

Placing the ear lightly yet firmly to the chest, allowing the head to rise and fall with the respiration, listen to the breath-sounds. The tidal-air murmur will first catch the ear as modified by the true respiratory murmur, and, as has been described, is like the sighing of the trees over our heads in the forest, when the boughs are gently stirred by the breeze. The character and quality of the respiratory murmur depend upon the absence or excess of one or the other of the composing elements. If the true respiratory murmur be maximum in fulness, the tidal-air sound will be short, only heard in inspiration, and will be of the soft, breezy character described as gently sighing.

While, if the broncho-respiratory be in excess, the tidal-air sound will be harsh, raised in pitch, and will be heard both in inspiration and expiration, and becomes a sign of disease as the other is of health.

BRONCHO-RESPIRATORY MURMUR.

Broncho-respiratory murmur may be studied by forcing the breathing, when it will be heard in both inspiration and expiration, and its harshness, loudness, and pitch, will depend upon the force given to the respiration. This murmur may be heard in its perfection, in the chest of a child, before the true respiratory murmur has been developed.

TRUE RESPIRATORY MURMUR.

The ear accustomed to auscultation, after a few moments of concentration of the attention upon the respiratory murmur, will recognize its dual composition. If the chest be perfect in condition, the tidal-air sound will be heard in inspiration only—soft and short, like breathing gently through the closed teeth—while the true respiratory murmur will be continuous, increasing in fulness in inspiration and diminishing in expiration. It is of low pitch, and is like the roaring of the sea at a distance, the waves breaking on an even shore of sand; or, better still, like the sound made by bees in cold weather, when the hive is tapped with the finger. It is like the innumerable vibrations of the wings of bees, increasing to maximum in inspiration like the coming waves on the sea-shore, and decreasing in expiration as they recede. If the breath be held, this murmur may be heard without admixture, for there can then be no bronchial murmur. The sound is the susurrus of the delicate muscular fibres of the true respiratory system, contracting and relaxing over the dilating and resisting residual air. If the breath be held after a full *inspiration*, the murmur will be at its maximum; if it be held after *expiration*, it will be at its minimum fulness. It cannot be exaggerated, as has been said of the so-called vesicular murmur. If the true respiratory system be unduly dilated, it loses its power to contract on the residual air, and the murmur wholly ceases. This is a sign of emphysema, and is proof of the muscular cause or origin of the sound which may return again after rest.

This murmur only commences to be developed in the child at eight years of age, becomes recognizable at twelve, but is only fully developed at maturity. A beginner in auscultation may recognize true respiratory murmur in a good subject with ease. But, when the chest has lost its excellent quality as an acoustic chamber by physical changes, resulting from inflammation, or when, from disease of the lung itself, the natural respiratory murmur has been altered or lost, or when the chest, although in its natural conditions, may be covered by thick and hardened muscles, the trained expert ear only can arrive at diagnostic truth.

Many love and enjoy music, and may assist in producing it, but the trained expert alone can lead an orchestra, and harmonize each instrument into a body of perfect song.

These facts, instead of being a matter of discouragement, should induce beginners to pursue auscultation with untiring assiduity, knowing that the end will crown them as masters in physical diagnosis. The ability to recognize true respiratory murmur under any conditions, to analyze its quality, and measure its power, gives its possessor the means of knowing even the approach of that most insidious disease, phthisis, and suggests the method of prevention. The true respiratory system, air-sacs, alveoli, nutrient artery, ganglia of the organic nervous system, with absorbents, etc., all require active use for the prevention of disease. Phthisis does not begin in the lower part of the lungs, which are constantly and actively in motion. If we insure the same kind of exercise in the upper part, we prevent and may even arrest incipient disease.

For more than ten years respiratory murmurs have occupied my diligent attention. The views put forth in this paper were not hastily formed. Some of them are new, and may receive the harsh judgment innovations ever provoke. I do not deprecate criticism, and I am not insensible to the opinions of my professional brethren. I earnestly desire their approval. Many, on whose judgment I rely, believe in true respiratory murmur as well as myself. But, wishing, above all things, for the establishment of truth, I submit the whole subject to this learned Academy, and through it to the profession.

ART. III.—*Lectures on Public Hygiene.* Delivered at the Bellevue Hospital Medical College, Summer Session, 1872. By WILLIAM A. HAMMOND, M. D., Professor of Diseases of the Mind and Nervous System, and Clinical Medicine, and of Materia Medica and Therapeutics.

LECTURE I.

THE SANITARY RELATIONS OF THE ATMOSPHERE.—EMANATIONS FROM DECOMPOSING ANIMAL MATTER.

GENTLEMEN: In a few lectures, I design to make you acquainted, as far as I can, with certain striking facts of public hygiene. I shall deal mainly with general principles, leaving you to work out the details for yourselves, and I shall endeavor to illustrate my remarks to a great extent by experiments and familiar examples. A more important science to man than hygiene scarcely exists; and yet, strange to say, it is a science which he regards with very little interest till some terrible scourge in the form of a pestilence frightens him into spasmodic attempts to abate the severity of a punishment which with timely care he could altogether have escaped. For a violation of the laws of health there is no pardon. The sin is committed, and the penalty follows with ruthless certainty, and of a degree of sharpness exactly proportioned to the offence. Look at the criminal disregard of the very first principles of sanitary science, evinced by those who have the rule over us in this city; see the masses of decomposing animal and vegetable matter which exist in every street; the occupations prejudicial to health which are permitted to be carried on in crowded localities; the faulty construction of our dwellings and of the drains which remove the excreta of their inhabitants—and then say if it is surprising that for week after week the deaths exceed the births, and that pestilence upon pestilence stalks almost unchecked among us. Even the reformers think hygiene a matter of small importance, for, in the charter recently submitted to the Legislature by a body of presumably well-educated gentlemen, a Board of Health is constituted, not a single member of which is required to be a physician! Think of a merchant selecting a clerk who knew nothing of figures; or of that same Committee of Seventy neglecting to provide that judges should be educated in the law, and you would not have a more striking instance of criminal folly than the one I have cited. Probably, however, the time will come when matters will be very different, and when the chief office of the physician will be to advise his patients and the public generally not how they may be cured of their diseases, but how they may avoid them.

The short course of lectures I am about to give you will be devoted to the sanitary relations of the atmosphere we breathe the water we drink and use for other purposes, and the soil upon which we live. These matters constitute, it is true, scarcely a moiety of the subject of public hygiene; but they are certainly secondary to no others in importance, and they form the groundwork of the science. With a good knowledge of them in their various hygienic connections, you will certainly make better physicians than you would otherwise be, even if you should not be called upon to act as members of boards of health.

The air exists in two distinct conditions. It surrounds the earth as a gas of variable density, and it is dissolved in the water which constitutes so large a portion of the surface of our globe. It is necessary to the life of all organic beings with which we are acquainted. It extends above the earth nearly forty-five miles, and at the level of the sea exerts a pressure of about fifteen pounds upon every square inch of surface. Owing to various causes, this pressure is subject to considerable variation, and most of the grand principles of terrestrial meteorology are connected with this fact.

By measure, one hundred parts of atmospheric air consist of 20.8 of oxygen and 79.2 of nitrogen mixed mechanically, and therefore not constituting a fixed and definite chemical compound. In all situations it contains small amounts of carbonic acid, ammonia, nitric acid, and iodine, which are subject to variation according to locality. It also contains other substances of local origin and distribution, some of which are in a greater or less degree deleterious to the health of those compelled to breath the atmosphere in which they exist. These arise from animal or vegetable decomposition, are products of the various industrial pursuits in which man is engaged, or are living organisms of animal or vegetable origin. Their nature, the conditions under which their evolution takes place, the degree of danger which attends their inhalation, and the means of avoiding or counteracting their noxious influence, constitute a most important part of the science of public hygiene.

Many of these emanations are of exceedingly unpleasant odor, but others, again, only reveal their existence by their effects in causing disease. These latter are therefore more insidious than the former, and there is reason to believe far more dangerous. Indeed, relative to the former, opinions and facts are not wanting which support the view that, as regards some of them, their injurious power has not only been over-estimated, but that, on the contrary, they are beneficial to health. Chief among the sources of such vapors, slaughter-houses, glue-factories, tanneries, soap-boiling and fat-melting establishments, graveyards, market-houses, stables, manure-factories, etc., are to be found. To the consideration of the question, How far are the emanations from such places injurious? I propose to devote the present lecture. In the first place, I will adduce some of the most remarkable instances on record which appear to have most bearing on the point at issue; I will then state the conclusions which appear to be established; and, finally, perform a few experiments in illustration of the principles brought forward.

During the war in the Spanish Peninsula the sick in the hospitals at Ciudad Rodrigo were affected with dysentery, hospital gangrene, and tetanus, to an extraordinary degree, consequent, as was alleged, upon the burial of many thousand dead within the limits of the city a short time before it was occupied as a hospital-station. This fact has repeatedly been used as an argument in support of the theory of the uniformly noxious character of the vapors given off from all dead animal matter; and yet, when typhus fever prevailed to a great extent at Paris, in 1814, the patients affected with this disease who were placed in the immense *abattoir* at Montfauçon recovered with much greater rapidity, and with more certainty, than those treated in the regular hospitals. The good results in this case may, however, have been owing to the fact that the situation is remarkably airy, and salubrious in all other respects.

In the year 1828 a commission was appointed by the French Government to inquire into the sanitary relations of the large knackeries in Paris. It was found that all the men, women, and children, connected with these establishments, were remarkable for the excellence of their health and fine, ruddy appearance. The workmen generally were long-lived, and many of them had attained to extreme old age. Persons who resided in the vicinity of these places were likewise ascertained to be unusually healthy. Epidemic fever seemed to be powerless to affect the persons engaged in employments by which they were exposed almost continually to the most offensive emanations from decomposing animal matters, and even the cholera passed them by. In the whole course of their examination the members of the commission were unable to obtain the least evidence tending to show that the disgusting odors produced in the establishments in question were ever the cause of ill-health. From these facts, and many others which he has adduced, Parent Duchatelet[1] unhesitatingly

[1] "Hygiène Publique."

states his conviction that the emanations from decomposing and putrid animal tissues are not prejudicial to health.

Patissier[1] is of the opinion that the emanations arising from decaying animal matters are not very deleterious unless they are confined within narrow limits, and thus absorbed into the system in an exceedingly concentrated form. Night-scavengers, and the manufacturers of manures from animal excrements, would appear to be peculiarly liable to certain diseases, such as gastro-intestinal inflammations, carbuncle, and gangrene. At the same time[2] he declares, what will I think be generally admitted, that butchers are of all other persons the most healthy and vigorous in appearance. He attributes their good condition to the fact that nutritive molecules, disengaged from the flesh and blood surrounding them, are absorbed in large quantities into their systems. During the summer they are, in his opinion, owing to putrefaction rapidly taking place in their *abattoirs*, subject to putrid and malignant fevers.

Londe[3] states that, while the emanations from blood and sound flesh are not injurious, those arising from the same substances in a state of decomposition exert a decidedly contrary influence. When the cleansing of slaughter-houses is neglected, and, as a consequence, putrefaction takes place in the *débris* collected in them, butchers, he says, are liable to visceral inflammations, carbuncles, boils, malignant pustules, etc. Dissecting-rooms are not unhealthy places when they are well ventilated, and when the bodies used are in a good state of preservation. It is well known that putrid animal substances introduced into the blood produce great constitutional disturbance, and often death. It is certainly reasonable to suppose that such matters, reaching the circulation through the respiration, must likewise be highly deleterious. An instance of the kind, given by Londe, is so striking and full of warning that I quote the details in full:

"Dr. Chambon was required, according to Percy, to demonstrate the anatomy of the liver and its appendages at the

[1] "Traité des Maladies des Artisans," etc., Paris, 1822, p. 105, *et seq.*

[2] *Op. cit.*, p. 162, *et seq.*

[3] "Nouveaux Éléments d'Hygiène," troisième édition, Paris, 1847, tome ii., p. 453.

time of his license, by the faculty of Paris. Decomposition was far advanced in the cadaver used for the demonstration, and Chambon called attention to the fact; but, notwithstanding his objections and those of the professors, the obstinate dean insisted on his making use of the subject. One of the four candidates, overcome by the putrid emanations which escaped as soon as the body was opened, fell into syncope, was carried to his home, and died in seventy hours. Another, the celebrated Foucroy, was attacked with a severe exanthematous eruption. The other two, Laguerenne and Dufresnoy, remained for a long time in a feeble state of health, from which the latter never entirely recovered. As to Chambon, excited to anger by the obstinacy of the dean, he remained firmly in his place, and finished his lecture while those who surrounded him were counteracting the offensive odor with handkerchiefs saturated with perfumes. Without doubt he owed his safety to the mental excitement which, after a slight fever, terminated in a profuse perspiration."

Pringle [1] ascribes the production of malignant fevers and dysentery to (among other causes) the emanations arising from sewers, from slaughter-houses, and from putrefying animal matters generally.

Monro [2] accepts the view of Portius, Ramazini, and other writers, that ascribes the origin of putrid diseases to the stench and effluvia arising from the excrements of men and beasts, and from the dead bodies of men, horses, and other animals, lying unburied in the neighborhood of camps.

Desgenettes [3] has known the insalubrity of the field-hospitals very much increased by their being placed in localities surrounded by dead bodies, some of which were scarcely covered with earth.

Hennen [4] calls attention to the fact that the hospital at

[1] "Observations on the Diseases of the Army." First American edition, edited by Benjamin Rush, M. D. Philadelphia, 1810, p. 285.

[2] "Account of the Diseases which were most frequent in the British Military Hospitals in Germany," etc. London, 1764, p. 344.

[3] "Histoire Médicale de l'Armée d'Orient," troisième édition." Paris, 1835, p. 85.

[4] "Observations on Some Important Points in the Practice of Military Surgery, and in the Arrangement and Police of Hospitals. Illustrated by Cases and Dissections." Edinburgh, 1818, p. 250.

Abrantes, in Spain, was situated on the Alemtejo bank of the Tagus, on a low, flat, moist ground, which was occasionally overflowed by the river. In its neighborhood was the great commissariat depot, where vast quantities of cattle were daily slaughtered, and where from the number of carts, oxen, and mules, hourly traversing the adjacent fields, the soil, intermixed with their food and ordure, and occasionally with damaged biscuit, was trodden down into a thick, tenacious, offensive compost, on which a burning sun acted almost constantly. In this hospital, gangrene had prevailed to a great extent.

Dr. John Bell[1] quotes Mr. George A. Walker, to the effect that he has frequently demonstrated that a single inspiration of the products of human putrefaction has in numerous individual and collective instances at once destroyed life; in others, giving rise to lingering consumption, typhus, scarlet fever, and other diseases, and again producing a permanently bad state of health.

Dr. Bell then refers to a case related by Mr. Chadwick, in which this gentleman, while walking with Prof. Owen, met a butcher who, in reply to some inquiries in regard to his health, stated the following particulars: This man had lived a long time in Bear Yard, near Clare Market, where he was exposed to two deleterious influences—shambles on one side, and a tripe-house on the other. His attention to his own impaired health under such circumstances was quickened, by observing that it was impossible for him to keep birds, of which he was extremely fond, in this place. "You may hang up a cage," said he, "in any window of the cow-houses round Bear Yard, and not a bird will live out the week." What most annoyed them among the congregation of odors was the vapor arising from the fat in the process of preparing the tripe. Some time before this, he had occupied a room in Portugal Street, overlooking a crowded church-yard, from which he often saw a dense vapor rise that had a very offensive odor. The butcher's birds died there in brief time, and the good man found that

[1] "Report on the Importance and Economy of Sanitary Measures to Cities." New York, p. 189.

he could only preserve new purchases by removing his quarters to Vere Street, beyond the range of deleterious emanations.

In April, 1773, in the church of St. Saturnin, at Saulieu, in France, two coffins being opened during an inhumation, an extremely offensive odor was disengaged, of so insupportable a character that the persons assisting in the ceremony were obliged to leave the building. Of one hundred and twenty young people of both sexes, who were being prepared for their first communion, one hundred and fourteen were taken dangerously ill, as were likewise the curate, the vicar, the grave-diggers, and more than seventy other persons.[1]

From a very early period in the history of the world, the belief has existed that the emanations from putrid animal substances were deleterious, and many instances of disease being traced directly to these vapors and gases have been adduced by the older authors.

Thus, Galen[2] ascribes one of the causes of malignant fevers to the stench arising from the dead bodies of men and animals left unburied on battle-fields.

Ambrose Paré[3] affirms that the air is poisoned by the fumes given off from the dead bodies left to putrefy after great conflicts.

Petrus Forestus[4] gives a very circumstantial account of a malignant fever which broke out at Egmont, in Holland, due, in his opinion, to the poisonous emanations from a whale which had been left on the shore, whereby the atmosphere for a great distance around was contaminated.

From these citations (and it would be easy to adduce many others) we see that the idea of the noxious character of the gases given off from putrefying animal substances is of great antiquity, and has held a firm hold over the human mind Very few persons, even among those regarded as well skilled in sanitary science, would at the present day venture to deny

[1] "Cours d'Hygiène fait à la Faculté de Médecine de Paris, par Louis Fleury." Paris, 1852, tome i., p. 228.

[2] "De Fibrium Differentiis," lib. i., cap. iv.

[3] The Works of Ambrose Paré, etc. London, 1691, p. 467.

[4] "Observationum et Curationum Medicinalium," lib. vi., observat. xxvi.

that the matters in question are prolific causes of disease. And yet, as we have already seen, there is no lack of evidence to show that the opposite doctrine is far from being untenable, and that the occupations which bring men in contact with the most disgusting animal substances, in all stages of the putrefactive process, are really healthy in their influence. We have only to call to mind the fact that butchers, tanners, soap-boilers, fat-melters, candle-makers, the sailors engaged in whaling-voyages, and persons employed in other occupations, which cause them to be exposed almost constantly to fetid odors from decomposing animal matters, generally enjoy good health, to convince ourselves that the outcry against the healthfulness of such emanations has been too indiscriminating and absolute. Instances are numerous of epidemics ravaging cities, and sparing those engaged in pursuits such as some of those specified.

In 1814, after the battle of Paris, four thousand dead horses remained on the soil during fifteen days, and were for that time subjected to a high temperature. Men were employed to gather them together in order that they might be burnt, but not one of those who undertook this digusting work suffered in health in the least degree.[1] During our own recent civil war many similar instances of dead men and animals remaining on the ground several days in a state of decomposition occurred, and yet in no case did the neighboring inhabitants suffer any deterioration of health. Like examples could readily be adduced from the late Franco-German War. Those of you who have attended dissecting rooms in which there were numerous bodies in all stages of decomposition, will probably not be able to recall a single instance in which any of the students or attendants contracted disease from inspiring the gases and vapors which abounded. Probably the emanations from bodies, dead of some highly-malignant disease, such as the plague, puerperal fever, or glanders, are more noxious than those which arise from animals which have died from accidents or non-putrid affections, but this even is a disputed point. Brayn, Howard, and others, who have carefully studied the plague, deny entirely that it causes any such result. And cer-

[1] Fleury, *op. cit.*, p. 227.

tainly the experience gained in the dissecting-room militates against the theory that any disease imparts such particularly deleterious qualities to the corpse that the emanations from it cannot be inhaled with safety; doubtless in this, as in most other disputed questions, the truth is to be found in the mean. There can be no doubt that, under certain conditions, not very clearly understood, though perhaps having their origin in the diseases with which mankind and the lower animals are affected, the effluvia from dead bodies are highly deleterious. Such instances, however, as that of Chambon and others which have been cited, are exceptional, and are not to be considered as of ordinary occurrence. It may also be regarded as true that the products of the decomposition of all animal matter are injurious to health when they are inhaled in a concentrated form. It is equally true that no apparent ill effects follow from exposure to such matters when diluted with a moderately large amount of atmospheric air. Indeed, Parent Duchatelet, Andral, Lallemand, Dubois, Dupuytren, Boyer, Desault, and many others in France, and Lawrence among others in England, have insisted that the dissecting-rooms attached to medical schools and hospitals, and which, until a comparatively late period, have not been remarkable for good ventilation, are by no means unhealthy.[1] A very excellent observer, Mr. Thackrah,[2] goes further, and asserts that, although sufficiently disgusting, the air of slaughter-houses, so far from being prejudicial to health, is frequently decidedly useful, especially to those predisposed to consumption. Still, although we may not believe in the noxiousness, under ordinary circumstances, of the effluvia from battle-fields, slaughter-houses, tanneries, glue-factories, and other places where animal matters are in warm weather undergoing putrefaction, there is no doubt that the odors from such places are disagreeable both to sick and well persons. It may happen too, that, from accidental causes, such emanations may be absolutely deleterious to health, and, as we can never determine before-

[1] "Cyclopædia of Practical Medicine," vol. i., article "Diseases of Artisans."

[2] "The Effects of the Principal Arts and Professions and of Civic States and Habits on Living, Health, and Longevity," etc. London, 1831.

hand how far they may or may not prove injurious, three is additional reason for avoiding them altogether.

With reference to the deleterious influences exerted upon health by the effluvia from graveyards and cemeteries, public opinion has for many years been very decided, and in this instance rightly so. Owing to the character of the soil, or to the confined air of tombs, decomposition goes on less rapidly and with a more or less admixture of its products with gases derived from the earth. As a consequence, there is a slow and almost imperceptible poisoning of the atmosphere; and, as there is little or no attendant bad odor, those living in the vicinity of burial-places have no warning of the noxious matter which they are constantly inhaling. In all large cities of the civilized world, except Philadelphia and perhaps one or two others, intra-mural interments are positively forbidden. It would be far better that all dead bodies should be burned, but it will require a long course of sanitary enlightenment to revive this practice among civilized nations.

Among the ancients it was at first customary to bury the dead near the houses occupied by the living. Experience, however, would seem to have demonstrated the evils of this procedure, for at a later period the Romans were prohibited from burning the corpses of the dead, or burying them within the limits of the city. This prohibition was continued until the end of the empire, and was enforced under Christian authority, until the idea was conceived that it was desirable to repose after death near the relics of martyrs, and under the altars in the churches. During the middle ages this practice was universally followed, and it was not until the year 1765 that a royal ordinance and a decree of the Parliament prescribed that for the future no interments should take place in France within the limits of any city.[1]

The whole subject of graveyards and intra-mural interments has been fully considered by Tardieu,[2] in his admirable monograph. It must be admitted that the fact of the deleterious character of the emanations from graves is well estab-

[1] "Traité Élémentaire d'Hygiène Privée et Publique," par A. Becquerel. Troisième édition, Paris, 1864, p. 231.

[2] Des Voiries et Cimetières, Thèse de Concours, Paris, 1852.

lished. Even the superposition of several feet of earth over the corpse is not sufficient to prevent the escape of gases capable of acting with great energy on human health. These gases are not entirely such as would be given off from a dead body fully exposed to the air, and there are other circumstances which modify their action. In passing through the soil they become mixed with substances of an animal, vegetable, and mineral character, and are evolved so slowly as to render their influence long continued. Among their exhalations are carbonic acid, carbonic oxide, carburetted hydrogen, sulphuretted hydrogen, phosphuretted hydrogen, and ammoniacal combinations.

Pellieux[1] has studied with much success the character and effects of the gases evolved from graveyards and tombs. According to this author, carbonic acid is the predominating substance, and he attributes much of the ill effects of these emanations to this compound. In this opinion he is, as will be shown hereafter, probably mistaken. It would seem more reasonable to regard the sulphuretted and phosphuretted hydrogen as the preëminently noxious agents.

It is perhaps well to impress upon the public the fact that hygienists, however much they may differ in regard to the injurious influence exerted by decomposing animal matters exposed to the open air, are of full accord relative to the highly-deleterious nature of the compounds evolved from graveyards and tombs. As is well remarked by Montfalcon and De Poliniere:[2] "It is thoroughly demonstrated that inhumations in cities seriously affect the public health; that the miasms disengaged from sepulchres may cause, as they have often caused, terrible catastrophes; and that not only do they increase the malignancy of existing diseases, but they give rise to contagious affections, the ravages of which are often frightful."

The gases which escape from tombs built of masonry would appear to be especially noxious. Here the products of decomposition have no opportunity for escaping, except when the tomb is opened for the purpose of receiving another corpse.

[1] "Observations sur les Gaz Méphitiques des Caveaux Mortuaires des bimetières de Paris," Ann. d'Hyg., 1e série, tome xli., p. 127, 1849.

[2] "Traité de la Salubrité dans les Grandes Villes." Paris, 1846, p. 213.

As a consequence, they become highly concentrated, and act with tremendous potency.

Londe[1] regards them as more dangerous than any other emanation whatever. He refers to a case which occurred at Dijon, in 1713, in which the accidental breaking of a coffin, which had been buried for six weeks, caused very severe disorders in one hundred and fourteen persons out of one hundred and twenty who were present at the opening of the tomb, and from which eighteen died.

Many other instances of the most deplorable effects following the opening of graves are on record. Even when the bodies have been for a long time underground, and when the cemetery is no longer used, it is dangerous to break up the soil or to erect buildings upon it. Of course, such places should not be used as sites for residences. They are not safe, although an interment therein may not have taken place for many years. Vicq d'Azyr[2] has stated that at Rion, in Auvergne, the ground of an old cemetery was broken up for the purpose of improving the city. A short time subsequently, an epidemic disease broke out, by which a great many persons, particularly from among the lower orders, perished. The mortality was especially severe in the immediate vicinity of the cemetery. Six years previously, a similar event had caused an epidemic in Ambert, a small town of the same province.

Tardieu[3] also cites an instance in which, at Paris, a number of shops were built on a situation where once the convent of the Filles de Sainte-Geneviève was placed. All those persons who lived on the first floors, especially the younger ones, were attacked with diseases which were attributed with correctness to the emanations from the dead bodies buried in the ground on which the buildings were erected.

And yet, notwithstanding the fact that danger almost invariably attends upon subjection to the emanations from graves and tombs, there are occasional instances of persons being exposed to these matters in a most concentrated and long-con-

[1] "Nouveaux Éléments d'Hygiène." Paris, 1847, p. 454.

"Dictionnaire d'Hygiène Publique et de Salubrité," etc., par Ambroise Tardieu, Paris, 1854, tome ii., p. 298, article "Inhumations."

Op. et loc. cit.

tinued form without any ill consequences following. The Church of the Capucins, at Rome, affords a notable example of the truth of this assertion. For several hundred years all the monks belonging to the monastery attached to that church, who have died, have been buried in a small cellar under the building. After the soft parts have become decomposed, the skeletons are taken up and placed in their monastic garments around the walls of the room. Subsequently, when the joints likewise have yielded to the inroads of time, the bones are arranged on the walls and ceiling in the form of rosettes, arabesques, and other fanciful figures. There are always ten or twelves bodies in the ground, and as many skeletons standing around the room. The whole apartment is a charnel-house made up almost entirely of the *débris* of human bodies in all stages of decomposition; and yet there is no instance of any sickness having occurred, among the thousands of persons who annually visit it, which could in the slightest degree be attributed to any influence exerted by the locality, and the monks who live over it and who visit it constantly are remarkable for their sleek and rotund appearance. Indeed, the good-natured friar, who has acted as the janitor and exhibitor of this strange chamber for several years, is a wonderful specimen of robust health. To be sure, the earth—which has long since been replaced by the *débris* of the dead—was originally brought from Jerusalem, and may, therefore, in the opinion of some persons, be supposed to possess peculiar disinfectant qualities.

In regard to the whole subject of putrid emanations, I will repeat that, in the present state of our knowledge, it is desirable not to locate habitations in situations where they can, in the least degree, reach the inmates, and that old camping-grounds, graveyards, cemeteries, and tombs, are to be especially avoided. But at the same time it must not be forgotten that public opinion and prejudice have exaggerated the ill effects which are liable to occur from exposure to the products of the decomposition of animal substances in the open air. The *résumé* given by Fleury[1] is so thoroughly in accordance with my own views on this matter that I quote it entire:

[1] "Cours d'Hygiène fait à la Faculté de Médecine de Paris," t. premier, Paris, 1852–'53, p. 229.

1. "When the putrefaction of animal substances goes on in a confined atmosphere, it sometimes gives origin to non-respirable or deleterious gases which cause either asphyxia or a more or less grave toxic effect.

2. "In exceptional circumstances, as yet imperfectly understood, animal bodies, in process of decomposition in the open air, disengage deleterious gases, probably formed by sulphuretted and phosphuretted hydrogen. This evolution especially takes place during the first stage of the putrefaction of the abdomen of animals.

3. "The disengagement of these deleterious gases constitutes a true mephitism, capable of causing serious diseases, or even instant death, such as that produced by sewers or privies. But this accidental mephitism does not necessarily imply the general and absolute injurious character of putrid emanations.

4. "In view of the immense centres of putrefaction which exhale their putrid vapors and gases over large populations without increasing the mortality, and without causing the development of diseases of miasmatic origin, we cannot avoid the conclusion that in general the emanations from putrefying animal matters are not deleterious, and that perhaps they are capable of exercising a favorable and prophylactic effect.

5. "Examples have been cited of diseases being more or less manifestly produced by putrid emanations. But the number of these instances is relatively extremely small. They do not invalidate the general rule, and are attributable to individual circumstances and particular predispositions.

6. "As M. Londe correctly remarks, a strong constitution of the individual, a good state of health, physical exercise, nutritious food, and habitual exposure to putrid emanations, diminish the aptitude to be injuriously affected by them, while the contrary circumstances augment this disposition."

In regard to the statements made in this last paragraph, I may state that they are applicable to all contagious or infectious diseases whatsoever.

Now, what is the nature of the gases given off by animal substances in a state of decomposition?

Analysis shows that they in part owe their deleterious properties to the presence of carbonic-acid gas, carburetted hydrogen, ammoniacal compounds, phosphuretted hydrogen, and sulphuretted hydrogen. These are all, especially the two latter, exceedingly poisonous when inhaled. Carbonic acid, however, is not a poison by reason of any action on the blood. When it is present in the atmosphere in large amount, the glottis spasmodically closes, and death ensues from asphyxia. When in small proportion, it is still not taken into the system, but it interferes, to the extent of its presence, with the absorption of oxygen, and the elimination by respiration of the carbon of the blood. Essentially, therefore, its action is that of a ligature to the trachea. Thus I put this mouse into a jar of pure carbonic-acid gas, and you observe that the animal dies in a few seconds. This other I place in an atmosphere containing forty per cent. of carbonic acid, and death takes place after a longer interval, but still in essentially the same manner as in the first instance. If, however, as in the next experiment, I remove the animal before death takes place, and expose it to a free current of atmospheric air, recovery follows very promptly. It is hence apparent that carbonic-acid gas is not a directly poisonous substance, but that it simply interferes with the performance of processes which are essential to life. When its influence is removed, provided the animal is not *in articulo mortis*, respiration goes on again as in health; just as, when a man hangs himself, and is cut down before life is extinct, recovery can generally be effected.

Under another head, however, I will return to the fuller consideration of the hygienic relations of carbonic acid.

Phosphuretted hydrogen—a form of phosphide of hydrogen—is given off from all animal bodies, and especially fish; phosphorus abounding in these animals. It is the element which renders luminous the emanations from decomposing substances, and is frequently seen in the form of masses of luminous vapor over graveyards and other places where phosphorized bodies are undergoing decomposition. It is exceedingly poisonous, and acts with great energy on the living animal body when absorbed into the blood through respiration.

Sulphuretted hydrogen is still more poisonous, and is a constant emanation from decomposing animal matter. Du puytren found that $\frac{1}{800}$ part of this gas in the atmosphere was sufficient to kill birds in a few seconds. In my own experiments I found that small animals died after a few minutes when the $\frac{1}{1000}$ part of sulphuretted hydrogen was present.

As you see now, I place this mouse under a large bell-glass, and introduce a very small amount of sulphuretted hydrogen. The animal dies quietly, after an exposure of less than five minutes.

This was probably the chief agent in causing death in the cases I have referred to as occurring in church-yards and dissecting-rooms.

The ammoniacal compounds and carburetted hydrogen are also highly deleterious, and when inhaled in concentrated forms are speedily fatal.

But, besides these, there is reason to suppose that there are present, in the atmosphere contaminated with animal emanations, certain living organisms. As these, in different but analogous forms, are given off from vegetable bodies in a state of decomposition, I reserve their consideration for another lecture.

Art. IV.—*Note relative to the Monobromide of Camphor.* By William A. Hammond, M. D.

The monobromide of camphor consists of one equivalent of camphor united with one of bromine ($C_{10},H_{16}O,Br$). It is a white crystalline solid, having the odor of camphor, and to a slight extent that of bromine. It decomposes readily when exposed to the atmosphere, to a heat of 100° Fahr., or to the action of ammonia.

Several months since, a statement was made in *The Doctor* to the effect that a Belgian physician had for more than ten years past made use of the monobromide of camphor in delirium tremens and analogous nervous diseases. Desiring to test its value in such affections, I requested Dr. Neergaard to obtain a quantity of the preparation for my experiments. Prof. Maisch, of the Philadelphia College of Pharmacy, very kindly undertook to manufacture it, and, over-coming the great difficulties of the process, succeeded in obtaining it in beautiful crystals free from the slightest yellow tinge.

My experience with the monobromide of camphor, though thus far limited, is eminently satisfactory. I have employed it in two cases of infantile convulsions due to the irritation of teething, with the effect in each instance of preventing the further occurrence of paroxysms which, previously to its administration, had been very frequent. In each case a grain was given every hour, rubbed up with a little mucilage of acacia. Three doses were sufficient in one, and two in the other case. The children were aged respectively fifteen and eighteen months.

In a very obstinate case of hysteria occurring in a young married lady, in the form of paroxysms of weeping and laughing, alternating with epileptiform and choreiform convulsions, I gave the monobromide of camphor in doses of four grains every hour. The influence was distinctly perceived after two doses were taken, but ten were necessary to entirely break up the attack. This was a very favorable result, as all previous seizures had lasted for from five to eleven days, uninfluenced by medication or moral suasion.

I have also employed it with excellent effect in several cases of headache occurring in women and young girls, and due to mental excitement and excessive study. One dose of four grains was generally sufficient to cut short the attack. In two cases, three doses at intervals of half an hour were necessary.

In wakefulness, the result as it so generally is of cerebral hyperæmia, the monobromide of camphor appears to be greatly inferior to the bromide of calcium or even the other bromides. But it is apparently indicated in delirium tremens. I have not yet had the opportunity of trying it in this disease, but I should not hesitate in a case of the affection to administer it in doses of five grains every hour or half-hour, with the confident expectation that sedation and sleep would result.

The monobromide of camphor may be given in the form of pill, with conserve of roses as the excipient, or as a mixture with mucilage of gum-arabic and syrup. The dose for adults ranges from two to five grains.

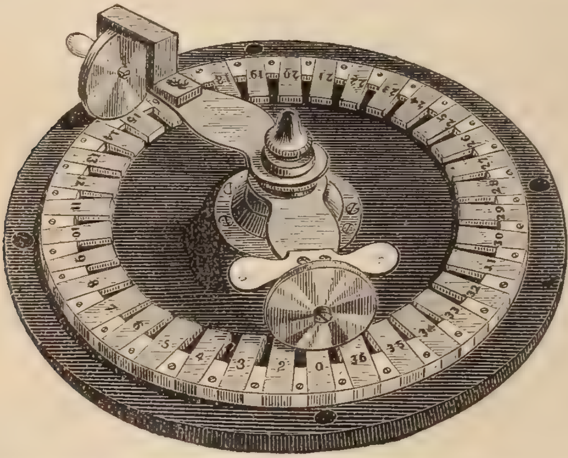
ART. V.—*Kidder's Circular Current-Selector for Galvanic Batteries.* By GEORGE M. BEARD, M. D.

THOSE who use the galvanic current in the treatment of medical or surgical cases feel the need of some arrangement by which the current can readily be *increased or decreased without interrupting it.*

In the treatment of sensitive patients, whatever may be the special affection from which they suffer, a frequent interruption of the current is disagreeable, painful, and sometimes, in cases of recent cerebral hæmorrhage, dangerous.

In the treatment of certain irritable conditions, neuralgic and spasmodic diseases, and in careful electro-physiological researches, the good effects of the applications may be neutralized, if not destroyed, by suddenly and violently interrupting the current, or by putting on many cells at once.

The circular current-selector, represented in the cut, is the most convenient of the various arrangements that have been devised to secure perfect and ready control over the galvanic current.



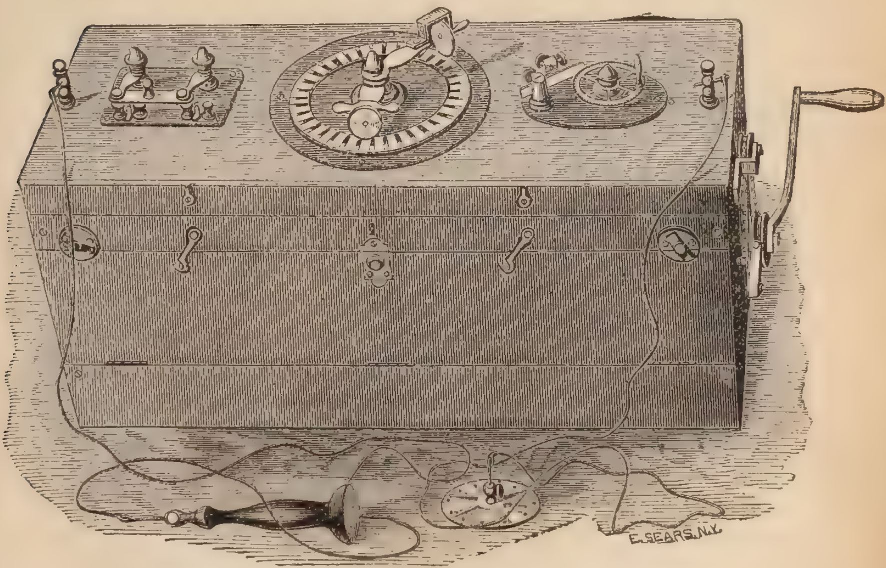
This arrangement consists of a circle with a hard rubber base, on which are small brass plates, of about one inch in length, connected with the different cells of the battery. These brass plates are numbered 1, 2, 3, 4, 5, 6, etc., corresponding to the number of the cells. In their outer portion these plates are separated from each other by pieces of ivory.

Two arms provided with wheels are connected with the circle, revolving on a pivot in the centre. One of these is long, the other short.

If the wheel of the *short* arm be placed on the plate marked 0, the current may be increased, a cell at a time, and *interrupting* it, as each cell is added, by turning the wheel of the *long* arm over 2, 3, 4, 5, 6, etc.

If the wheel of the *long* arm be placed on the plate marked 0, the current may be *increased without interrupting* it, by steadily turning the wheel of the short arm with firm pressure over plates marked 2, 3, 4, 5, 6, etc.

By this very simple contrivance it is possible for the operator sitting or standing by his patient, holding an electrode in one hand, with the other hand to slowly increase or diminish the strength of the current, as the varying sensations of the patient require.



The satisfaction that this arrangement will give, will depend on the perfectness of the workmanship. If the connections are completely and firmly made, and all the parts are properly adjusted, it is an ideal arrangement absolutely meeting the requirements of the electro-therapeutist.

It may be connected with any form of galvanic battery. Kidder connects it with a zinc-carbon battery of thirty-six cells. It is placed in the battery-cover, which is also provided with a current-reverser at one end, and a tooth-wheel current-interrupter at the other.

The arrangement might just as well be connected by wires with any number of cells in the cellar, or in a closet.

The size thus far constructed has been adapted only for office or hospital batteries. There is no reason why our portable galvanic batteries might not be provided with a similar arrangement of smaller size.

We have used a thirty-six cell zinc-carbon battery, provided with this arrangement, for six months, and nothing that we have ever used has been so convenient. By my suggestion, these batteries were last summer introduced into the army hospitals, where they have thus far given excellent satisfaction.

Clinical Records from Private and Hospital Practice.

1.—*Report of Cases treated in the Brooklyn City Hospital.* Reported by J. H. RAYMOND, M. D., late House-Surgeon.

Fracture of the Base of the Skull; Paralysis of the External Rectus; Recovery. Service of Dr. S. FLEET SPEIR.—J. L., aged forty years, a truck-driver by occupation, was admitted to the hospital November 30, 1871. He was thrown from his truck and fell a distance of four feet, striking upon the left side of his head. On admission, he appeared to be suffering from compression of the brain; there was bleeding from both ears and nose, and also a scalp-wound over the left parietal region—which, however, gave no trouble, healing by first intention. The following morning he was conscious; his speech was indistinct; his left side was paralyzed; the power of motion over the limbs being just appreciable, while the facial paralysis was complete. It was discovered for the first time that there was a discharge of a serous fluid from the left ear, which continued for several days; the left eye was turned in toward the nose, and could not be directed outward; the patient complained of "seeing double." He gradually regained the use of his limbs, and of the muscles of his face, though the improvement in the facial paralysis was less rapid. By January 1st he was able to leave his bed, complaining only of severe headache on the left side at night, of deafness in the left ear, and double vision. His headaches disappeared after a few weeks, while his deafness remained up to the time of his discharge. With the return of the power of motion in limbs and face, no improvement in his external rectus took place. About this time attention was attracted to an article in the London *Practitioner*, for the month of January, on the use of the Calabar-bean. On the 19th of January a solution of the alcoholic extract was ordered, of one grain to the ounce of water; one or two drops were daily dropped into the eye, and at the expiration of a week the gain of power over the rectus was appreciable, both to patient and physician. The only other effect was to contract the pupil. At the time of the patient's discharge the improvement was very marked, he being able to carry the centre of the pupil three lines to the left of the median line. After the drops had been in use about one week, he discovered that, by closing the nose and mouth, and blowing, he could cause the air to pass out through the puncta of both eyes. He was discharged from the hospital February 19, 1872, able to resume his occupation. This open condition of the lachrymal canal did not appear till the Calabar-bean had been in use one week, and the question naturally was asked whether it was a result or a coincidence only. Forty-three persons were examined for the purpose of ascertaining the condition of the lachrymal canal when no Calabar-bean had been used, and it was found that of this number six could blow air through the puncta when the mouth and nose were closed, though but one seemed to have the passage as open as the patient.

Fracture of Pelvis; Rupture of Urethra; Puncture of Bladder through Rectum. Service of Dr. J. C. Hutchison.—H. D., aged twenty-eight, laborer, was admitted October 23, 1871. In attempting to escape from a falling derrick, he jumped from an elevated platform to a platform beneath, a distance of thirty-five feet. On admission he was suffering from shock; there was a fracture of both bones of the right forearm. While being undressed he complained of great pain over the right hip; the least movement of the leg caused him to cry out. Measurement detected a shortening of about one-eighth of an inch; the trochanter rotated with the femur; about a teaspoonful of blood came from the penis. During the whole day he was in great agony, and seemed unable to remain quiet in any one position, tossing about the bed continually. In the evening, no urine having been passed during the day, examination was made of the abdomen, and a tumor was found, reaching nearly to the umbilicus, painful on pressure, perfectly flat on percussion, and bounded above and on the sides by tympanitic resonance; the patient expressed great desire to pass his water. A catheter was passed naturally enough till it reached a point about one inch in front f the prostate, when the point of the instrument was turned o one side, and evidently passed out of the canal; upon withdrawing it a few drops of blood came away, but no urine; many attempts were made, with various sizes and forms of instrument, but all to no purpose. Twice a few drops of urine were discharged from the urethra between its walls and the catheter. The ecchymosis which now appeared in the perinæum rendered it beyond doubt that the urethra was ruptured, and it was evident that other means must be adopted to relieve the distended bladder. Accordingly, a curved trocar and canula were introduced into the rectum, and the bladder punctured from behind, but no urine came away—why, was inexplicable—and puncture over the pubes suggested itself, but was not performed, as the man was at the time in collapse, and died within thirty-six hours from the time of admission.

A partial autopsy only was allowed. On opening the abdomen, the pelvic cavity was found filled with fluid and coagulated blood; there was diffuse peritonitis; the bladder was firmly contracted and contained two drachms of urine, not at all tinged with blood; the puncture made by the trocar was seen between the openings of the ureters; the urethra was torn across one inch in front of the prostate. The pelvic bones were very much comminuted. A small spicula of bone projected at the point where the laceration of the urethra took place.

The case presents many points of interest, and illustrates very forcibly the fact that blood effused into the peritoneal cavity can simulate perfectly a distended bladder.

Compound, Comminuted Fracture of the Skull; Laceration of the Brain and Meninges; Recovery. Service of Dr. S. FLEET SPEIR.—P. F., aged sixteen years, factory-boy, was admitted June 17, 1871. While lowering a sewing-machine from the second story, the rope gave way, and the machine fell to the bottom floor, closely followed by the patient, who struck his head against the corner of the stand. He was found to have a compound, comminuted fracture of the skull in the left temporal region; the membranes were lacerated, and a small portion of the brain which protruded from the wound was removed. A small piece of bone which was depressed was raised and removed. From the time of his admission till the day of his discharge, August 5th, he had not a bad symptom.

Compound Fracture of Radius and Ulna; Wound sealed by Carbolized Collodion. Service of Dr. S. FLEET SPEIR.—J. F., aged fifteen years, a laborer by occupation, was received into the hospital on the 25th of November, 1871, suffering from a compound fracture of both bones of the left forearm at a point about two inches from the end of the olecranon. The upper fragment of the ulna was protruding through the wound, which was on the anterior surface of the forearm. The fracture was reduced, and the wound closed by a piece of surgical lint thoroughly soaked in carbolized collodion; the forearm was put on a straight splint. After three weeks the sealing was removed, and the wound found entirely healed; there was excellent union. On the 11th of January the patient was discharged with a good arm, the case having pursued the course of a simple fracture.

II.—*Electro-therapeutics, Electrolysis, and Electro-chemical Action on Cell-tissues. With a Case of Scirrhus of the Breast.* Read by John J. Caldwell, M. D., before the Electro-Therapeutical Society of New York.

September 25, 1871.—Mrs. J. N. B. called at my office, for treatment of a tumor of the right breast, which her physician (Dr. Thompson, of Islip, L. I.), and several other medical gentlemen of that neighborhood, had pronounced cancerous. From all its clinical bearings I fully concurred in their opinion, and commenced treatment by application of a constant current of electricity from eight to ten cells, duration from ten to fifteen minutes.

Mrs. B. informed me that her attention was drawn to her trouble in April last; first noticed a small, hard growth, about the size of a walnut, which became larger and more painful, until it reached its present size, occupying the base of the gland, and involving the nipple; surrounding tissues were hard and irregular, skin tense and purple, with two or three ulcerating points discharging the ichorous matter pathognomonic of carcinoma. In this case I could trace no hereditary taint.

September 27th.—Continued the galvanic treatment, and ordered her a wash of carbolic acid and glycerine as a disinfectant.

September 29th, October 1st, 3d, 7th, 12th, 14th, 16th, 18th, 22d, 27th, and *30th.*—Continued treatment.

October 5th.—Consulted Drs. Beard and Gregory. Dr. Gregory takes a specimen for microscopic examination.

October 18th.—Two or three small nodes are all that is left of the tumor.

Last evening her case, as well as one of Dr. Beard's, was presented to Kings County Medical Society, when several of the medical gentlemen concurred with me as to its being truly scirrhus, judging alone from its clinical appearance, which Prof. Krackowitzer has declared the most reliable.

October 30th.—Consulted Drs. Beard and Fleet Speir. A specimen taken by Dr. Speir, with a trocar, for examination.

(Copy of Dr. Speir's letter in reference to the specimen, November 1, 1871.)

MY DEAR DOCTOR: In the specimen removed I can only find evidence of condensed connective tissue. There is no indication of cancer so far as this specimen goes. Very truly yours, etc.,

(Signed) S. FLEET SPEIR.

November 1, 1871.—Mrs. B. discharged cured.

(Copy of Dr. Gregory's letter.)

DEAR DOCTOR: The specimen you sent me for examination I find to contain cancer-cells. Yours, etc.,

(Signed) DR. J. E. GREGORY.

The following is from a letter of Dr. Thompson, of Islip, L. I.: "I omitted to say that the improvement in Mrs. B.'s case has been very satisfactory. Have advised her to continue your treatment six weeks to two months longer." Also the annexed from Dr. W. S. Preston, Patchogue, L. I.: "Mrs. B. has given you more of a history of her tumor than I can. I consider it truly scirrhus."

The argument that, because cancer is a constitutional taint, there is no use in treating it locally, now belongs to the light of other days. Who for one moment would hesitate to treat chancre locally, because the patient might have syphilitic taint? Again, who knows but in cancer, as well as in syphilis, the local manifestations are primary, and the constitutional symptoms secondary? The ichorous cancer-matter may be absorbed from the tumor, thus poisoning the system; so far as we know at present, it is the "oak and the acorn, or the acorn and the oak."

In my opinion, the whole secret of treatment consists in the electrolytic power, or influence over a drop of water, the oxygen being given off at the positive pole, and the hydrogen at the negative. In applications to tumor I use electrodes with sponge-tips, saturated with a strong solution of chloride of sodium. This may have some remedial agency.

Commencing from the border of the tumor, and gradually approaching the more solid particles, suppuration sets in and destroys the growth.

After a dissolution in the manner above mentioned, there must be a deposit and coagulum. These, being foreign matters, must cause suppuration or absorption. Thus the tumor is destroyed.

To prove this, place a piece of beef in a solution of chloride of sodium at a temperature of 100° to 110°; this fluid must be maintained at the above temperature, in a sand-bath, or in any other convenient manner, in order to make the decomposition more satisfactory and rapid; then, by bringing this tissue in contact with the positive and negative poles (platinum wire) of a constant-current battery of some eight or ten pairs, the process of electrolysis is vividly and beautifully demonstrated.

Histologically speaking, the oxygen and hydrogen of these cells being divorced, the albuminous elements, alkaline and earthy salts, fatty matter, etc., are dissolved or precipitated.

Physically, the process is highly interesting. From the positive pole will be seen flakes of coagulum, with a florid-red stain of hematine, or hydrochlorate of cruentine; while, at the negative pole, hydrogen bubbles away, with a snow-white fleecy cloud, making an ebullition, like boiling water, quite audible to the by-stander.

Chemically, the chloride of sodium is robbed of its chlorine, part of which is set free, while other portions of this gas unite with oxygen, forming $Cl\frac{O}{5}$ chloric acid, while at the negative pole we have still another portion uniting with hydrogen forming HCL, or hydrochloric acid. This action, together with the thermic influence, much resembles the digestive forces of the stomach. The electricity represents the vital forces, the chloric and hydrochloric acids the digestive fluids, the artificial the animal heat.

We here present the beef fluidized, after thirty-six hours of electro-chemical action, as above described; you will see it presents the appearance of a fresh sample of the fluid extract; no doubt, this will some day be one of the modes of manufacturing this, and perhaps other extracts, as well as of cooking our food in summer-time, saving much heat and annoyance. The instrument used on this occasion was a portable galvanic battery, manufactured by the Galvano-Faradic Manufacturing

Company of New York. It is a very reliable instrument; the thirty-cell battery is most powerful, and far more continuous than Smee's sixty-cell. It is in many respects superior to Stohrer's.

The hydrostat is also a valuable addition. This ingenious contrivance prevents an overflow of the battery-fluid, when the instrument is carried from place to place.

Mrs. McG., of Glen Cove, L. I., aged fifty-three, called at the office, and stated that, when a child, she noticed a little hard, flat point, about the size of a nickel cent, which remained the same until about three years since, when it began rapidly to increase in size, without pain. Drs. Wright and Aughtmuller informed her that it was a cancer-tumor, and advised her to go to the city for its excision or removal.

Now it presents a most formidable appearance, the whole gland being involved, with a scirrhous attachment to the rib, and the same deposit along the intercostal spaces, also infiltrating the axillary glands, the respiratory movement of both sides being free. Upon the base of the gland there is a denuded surface, circular in form, of two and a half inches in diameter, from which (during the last sixteen months) she has suffered, at intervals of several days, quite free hæmorrhage. She has never suffered the slightest pain; the appearance of the latter part of the tumor looks to be much like fungus hæmatodes. Her general health is good, no hereditary taint; though she informs me that her sister has the same characteristic painless tumor in the womb, and that the doctors have failed to relieve her. To-day commenced electrolytic treatment with the Galvano-Faradic Manufacturing Company's battery. Application of eight cells, sponge-tip electrodes, saturated with chloride of sodium.

After three applications, she discontinued treatment.

The two cases here presented represent the extremes of this malady: the first the milder one, when the trouble is local; and the second, the last stage, wherein the constitutional taint is manifested plainly throughout, and where but little hope can be held out, save by applying the constant current to modify the severe pain, and by anodynes to smooth the path to the grave.

In the early stages, electrolysis offers the only safe and true mode of removal.

I may say, in conclusion, that the specimen of what I have called hydrochlorate of cruentine was pronounced to be such by Dr. S. Waterman, after examination with the micro-spectroscope.

III.—*Birth of a Child without Limbs.* By L. Millspaugh, M. D., Richmond, Staten Island.

On the 1st of March last, Mrs. H., the wife of a mechanic in comfortable circumstances, aged about thirty-five, in her fifth pregnancy, was delivered of a still-born female child, which presented the peculiarity of a well-developed head and trunk entirely without limbs. The clavicles and scapulæ were perfect, but no vestige of a humerus; also the hip-bones were entire, but no femur.

The labor was protracted, owing to feeble expulsive pains, and the delivery was accomplished chiefly by artificial means. The presentation was by breech, the position being left occipito-posterior.

The child was alive until a short time before its birth, the cause of its death being compression of the umbilical cord as the fœtal abdomen came within the constriction of the superior pelvic strait. Judging from the mother's reckoning, as well as from the imperfectly-developed cranial bones, it is probable that it had not passed the eighth month of fœtal life.

In this case, conception occurred at a time when the mother was grieving deeply over the loss of a little girl a few months before. Could that circumstance be regarded as in any degree productive of the deformity, or was it simply a *lusus naturæ?*

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, March 25, 1872.

DR. ABRAHAM JACOBI, President, in the chair.

THE following physicians, recommended by the Comitia Minora, were elected to membership: James Clark Thomas, Colin Mackenzie, George M. Schweig, Henry Raphael, Francis Simrock, and Peter B. Wyckoff.

DR. C. S. BULL, from the Committee on Intelligence, read a report on the Progress of Otology.

DR. L. D. BULKLEY, from the same committee, read a report on the Progress of Dermatology and Syphilography.

DR. GOODWILLIE read the report of the Committee on Meteorology.

VERATRUM.

DR. E. PEUGNET read portions of a long and elaborate paper, entitled "A Contribution to the Knowledge of the Chemistry, and of the Physiological, Therapeutical, and Toxical Actions of Veratrum Album, Veratrum Viride, and their Alkaloids," which appears in full in the current number of the *Medical Record.* Besides reviewing the work of previous investigators, the speaker detailed upward of fifty experiments of his own, and ended with the following summary of conclusions drawn from them:

"1. They confirm the views of Richardson and Scattergood as to the identity of the veratroida in both [veratrum album and veratrum viride].

"2. They confirm the views of Bullock as to the veratroida's being distinct from the veratria (sabadilla).

"3. They confirm the existence of Simon's jervina in veratrum album, and of Bullock's viridia in veratrum viride, and further tend to establish the chemical and physiological identity of both.

"4. They establish that Simon was probably mistaken as to the existence of baratyna in veratrum album; for in exhausting the root a large amount of lime is extracted by the acetic or phosphoric acids, and held in solution in the form of acetate or phosphate, and the addition of sulphuric acid will precipitate it in the form of sulphate, which is insoluble; in case the sulphates are used to precipitate the lime, the acetates or phosphates of ammonia, magnesia, potassa, or soda, are formed and held in solution.

"5. The veratroida is the active sedative principle of veratrum viride, and Bullock and Wood were mistaken in asserting that it was the viridia.

"6. The combination of the two alkaloids as they exist in their natural state or artificially combined, is the most reliable and decided method of obtaining the therapeutical virtue of either veratrum album or viride, as shown by the experiments of Scattergood, Percy, and Oulmont, with the resinoid of veratrum viride, also Bullock's and Wood's with the combined alkaloids, and confirmed by my own.

"7. The essential and characteristic difference between the varieties of veratum resides in the resinoid of veratrum album, and the characteristic action of that plant on the alimentary canal is due to it.

"8. In order to obtain the most active root, great care should be taken in selecting it; the time of gathering it is essential in both, for in extracting the alkaloids from two pounds of powdered veratrum album, purchased from a German importer, I obtained but two grains of veratroida, not a trace of jervina, an abundance of earthy salts, and a resin almost inert. I did not find two specimens out of five to yield the same result; this accounts for the discrepancy between the views of such high authorities as Pereira, Christison, and others, and mine, as to the constant effect of veratrum album; for I maintain that, if it has been collected at the proper season, and has not deteriorated by keeping, and if it is either given in substance, or its active principles are administered, it will invariably cause vomiting, purging, great prostration, and gastro-intestinal hyperæmia; in many cases gastro-enteritis will ensue as a secondary result. These effects, of course, vary with the amount taken.

"In case the tincture of veratrum viride or its fluid extract is preferred, the rootlets should be separated, as they probably contain less of the alkaloids. As the tincture or fluid extract of veratrum album would contain the veratrine or resinoid, the obnoxious principle, it would be necessary to extract the alkaloids and make use of them.

"9. In cases where alarming symptoms manifest themselves, the basis of the treatment should consist of stimulants, rubefacients, and opiates. In poisoning by veratrum album, the gastro-intestinal hyperæmia should be early overcome by the tincture of cantharides, as suggested by Percy in reference to the dilatation of the capillaries, by general and local electrization, and by the early use of warm enemas to wash away the effused blood and mucus.

"10. The presence of the alkaloids can be detected in the urine.

"11. The ethereal extract of the resin of veratrum album has a remarkable anæsthetic power."

The exhaustive character of the paper gave little opportunity for discussion. DR. PERCY, to whose researches the writer had often referred, expressed his high admiration of the investigations reported, and would have remarked further upon a few points, had not an extreme hoarseness prevented. DR. J. C. PETERS exhibited drawings of the different species of veratrum, and spoke briefly of some therapeutic uses of veratria.

Nominations were made for delegates to the American Medical Association, and the President and Secretary were authorized to fill any remaining vacancies in the delegation.

Adjourned.

Poisoning by Antimony.—A woman, the wife of a puddler at Bilston, has just been committed for trial at the next Staffordshire assizes, on the charge of having poisoned three of her children with antimony. What makes this case of singular importance is the fact that death took place in each instance without the cause being ascertained, the inquest and *post mortem* of one leading to the exhumation and analytical examination of the abdominal contents of the others, with the result of finding sufficient antimony to account for death.—*Lancet.*

Bibliographical and Literary Notes.

Art. I.—*Cancerous and other Intra-thoracic Growths; their Natural History and Diagnosis.* By James Risdon Bennett, M. D. London: J. & A. Churchill.

This little volume contains a concise history of intra-thoracic cancer, with cases illustrating its various modes of development. The subject is divided into three parts: 1. Disseminated cancer; 2. Localized or circumscribed cancer; 3. Cancerous and other tumors of the mediastinum. In the diagnosis of disseminated cancer our author attaches great importance to the character of the dyspnœa. If it is paroxysmal, and unattended by the ordinary physical signs of obstructed bronchial tubes, he considers it almost positive evidence of cancer. Stokes also speaks of the same symptom, but does not attach a like importance to it.

An examination of the clinical cases shows that the cancerous deposit is not confined to the lungs, but is found also in other organs. Nor is there any reason for supposing that the disease manifested itself primarily on the lungs. Dr. Bennett believes that the disseminated variety of lung-cancer does not ulcerate or produce any morbid change in the lung-tissue. This view is not sustained by other observers. Cancer in all its forms is an irritant, and as such usually excites inflammatory action in its neighborhood to a greater or less extent. Degenerative changes likewise ensue from the mere pressure of the deposit. In those instances where morbid changes in the intervening tissues are absent, the cancerous formations are of such extent, and involve so many different organs, that life is destroyed before these peculiar changes can occur.

The clinical examples throughout the volume are very carefully analyzed, and the practical deductions in diagnosis and treatment are given suitable prominence. Recognized pathological features are grouped in an intelligible form, without any attempt at useless theorizing. The author has done a good work in thus throwing more light on the obscure subject of cancer, and has made a very readable book, valuable alike to the specialist and general practitioner.

Books and Pamphlets Received.—Practical Lessons on the Nature and Treatment of [the Contagious Diseases; an Account of the Primary Syphilitic Poison, and of its Communicability. With an Appendix on the Recent Report of the Royal Commission on the Contagious Diseases Act, and its Application to the Voluntary Hospital System. By John Morgan, M. D., F. R. C. S., Surgeon to Mercer's Hospital, and to the Westmoreland Lock Hospital, Dublin; Professor of Surgical and Descriptive Anatomy, Royal College of Surgeons, Ireland, etc., etc. Sixty-seven Illustrations, plain and colored. London: Baillière, Tindall & Cox, 1872.

A Treatise on Diseases of the Bones. By Thomas M. Markoe, M. D., Professor of Surgery in the College of Physicians and Surgeons, Surgeon of the New York Hospital, Surgeon of Bellevue Hospital, Surgeon of the Roosevelt Hospital, Consulting Surgeon of the Mount Sinai Hospital, of the Strangers' Hospital, of the State Woman's Hospital, and of the Nursery and Child's Hospital, etc., etc. New York: D. Appleton & Co., 1872.

A Practical Treatise on the Diseases of Women. By T. Gaillard Thomas, M. D., Professor of Obstetrics and Diseases of Women and Children, in the College of Physicians and Surgeons, New York, etc., etc. Third edition, enlarged and thoroughly revised. With two hundred and forty-six Illustrations on Wood. Philadelphia: Henry C. Lea, 1872.

The Urine and its Derangements, with the Application of Physiological Chemistry to the Diagnosis and Treatment of Constitutional as well as Local Disease. By George Harley, M. D., F. R. S., late Professor in the University College, London, etc., etc. With Illustrations. Philadelphia: Lindsay & Blakiston, 1872.

History of Medicine, from the Earliest Ages to the Commencement of the Nineteenth Century. By Robley Dunglison, M. D., LL. D., etc. Arranged and edited by Richard J. Dunglison, M. D. Philadelphia: Lindsay & Blakiston, 1872.

Consumption and the Breath Rebreathed. Being a Sequel to the Author's Treatise on Consumption. By Henry MacCormac, M. D., Consulting Physician to the Belfast General Hospital, etc. London: Longmans, Green & Co., 1872.

Injuries of Nerves and their Consequences. By S. Weir Mitchell, M. D., Physician to the Philadelphia Orthopedic Hospital and Infirmary for Diseases of the Nervous System, etc., etc. Philadelphia: J. B. Lippincott & Co., 1872.

The Education of the Deaf and Dumb, by means of Lip-reading and Articulation. By W. B. Dalby, M. D., F. R. C. S., M. B. (Cantab.), Aural Surgeon to St. George's Hospital. London: J. & A. Churchill, 1872.

On the Pathology and Treatment of Gonorrhœa. By J. L. Milton, Surgeon to St. John's Hospital for Diseases of the Skin. London: Robert Hardwick, 1871.

Medical Register and Directory of the United States. By Dr. J. M. Tower, of Washington. Philadelphia: L. W. Butler, M. D., 1872.

Zymotic Diseases: Their Correlation and Causation. By A. Wolff, F. R. C. S. London: J. & A. Churchill, 1872.

Miscellaneous and Scientific Notes.

Catheterization of the Uterus, to incite and increase Contraction. (*Wien*, 1871, *Med. Chir. Rundschr.*, 71, 12.)—Since 1860 M. Valenta has published a series of articles in the Vienna Medical Press on this subject, and has now collected these and the results of the experiences of twelve years with catheterization of the womb, where the pains have commenced, for the purpose of increasing them, and also as a means of producing abortion or premature labor (known as the Simpson-Krause method). As to catheterization of the womb as a means of arousing pain, it may now be regarded as a fixed and recognized method: it takes the first rank to-day among the means for the induction of premature labor or abortion, and also as a means for exciting the contractions. It has already found many and warm adherents. Thus, for example, Scanzoni says on this point: "As a means acting directly upon the womb, one that seldom fails, and is at the same time harmless, we must recommend the so-called intra-uterine catheterization. This means was at first advised for the artificial induction of premature labor, and proved so useful that in the course of a few years all previous methods were almost entirely abandoned in its favor. It being once certain that it was possible to call up uterine contractions by the introduction of a catheter along the inner surface of the womb when quiescent, the step was a short one to assume that it might be possible to increase the already commenced uterine contractions by this means. And in fact this assumption was so clearly confirmed at the bedside that we must now prefer the catheterization of the womb to all other means for increasing the uterine contractions. Having been in clinical use for about two years, it has rarely disappointed, and, where it did not suffice to terminate the labor naturally, nevertheless its action in increasing the contractions has been unmistakable." Scanzoni goes even further than Valenta, in that he has employed this means successfully in many cases even after rupture of the sac of waters.

Prof. Schroeder, of Erlangen, in his book on Obstetrics, published the past year, says on this subject (p. 256): "With the same certainty with which the catheter interrupts gestation, it produces an increase in the contractions during labor, although we have not been able to assure ourselves of this result after rupture of the membranes, and the partial loss of the waters. In every case where the membranes are still intact this method deserves a thorough trial, since it acts with certainty, and produces no ill results." In the same manner we find this means recommended in Cohnstein's treatise on Obstetrics, p. 155. According to Valenta, this operation is to be employed also in those cases where a *sectio Cæsarea post mortem* is in contemplation, in order thus to preserve the life of the child with greater probability.

Of course, the employment of catheterization in no way interferes with the use of other measures for exciting the pains. As to the technical manner of this operation, nothing new is to be said. As to the duration, the author advises always to leave the catheter only until the activity of the pains has become fully regulated, that is, until the pains have reached the quality and quantity appropriate for the stage of gestation, for, by leaving it too long *in situ*, the womb might be too much irritated.

Appointments, Honors, etc.—Dr. Henry S. Hewit has been appointed Clinical Professor of Surgery, in the University Medical College, of this city; Dr. Arnold, Professor of Pathological Anatomy; and Dr. Kammerer, Professor of Diseases of Women and Children, *vice* Dr. F. D. Lente, resigned. The two vacancies among the eight Foreign Associates of the Académie des Sciences, Paris, created by the deaths of Sir John Herschel and Sir Roderick I. Murchison, have just been filled by the almost unanimous elections of Mr. Airy, the Astronomer Royal, and Prof. Agassiz. A close contest is anticipated for the rectorship of St. Andrew's University.

There are now in the field the Dean of Westminster, Lord Neaves, Sir Roundell Palmer, and Prof. Huxley. The name of Lord Salisbury has been withdrawn. The Fothergillian gold medal has been awarded by the Council of the Medical Society of London to Dr. Edwards Crisp, for his essay on Croup. One of the two silver medals is given to Dr. Thorowgood, for services rendered to the Society as secretary; and the other, which is always awarded to a gentleman not a Fellow of the Society, to Dr. Silver, in recognition of the value of his paper on Mitral Stenosis. Sir Henry Holland retains his public spirit and intellectual versatility in a remarkable degree. To the post of President of the Royal Institution, he has just added that of Vice-President of the Society for the Encouragement of the Fine Arts. The Queen has formally conferred the honor of knighthood on Dr. John Rose Cormack. Prof. Virchow has received the Cross of Commander of the Italian Order of the Crown, from King Victor Emmanuel. Prof. Esmarch, of Kiel, has married the Princess Henrietta, of Schleswig-Holstein, sister of Prince Christian. Prof. Traube, of the University of Berlin, has at last obtained a chair of the Faculty. He had previously been allowed only an extraordinary professorship, having been kept purposely back because of his Jewish faith. Dr. A. O. Kellogg, for nine years an Assistant Physician in the New York State Lunatic Asylum, has resigned, and accepted a like position in the Hudson River Hospital for the Insane; Dr. J. M. Cleveland, Superintendent. Subsequently, Dr. Judson B. Andrews, Second Assistant Physician in the New York State Lunatic Asylum, was appointed First Assistant; Dr. Walter Kempster, Third Assistant Physician, was appointed Second Assistant, and Dr. Daniel H. Kitchen, of New York City, was appointed Third Assistant Physician, to fill the vacancy. Dr. Brown-Séquard was married in Cincinnati, on March 14th, to Miss Maria R. Carlisle, of that city, and sailed for Europe on the 20th ult.

Arsenic in Multiple Lymphoma. (*Wien. Med. Wochsch.*, 1871, 14.)—Billroth reports the case of a woman, aged forty, who had been in good health during her youth. The disease began ten months before her reception into the clinic, with enlargement of the glands of the neck; those of the axilla and groin soon followed. When the patient was received into the clinic, the tumors in the neck on both sides had reached the size of the fist, those of the axilla and groin formed swellings of the size of an apple, those at the elbow the size of a hen's-egg; and the mesenteric glands seemed enlarged also. The spleen was double the normal size. Examination of the blood showed that no leucæmia was present.

An experiment was at first made with quinine, commencing with twelve grammes, gradually increasing to twenty grammes, daily. As this treatment produced no effect, Billroth decided to employ arsenic, which so frequently causes the reabsorption of splenic enlargement in intermittent fever. A mixture of equal parts of Fowler's solution and water was given in doses of five drops, at first, gradually increasing, in the course of four weeks, to twenty drops, and then again returning gradually to the original dose. Two weeks after the commencement of this treatment there was already a marked diminution of all the tumors; and, when, after two months, the patient was dismissed, there remained only a single gland, of the size of a hazel-nut, all the others having been entirely reduced.

Disease of the Kidneys produced by Urethral Gonorrhœa. [*Oesterr. Zeitsch. Prat. Heilk.*, xvii., 4 Rundsch, August, 1871.] —This obscure disease, consequent upon gonorrhœa, Dr. Zeissl remarks, is frequently to be ascribed to the large doses of the resinous diuretics given during gonorrhœa. In the demonstration of the ill effects of these medicines upon animals, Schroff and Beckmann were unsuccessful, because the animals employed in the experiments are tolerant of the acrid diuretics, while in man, sometimes after a slight use of these medicines, hæmorrhages and ecchymoses of the neck of the bladder occur. From these latter (which, however, may occur in gonorrhœa without the previous exhibition of diuretics) the hyperæmia may certainly extend *per continuitatem* as far as the papillæ of the kidneys, and into the tubuli, and thus produce a nephritis desquamativa, though not, however, a nephritis diffusa.

Diffuse Spongy Hyperostosis. [*Huppart. Wunderlich's Arch.*, 71, 2.]—At the autopsy of a scrofulous and epileptic idiot, aged twenty-seven, on the outer portion of the cranium were found four symmetrical flat osseous tumors, of which the two anterior ones were seated at the upper and inner angle of the originally non-united right and left pars frontalis of the frontal bone; the two posterior and larger ones were seated upon the parietal bones covering almost their entire surface; all four hyperostoses, which did not extend internally to the diplöe, were separated from the neighboring sutures by a thin edge of normal bone; they had the consistency of porous bone-substance, so as to be readily cut with a knife, and presented at the sections a convolution of interlacing vascular canals. The internal surface of the cranium was entirely normal. The author was not able to discover the cause of these neoplasms, which of course had no direct influence upon the mental condition of the patient.

Clay-Dressings for Variola.—Dr. E. S. Bunker, in a note to the *Medical Record*, says: "During the recent epidemic I used clay-dressings for two pretty decided cases of confluent small-pox. Both patients were young women. One, a married lady, aged twenty-three (delivered on the second day of a six months' fœtus), made a fair recovery, took cold after getting up, and in a few days died suddenly of empyema and pericarditis; diagnosis confirmed by autopsy. The other, single, aged twenty-one, had the disease with great violence, recovered rapidly, and is now well. In each case I dusted finely-sifted pipe-clay over the face as soon as the pustules became fairly developed. This formed immediately a clean, dry, wholesome scab, abolished the intolerable itching and burning, served apparently as a good absorbent of infectious material, and scaled off during convalescence, leaving underneath a soft, natural integument. There was no disfigurement in either case."

The Etiology of Eclampsia. [*Halbertsma. Cbl. f. Med. Wiss.*, 71, 27.]—The author presents the following hypothesis: The majority of cases of puerperal eclampsia are in consequence of retention of the contents of the urine, resulting from the pressure upon the ureters. The two ureters, before reaching the fundus of the bladder, include the lower part of the uterus between them. If the latter is greatly enlarged, the ureters must be compressed, and thus retention of urine caused. Hence, it follows also that, by the action of the abdominal pressure and the contraction of the uterus, inasmuch as the uterine walls become each time anæmic, while, in the rest of the vascular system and especially in the region of the kidneys, a collateral hyperæmia must ensue, through the latter, as well as from diffuse nephritis, the production of eclampsia is effected.

Graduates in Medicine for 1872.—We give below the number of recent graduates at the various medical colleges of which we have thus far received reports:

Bellevue, New York	129
Jefferson, Philadelphia	115
Medical College of Ohio	87
University of Louisville	84
University of Nashville	84
University of Pennsylvania	83
University of Michigan	82
College of Physicians and Surgeons New York	78
Rush Medical College, Chicago	77
University, New York	75
St. Louis Medical College	68
Miami Medical College, Ohio	67
Missouri Medical College	40
Buffalo Medical College	34
Chicago Medical College	32
Woman's Medical College, Pennsylvania	13
Geneva Medical College	12
Woman's Medical College, New York	8
Kansas City College	5

On the Absorption of Gray and Corrosive Sublimate Ointment by the Uninjured Skin. [*Allg. Wien. Med. Zeit.*, No. 43.]—Dr. Neumann publishes some microscopico-chemical studies on this subject. By the inunction of gray ointment upon the uninjured skin, the globules of quicksilver penetrate into the hair-follicles as far as the bulbs, into the commencement of the sudoriparous glands, into the open fat-glands (at least into those opening into the hair-follicles). How they then penetrated into the circulation, and in what form, could not be determined; they were probably converted into the sublimate, and absorbed by the superficial lymphatic system. In the blood, however, and in the internal organs, mercury rubbed into the uninjured skin as gray ointment, as well as the sublimate, is demonstrable only by chemical means. In the subcutaneous cellular tissue and in the cutis the globules were never discoverable. Mercury does not penetrate the cartilaginous tissue.

The Popular Science Monthly.—The rapidly-increasing taste for science among all classes has led to a demand for popular instruction that is not supplied by any journal hitherto published in this country. In order adequately to meet this want, a new journal, THE POPULAR SCIENCE MONTHLY, conducted by E. L. Youmans, has been established by D. Appleton & Co. The first number, for the month of May, presents a highly-interesting table of contents, including among contributors the names of Herbert Spencer, R. G. Proctor, A. de Quatrefages, Arthur Leared, and others well known to scientific fame. The new magazine enters a wide field of usefulness, and we doubt not will receive from the reading public the hearty encouragement it so well deserves. The reputation of its editor is a sufficient guarantee of future excellence.

Contagiousness of Petechial Typhus. [*Rud. Virchow*, *Virchow's Arch.*, 53, 1.]—In a slight epidemic (fifteen cases) of this disease the portability and contagiousness of the malady were evident as a rule; there was no evidence in favor of its spontaneity; the eruption generally appeared early—twice on the third day, twice by the second day after the initial rigors. Most frequently diarrhœa occurred in the course of the disease, once to such an extent that cholerine was suggested. Cholera fungi were present in the fæces. The incubation varied between nine and fourteen days, and perhaps longer. From one case it appeared as though the disease were contagious even in its latter stages. The most striking case was that of one of the attendants who died, and who, as nearly as could be ascertained, had come in contact with the bodies of fever-patients only.

The German University of Strasbourg.—The Faculty of Medicine is almost completely organized. Prof. Hering, of Prague, has lately been appointed; also the following: Profs. Oscar Schmidt, of Gräz (Zoology), Waldeyer (Anatomy), Hoppe-Seyler (Physiological Chemistry), Von Recklinghausen (Morbid Anatomy), Schmiedeberg (Materia Medica), Leyden (Medicine), Lücke (Surgery), Gusserow (Midwifery and Diseases of Children), Von Kraft-Ebing (Mental Diseases); Laqueur (Ophthalmology). Cohnheim, whose researches are well known in this country, goes to Breslau, to take the chair vacated by Prof. Waldeyer.

A Nuisance abated.—We take sincere pleasure in giving publicity to the fact that, on the 20th of March last, the Legislature of Pennsylvania, by a unanimous vote of both Houses, revoked the charters of the two institutions known respectively as the Philadelphia University of Medicine and Surgery, and the American University of Medicine. The infamous traffic in diplomas so long carried on with impunity by these concerns is thus brought to an end. We join the Philadelphia *Medical Times* in regretting that the guilty parties are likely to escape the punishment they so richly deserve, but it is a satisfaction to know that they can do no further mischief to the public in the way of issuing bogus diplomas.

A Good Appointment.—Dr. J. H. Baxter, who has for some time filled the position of assistant medical purveyor, has been promoted to the responsible post of chief medical purveyor, U. S. Army, and confirmed in that position by the Senate. Dr. Baxter is a highly-efficient officer, and admirably fitted for the duties of his new office. He has been engaged for some time in the preparation of a report on the medical statistics of the Provost-Marshal-General's bureau.

Journalistic Notes.—The *National Medical Journal* is temporarily suspended for want of editors. The recent editors of that journal, Drs. Busey and Lee, announce a new one, to be called the *Washington Medical Monthly*. *The Medical World*, which recently lost its cover and its editor at the same time, announces that a new editor "of superior talents and ability" has been obtained; hence the *World* appears with its accustomed regularity, naked but not ashamed.

American Medical Association.—The following are the terms on which tickets will be issued to those wishing to attend the twenty-third annual session of this Association, to be held in Philadelphia, on Tuesday, May 7th: Camden & Amboy, excursion tickets at $4 from New York to Philadelphia and return, if fifty tickets are taken. For this ticket send money to Dr. A. E. M. Purdy, 123 East Thirty-eighth Street.

Medical Preparations.—We call attention to the announcement, in our advertising columns, of various pharmaceutical preparations, by E. Fougera & Co. They have long enjoyed the reputation of furnishing drugs of the finest quality, and their preparations may be relied upon. We know by experience that their cod-liver oil is uniformly pure and sweet.

A Large Medical Library for Sale.—The medical library of the late Prof. George C. Blackman, M. D., of Cincinnati, containing over three thousand volumes, besides a large number of journals, papers, pamphlets, etc., is now in the hands of Mr. Robert Clark, of Cincinnati, for sale.

University Appointment.—Dr. Joseph W. Howe has been appointed Clinical Professor of Surgery in the Medical Department of the University of New York.

Lectures on Hygiene.—We publish in this issue the first of a series of lectures on hygiene, in course of delivery, by Prof. Hammond, in Bellevue Hospital College.

Whither are we drifting?—Under this title the *Philadelphia Medical Times* of January 15th gives the following editorial review of the sentiments expressed by Dr. H. J. Bigelow in his recent pamphlet on "Medical Education:"

Some little time since, chance, or an evil star of destiny, led us into the rooms of the Pennsylvania Society for Prevention of Cruelty to Animals, and we found ourselves confronted by a crowd of ladies, eagerly desiring each to have her speak on the horrid cruelty of vivisections.

"Dr. —— says there never was any thing learned from vivisections," said one. "Yes, and if any thing ever was learned, Brown-Séquard says all is now known that is worth knowing," echoed a second. "Magendie on his death-bed regretted it bitterly, and confessed it to be the sin which had ruined his soul," chimed in a third. "How can you do it?" And mild eyes looked so reproachfully that we really did begin to think ourselves a sort of a monster, and certainly appreciated most fully the feelings of their possessor.

It may seem strange, when the poor are dying in our streets for very cold and poverty, when the sick are languishing in half-furnished hospitals or perishing in the by-ways and alleys, calling down God's curse upon the city that leaves them so, when the pestilence is deepening the gloom in many a stricken household, when in almshouse and alley the patient sufferers are longing for human sympathy and some one to break to them the bread of life—when these things are so, it may seem strange that women of refinement and culture, clad in their fine raiment and rejoicing in their happy homes, should pass by their own suffering fellow-beings and devote themselves with all their earnestness to brutes, weighing the life and suffering of man against the life and suffering of the brute, and casting in their strength and powers with the latter. But when a woman has once become a professional protector of the dog—with her whole-heartedness, her mine of tender feeling, her inborn tendency to see only one side of a question—it is but natural that she should get most narrow and distorted views, ending in that bigoted ignorance which hesitates not—knowing no better—to advance with a ludicrous persistence the most transparent falsities as proven facts.

Such things any one may feel disposed to pardon in an elegant lady; but what shall we say when a member of our profession, driven by some nightmare chimera of his imagination, oppressed by a morbid sensibility, puts on record such sentiments as are contained in Dr. H. J. Bigelow's pamphlet on "Medical Education in America?" As these paragraphs have been in all our daily papers, and as space is precious, it is unnecessary here to quote them. We know it will be said it is of very little importance what a man who writes such sentences thinks; and so we acknowledge it to be. Yet office throws a halo around the most contemptible. In America, public opinion must finally uphold or crush out every thing brought to its bar. To-day the practice of vivisection is at the bar of public opinion; and far and wide over our land, to the sad detriment of truth, the dictum of a professor in Harvard College is being quoted by hundreds who never before heard of the Boston surgeon. The false, poisoned arrow which he has shot has far outstripped all the truth a lifetime has sped on its way.

Much that our professor writes is so childish as to be beneath criticism. Witness the proposition that, when a vivisector is experimenting, he should always cut one-eighth the distance into his own flesh that he does into that of his victim! It is possible that the professor, when he wrote this, thought it fine, ingenious, and no doubt the F. B. of the Pennsylvania Society for Prevention of Cruelty to Animals thinks it so; but, we say, whither are we drifting, when a leader of the profession can stand up and speak such nonsense before one of its chief societies?

But we have a graver charge than that of nonsense to make against this writing: much of it is false; and, coming from one so high, it must be *knowingly* false. It is a direct attempt to hide the truth: on the one hand he draws a picture of brute suffering wrought up to a pitch far above what it is in nature, heightened with sketches drawn from the veterinary school of Alfort, and on the other he makes the blank assertion that no good has of late years come out of vivisection. Must we not repeat *knowingly* false? for is it possible that the professor does not know that Alfort was a veterinary school, and that it was chiefly, or at least largely, through the efforts of the medical profession—the vivisectors—that it was broken up? We say *knowingly* false; for is it possible that a Harvard professor has not heard of vaso-motor nerves, of spinal cord, or of modern nervous physiology, which rests almost exclusively on vivisection? We say *knowingly* false; for is it conceivable that he should lecture on tetanus and not have heard of Calabar-bean or chloral, whose action and powers were first learned by means of vivesection? Once more we say *knowingly* false; for is it possible that to his ears not yet have come sounds of the great truths about inflammation and tuberculosis and other general nutritive disturbances which are being wrought out through vivisection?

It is needless for us to say more on these paragraphs of Dr. Bigelow; their own condemnation is stamped so deep and sharp in their very fibre that every professional man will at once perceive it; but to let them go unchallenged, to let the public think that they are true, or represent the opinions of a large body of the American profession, were an outrage to that profession, and especially to the few who, without hope of direct reward, spend hours of toil in search, by the repulsive method of vivisection, for precious truth and human boon.

If such words have astonished and stirred us, much more deeply and indignantly have we been moved for the honor and fair name of our American profession by other paragraphs in this most remarkable pamphlet.

The Roman philosopher, when sore sickness, dull business, or a scolding wife, made life seem weariness, threw off the load, and the world praised his courage. But times have changed, and murder and suicide have been rendered unfashionable by Christianity. Napoleon, in Egypt, shot down his Arab prisoners by thousands, justifying himself by a stern necessity; but the world execrated him.

Some time since, a leading practitioner of this city—a plain Quaker gentleman, marked by his peculiar garb—was horrified by being asked whether it was true that, when a baby was suffering, and he thought that it would not recover, he put it to death! If the mere suspicion sent a shudder through him, what must be his astonishment when a learned professor states that such or very similar action was his habitual practice? We will not put Dr. Bigelow in a false light, and therefore quote the passage in full from page 35 of his pamphlet:

"In a surgical practice of twenty-five years, I have never intentionally given a patient, unless by his own choice, any unnarcotized pain, *nor have I allowed a patient to die a death of pain, when opium would lull him into his long sleep. I share the responsibility of this with the surgeon who walked about the battle-field distributing morphia to the hopelessly wounded, and with the soldier of Ambrose Paré, who did more.* It has been my lot to see a friend, at the end of a painful and hopeless malady, to whom, when the hour of death seemed to be near at hand, I had given the morphine largely, twice awaken with a week's new life, due to eighteen or twenty-four hours' deep sleep and continued exemption from pain."

If English means any thing, these sentences certainly mean that, when, in his finite judgment, death is at hand, Dr. Bigelow, driven by the same morbid sensibility that has so warped and distorted his vision in regard to vivisection, hesitates not to take into his feeble hand the issues of life and death, which are said to belong to the Almighty. The inference from the last sentence, taken with what goes before, is inevitable—that his friend did not wake up from the third poisonous draught.

In Philadelphia we have not as yet progressed so far as to lose respect for a certain book which says, most emphatically, "Thou shalt commit no murder." And we had thought that, even in self-complacent Boston, the centre of progress, this old-fashioned maxim still held sway in the profession. But with sorrow and shamefacedness we hang our heads; nay, rather, with determined earnestness let us raise our voices in indignant protest at this last and vilest prostitution of our sacred calling. Time was when physicians were earnest men, full of sympathy and true zeal for the welfare of their kind; but alas! we seem to be drifting away from the ancient moorings; the profession is fast becoming a trade; but infinitely more welcome were the simple commercial basis than this maudlin sensibility which would utterly pervert the truth, and hesitate not to trifle with human life.

The Nervous Systems of the Carolina Sisters.—During a recent visit to Scotland of Millie-Christine, known as the "Carolina Sisters," experiments were made by Prof. Struthers and Dr. A. Buchanan on their forms of sensibility and other peculiarities of function in their nervous systems. The following letter by Dr. Buchanan is a portion of the correspondence on the subject, as published in the *Lancet* of February 24th:

The reason of my taking so much interest in the nervous development of the two sisters is, that it appears to me to afford a most beautiful illustration of the separate existence of two forms of sensation, which we usually see combined, but which I have long held, both on physiological and psychological grounds, to be quite distinct from each other. I give them the names of *passive* and *discriminative* sensation. By sensation we mean the process or action by which certain changes that take place in the condition of the body excite corresponding changes of consciousness in the mind. In *passive* sensation, the mind, although cognizant of a change, gives no heed to it as it glides past on the ever-varying stream of consciousness. In *discriminative* sensation, on the other hand, the attention is aroused, and the mind voluntarily directs the sentient organs to assist in the performance of an act of judgment as to the nature of the sensations. Discriminative sensation, as implying both volition and judgment, requires indispensably the brain as its chief instrument. Passive sensation, again, has nothing to do with the brain, requiring only the medullary cord, with its nerves and ganglia, for its instruments. This has been demonstrated experimentally by Flourens and other physiologists, who found that, after removing the cerebral and cerebellar hemispheres, while all the higher faculties of the mind were abolished, mere bodily feeling, or passive sensation, continued as acute as before. Now, the Carolina Sisters exhibit to us a similar experiment made by the delicate hand of Nature, and as far surpassing the coarse manipulations of the dissecting-room as the answers given by these bright and intelligent girls are more worthy of reliance than the deductions made from the cries and writhings of a mutilated and half-unconscious animal.

If I do not misinterpret your investigations, they lead to the two following conclusions: 1. That each sister has, in her own person, the functions of the nervous system quite complete, having, in particular, perfect sensation over the whole body, and full command over all the voluntary muscles. 2. That each of them, in addition to perfect sensation in her own person, has passive sensation all over the lower limbs of her sister; that she gets from this last source the sense of touch or contact, the sense of pain, the sense of heat and cold, and the sense of movement in any part of the limbs; but that all these sensations are merely passive, and so indefinite that they cannot be made the basis of any act of judgment—as, for instance, to determine whether a touch felt has been made on the great-toe, the ankle, the knee, or the thigh.

As to the state of the nervous system which accompanies these wonderful phenomena, we can only guess. According to the medical men who have had the best opportunities of judging of the mode of union of the two sisters, there is no osseous union higher up than the lowest third of the sacrum; but there can be little doubt that the lower parts of both vertebral canals are still open behind, as we see them in the embryo, and as they continue permanently in cases of spina bifida. The coalescence of the two embryos must have taken place at a very early period of development, and the bones would thereafter adapt themselves to the condition of the soft parts. We have simply, however, to inquire what mode of union of the true nervous systems would produce the phenomena described above, and none other. We must, I think, put aside the hypothosis that the two spinal cords are united posteriorly at their lower ends, or that there is a coalescence or intermingling of the posterior roots of the two cords; for each sister has the functions of the nervous system perfect in her own person, which she could not have without a normal structure of the cord, and normal distribution of the nervous fibres. It seems to me more probable that there is fusion of the lowermost intervertebral and sympathetic ganglia. There might be a necessity for the conjunction of the latter, to give unity of action to the conjoined pelvic viscera. We have, however, at present only to do with the former, which we must suppose to be coalescent, and so to form only a single instead of a double series, from the second lumbar vertebra downward. In this way each ganglion becomes the common centre to the same nerves of the right and left limbs, alternately, of the two sisters, and sends a commissure (posterior root) to the alternate sides of the two spinal cords; and every impression made on the nerves of the lower limbs follows a single tract as far as the intervetebral ganglion, where, divaricating, it passes by the two commissures to the alternate sides of the two spinal cords. By this arrangement the two sisters, speaking of them together, have sensation and cross reflex action everywhere over the four lower limbs; but, speaking of them individually, each sister has perfect sensation, voluntary motion, and direct reflex action, in her own limbs, while over the limbs which do not belong to her she has no voluntary power of motion, and derives from them only passive sensation and cross reflex action.

Speculations like these are of little moment compared with the unquestionable fact that you have ascertained that each sister has one kind of sensibility in her own limbs, and another quite different throughout the limbs of her sister. This seems to me so important that you should lose no time in communicating it to the medical public, who are manifestly not aware of it, nor alive to its important bearings. If we are still to look upon the head as "the dome of thought," are we no longer to regard it as "the palace of the soul?"—that is, are we to assign to consciousness a wider residence in the three layers of the germinal membrane and the tissues developed from them? British physiologists could not but regret deeply, if these bright girls were allowed to leave this country before every fact connected with their complex organization had been carefully ascertained; more especially as centuries may elapse before such another opportunity presents itself, of cross-questioning Nature, as to the laws by which she governs the nervous system.

A. BUCHANAN.

Syphilitic Aneurism.—At a meeting of the New York Pathological Society (*Medical Record*), held January 10th, Dr. Flint exhibited a specimen of aneurism of the abdominal aorta. The history of the case was very incomplete, the patient being received in the hospital only a few hours before his death, and then in a state of almost complete exhaustion. It was ascertained, however, that he had had syphilis fourteen years before, followed by certain constitutional symptoms, affection of the throat, eruptions, etc. During last summer he had complained of pain in the back and abdomen. After his admission he rallied somewhat, on the use of stimulants, so that the facts above narrated were obtained. The house-physician ascertained the presence of a tumor which he found to be pulsating, but he discovered neither thrill nor murmur. Shortly after his temporary improvement he raised up in bed, when he complained of feeling faint, was laid back, and expired almost immediately.

The abdominal aorta, just above the renal arteries, showed a large circular opening, perhaps an inch and a half in diameter, which communicated with an aneurismal sac the size of a small orange. There was a perforation in the sac large enough to admit the end of the little finger. There was some blood infiltrated in the areolar tissue beneath the artery, and the peritoneal cavity contained a large quantity of blood. The specimen was of no particular interest, except from the fact that it occurred in a patient who had syphilis, and that, in all probability, there was a certain amount of hæmorrhage from rupture before he was admitted into the hospital, which hæmorrhage was arrested by temporary coagulation. The internal coat of the artery was lined with atheromatous deposits. The patient was thirty-seven years of age.

Dr. Janeway believed that syphilis was not only a very common cause of aneurism, but also of valvular disease. He had met with two cases of thickening of the valves which could be explained in no other way.

Dr. Flint had not had his attention drawn particularly to syphilis as a cause of valvular disease, but thought that it might explain many of the cases of the latter lesion, in which none of the usual causes were made out.

Dr. Mason had seen several cases of aneurism which appeared to be the direct result of constitutional syphilis. He referred to one case in point, occurring in a man aged thirty-three.

Dr. Loomis alluded to a long list of cases published within the last two years in the *Lancet*, in which all under the age of forty gave a syphilitic history.

Dr. Rogers was reminded of a case presented to the Society ten years ago, of very extensive aortic disease, in a young man twenty-eight years of age, who had contracted syphilis only eighteen months before his death. The patient eventually died of extensive aortic disease. In that case no other cause for the arterial and cardiac lesions was assigned than syphilis.

Physiological and Therapeutical Action of Quinine.—The following is a summary of the results of recent investigations on this subject by Binz, Ranke, Kerner, Schulte, Zuntz, and Scharrenbroich:

Binz finds that quinine has the power of arresting the processes of putrefaction and fermentation in a high degree, and that it is an active poison for all low organisms, animal and vegetable. According to Cohnheim's views, pus being mainly a collection of white blood-globules, which have passed through the walls of the vessels—further, quinine having the power of arresting the motions of the white corpuscles, and hence preventing their exit from the vessels—the alkaloid arrests, or at all events diminishes, the formation of pus during the course of inflammation. Moreover, it destroys the ozonizing power of certain substances; and, as the red corpuscles have this power, quinine in the blood probably diminishes oxidation of tissue, and lessens the production of heat. Ranke and Kerner, indeed, have found that quinine in large doses diminishes tissue-changes, as is shown by the smaller quantities of urea and uric acid excreted; and there are many observations to show that in fevers it produces a decrement in temperature. Ranke and Kerner's experiments do not show, however, how far the lessening of tissue-waste is due to the direct action of quinine on oxidation, and how far to the indirect action of the alkaloid through the nervous system. Two methods have been employed for ascertaining the direct influence of quinine on oxidation. Harley added quinine to the blood, and found that this, when so treated, took up less oxygen, and gave off less carbonic acid, than blood which had not been so treated. This method is inconvenient of application, and liable to error. Zuntz employed the changes in the alkalinity of the blood for arriving at the same results. Schulte has extended these researches. If fresh blood be drawn, a development of acid begins in it, and continues, at first rapidly, then more slowly, till putrefaction sets in. Of course this acidification depends on oxidations; and the diminished alkalinity of the blood, thereby produced, furnishes a test of the rapidity with which oxidation proceeds. Schulte has confirmed the observation first made by Zuntz and Scharrenbroich, that quinine and berberine lessen the production of acid. Harley's observation is thus confirmed. Cinchonine produces similar results to quinine, though in a very inferior degree. Picrate of sodium is nearly as powerful as quinine. Zuntz found, as Ranke and Kerner had previously done, that quinine in ten-grain doses lessens the daily excretion of urea by one-third or more. Unruh has found the same to occur when quinine is administered in fevers; but his observations are open to objection. Binz's experiments are curious, and show that, when putrefying liquids are injected into the circulation, the temperature of the body rises; but, if the fluids be previously mixed with quinine, whereby the putrefactive processes are arrested or destroyed, the rise in temperature is either entirely arrested or considerably diminished.

Mental Influence in Disease.—Prof. Austin Flint, in an article in the *American Practitioner* for January, 1872, on the agency of the mind in etiology, prophylaxis, and therapeutics, says:

We meet sometimes with cases in which recovery from disease seems fairly attributable, in a great measure, to a resolute determination on the part of the patient to recover. An unfavorable prognosis is communicated to a patient. He declares that he will not die, and he gets well, when, according to prognostics, which are not thereby invalidated, he ought to have died. Examples of this kind have doubtless fallen under the observation of my readers. It has fallen to my lot to observe a large number of cases of tuberculous disease of the lungs; and I have been struck with the fact that, in the comparatively few which have ended in recovery, the patients have generally been persons of a strong will, who, appreciating fully the disease, have resolved, if possible, to overcome it. I cannot but think that the proportion of cases ending in recovery would be larger than it is, were it not that the disease carries with it so often either a delusive expectation of recovery without exertion, or a passive acquiescence in a fatal termination.

The management in many cases of disease embraces measures not to effect a cure, but to secure, as fully and for as long a period as possible, tolerance of the disease. To prolong life, and to render as comfortable as may be life as long as it lasts, are the grand object of management when diseases exist which are incurable, and which, sooner or later, will end fatally. Now, mental conditions have much to do toward the accomplishment of these objects. Occupations which engage the mind sufficiently to prevent undue attention to symptoms, over-anxiety, and apprehensions, are often of great utility. Knowledge of the existence of an incurable disease is sometimes extremely unfortunate for the welfare of the patient. I am convinced that the duration of life is sometimes shortened, and comfort during life lessened, by this knowledge.

Virgin and Humanized Lymph.—From a series of careful experiments and observations, published in full in the Philadelphia *Medical Times*, of March 1st, Dr. Benjamin Lee arrives at the following conclusions:

1. That virgin vaccine lymph direct from the heifer is not easily absorbed into the human system, and that therefore it is not advisable to attempt its general use in the face of an epidemic or the presence of direct contagion.

2. That it is less readily absorbed in the case of infants than of older children and adults.

3. That, when absorbed, it produces the vaccine disease in both its local and constitutional manifestations in its most normal and perfect type, without unusual severity or complication.

4. That virus produced by the inoculation of a single human being with virgin lymph, for the first time, is absorbed into the human system with excessive readiness, constituting the most active virus that can be procured.

5. That such virus is more likely to induce true vaccinia in persons already vaccinated than ordinary humanized virus of long descent, and that it is therefore reasonable to suppose that its prophylactic power is in all cases greater.

6. That what is true of lymph of the first remove is also measurably true of that of several subsequent removes, but to what extent these experiments do not determine.

7. That, accepting the foregoing conclusions, it is a matter of primary importance that every large centre of population should be provided with the means for frequently revivifying its supply of virus by a return to the original source.

Rapidity of the Circulation in the Veins.—A translation of a paper contained in the Bulletin of the St. Petersburg Academy of Sciences appears in the *Journal of Anatomy and Physiology*. It contains the results of the experiments of MM. Cyon and Steinmann, who have recently found that the amount of blood which flowed in a given time through the jugular or crural veins of dogs, narcotized with opium or chloral, was nearly the same as that which flowed through the corresponding arteries. The velocity of the current in veins is therefore much greater than that calculated by Volkmann. The rapidity of the current in the veins undergoes variations similar to that in the arteries, so that it is much diminished in the jugular when the carotid of the same side is compressed, but is increased by compression of the carotid of the opposite side. Division of the spinal cord lowers the blood-pressure, and thus lessens the rapidity of the current. Irritation of the cord generally raises the blood-pressure in dogs only to a slight extent, and, as it causes contraction of the arterioles, it generally lessens the rapidity of the current. Sometimes division of the cord is followed by a slightly-increased rapidity of the current. Irritation of a sensory nerve produces general contraction of the arterioles, and temporary contraction succeeded by dilatation in the part supplied by the nerve. When the tibial nerve was irritated the rapidity of the current in the crural vein was increased on one occasion so much that Cyon thought it due to local dilatation of the arteries occurring at the same time as a general increase of blood-pressure. At other times the rapidity became greater as the blood-pressure sank, apparently from the contraction of the arterioles, which had slowed the current in spite of the increased blood-pressure having given way to dilatation. If no rise of blood-pressure occurred, the stream in the veins was slowed by the contraction of the arterioles. Stoppage of artificial respiration in curarized dogs increased the rapidity of the current, and it only began to diminish when they were becoming asphyxiated. If the tracheæ of dogs which were not narcotized were closed when their lungs were distended with air, the rapidity of the current in the veins became rapidly diminished in consequence of venous congestion.—*Lancet*, December 23d.

The Medical Register of New York.—The tenth volume of this important work, to be issued June 1, 1872, under the auspices of the New York Medico-Historical Society, will contain a likeness of Dr. George H. Tucker, deceased, editor of the first *Medical Register* published in this country; also a condensed description of the objects, character, and field of operation of the hospitals, infirmaries, dispensaries, asylums, and medical and other scientific associations of New York City and its vicinity, and the charitable institutions of New York, New Jersey, and Connecticut, etc. It will, as usual, also contain information respecting the colleges of these States, a list of physicians in good standing, practising in New York City and adjoining places, their residences and office-hours; also a list of county medical societies of New York, New Jersey, and Connecticut, their officers and members, with post-office addresses, and short obituaries, as far as can be obtained, of the physicians of these counties, recently deceased.

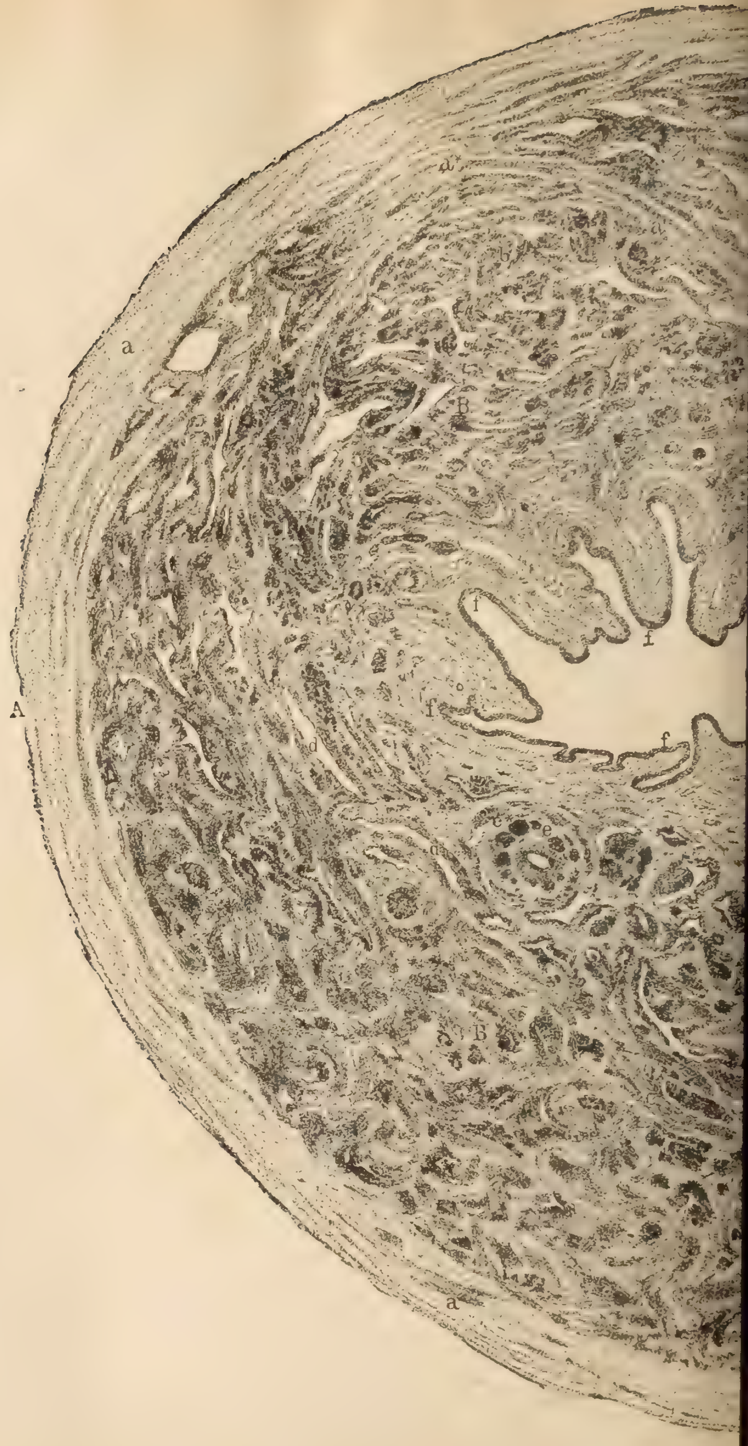
Committee of Publication.—Drs. Frederic Elliot, Ellsworth Eliot, Edward C. Woodbury, William T. White, *President*, and Alfred E. M. Purdy, *Editor*.—*Medical Record*.

Medical Teaching in Austria.—The Minister of Public Instruction of Austria has requested the professors to send in an annual report touching their labors, both as to the advancement of science and the teaching of pupils. Minute details are expected, and promotion will be regulated by the importance of the reports.—*Lancet.*

Obituary.

Dr. Samuel Jackson, Emeritus Professor of the Institutes of Medicine in the University of Pennsylvania, died, April 6th, at his residence, in Philadelphia, at the age of eighty-two. He was born in Philadelphia, on the 22d of March, 1787. He devoted his time with great energy to the study of his profession, and was successful in acquiring a high standing in its practice. In 1835 he was elected professor of the Institute of Medicine in the University of Pennsylvania, a position which was filled by him during a period of twenty-eight years, with great credit both to himself and to the institution. He was a very popular lecturer and profound scholar, and occupied a front rank in the corps of professors of the university. In 1863, in consequence of his advanced age, he abandoned the active duties of his professorship. Prof. Jackson wrote several works on medical and physiological subjects, of which a treatise on the "Principles of Medicine," first published in 1832, and an "Introduction to Lehman's Chemical Physiology," published in 1856, are the most widely known.

Miss Ann Preston, M. D., of Philadelphia, died in that city on the 19th ult. Miss Preston was born in 1814, and early in life manifested a taste for studies not usually pursued by women. She was one of the first applicants for admission to the Woman's Hospital of Philadelphia, founded in 1850, and afterward became professor and dean of that institution.



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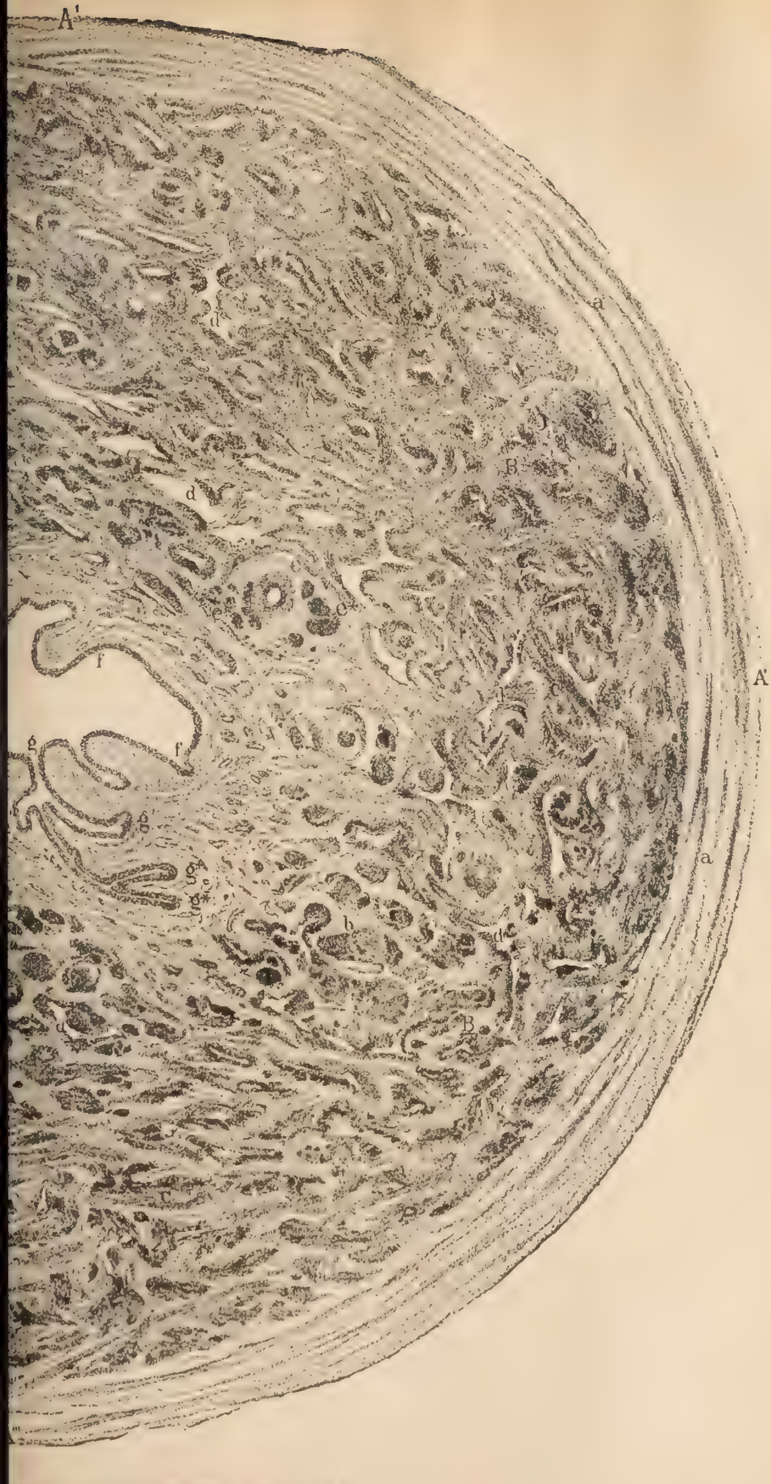

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Original Communications.

Art. I.—*Normal Presence of Alcohol in the Blood.* By Wm. Hutson Ford, M. D., Professor of Physiology in the New Orleans School of Medicine.

During the winter of 1854–'55, a course of lectures on "Experimental Physiology" was delivered at the College of France, by M. Claude Bernard, of which a condensed report by one of his pupils, under the supervision of the distinguished physiologist, was shortly afterward published. In these lectures, M. Bernard reviewed before his class, both didactically and experimentally, his most important investigations concerning the glycogenitic function of the liver, and the influence of the nervous system, in certain morbid states, upon this remarkable secretory process. I had been fortunate enough, in previous years, to attend similar courses, in private and in public, by M. Bernard, and continued to feel very deeply interested in his untiring physiological investigations. I could not, however, recollect that he had ever expressed himself definitely with regard to the purposes and mode of destruction, in the economy, of the glucose he had discovered; and, upon obtaining a copy of the publication above referred to, I sought through the volume for a declaration of his views upon these points. On page 242 he states that it is difficult to designate the uses of hepatic glucose in the body; and on the succeeding page emphatically avows his disbelief in its calorifaciant destination; expressing a conviction that it is devoted to purposes of "*far greater importance*," viz., to what he terms "animal germination." As many reasons inclined me to adopt a different opinion, I determined to investigate the matter, and accordingly conducted an experimental series with this view, whose principal results were published in 1859, under the auspices of a scientific body, of Charleston, S. C. This paper is in part a *résumé* of the monograph referred to. M. Bernard had instituted some experiments, to which I must refer the reader (*loc. cit.*, pp. 233–236), upon the disappearance of sugar in blood after a lapse of time varying from some hours to two or three days, not with a view of determining the mode of its destruction, but to ascertain the influence exerted by various gases upon its disappearance. He found that in blood placed under these gases, or through which at one time oxygen, at another carbonic acid, nitrogen, hydrogen, arseniuretted hydrogen, or air, was diffused by agitation, sugar, sooner or later, but *invariably*, disappeared. With regard, however, to the mode of destruction of sugar in these cases, or the nature of the product of its transformation, M. Bernard was silent.

On attentively considering the general fact of the disappearance of glucose, under circumstances apparently so opposite, I suspected that it might be due to a fermentative process of some sort. If so, the two fermentations most likely to have occurred were the *lactic* and the *alcoholic*. For a variety of reasons, which I have elsewhere stated, I felt myself obliged to dissent from the prevalent physiological hypothesis, which maintains that sugar disappears in the economy by the lactic-acid fermentation. The most prominent facts before me were the following: At the temperature of the air, and in a fluid containing nitrogenous matter undergoing change, as well as oxygen (venous blood six per cent. by bulk), the most fermentescible of all sugars had disappeared: may it not have undergone the alcoholic fermentation, the resultant alcohol being mingled with the blood, and the carbonic acid simply dissolved in that fluid? And if such should be the mode of disappearance of glucose, could not the alcohol generated be recovered by distillation?

When I reflected upon the minuteness of the quantity of glucose present in systemic blood, during digestion, or even in the heart and lungs, at any given moment, only *one-half* of which, at the utmost, could be recovered as *alcohol*, I was somewhat dismayed. This quantity may be estimated as follows:

We know that starchy matters of the food are gradually transformed by the duodenal and intestinal juices into glucose, which is taken up by the radicles of the vena portæ, and passed through the capillaries of the liver into the hepatic veins. But when the intestines are empty, as well as when an animal is fed exclusively on meat or other matters devoid of starch and sugar, glucose is still found in the blood of the hepatic veins; the liver-tissue itself containing about two per cent. of it. Glucose, found in the economy during abstinence, is plainly derived from nitrogenous matter, so that, by the metamorphosis in the liver of some proteinoid body, perhaps fibrine, a continual supply is poured into the hepatic veins; this true hepatic secretion being entirely independent of the presence or nature of the contents of the stomach or intestines. The hepatic veins, therefore, contain glucose, of hepatic origin, at all times; while, during digestion, they contain, besides this portion, a quantity representing the starch and saccharine matters ingested. This *conjoint glucose*, consequently, represents not only the starch, but likewise a very considerable proportion of the nitrogenous constituents of the food; for these are destined, after subserving their special economical purposes, to be converted into that body, by the glycogenetic action of the liver.

Liebig estimates the carbon consumed by each prisoner, in the House of Arrest, at Giessen, in his daily ration of $1\frac{1}{2}$ lb. bread, 1 lb. soup, and $\frac{1}{2}$ lb. potatoes, at 9 oz. Hessian (very nearly the same quantity av.). Assuming 330 grains of urea to be daily eliminated by such a prisoner, of which exactly $\frac{1}{5}$ is carbon, we should deduct from the above $\frac{1}{7}$ of an ounce, leaving 8.86 oz. of carbon devoted to respiratory purposes, if *fat* be neglected, not very abundant in food like the above. By the formula for glucose ($C_{12}H_{14}O_{14}$), this amount of carbon represents 24.36 oz. of glucose from both sources, all of which passes through the hepatic veins within twenty-four hours. Now, if this amount be fermented into alcohol and carbonic acid, it would correspond to no less than 12.45 oz. (av.) = 5,976 grains of alcohol. As all of this passes through the right side of the heart, there would be present in the pulmonary blood, *during* one minute, $\frac{1}{1440}$th part of it, or 4.15 grains. According to Müller, ten pounds of blood (or 76.800 grains) traverse the lungs in the same time. The pulmonary blood, therefore, of such a prisoner, obviously underfed, cannot contain more than $\frac{4.15}{76,800}$ = 0.5403 of a grain for 10,000 grains.

We may call this a calculation by "*ingested carbon.*" Let us compare it with a similar one based upon the "*expired carbon.*"

Scharling estimates the carbon given off from the lungs of a man, in twenty-four hours, at 325.31 grammes. Supposing that all of this carbon (which is not strictly true) results from the destruction of hydrocarbon, and that this hydrocarbon is hepatic sugar, 813.26 grammes of this substance would be fermented, producing 415.65 grammes of alcohol, or 6,415 grains. As this quantity represents the entire supply during twenty-four hours, $\frac{6415}{1440}$ = 4.45 grains of alcohol would pass through the lungs in one minute; and as by Müller's estimate 76,800 grains of blood is borne through those organs in the same time, we have, by this calculation, $\frac{4.45}{76,800}$ = 0.5794 of a grain of alcohol present in the lungs to 10,000 of blood. The two results approximate closely enough for such calculations. So that, while no less than 23.05 oz. (Scharling) to 24.36 oz. (Liebig) of glucose should be poured into the circulation within twenty-four hours, representing *about* 13 *ounces* (av.) *of alcohol*, a quantity amply capable of subserving most important functions, whatever these may be, it is impossible that the blood of the vena cava or the lungs should *contain* more than the *half of one part of alcohol*, in 10,000 parts, even if no particle of alcohol so formed be oxidized. And this I premise, requesting particular attention to the obvious fact, that only exceedingly minute quantities of alcohol can exist in animal fluids or organs, or be obtained from them; and consequently, that only the most accurate and careful processes, and the manipulation of tolerably large amounts of material, can enable us to procure that body if it be present. Impressed with these considerations, in a series of preliminary experiments, I determined:

1. That organic matters could be distilled, and the associated matters separated from the distillate, by certain agents.

2. That a *few drops* of alcohol, thrown into a quart of distilled water, could be entirely recovered.

3. That when one part of alcohol was added to three or four thousand of blood, it could likewise be recovered.

4. That in blood somewhat stale, alcohol, added in similar proportion, is *not destroyed within several days.*

Advancing, then, to the solution of the problem, above alluded to, which Bernard had not attempted, with a view of obtaining alcohol, if it existed, from portions of blood, in which, after a certain time, sugar had disappeared, the following experiments were conducted:

I.—Six hundred and twenty-two grammes of ox-blood from the slaughter-house, containing sugar, by the cupro-potassic test (*see* Bernard's "Nouvelle Fonction du Foie," Paris, 1853, p. 22), as it does invariably, was agitated with air in a large flagon, and set aside at a mean temperature of 62° Fahr. for forty hours. At the expiration of this period, sugar had almost entirely disappeared; the blood was distilled over the bath of common salt, and yielded 0.0162 grammes of alcohol.

II.—Six hundred and twenty-two grammes of similar blood, *abundantly* charged with sugar, was set aside, at a mean temperature of 60° for ninety hours. The blood was occasionally agitated, and the air within the vessel renewed from time to time with a bellows. Although sugar was still traceable at the expiration of the above period, the blood was distilled over the common-salt bath; the final distillate weighed 1.2940 grammes, and contained 0.1552 grammes of alcohol.

III.—Nine hundred and thirty grammes of similar blood was introduced into a large glass-stoppered flagon. The blood contained sugar, upon examination. The vacant two-thirds of the flagon was filled with carbonic acid; the blood, well agitated, becoming almost black. The flagon being securely stoppered, melted wax was poured over the line of juncture of the stopper and the mouth of the vessel, and the blood was set aside at a mean temperature of 66° Fahr. for forty-two hours. It was then distilled over the bath of common salt, although sugar was still traceable. The final distillate weighed 1.6173 grammes; alcohol burned in a lively manner in the test-tube; its quantity was 0.0647 grammes.

IV.—Eleven hundred and twenty grammes of similar blood, containing sugar, by examination, was introduced, as before, into a large glass-stoppered flagon, securely shut off from the air, and set aside at a mean temperature of 60° Fahr. for seventy-two hours. The blood was well shaken with the air in the flagon, from time to time, but no fresh air was allowed to enter. After the above lapse of time, the blood was distilled over the bath of common salt; the final distillate weighed 1.4880 grammes. The vapors of alcohol burned in a lively manner in the test-tube; the quantity of alcohol was 0.0446 grammes.

V.—Nine hundred and thirty grammes of similar blood, and containing sugar by examination, was well agitated with air, and set aside at a mean temperature of 58° Fahr. for sixty-five hours. It was then distilled over the bath of common salt. The final distillate weighed 1.0352 grammes; and the vapor of alcohol burned in a lively manner in the test-tube; the quantity of alcohol was 0.0414 grammes.

A table of these experiments is given below:

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Temperature of air (Fahr.)	62°	60°	66°	60°	58°
Quantity of blood (grammes) ..	622	622	930	1120	930
Quantity of alcohol (grammes)..	0.0162	0.1552	0.0647	0.0446	0.0414
Duration of experiments (hours).	40	90	42	72	65

Having determined, therefore, that during the incipient putrefaction of blood the disappearance of glucose is due, in part at least, to its conversion into alcohol and carbonic acid, I concluded that such must have been the mechanism by which it was destroyed in the experiments of Bernard; but, as I was still further desirous of investigating the influence of the first stages of putrefaction upon sugar, in other substances, in one of which, *the liver*, it is always to be found, while Bernard denies its existence in the other, viz., the *tissue of the lungs*, I proceeded as follows:

VI.—Seven hundred and fifty grammes of fresh ox-liver was comminuted in a meat-mincer (used only for these purposes, well washed, and completely dried over the furnace, after each operation), and set aside at a mean temperature of 75° Fahr. for ninety hours. At the end of this period, sugar was no longer discoverable, and putrefaction was well established. While making a decoction of a portion of the tissue in testing for sugar, acid vapors (probably of acetic acid) were given off, the mass itself being strongly *acid* and *effervescing vigorously* with bicarbonate of soda. The mass of liver was now distilled over the common-salt bath, and yielded 0.3235 grammes of alcohol. The final distillate burned continuously at the mouth of the test-tube, upon ebullition, and the chromic-acid test struck with it a vivid emerald-green tint.

VII.—One hundred and ninety grammes of fresh ox-liver was laid aside, sugar being present, by examination, *without* comminution, at a mean temperature of 60° Fahr. for one hundred and twenty hours. When distilled over the bath of salt, it yielded a large quantity of alcohol—the final distillate upon ebullition giving off vapors, which burned continuously at the mouth of the test-tube as long as heat was applied.

Having, therefore, ascertained that the decomposition of sugar in blood and liver-tissue, during the early stages of the putrefactive process is, at least mainly, into alcohol and carbonic acid, I inquired whether the lung-tissue in a state of incipient putrefaction might not furnish alcohol by similar procedures. Sugar is constantly formed in the liver, and borne through the heart into the lungs by the circulatory stream; and, though it disappear in those organs, a minute quantity must be always present, not yet "*destroyed.*"

The large thoracic vessels are almost invariably empty in lung-tissue obtained from the slaughter-house, or are easily emptied of their fluids upon comminution of the material. These drainings I have always rejected, in experiments upon this tissue, using only the superficial (pleural or interlobar) portions of lung-substance, and these only when not over-charged with blood. I determined, therefore, to submit fresh and cortical lung-tissue to incipient putrefaction, in order to ascertain the change occurring in hepatic sugar, as well as to determine its presence in that substance; for, if alcohol should be obtained by distillation, contrary to Bernard's assertions, healthy lung-tissue must really contain sugar.

VIII.—Five hundred grammes of fresh lung-tissue from the ox was comminuted with a pair of curved scissors, to the size of an acorn; the peripheral portions *alone* of the lung-tissue were used; all vessels and bronchial tubes, as well as fluid blood, being rejected. This lung-tissue was set aside at a mean temperature of 60° Fahr. for seventy-two hours. It was then distilled over the salt-bath. The vapor of alcohol burned in a lively manner upon ebullition of the final distillate, in a test-tube. The quantity was 0.0323 grammes.

IX.—Seven hundred and fifty grammes of ox-lung tissue, similarly treated, was set aside at a mean temperature of 66° Fahr. for ninety-six hours. At the end of this period, the odor exhaled was faintly disagreeable. It was then distilled upon the salt-bath. Alcohol was seen to pass over during the final distillations, and, when the distillate was boiled in a test-tube, burned vigorously at its mouth. Its quantity was 0.0485 grammes.

X.—Two thousand one hundred and eighty grammes of ox-lung tissue was set aside at a mean temperature of 76° Fahr. for seventeen hours. Upon distillation over the salt-bath, alcohol was seen to pass over, burning in a test-tube upon ebullition of the final distillate. Its quantity was too small for measurement.

XI.—Five thousand one hundred and sixty grammes of ox-lung tissue was set aside at a mean temperature of 66° Fahr. for sixty-five hours. At the end of this time, putrefaction having fairly set in, it was distilled over the salt-bath. The final distillate weighed 3.8820 grammes, and contained 0.1640 grammes of alcohol; the vapor of this fluid burning in a continuous manner at the mouth of the test-tube after removal of the taper, upon ebullition of the final distillate, and striking a vivid-green tint with the solution of chromic-acid.

A table of these experiments is subjoined:

	No. 8.	No. 9.	No. 10	No. 11.
Temperature of air (Fahr.)	66°	66°	76°	76°
Quantity of lung-tissue (grammes)	500	750	2180	5160
Quantity of alcohol (grammes)	0.0323	0.0485	Least.	0.1640
Duration of experiments (hours)	72	96	17	65

We observe—1: That alcohol was obtained in every case. 2. The least quantity of alcohol was obtained in the experiment whose duration was least; while lung-tissue remaining four times as long as in No. 10, at the *same* mean temperature, furnished a large amount of alcohol. 3. From experiments No. VIII. and No. IX. it will appear that the quantity of alcohol obtained varied as the weight of lung-tissue employed. 4. The results accord entirely with those obtained by a similar use of blood and liver-tissue.

Alcohol is, therefore, present in decomposing lung-tissue, and *sugar*, consequently, exists in the fresh lung-substance. Its quantity must, however, be quite too small for the employment of the cupro-potassic test, used by Bernard. Alcoholic fermentation and distillation is a more delicate test for the presence of sugar in an organic body than any method addressed to sugar directly. Besides, Bernard operated upon the very small quantity of twenty-four to thirty grammes of lung-tissue. I have tested in this "indirect" manner no less than five thousand one hundred and sixty-one grammes. The quantities of most important substances, in the animal body, are frequently so minute that inferences drawn from the examination of small weights of organic matters must be received with great caution.

The liver, the blood, and the lungs, which naturally contain sugar, had now been examined in a state of incipient putrefaction; the results of the experiments, as detailed, evidently warranting the conclusion that, *in decomposing organic matter, sugar, if present, is naturally changed, in great part at least, into alcohol and carbonic acid.*

These experiments had been instituted as a preparation for the important question, shortly to be proposed, concerning the normal presence of alcohol in the organs and blood of animals. The process, by which, after death, organic matter advances to destruction, is similar to that by which, during life, its constituents are conducted through their natural metamorphoses. In both cases, nitrogenous colloids are eventually resolved into crystalloids. The first step is always the absorption of oxygen. If glucose be in contact with a nitrogenous colloid undergoing oxidation, as a preliminary to further change, its elements group themselves into *alcohol* and *carbonic acid.* I have enjoyed quite unusual opportunities for studying the common processes of distillation, and am convinced that Liebig's views are essentially correct. The formation of alcohol in the fermenting-tun begins with the oxidation of gluten, which consists mainly of *vegetable fibrine*, advances with it, *pari passu*, and ceases with it. If animal fibrine be oxidized in the lungs, it must act similarly, and with equal power; indeed, be far more effective, inasmuch as hepatic sugar is greatly more fermentescible than "grape-sugar." Adopting Virchow's views respecting its tissual origin, *fibrine* is emptied by the lymphatics into the venous system, and borne into the lung-capillaries, where it absorbs oxygen with great avidity. It is in the act of decay, or Liebig's "*eremacausis.*" If hepatic sugar be in contact with it at this moment, it must surely be resolved into alcohol and carbonic acid. This alcoholic fermentation must, therefore, be a constant phenomenon, varying directly in rapidity and completeness, as the amount of fibrine present, and the intensity of oxidation.

After oxidation, if fibrine be left to itself, out of the body, it putrefies very soon. Within the organism, it is likewise broken up into simpler bodies, most probably in the liver; for fibrine disappears in this organ, which it reaches by two channels—with a simultaneous formation of glycogen and fat, biliary acids, and probably uric acid. Before advancing, however, to the *experimental test* of such theoretical considerations, I must describe the methods and appliances employed in general, for the determinations already quoted, and those about to be set forth:

Distillation is the only available process for obtaining alcohol from aqueous fluids; it is, moreover, the proper method for experimentation of the kind proposed, as it involves a complete and prompt separation of alcohol, with a simultaneous arrest of all fermentative changes by the necessary use of heat.

I have never attempted any preliminary removal of the solids or coagulable matters of an organic substance from its natural liquids. Tedious and unavoidable filtrations would consume too much time, and tend very materially to invalidate the significance of an experiment. I have always, therefore, submitted organic matters to distillation, without previous admixture, except in certain cases; and, when substances have been mingled with blood before distillation, they have not been intended to effect changes in that fluid of the sort referred to, nor has their employment at any time lengthened the period elapsing between the death of an animal and the elevation of its blood to 212° Fahr. The apparatus varied somewhat with the nature of the experiments. For organic matters, copper balloons, holding from two to sixteen quarts, were employed. Blood, and organic substances generally, cannot be distilled in glass: as soon as heat is applied, a firm coagulation takes place; the mass becomes solid; vapor is generated beneath, and the vessel is broken, although not filled with blood, for example, to more than a tenth of its capacity. On the other hand, where reagents of a corrosive nature have been added to the distilled fluids, and for all fluids not of a coagulable nature, glass balloons alone have been used, varying in capacity from four quarts to one *fluid-drachm.* Liebig-condensers, of sizes proportionate to the bulk of fluid to be distilled, and for the later distillations of a series, traversed by glass tubes, of very small calibre, were used; the tubes reaching, without joints in most cases, by a double bend, from the vessel submitted to heat to the centre of the receiver, which was, if possible, the balloon for the succeeding distillation. As a source of heat, for large quantities, a charcoal-furnace, and for smaller portions of organic material, a lamp, was employed. One-half of a distilland was brought over in the secondary distillations; but, on account of the large quantity of solid matter contained in blood, when the bath of common salt was used, *one-fourth* only was brought over. When the heat of the furnace was *directly* applied, distillate to the amount of one-half of the original weight of the blood or organic matter was drawn over. Fifteen or more successive distillations were frequently required, for the proper concentration and purification of the final distillate. The quantity of material used varied from a pound or less to twenty pounds, each experiment occupying some days. Before every operation, the balloons, both of copper and of glass, were carefully washed in pure water, and thoroughly dried over the furnace, air being drawn into them with a bellows, until the watery vapor was entirely expelled; they were then corked and set aside. All tubes, corks, and funnels, and the tubes of the condensers, were carefully washed, rinsed with distilled water, and thoroughly dried over the furnace, after being wiped with tissue-paper or raw-cotton. In no experiment whatever, reported in this paper, were these precautions neglected; the preparation of the main pieces of the apparatus being made just before the institution of an experiment. Distilled water for *washing precipitates, or for any other purposes, was always boiled until a half of its bulk had evaporated*, and was used when *freshly cooled.* Reagents warranted chemically pure alone, and different parcels of *these*, were employed in successive operations; commercial articles were *never used.* Solutions of salts were made with freshly-boiled distilled water, and the solutions themselves afterward boiled and filtered. By these precautions, *it is inferred that no alcohol from without could by any chance have become mixed with the distilled fluids.* If fluid or froth (under the circumstances of these experiments, containing *sugar*) escaped from the balloon into the tubes or receiver, as occasionally happened when the balloon was too full, the experiment was discontinued; and so likewise for accidental admixture of any sort whatever. When a distillation was commenced, it was invariably completed, unless interrupted by accident, as soon as possible. If loss of vapor took place by leakage of joints, or, if, in consequence of too rapid a distillation, it may have escaped uncondensed through the condenser, thereby involving perhaps a loss of alcohol, the experiment was abandoned. Transference of distillate from vessel to vessel was avoided as much as possible; and when filtrations were requisite the funnels were covered, and the receivers narrow-mouthed. The analytical balance used was imported from Germany; it was provided with platinum weights according to the French metrical system; it was capable of showing the tenth part of a milligramme when lightly loaded. The thermometers employed were carefully proved by introducing their bulbs into the centre of a block of melting ice.

The upper portions of the bodies of the glass-balloons, *throughout the distillations*, were covered with hoods of tinned iron; the first bend of the conducting tubes, and the bodies of the small glass balloons containing distilland, being previously warmed by the flame of a lamp, directed by a large-orificed blow-pipe. This was done to prevent the first portions, coming over, from being condensed by the cold tubes, and trickling back into the distilland.

I pass on now to describe the procedures by which a tolerably pure mixture of water and alcohol may be obtained from organic fluids and solids; always possessing, however, a faint odor, due to the presence, in small quantity, of some peculiar volatile matter.

The organic substance, having been weighed, is introduced into a copper balloon, of a conical shape, with a broad and slightly-rounded bottom, furnished at its apex with a single orifice, which should be no larger than is requisite for the removal of residuum with a hook and scraper. This orifice is closed, during the distillation, with a cork, through which passes the glass-tube reaching to the "Liebig-condenser." The balloon should not be more than one-sixth filled *with blood;* and, if it be shaped as above described, the fluid will be spread, in a shallow layer, upon its broad bottom. It may, however, for lung-tissue, or liver-tissue, be filled to one-half, or even *more*, of its capacity; too great a repletion should be generally avoided, however. As a general rule, organic matters, and especially blood, should be spread out as thinly as possible upon the heating surface, as they are poor conductors. Baths of sand, of common salt, or of chloride of calcium, may be used or not; but, in all cases, strong heat and compact vessels are necessary. Upon the application of heat, blood becomes solid, and, being perforated by gases and vapors, assumes a honeycomb texture, and is thus rendered very inefficient in the conduction of heat from without. As this happens very soon, and first takes place next the bottom of the balloon, the conduction of heat is so much impeded that, when the fluid baths are used, the distillation proceeds slowly; but, when the balloon is placed directly upon the furnace, its whole contents yield up their vapors, and the blood becomes dry, crumbly, and perforated by sinuous vacuities throughout its entire mass. Although a certain quantity of empyreumatic matter, not so much, however, as might be imagined (even when one-half of the weight of the organic substance is brought over), is generated by the *direct* exposure of the copper balloon to the heat of the furnace, yet, as it may be afterward removed, the quickness and completeness of this method, as well as the rapid coagulation of albuminoid matters which it involves, and the prompt arrest of all *post-mortem* changes, outweigh its inconveniences.

A. When heat is directly applied in this way, the first distillate comes over opalescent, and strongly alkaline. It contains free ammonia, carbonate of ammonia, sulphuretted hydrogen (especially when decomposing matters are experimented upon), two or three oily substances, and one concrete oily body; a very volatile fluid coming over *before* general ebullition, and perhaps some acid substances in union with ammonia. For the separation of these matters, the distillate is treated in the following manner:

1. Hydrochloric acid is added in considerable excess, and the fluid set aside for twelve hours; the acid fixes ammonia, drives off carbonic acid in large quantity, and combines with some of the empyreumatic bodies, forming compounds which impart a bright-pink hue to the liquid; drops of animal tar, which floated upon the distillate, are found to have disappeared, after the expiration of the above period. The distillate is then distilled in a glass vessel.

2. The distilled fluid, newly brought over, is treated with excess of a cold saturated solution of acetate of lead; five per cent. of its bulk of this solution is generally sufficient. A brownish or coffee-colored precipitate is thrown down, of sulphide and chloride of lead. The fluid is then filtered through a moistened filter, and the precipitate washed. Since the filtrate contains an excess of acetate of lead, in order to decompose this, as well as to remove empyreumatic matters, which this acid combines with and fixes in the distilland, hy-

drochloric acid is again added in considerable excess, and the chloride of lead thrown down is removed by another filtration; after due washing of the precipitate, the washings being added to the distillate, it is distilled. Upon ebullition the distilland changes from transparency to a madeira-tint, passing into dark red.

3. The distillate comes over milky, at first, but gradually clears up. Hydrochloric acid is added as before, several times in succession; when heated, the distillate always becomes colored, gradually, however, less and less so, as more of the empyreuma remains behind—*this treatment* is continued until the transparency of the *distilland* remains nearly or quite unchanged throughout its ebullition. The distillate is thus concentrated, and foreign matter in great part, though not entirely, separated.

4. A concrete, shining, yellowish, fatty-looking substance is still seen in the distillate, which is removed by adding a few grains of crystalline nitrate of silver, filtering and washing as usual. Upon ebullition, the distilland becomes dark, oxide of silver, with metallic silver also, most probably, being thrown down. Neither the concrete matter above described, nor a volatile substance, which up to this time had made its appearance in the tubes, just before ebullition, and which I suspect to be *aldehyde*, is seen again, in most cases. If either of them should pass over, the treatment by nitrate of silver must be repeated. If alcohol be present, it now begins to show its *characteristic* play in the conducting-tubes, *during* the first moments of ebullition.

5. The distillates still continue to come over somewhat milky at first, owing to the suspension of a pale-yellow oil in the liquid; drops of fluid oil are seen to float upon its surface. By two or three filtrations through *freshly-calcined* animal charcoal, and distillation in a very small apparatus, these oils are finally separated, and the liquid sufficiently concentrated and purified for examination. This *final distillate* should weigh from fifteen to thirty grains; *should have come over perfectly transparent from the commencement of the final distillation; must be neutral to litmus-paper*, or may be rendered so by distillation over a little precipitated chalk; and will be found to possess a faint odor.

B. When baths containing common salt, chloride of calcium, oil, or other fluids, are interposed between the copper balloon and the furnace, the primary distillate comes over transparent from the first, and, as no empyreuma is generated, its rectification is expeditious and simple. If semi-putrescent bodies have been distilled, hydrochloric acid must be employed to fix ammonia; acetate of lead, to decompose sulphides, and animal charcoal for the separation of whatever volatile oils may be present, in the same way as already described, for the distillate obtained by *direct heat.* When *fresh* animal matters are distilled, a *few* repetitions of these procedures will suffice to purify the final distillate. They must, however, be repeated in all cases, until the liquid possesses the characters above set forth.

The final distillate is now carefully weighed in a very small glass balloon; its weight should not ordinarily exceed thirty grains.

C. Its *specific gravity* must now be determined, on account of its small quantity, and, as great accuracy is required, ordinary methods of determining specific gravities are not admissible; I have therefore employed a mode of doing this which was similar in principle to that by the common specific-gravity bottle, though far more delicate. A thermometer with a very fine bore, and a bulb not more than one-third of an inch in diameter, was selected, and the tube cut off and polished at the 120° Fahr. The mercury having been expelled, the tube was filled in the ordinary manner with freshly-boiled distilled water, two or three times successively. Being thus well rinsed out, while filled again with water, its bulb was held over the flame of a small spirit-lamp, and, as soon as the boiling liquid had been entirely ejected, its extremity was suddenly immersed into the distillate. It was then in like manner filled, two or three times, with the distillate, in order that any remaining vapor of water might be so diluted on the mass of the distillate as to give a uniform specific gravity to the liquid, within and without the bulb tube. Care was taken to expel the minute globules of air which are apt to remain in the tube; after their complete expulsion, and while the fluid in the bulb-tube *was expanding*, its point was immersed beneath the surface of the distillate, and it was allowed to cool and fill itself, at the temperature of the air. A glass jar of water being provided, about four inches in diameter, and nine inches high, and a thermometer suspended in it, the water was brought to 62° Fahr. if the weather was cool, and to 58° if the air was above 60°; the specific gravity being taken for this latter temperature. The bulb-tube, completely filled with distillate, was now attached to a thread so arranged as to allow its bulb to lie in the same horizontal stratum of water in which that of the standard thermometer was situated, when submerged for the whole length of its stem except about a quarter of an inch. Thus suspended, *before being placed in the jar*, the bulb was held for a moment over a flame, and when a minute drop of its contents appeared, in consequence of expansion, upon its extremity, *a drop* of final distillate, from the end of a glass rod, was added to it, and the bulb-tube at once lowered to its proper place into the jar of water, where it hung by the side of the standard thermometer. It was allowed to remain until the water of the jar had risen or fallen to 60° Fahr.; and when the thermometer showed exactly this temperature, the *drop upon the extremity* of the bulb-tube was suddenly wiped off; the tube itself removed, rapidly dried with a fine linen cloth, and immediately weighed. The weight of the bulb-tube filled with distilled water at 60° being previously ascertained by a similar process, the weight of the empty bulb-tube being also known, its weight at 60°, when filled with a final distillate, furnished all the data necessary for the determination of the specific gravity. Exactly equal bulks of very small quantities of liquid can thus be measured with great accuracy, without appreciable loss by evaporation, or gain in weight by precipitation of dewy particles, in warm weather, during the weighing; for in the glass-case of the balance, always shut, the air was kept constantly dry by chloride of calcium or sulphuric acid. The table of Drinkwater furnished the percentage, by weight, of absolute alcohol, in any aqueous fluid, corresponding to the specific gravity, up to ten per cent.; a range amply sufficient for my purposes.

The specific gravity having been thus determined, and the weight of the entire final distillate being known, a simple calculation gave the quantity of alcohol present in the distillate. In order to apply certain tests, however, the fluid was expelled by heat from the bulb-tube into the vessel from which it had been originally taken; the quantity of final distillate, being thus undiminished, was always sufficient to allow the application of the following *qualitative* tests:

1. *Chromic Acid dissolved in Sulphuric Acid.*—By adding 0.25 grammes of crystallized bichromate of potassa to 100 grammes of concentrated and chemically pure sulphuric acid, the salt is decomposed, chromic acid liberated, and a rich brownish-yellow liquid obtained. When to any fluid, containing the feeblest trace of alcohol, an equal bulk of this reagent is added, much heat is evolved, and, by deoxidation of the chromic acid, sesquioxide of chrome is produced, imparting to the fluid an emerald-green hue. This is an exceedingly delicate and reliable test; nothing similar occurs with other volatile fluids, obtained from fermented or organic matters; sugar, dextrine, and gum, uric acid, urea, and albumen, equally effect this reaction, but must be obviously absent in distilled fluids.

2. *Inflammation of the Vapor of Alcohol in a Test-tube upon Ebullition of the Final Distillate.*—This most striking and important test I have found applicable when not less than two per cent. of alcohol exists in an aqueous fluid; below this percentage the alcoholic vapor is too much diluted with steam to burn even at the first moments of ebullition. Some care is requisite in the use of this test. The quantity of liquid should be about one-tenth of the capacity of the test-tube; the flame over which the tube is held should be very moderate and steady, and should give but little light; the room should be darkened; a wax-taper ready lighted should be held just above the upper edge of the mouth of the tube; the entire length of the test-tube should be heated before the liquid is made to boil, and the tube held in an oblique position, by a bit of bent wire, that it may be closely watched, as the least discomposure may prevent our observing the inflammation of the vapor, which is sometimes only momentary. When all is properly done, if alcohol is present in more than one and a half or two per cent., a flash of flame is seen to descend into the tube at the moment of ebullition, or a little after it; and, if alcohol be

present in greater quantity, the vapor continues, for a longer or shorter time, to burn at the mouth of the tube, during the continued boiling, with a characteristic flame. No other substance met with in the distillate of animal matters is capable of inflammation in this way. The *primary distillate*, and *all successive ones*, until the last portions are reached, constantly refuse to burn as above described, no matter how abundant the empyreumatic substances may be. Some organic acids, as formic and acetic, are inflammable, in the state of vapor, but only when pure or nearly so; the greater part, however, of the final distillate of these processes, is water, and *acids are not* present, as it is neutral or *must be made so*, previously to weighing or testing.

3. *The Optical Appearance within the Conducting-Tubes, when the Distilland just begins to boil.*—Alcohol comes over in greatest strength in the first moments of distillation, and, if present, may always be seen to play in a mobile manner just in advance of the watery dew-drops, as both progress rapidly along the interior of the conducting-tube. To observe this peculiar play of the successively condensing and vaporizing alcohol, it is essential that the drops of water, previously deposited from the humid air within the interior of the tube, should have been dissipated by a gentle heat from the flame of a lamp; the conducting-tube being allowed to cool again before ebullition. This appearance is of course only to be observed between the balloon and the condenser. It is a trustworthy guide for very minute quantities of alcohol, and should be studied in the distillation of dilute mixtures of alcohol and water. If carefully looked for and not observed, alcohol is hardly present; especially as the terminal distillations of a series are approached. Other volatile substances likewise condense at the first bend of the conducting-tube, but their "play" is entirely different, and they invariably *precede* the appearance due to alcohol; they are, moreover, often milky, or yellowish, when direct heat has been applied to the organic matter experimented on. It is hardly necessary to remark that the purification of the final distillate *is not complete*, so long as this ante-alcoholic "play" is observable.

Such, then, are the qualitative tests upon which I have mainly relied: whenever a final distillate, whose specific gravity was less than that of water, fulfilled their requirements, I have considered the presence of alcohol established. By the practical experience, moreover, of several years of experimentation, in a great number of experiments similar to those quoted from my journal in this paper, as well as by determination of the expansibility of some strong distillates, the reasonable skepticism which I have always endeavored to maintain has been entirely dissipated. Since these experiments were concluded, spectrum analysis has furnished to science its wonderful determinative powers; not having been able to apply this mode of research to the present inquiries, I can only suggest its application to the distilled fluids, and to their *flame produced* as above described. I now proceed to detail some of the experiments directed toward a solution of the question whether alcohol exists normally in the blood and tissues of animals.

I have already shown that alcohol can be obtained from organic matters, originally containing sugar, after incipient putrefaction, and have stated that *post-mortem* changes are not different in *their first stages* from those taking place in the living body; if, therefore, the alcoholic fermentation really occurs within the organism during life, all blood, whether capillary, or contained in the larger vessels, which naturally contains sugar, should likewise yield alcohol by distillation. As far as practicable, I endeavored to submit organic matters to the distillatory process, within the shortest possible time, so as to prevent any *post-mortem* fermentation—with what success the subjoined experiments will show:

XII.—Seven hundred and fifty grammes of healthy ox lung-tissue was comminuted in a meat-mincer, and raised, over the bath of common salt, to 212° Fahr. in ninety minutes after the animal was struck down. Alcohol was seen to pass over during the later distillations. The final distillate burned within the test-tube upon ebullition, but feebly. The quantity of alcohol was 0.0259 grammes.

XIII.—Eight hundred and seventy grammes of ox lung-tissue, while still quite warm, was comminuted as above, and raised, over the bath of common salt, to 212° Fahr. in fifty-five minutes after the animal was struck down. The tissue was normally full of blood. During the later distillations, alcohol was seen to pass over. The final distillate weighed 0.8411 grammes, and burned, though faintly, in a test-tube upon ebullition. The quantity of alcohol was 0.0168 grammes.

XIV.—Twelve hundred and forty grammes of ox lung-tissue, comminuted to the size of a pigeon's-egg, was raised, over the bath of common salt, to 212° Fahr. in seventy-five minutes after the animal was struck down. The tissue was normally full of blood. During the later distillations alcohol was seen to pass over. The final distillate weighed 0.6470 grammes, and contained 0.0453 grammes of alcohol; it struck a vivid emerald-green tint with the chromic-acid test.

Alcohol may, therefore, be obtained from lung-tissue within fifty-five minutes after the death of the animal. Its quantity may seem minute, but must be so for considerations already urged, and is, indeed, as I shall hereafter show, quite as large as we have any right to expect. We are dealing with essentially small quantities; nor is alcohol by any means unique in this respect. The determination of glucose in the intestines presents quite an analogous case. We are tolerably well acquainted, at the present day, with the changes occurring in alimentary substances, and the agencies of the several secretions with which they are mingled after being taken into the mouth, although much remains doubtful, notwithstanding exhaustive researches in this department. After rupture of its envelope and hydration in the stomach, starch is passed into the duodenum, and is acted upon by the pancreatic juice, and the secretion of Brunner's glandules, and during its progress through the intestines by their proper juices. A prompt conversion into dextrine and partly into glucose takes place, but starch and dextrine may be recognized throughout the whole length of the intestinal canal. The change from dextrine into glucose is especially slow. This is likewise the case in the saccharification of starch by acids, where I have found that the complete conversion of dextrine into grape-sugar requires about eight times as long a maceration, as to convert the original starch into dextrine. In the intestines, as soon as gluclose is formed, it is absorbed by the veinules of their walls, and so rapidly, that it is hardly possible to find it, except in the midst of the intestinal contents. The quantity present, therefore, at a given moment must be exceedingly minute. For, if 11,000 grammes (23 oz.) of glucose is distributed through 384 inches (32 feet) of intestine during twenty-four hours, the whole intestinal canal can only contain eight grains during any one minute ; and any linear inch, during the same time, but $\frac{4}{384} = 0.0209$ of a grain, while this is rapidly removed by absorption. The rapidity of absorption in this instance is represented, in the case of sugar and alcohol, by the velocity of the circulation, and the extreme dilution of the saccharine blood of the hepatic veins by that from other sources. Inasmuch, however, as the alcohol obtained was doubtless derived from the blood of the capillaries and small veins of the lung-tissue, and as this blood must be added to that from the great vessels, when they are freely incised, as in the mode of slaughtering commonly practised, and as I had already determined the presence of alcohol in such blood when putrescent, in order to obtain an absolutely greater quantity of alcohol, I determined to employ it. In the mode of slaughtering which I have witnessed, the animal is struck upon the head, and, as it falls, the knife is passed across its throat just above the sternum, and introduced with free lateral movements into the thoracic cavity. The flow of blood is overwhelming, ten pounds escaping within a second or two. This is derived, apparently, from the innominate veins and vena cava, and in some degree from the parenchyma of the lungs. The aorta and its greater branches must likewise be frequently wounded. The slaughtering was often superintended in person, and the time of its occurrence carefully noted. The blood was received directly from the throat of the animal into a vessel constructed for the purpose, previously dried over the furnace, after careful washing with boiled distilled water, and wiping with linen and tissue-paper.

XV.—Six thousand nine hundred and seventy grammes of ox-blood was raised to 212° Fahr. in sixty minutes after the animal was struck down. When set upon the furnace the blood was quite warm, coagulated, and of a dark-red color. The distillation was conducted over the salt-bath. Alcohol was seen to pass over during the terminal distillations, and its vapor burned in the most satisfactory manner in the test-tube upon ebullition of the final distillate, which also struck a bright-green tint with the chromic-acid test. The quantity of alcohol was 0.0650 grammes.

XVI.—Nine thousand seven hundred and thirty-four grammes of ox-blood was raised to 212° Fahr. in fifty-six minutes after the animals, two in number, were struck down. The temperature of the blood when set upon the furnace was 101° Fahr. During the terminal distillations alcohol was seen to pass over. The final distillate weighed 0.8416 grammes, and struck the green tint characteristic of alcohol with the chromic-acid solution. The quantity of alcohol obtained was 0.0198 grammes.

Since it is possible that the oxidizing power of the blood might destroy, within a few moments after death, a certain portion of alcohol formed during life, if this oxidizing power could be abolished by the addition of some chemical agent to the blood, at the moment of its extraction, a greater quantity of alcohol should be obtained. Sulphuretted hydrogen, we have reason to believe, by combining with the hæmato-globulin of the red corpuscles, is able to act in this way; it was, therefore, employed as below.

XVII.—Nine thousand one hundred and thirty-seven grammes of ox-blood, from two animals, was used in this experiment, and raised to 212° Fahr. in seventy minutes after the animals were struck down; 250 grammes of a saturated solu, tion of pure and dry sulphuretted hydrogen in boiled distilled water was added, portion for portion, to the blood, during its collection. The blood coagulated loosely and became very dark, or even black. Notwithstanding the addition to the blood of so much liquid, at about 70° Fahr., the temperature of the whole mass when set upon the furnace was 100.3° Fahr. Alcohol was seen to pass over during the terminal distillations. The final distillate weighed 1.6218 grammes, and struck the usual tint with chromic acid. The vapor of alcohol burned within the test-tube, and at its mouth, when the final distillate was boiled; its quantity was 0.0605 grammes.

XVIII.—Nine thousand two hundred and thirty-six grammes of ox-blood, from two animals, was raised to 212°

Fahr. in seventy-seven minutes after the animals were struck down; 250 grammes of a similar solution of sulphuretted hydrogen was added as before. The blood coagulated loosely, and became very dark. Its temperature when set upon the furnace was 99.5° Fahr. Alcohol was seen to pass over during the terminal distillations. The final distillate weighed 3.6130 grammes, and struck the usual tint with chromic acid; when boiled in a test-tube, the vapor of alcohol burned satisfactorily. The quantity of alcohol was 0.0444 grammes.

As the main data of XIX., XX., and XXI., will be presented shortly in a tabulated statement, I will omit their detail, passing on to

XXII.—In order to illustrate the successive steps of the distillation when *direct* heat was applied to the copper balloon, and also the procedures of rectification and dosage, I quote this experiment *in extenso* from my journal:

February 22, 1858.—Without HS. Blood firmly coagulated. Two oxen struck down at 3.43 P. M. Temperature of blood when set upon furnace, 98° Fahr.; 9,112 grammes used. Set on furnace without bath at 4.8 P. M. Raised to 212° Fahr. at 4.44 P. M., sixty-one minutes after the animals were struck down; 1,555 grammes brought over at the first distillation, which occupied six and a half hours. No froth passed over. H.Cl. added in excess, and, when effervescence had ceased, the liquid was set aside until the following morning.

February 23*d.*—Distilled in glass over sand-bath. The color of the distilland, which had been a bright pink, darkened to an ochreous hue during the ebullition. The second distillate came over slightly milky, remaining afterward somewhat cloudy. Excess of acetate of lead was added, about 30 grammes of the saturated solution being required. A coffee-colored precipitate was thrown down; filtered; came through quite transparent.

February 24*th.* — H.Cl. added in considerable excess. Dense white precipitate of chloride of lead thrown down; filtered, and the precipitate washed; came through perfectly clear. Distilled in glass over sand-bath. Upon ebullition assumed a Madeira-wine tint. The third distillate came over somewhat cloudy; reaction strongly acid. H.Cl. added in considerable quantity; distilled; upon ebullition assumed a Madeira-wine tint. The fourth distillate came over, at first, quite milky; drops of light-yellow oil floating upon its surface; distillate became gradually more transparent, but remained slightly cloudy; H.Cl. added; distilled; upon ebullition, assumed an amber-hue. The fifth distillate came over, at first, quite milky, clearing up as the distillation progressed, and becoming finally almost completely transparent; reaction acid; treated with newly-calcined animal charcoal; filtered through a moistened filter, and washed; came through clear and *neutral*. Distilled without addition of H.Cl. The sixth distillate came over transparent from the first; a crystal of nitrate of silver being thrown into it, it threw down a dense white precipitate; filtered and washed, came through clear; distilled; at the moment of ebullition, the distilland assumed a brownish tint, and, after some time, a black deposit was precipitated within the balloon, the fluid becoming clearer. The mobile substance up to this time seen in the conducting-tubes, *just before* the distilland began to boil, was not again observed. The seventh distillate came over perfectly transparent from the first; alcohol was seen to pass over during the first moments of ebullition. The eighth distillate came over transparent from the first; weighed two grammes nearly; set aside until the following morning.

February 25th.—Filtered anew through animal charcoal, recently calcined; washed; came through perfectly transparent and neutral; distilled. The ninth distillate, tenth, eleventh, and twelfth (final) distillates, all came over from the first, perfectly clear; alcohol being seen to pass over at each distillation. The quantity of alcohol present was determined as follows:

Weight of final distillate	0.9552	gram's.
" empty bulb-tube	5.4856	"
" bulb-tube filled with final distillate at 60° Fahr.	6.0426	"
" bulb-tube filled with distilled water at 60° Fahr.	6.0463	"
" final distillate contained in bulb-tube at 60° Fahr.	0.5570	"
" distilled water " " "	0.5607	"
Specific gravity of final distillate at 60° Fahr.	0.9934	
This spec. grav. by Drinkwater indicates pr. ct. by weight.	0.0367	
Weight of alcohol in entire final distillate	0.0350	gram's.

The chromic-acid test gave the green reaction characteristic of alcohol. The vapor of alcohol burned satisfactorily in the test-tube upon ebullition of the final distillate, *remaining inflamed at the mouth of the tube, after the removal of the taper, and being relighted several times, as the liquid was made repeatedly to boil.*

In the preceding eight experiments, 1,500 grammes of first distillate was in each case brought over, from about 9,000 of blood. As a portion of alcohol might still remain in the organic matter, *all* the alcohol of a spirituous liquid being driven over only by the evaporation of rather *more than half* the original bulk, it was determined to bring over 4,650 grammes of first distillate in a series of operations. To do this, about seven hours' distillation was necessary. The procedure was as follows:

XXIII.—Twenty-seven thousand three hundred and thirty grammes of ox-blood was distilled in portions of about 9,000 grammes each—the respective intervals between death and elevation of the blood to 212° Fahr. being 50m., 45m., and 50m. About 4,700 grammes of first distillate in each case brought over. The first distillates were rectified and concentrated to 120, 130, and 500 grammes. The three fluids were mixed and treated as usual.

The conjoint final distillate weighed 10.6883 grammes. It struck a bright-green tint with the chromic-acid test; when boiled in a test-tube, the vapor of alcohol burned satisfactorily, remaining lighted after the removal of the taper. Not more than a gramme was thus used; the remainder was set aside. The quantity of alcohol was 0.2928 grammes.

XXIV.—Thirty-six thousand three hundred grammes of ox-blood was distilled in portions of about 9,000 grammes each. The intervals between death and elevation of the blood to 212° Fahr. were respectively 55m., 56m., 50m., and 45m.; 4.900 grammes of first distillate was in each case brought over. Each first distillate was rectified and distilled down to about 120 grammes. The four resulting fluids were then mixed, as in the last experiment, and distilled.

The conjoint final distillate weighed 14.0606 grammes, manifesting the characteristic reaction of alcohol with the chromic-acid test; the vapor of alcohol burning as before in the test-tube and at its mouth. The quantity of alcohol obtained was 0.5652 grammes. The final distillates of experiments XXIII. and XXIV. were mixed and set aside, a crystal of nitrate of silver having been thrown in for *eleven months.* At the end of this time, the liquid contained a *slight* black precipitate, and, upon concentration by two distillations to a specific gravity of 0.986, upon ebullition in the test-tube, the vapor of alcohol burned at its mouth and within it, as before, though with far greater intensity.

Such are the procedures by which I was able to obtain alcohol from mixed thoracic blood; I have never failed to do so. Notwithstanding all efforts, the final distillate retains a peculiar and disagreeable odor. Bernard found that the alcohols obtained by the reaction of yeast upon liver-decoctions possessed in each case an unpleasant odor, especially when prepared from the liver of fishes.

The most important data of the last ten experiments are subjoined:

No.	Weight of Blood.	Interval from Death to 212°.	Temperature when distilled.	Weight of First Distillation.	Weight of Final Distillate.	Weight of Alcohol obtained.	Weight of Alcohol for 10.000 parts of Blood.	With or without H. S.
15	6970	60 m.	....	....	......	0.0650	0.0932	without.
16	9734	56	101°	1602	0.8416	0.0198	0.0203	without.
17	9137	70	100.3°	1636	1.6218	0.0605	0.0662	with.
18	9236	77	99.5	1623	3.6130	0.0444	0.0480	with.
19	8988	60	99	1555	2.6092	0.1357	0.1509	with.
20	8854	60	98	1555	1.7320	0.0760	0.0858	with.
21	9423	45	96	1560	1.8722	0.0708	0.0751	without.
22	9112	61	98	1550	0 9552	0.0350	0.0384	without.
23	27330	48	98	14050	10.6883	0.2928	0.1071	with.
24	36300	51	99	17600	14.0606	0.5652	0.1556	with.

The mean quantity of alcohol obtained for 10,000 parts of blood, when sulphuretted hydrogen was *not* added at the moment of its collection, was by the above table 0.0567; and when it *was* so used, 0.1028; or about twice as much. Sulphuretted hydrogen seems, therefore, to destroy the oxidizing powers of the blood (of its corpuscles). The solution of this gas in all the later experiments was placed in the blood-receiver, so that, when this was held to the throat of the animal, the blood was, I may say, instantaneously mingled with it. The blood-receiver was a vessel of tinned iron in the shape of a cylinder whose altitude was four and a half inches, and base twelve inches in diameter. It was entirely closed above, except an opening of four and a half inches across, fitted with a cover; this orifice was situated as near as possible to the edge of the vessel, so that it might be conveniently held to the wound in the neck.

According to the hypothesis under which the preceding experimentation was conducted, alcohol should coexist with sugar in the circulation. As sugar is formed in the cells of the liver and thence passed into the hepatic veins, its decomposition should *begin* even before the blood escapes from the organ, and alcohol should be obtainable by the distillation of recent liver-substance. When we recollect that alcohol can be present only in the *capillary blood* of the liver, as the hepatic veins always stand agape, emptying themselves spontaneously when the organ is excised, we will be prepared to find only very small quantities. I subjoin the following experiments on this point:

XXV.—Two thousand nine hundred and eighty-five grammes of ox-liver tissue raised to 212° Fahr. in forty minutes after the animal was struck down: 1,400 grammes of first distillate brought over. The final distillate weighed 1.1050 grammes; its specific gravity was 0.9989, corresponding to 0.0064 grammes of alcohol. This quantity of alcohol could not have been inflammable in the test-tube, but the final distillate was colored a bright green by the chromic-acid test.

The quantity of organic substance experimented upon was no doubt too small.

XXVI.—The entire liver of a dog, weighing 650 grammes, the animal having been killed by puncture of the medulla, was raised to 212° Fahr. in *seven* minutes after the sacrifice of the animal. It was distilled, and the distillates concentrated and purified as usual. The final distillate weighed 0.8600 grammes, and struck a bright emerald-green tint with the chromic-acid test, but did not burn in the test-tube. The quantity of alcohol was 0.0024 grammes. The result of this experiment upon the liver of a different animal led me to similar conclusions. The quantity of liver-substance used was obviously too small. A much larger portion was, therefore, employed as follows:

XXVII.—Eighteen thousand grammes of fresh ox-liver tissue was distilled in three portions:

1. Six thousand grammes raised to 212° Fahr. in thirty-five minutes after the animal was struck down—1,550 grammes of first distillate being brought over. H.Cl. added and distilled down to one-fourth.

2. Six thousand grammes raised to 212° Fahr. in forty minutes after the animals were struck down. A like quantity of first distillate was brought over, and was similarly treated.

3. Six thousand grammes raised to 212° in forty-five minutes after the animals were struck down. A like quantity of first distillate was brought over, and similarly treated.

These three fluids were mixed and concentrated and purified as usual. The final distillate weighed 1.2304 grammes. It struck a vivid green with the chromic-acid solution. The *vapor of alcohol burned brightly and repeatedly in the test-tube.* The quantity of alcohol was 0.0324 grammes. Its ratio to 10,000 parts of liver-tissue was 0.0180 grammes.

By referring to experiments XII., XIII., and XIV., it will be found that the ratio of alcohol obtained to 10,000 parts of fresh lung-tissue was 0.3084 grammes. Normal lung-tissue, therefore, contains about *seventeen times* as much alcohol as the liver-substance. As in both cases the alcohol should exist in the capillary blood, the lung-tissue must contain a greater proportion of such blood—a supposition which harmonizes with what we know of the great vascularity of the lungs; but as this consideration could not, I think, altogether suffice to explain the great differences observed, I am disposed to credit them, likewise, to a highly-developed fermentative power of the lungs, with which the air they contain, even shortly after death, is much concerned.

In the experiments thus far cited, alcohol had been sought in blood and tissues in which sugar always exists, upon the supposition that, if sugar were present, alcohol should have been likewise. During digestion, as Bernard has established, hepatic sugar is disseminated throughout the system. We may suppose that, in consequence of a temporary superabundance, it cannot be entirely fermented before it reaches the left ventricle, and that it is, therefore, propelled in the bloodtorrent to all the tissues. The liver exerts a kind of assimilative influence upon the glucose absorbed by the radicles of the vena portæ from the intestines, in virtue of which, intestinal glucose is invested with the peculiar fermentescibility of hepatic sugar proper; while the liver seems itself to act during digestion with especial vigor, so that its saccharine product is nearly twice as abundant in the hepatic veins as during the fasting state (Bernard, *loc cit.*, p. 1118). However it be destroyed, therefore, we find that, during digestion, instead of disappearing before the blood reaches the pulmonary veins, sugar passes into the general circulation and is distributed to all the organs. If, now, alcohol be the unfailing attendant of sugar in the blood, we should be able to find it during digestion in the systemic blood and in all the tissues; though it be obtainable, when the animal *is fasting*, only between the liver-cells and the pulmonary veins. Experimenting as follows upon this point, I have found traces of alcohol in the systemic blood during digestion.

XXXI.—Thirteen hundred and sixty-eight grammes of the pancreatic tissue of the ox, rapidly and carefully deprived of accompanying adipose tissue, the animals being in digestion (fodder and grass), was raised to 212° Fahr. in sixty minutes after the animals (two) were struck down. The final distillate weighed 1.0246 grammes, and struck a vivid emerald-green tint with the chromic-acid test. The quantity of alcohol was very small and was not estimated, nor did its vapor burn in the test-tube.

XXXII.—Two hundred and eighty grammes of blood were drawn from the cephalic extremity of the jugular vein of a small bitch, and raised to 212° Fahr. in *ten* (10) minutes after the vein was opened. The animal was in full digestion. The final distillate weighed 0.6000 grammes, and struck a bright emerald-green tint with the chromic-acid test. The quantity of alcohol was too minute for inflammation in the test-tube.

Having already exceeded the intended limits of this paper, for which I must beg the indulgence of my readers and of the editor, I will conclude with a comparative statement of the results obtained, and a few remarks concerning the mode of destruction of alcohol in the economy, and its purposes.

It has been premised that, even if oxidation were in complete abeyance, not more than *half a part* of alcohol could exist, at any given moment, in ten thousand of pulmonary blood. This statement was based upon the result of a double calculation, whose elements were so different that their common evaluation must be accepted as something *very near the truth.* These two figures I now subjoin, with those exhibiting the results practically obtained in the experiments detailed, all for 10,000 parts, as follows:

Alcohol in the capillary blood of the lungs—		
	calculation based on "*carbon ingested*"	0.5403
	" " "*carbon exhaled*"	0.5794
"	putrescent lung-tissue (mean of ex. 8, 9, and 11)	0.3819
"	fresh " (" " 12, 13, and 14)	0.3076
"	putrescent thoracic blood (mean of ex. 1, 2, 3, 4, and 5)	0.7625
"	fresh " (mean of table)	0.0841
"	putrescent silver-tissue (ex. 6)	4.3138
"	fresh " (mean of ex. 25, 26, and 27)	0.0190

An examination of the above figures will show:

1. That from putrescent liver-tissue the largest yield was obtained. Much of the alcohol in this case must have been lost by acetification, and partly, no doubt, by transformation into lactic acid and other products.

2. That the least quantity obtained was from fresh liver-tissue. It would appear that the fermentation of hepatic sugar merely *begins* in the hepatic capillaries.

3. That a far greater quantity of alcohol was obtained from blood in a state of incipient putrefaction than from fresh blood—about nine times as much. Under the circumstances of the slaughtering, blood from the inferior cava, laden with sugar, must become mingled with that from the lungs and other great vessels; it may even escape upward through the auricle and superior cava: mixed thoracic blood must, therefore, contain a large quantity of alcohol after *putrefaction* has advanced sufficiently far to cause the fermentation of all its original sugar. On the other hand, as *fresh thoracic blood* cannot be supposed to have traversed the lungs, it must only contain a small proportion of alcohol, for its sugar has *not been yet fermented;* it must likewise have been largely diluted with aortic blood, which, in the *fasting* state

especially, contains no sugar, nor alcohol, probably. As the thoracic blood employed in ten of these experiments should be composed of blood from many different vessels, which are divided at random by the knife, it must contain very variable quantities of sugar and alcohol, always, of course, however, *less* than the blood of the inferior cava.

4. That the greatest amount of alcohol obtained from any of the recent substances was recovered from fresh lung-tissue. Lung-tissue newly extracted from the body retains about half its weight of blood the number indicating alcohol should, therefore, be *doubled;* this would show that about as much alcohol had been actually procured as calculation warrants us to suppose present. Contrary to what we have just stated with regard to fresh and putrescent blood, we find that *fresh lung-tissue* contains nearly as much alcohol as *putrescent lung-tissue.* The figures show that only about *a fifth* of the sugar originally present remained unfermented after seventy-three minutes, when the fresh tissue was distilled; while no greater increase than *this fifth* took place in the seventy-seven hours during which similar tissues were submitted to putrefaction. Fermentation seems thus to be more active in the lungs *immediately after death*, and probably at its *highest term* during life. This deduction receives tangible support from the fact just now stated, viz., that in *fresh thoracic blood*, which the test by decomposition proves to be rich in sugar, *very little alcohol* is to be found; such blood has mostly been derived from the great vessels, and has not yet passed through the lung-capillaries; its sugar has not consequently been fermented. Other considerations, in too great variety to mention here, would lead us to conclude that the capillary system in the lungs is the proper seat of the alcoholic fermentation. As this must vary directly as the oxidation of its ferment (fibrine), if the aëration of the blood be impeded or arrested by the action of toxic or medicinal agents, such as woorara, the ethers, chloroform, etc., or by injury to the M. Flourens's "*nodus vitalis*," which paralyzes the pulmonary vaso-motor nerves, and induces pulmonic congestion, sugar becomes so abundant in the circulation as to pass into the urine—the animal becomes glycosuric. Alvaro Reynose has published a very interesting little

pamphlet on this point ("*Mémoire sur la Présence du Sucre dans les Urines*," Paris, 1853), in which he seems disposed to accept M. Mialhe's well-known hypothesis of saccharine oxidation in the lungs, in virtue of the alkalinity of the blood. The hypothesis of the alcoholic fermentation of hepatic glucose in the pulmonary capillaries is quite adequate to explain the above phenomena: an animal becomes glycosuric in cases like those alluded to, *not* because glucose cannot be oxidized in the lungs for want of oxygen, but because the alcoholic fermentation is slackened or suspended in consequence of the failure or deficiency of the oxidation of its proper ferment; the constant oxidation of its ferment being a law of this fermentation, as already explained.

4. That if, as before, we consider the lung-tissue operated on as containing one-half its weight of capillary blood, by *doubling* the alcoholic ratio, we have, for the *capillary blood of putrescent lung-tissue* (where we suppose all the sugar to have been converted) the figure $0.7638 = (0.3819 \times 2)$, which is nearly exactly equal to the mean product of putrescent thoracic blood, all of whose sugar has likewise been converted.

The general accordance of the results above set forth, with theoretical anticipations, and their consistency with each other, strongly tend to substantiate the truth of the hypothesis under which the whole experimental series was conducted, as well as to establish the *high degree of accuracy of the methods employed.*

It is, therefore, from the lung-tissue of *animals in digestion* that we are to expect the largest yield of alcohol. To obtain a sufficient quantity for verification and dosage, until considerable experimental facility is acquired, at least ten pounds of tissue should distilled be in one or several operations; the final distillate being reduced to twenty-five grains. All procedures embarrassing respiration, instituted with a view of impairing the oxidation of alcohol after it is formed in the lungs, in order to obtain a greater quantity of this substance, would seem to me quite unnecessary. I have not found that more alcohol is obtained by such measures; oxidation appears to cease at the instant of somatic death, while the sugar present, or still passing through the lungs, continues its fermentation.

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The tissue must be brought to 212° Fahr. with all possible dispatch, either by the use of direct heat applied to a large copper balloon, or by the steam of water previously evaporated to less than half its bulk, introduced by a coil of perforated pipe into the charged balloon. No empyreuma will be generated in this case, but at least *three-fifths* of the matter operated on must be brought over in the first distillate.

The *destination* of alcohol, whose presence in the economy I have thus demonstrated, must be to a *hæmal oxidation* or "combustion," as a *main source of animal heat.* This combustion is maintained, not only by glucose derived from amylaceous food, but likewise from the proximate products of change in the nitrogenous tissues. After the extrication of *polar forces*, one of these products, *fibrine*, is oxidized in the lungs, and finally metamorphosed in the liver. I would suggest the usual stages of alcoholic oxidation, viz., aldehyde, acetic acid, formic acid, oxalic acid, carbonic acid, and water, as those by which alcohol is consumed in the blood. It is worthy of mention that *oxalic acid* appears in the urine in combination with lime, whenever oxygenation of the blood is defective. Such an oxidation of alcohol in the blood, in the presence of nitrogenous bodies, themselves undergoing oxidation, is quite in accordance with familiar chemical processes.

I am fully aware that neither the number nor variety of the experiments reported in this paper is such as the importance of the subject demands, and I sincerely hope to be able to supply some of these deficiencies before long. Meanwhile, I trust that other experimentalists will not leave this extensive field of inquiry to my unaided efforts.

Art. II.—*The Histology and Physiology of the Penis.*[1] By Alex. W. Stein, M. D., Attending Physician to Charity Hospital, Professor of Visceral Anatomy and Physiology in New York College of Dentistry and of Comparative Physiology, New York College of Veterinary Surgeons, late Lecturer on Genito-Urinary Organs in University Medical College, etc.

Histology.—The erectile tissue of the penis may be said to consist of venous cavities or cells which freely communicate with each other, are continuous with the general venous system, and are lined with squamous epithelium. The direct connection of these cavities with the veins is clearly demonstrable in such preparations, in which the cavities are somewhat filled with blood (Fig. 8). In the corpus spongiosum these cells are quite large near the surface or immediately beneath the external fibrous investment, while toward the axis of the urethra they are short and narrow. In the bulb they are larger than at any part anterior to the same.

The interspaces between these cavities are occupied principally by non-striped muscles, which form, as it were, an external muscular tunic for these cavities. In these interspaces may also be recognized connective tissue forming the connecting medium of the muscles, blood-vessels, nerves, and lymphatics of the part.

The sheath of connective tissue, known as the albuginea of the corpus spongiosum, which lies beneath the subcutaneous areolar tissue, and which surrounds the corpus spongiosum in its entire length, is interspersed with innumerable fasciculi of organic muscles which are attached to or originate from the albuginea at innumerable and various points. From this origin they pass in a devious manner in the interspaces between the venous cells, inward toward the deeper portions of the spongy substance, verging suddenly from one direction into another, and by their manifold directions and connections with each other form the intricate trabecular structure or trellis-work of the corpus spongiosum.

[1] Read before the New York Dermatological Society, and illustrated by microscopical specimens prepared by my friend John Busteed, M. D. and myself.

Many of these fasciculi run horizontally internal to the albuginea, so that upon transverse section the periphery of the corpus spongiosum appears, at first glance, to be surrounded by a circular layer of organic muscles. Upon more precise examination, however, we find that this muscular layer does not form an absolute ring, but only an approximation to one. On the contrary, not a single fasciculus runs continuously around the spongy substance, but embraces only a small portion of its periphery in a horizontal direction, and that at those points where one fasciculus turns from one horizontal plane into another, others arise and continue in an analogous manner the horizontal course left by the first, and thus contributing to the formation of a more or less interrupted muscular ring upon the periphery of the corpus spongiosum.[1]

In the section of the corpus spongiosum before you can be seen the arteriæ profundæ corporis spongiosi, equidistant from the external fibrous covering and urethral canal in each lateral half of the corpus spongiosum. In this instance they are almost on a level with the urethra, though sometimes we find one artery a little below, the other a little above, the level of the urethra. At the bulbous portion we always find the arteries below the level of the urethra, because of the greater thickness of the parts below the canal at this part.

According to Müller, there are two sets of arteries, differing from one another in their size. The first are the rami nutritii, which are distributed upon the walls of the veins and throughout the spongy substance, differing in no respect from the nutritive arteries of other parts; they anastomose freely with each other, and terminate in capillaries. The second set he calls arteriæ helicinæ, from their supposed resemblance to the tendrils of the vine. They are given off from the larger branches as well as the smaller twigs of the arteries. They are especially to be seen in good longitudinal sections, sending out short branches somewhat like a ram's-horn, several going off from one point in a stellate form, or as the arms of a chandelier, and terminating in an expanded or knob-like extremity, which project freely into the venous cavities (Fig. 5). They are not entirely naked, but are covered with pavement epithe-

[1] B. Stilling, "Harnröhren-stricturen," Fig. 1.

FIG. 1.—THIN TRANSVERSE SECTION OF THE CORPUS SPONGIOSUM URETHRA. TAKEN FROM THE PENIS OF A MAN AGED TWENTY-SEVEN, MIDWAY BETWEEN THE GLANS AND BULBOUS PORTION. PREPARED IN ALCOHOL, AND TREATED WITH CARMINE. MAGNIFIED 20 DIAMETERS. (*See* Frontispiece.)

The albuginea or external connective-tissue sheath of the corpus spongiosum is shown at A A′ A″ A‴. The upper half, which is connected with the corpus cavernosum, is seen at A A′ A″; the lower half at A, A‴, A″. It is abundantly interspersed with (dark) fasciculi of organic muscles, *a*, *a*, *a*, *a*, which do not run completely around the periphery of the corpus spongiosum, but form only fragments of a muscular ring. These fasciculi are continuous with those seen in the substance of the corpus spongiosum at B, B, B, B. At *A**, *A*† (between A and A′), may be seen such fasciculi, extending from the albuginea directly into the substance of the corpus spongiosum. At *b*, *b*, *b*, can be seen the cut surfaces of a large number of muscular bundles appearing as round, oval, or irregularly-shaped dark spots. At *c*, *c*, *c*, *c*, may be seen other muscular fibres running in continuity. The latter run in a parallel direction with the section, while the former run in a vertical direction, and are consequently cut by the section. Between these bundles of muscles can be seen *d*, *d*, *d*, *d*, innumerable empty spaces or meshes of irregular shape. The walls of these spaces have become separated in preparing the specimen, while during life they are generally in immediate contact. These spaces are lined with pavement (Pflaster) epithelium, which can be distinctly seen, in suitable preparations, with a magnifying power of 60 to 200 diameters. Some of these meshes are quite near the mucous membrane, separated from it only by a thin layer of connective tissue, i. e., in the upper wall of the urethra in this figure. Transverse section of the corpus spongiosum will show as a rule two large arteries, *e e′*, the arteriæ profundæ corporis spongiosi. Each of these is surrounded by a number of organic longitudinal muscular bundles, *e**, *e**, which appear as round masses, in whose centre the artery passes. In the middle of the section is seen an empty space, C, the canal of the urethra. The epithelial covering of the mucous membrane is indicated at *f*, *f*, *f*, and is seen extending into its lacunæ. The existence of a real space in the accompanying figure is the result of the preparation. It does not exist during life. At *g*, *g′*, *g″*, *g*†, *g**, is seen the cut surface of a lacuna which extends deeply into the substance of the corpus spongiosum at *g**, and sends off several branches from its sides, *g′*, *g″*, *g*†. The largest meshes are at the external portions of the corpus spongiosum; the smaller ones near the urethra. The substance of the corpus spongiosum near the urethra is more transparent than the rest, because it consists of finer fibres than the parts more external.

FIG. 2.—LONGITUDINAL SECTION FROM THE SUPERFICIAL PARTS OF THE CORPUS CAVERNOSUM PENIS (MAN). ALCOHOL PREPARATION, TREATED WITH CARMINE, ACETIC ACID, AND GLYCERINE. MAGNIFIED 15 DIAMETERS.

This figure represents the trellis-work of the corpus spongiosum formed by the manifold directions and interlacing of its muscular fibres.

lium. These arteries are more easy of detection in the corpus cavernosum than in the corpus spongiosum. In the latter body they are most numerous in the bulbous and posterior portions.

In most of these terminal knobs can be seen a triple fissure, the form of a Y—like the three-horned figure in the crystalline lens of the human eye (Fig. 7). The smaller branches often present but a simple transverse fissure. Stilling is inclined to regard these fissures as the openings or mouths of the arteries, which are closed in the ordinary condition, but in the state of erection are opened and empty the arterial blood into the venous cavities or cells.

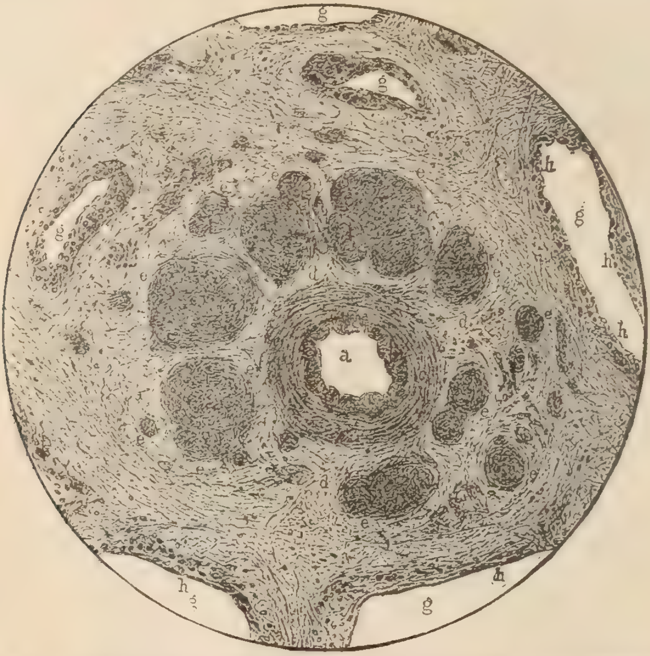

Fig. 3.—Thin Transverse Section of the Corpus Spongiosum (Man) prepared in Alcohol. Treated first with Carmine, and afterward with Acetic Acid, and preserved in Glycerine. Magnified 160 Diameters.

In the centre of the picture at *a* is seen a transverse section of the arteriæ profundæ corporis spongiosi. At *b*, the inner coat of the artery. At *c*, *c*, its very darkly-stained middle coat, with the nuclei of its individual muscular fibres. At *d*, *d*, its transparent external connective-tissue coat. The artery is surrounded by quite a number of organic muscular bundles, like a tube surrounded by a bundle of staves, *e*, *e*, *e*, *e*, *e*. At many points may be seen a portion of these fasciculi connecting and mixing with the muscular coat of the artery, as for example at *e**. The muscular bundles surrounding the arteries are so distinct from the other longitudinal muscular bundles that they must be recognized as a set belonging exclusively to the arteries. The meshes of the spongy substance *g*, *g*, *g*, *g*, are seen to be lined by pavement epithelium, *h*.

These arteries are accompanied by a special system of longitudinal bundles of muscles, whose fibres become inserted at various points into the middle coat of these vessels. Stilling, I believe, was first to call attention to this anatomical fact. These bundles may be seen in both transverse and longitudinal section (Figs. 3 and 4). The number and size of these bundles vary in different sections, in consequence of the changes which they undergo by the insertion of their fibres into the walls of the arteries on the one hand, and again by the addition of new fibres to these bundles from other planes.

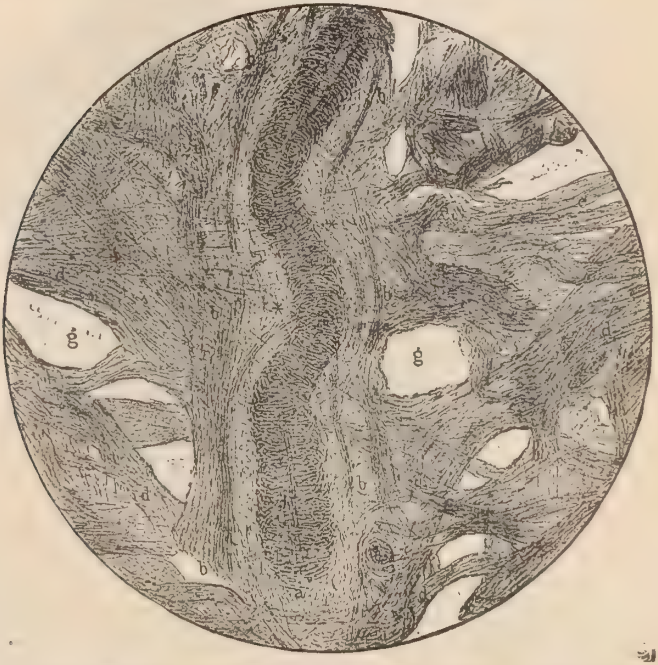

FIG. 4.—THIN LONGITUDINAL SECTION OF THE CORPUS SPONGIOSUM (MAN). PREPARED IN ALCOHOL, AND TREATED WITH CARMINE. MAGNIFIED 160 DIAMETERS.

In the centre of the figure is seen a branch of the arteriæ profundæ corporis spongiosi, *a*, *a*. It is accompanied upon both sides by organic longitudinal bundles of muscles, *b*, *b*, *b*, *b*, which are seen running for some distance in continuity near the artery. Some of these fasciculi pass directly into the walls of the artery, as at *, *, *, and are inserted in the same. Transversely-running muscular fasciculi, *c*, *c*, *c*, as well as those which bend from the longitudinal to the transverse direction, *d*, *d*, *d*, *d*, are very distinct. Also the meshes between these fasciculi, *g*, *g*.

The bundles accompanying the large arteries are comparatively numerous, while those accompanying the smaller ones are few, and not so distinct.

The knobby terminations of these arteries possess muscular fibres closely packed in concentric circles, which probably act as sphincters to the mouths of these vessels.

There is one other anatomical fact mentioned by Stilling, to which I will briefly refer, on account of its pathological bearing.[1] It is the intimate relationship of the epithelial elements of the urethra with sub-mucous tissues of the corpus spongiosum.

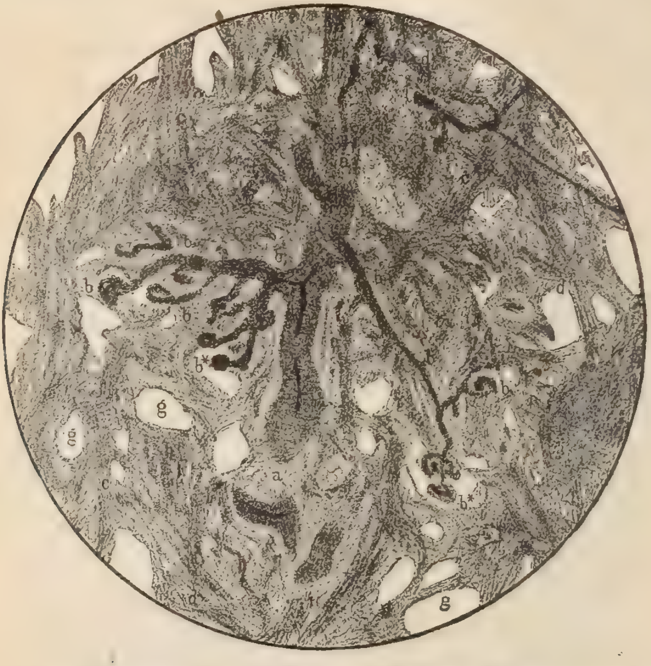

FIG. 5.—THIN LONGITUDINAL SECTION FROM THE POSTERIOR PORTION OF THE CORPUS CAVERNOSUM PENIS (MAN). MAGNIFIED 15 DIAMETERS. THE PENIS WAS INJECTED THROUGH THE ARTERIA DORSALIS WITH BEALE'S BLUE FLUID, AND THEN HARDENED IN ALCOHOL. THE THIN SECTION WAS AFTERWARD TREATED WITH CARMINE, ACETIC ACID, AND GLYCERINE.

In the centre of the figure is seen the artery *a*, *a*, running from below upward. The dark injection in its cavity is well shown. From the right and left of this artery are given off the arteriæ helicinæ, *b*, *b*, *b*, *b*. They assume different forms. Their terminal knobs lie free in the meshes *b** *b**, and are covered with pavement epithelium, which by higher magnifying powers are very distinctly brought into view.

That certain epithelial cells possess prolongations by which they are in intimate connection with the underlying tissues, Stilling claims to have indicated as early as 1857, since which time his statements have become confirmed by the researches of others, who have represented the passage of nerve-fibres to the epithelial layers of certain tissues, as for example in the

[1] This I will reserve for another occasion when I shall have completed some observations now engaged in.

cornea.[1] He affirms that the epithelial cells of the urethra are in manifold connections with the muscular fibres, nerves, and other tissues of the corpus spongiosum. "The most superficial cells," he says, "terminate in a pedicle or filiform prolongation, which often remain connected with the mucous membrane after the body of the cell has separated from its connec-

FIG. 6.—PART OF A FINE LONGITUDINAL SECTION FROM THE POSTERIOR PORTION OF THE CORPUS CAVERNOSUM PENIS (MAN). PREPARED IN ALCOHOL, AND TREATED WITH CARMINE. MAGNIFIED 160 DIAMETERS.

In this section the artery *a, a*, is seen running along the entire length of the section, sending off a number of branches which terminate in thickened ends, *c, c, c*. The red knob-like ends of this artery are quite conspicuous in consequence of the imbibition of carmine. To the left of the artery is seen the course of a longitudinal muscular bundle, *d*, which is inserted in the upper third of the artery. Transverse and oblique muscular bundles are seen at *e, e*.

tions. A good longitudinal section will often show three or four of such cells, one above the other, hanging like pears by their stalks, the cells directed toward the bladder, the stalk toward the external meatus. To state the precise manner in which these connections occur is reserved for future research. Especially will it have to be ascertained whether certain epithelial cells of the urethral mucous membrane are real terminations of nerves, and others real terminations of muscular fibres.

[1] Frey, "Handbuch der Histology und Histochemie," 3. Aufl., Leipzig, 1870, p. 608.

If we consider the extreme sensitiveness of the epithelial layer of the healthy cornea to the contact of ever so fine a foreign body; if we consider that the most superficial contact of the point of a needle with the cornea produces the most severe pain and reflex movements; while, after destruction of the most superficial layer at one point, the needle produces at that point only an inconsiderable amount of pain—facts which every physician has opportunity to observe in removing foreign bodies (dust, iron filings, particles of coal, etc.) from the cornea—the conclusion is almost irresistible that the epithelial covering of the cornea consists of cells which are to be regarded as nerve-terminations."

FIG. 7.—THE KNOB-LIKE END OF AN ARTERIA HELICINÆ, FROM A FINE LONGITUDINAL SECTION OF THE CORPUS CAVERNOSUM PENIS (MAN). PREPARED IN ALCOHOL, AND TREATED WITH CARMINE.

The terminal end *a* appears to lie free in one of the meshes. The mouth of the vessel is seen at *, which appears as a Y-shaped fissure.

Physiology.—The cause and mechanism of erection may be said to depend upon two phenomena occurring simultaneously:

1. Upon an increased influx of blood through the arteries which empty by the arterial helicinæ into the venous cavities.

2. Upon mechanical pressure affecting the veins which convey the blood from the penis, whereby a retardation of the venous circulation is induced.

In the passive condition of the penis the same quantity of blood flows to and from the organ, but during erection the entire arterial system becomes distended, especially those known as the arteriæ helicinæ become actively dilated, and empty themselves into the venous cavities. The dilatation of these vessels is supposed to be effected through the agency of the special system of longitudinal bundles of muscles which accompany, and whose fibres are inserted into the walls of these arteries.

The muscles chiefly concerned in arresting the efflux of blood, or at least preventing it from being as great as the influx, are the acceleratores urinæ and erectores penis.

The contraction of the acceleratores urinæ muscles impedes the return of blood through the vena corporis spongiosi, by pressure upon the bulbous portion of the corpus spongiosum.

The erectores penis act as erectors, by compressing the crura and vena dorsalis. They embrace the root of the penis and compress it. The pressure upon the vena dorsalis impedes the return of blood by this vessel, and the pressure upon the crura produces the same effect upon the veins of the corpus cavernosum.

The transversus perinei probably assists the posterior fasciculi of the acceleratores urinæ in producing turgescence of the corpus spongiosum, by virtue of its insertion into the fibrous tunic of the bulb.

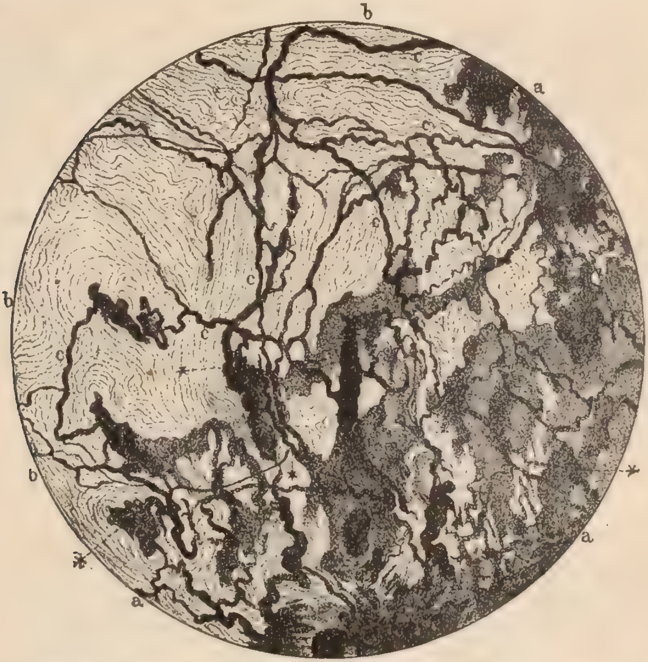

FIG. 8.—TRANSVERSE SECTION OF THE CORPUS CAVERNOSUM PENIS (MAN). PREPARED IN ALCOHOL. TREATED AT FIRST WITH CARMINE, AND AFTERWARD WITH ACETIC ACID AND GLYCERINE, AND PRESERVED IN FARRANT'S LIQUOR. MAGNIFIED 160 DIAMETERS.

The object of this figure is to represent the communication of the veins with the meshes of the corpus cavernosum (and spongiosum). The dark portion of the picture indicates the corpus cavernosum, *a*, *a*, the lighter portion the albuginea of the corpus cavernosum, *b*, *b*, *b*. The meshes are all filled with blood, in consequence of which the structure of this part appears considerably more distinct than when these cavities are empty. Internal to the albuginea of the corpus cavernosum are seen a large number of veins, *c*, *c*, *c*, forming a plexus. The direct connection of some of these veins with the meshes of the corpus cavernosum can be seen at *, *.

These muscles are supplied with nerves by the pudic branch of the sacral plexus, and it is an interesting fact, capable of demonstration upon the lower animals, that after division of this nerve the penis is incapable of erection.

In the passive condition the natural tonicity of the muscular trellis-work of the penis is sufficient to maintain the walls of the venous cells in apposition; and they, together with the sphinctoric action of the circular fibres around the mouths of the arterial helicinæ, prevent the flow of blood into these cells. But, when the parts are stimulated to erection, the muscular bands are obliged to yield to the distending force of the

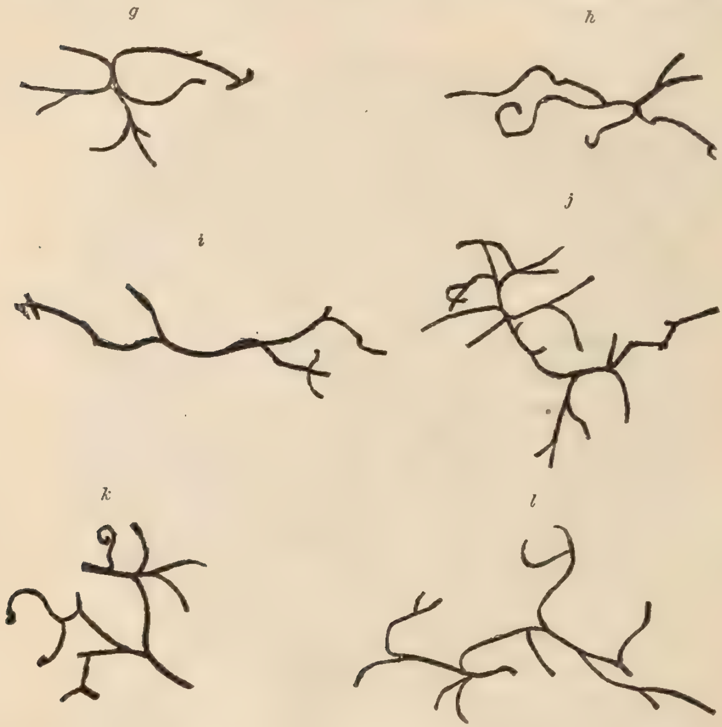

FIG. 8—*g, h, i, j, k, l*—represents the various shapes of the arteriæ helicinæ, which are frequently found in injected preparations of the corpus cavernosum. Magnified 15 diameters.

The description of the plates is taken from "Die Rationelle Behandlung der Harnröhren-stricturen, by Dr. B. Stilling.

blood (according to Müller, it appears that the blood accumulating in the penis during erection is subjected to a pressure equal to that of a column of water six feet in height). The meshes become filled, and remain so until the stimulus to erection subsides, when the acceleratores urinæ and erectores penis muscles relax, and remove the pressure from the veins. The trabeculæ (non-striped muscles) of the penis now contract and expel the blood from the dilated venous cells.

The contractile force of the corpus spongiosum is well displayed in persons who, for the first time, submit to the introduction of a catheter or sound into the urethra; the entrance of the instrument is often sensibly opposed, and during withdrawal it is forcibly expelled.

This phenomenon cannot be attributed entirely to the action of the acceleratores urinæ and compressor urethræ muscles, for it is manifest even within an inch of the external meatus.

The action of the muscular trellis-work of the corpus spongiosum, as it affects micturition and the ejaculation of the spermatic fluid, is of interest. During micturition, the entire corpus spongiosum, as well as the urethra, becomes somewhat stretched. At the end of micturition the tonicity of the trellis-work is sufficient to coaptate the walls of the urethra, and to expel the last drops of urine which may remain in the anterior portion of the canal. This pressure is strongest at those parts where the urethra and its surrounding meshes are narrow; and weakest where these cavities are wide, and consequently more dilatable, as at the bulb.

As the external fibrous investment of the corpus cavernosum consists of broad, thick bands of connective tissue, it becomes firmer, harder, and more unyielding during erection, than the corpus spongiosum. The latter being smaller in circumference, with its external fibrous investment interspersed with non-striped muscular bands, it remains even during the most complete erection, and during the ejaculation of the sperm, sufficiently dilatable or distensible to permit the seminal fluid to flow through the canal. Should the corpus spongiosum attain that degree of hardness which the corpus cavernosum acquires, the canal would not yield to the pressure of the sperm, and ejaculation would be obstructed.

The pressure which the corpus spongiosum is capable of exerting upon the urethra becomes quite considerable, when to the natural tonicity of its muscular trellis-work above mentioned there is added a special stimulus to contraction, which obtains when the penis is brought to a state of erection. Each new pulsation increases the quantity of blood in the meshes or venous cavities, and this—as it were—supplementary body within these cavities stimulates the muscular bands to increased activity. The pressure now brought to bear by these bands very materially assists the acceleratores urinæ and compressor urethræ muscles in communicating an expulsive impetus to the outflowing stream of urine or seminal fluid.

The greater the special force exerted by the contraction of the acceleratores urinæ during ejaculation, the more is the erection of the penis augmented, and the concentrical pressure of the muscular trellis-work upon the spermatic fluid increased.

For this reason micturition is so difficult during complete erection, and *immediately after* ejaculation.

Thus I have endeavored to lay before you, in a somewhat condensed form, a synopsis of some views at present entertained upon the histology and physiology of the penis. This subject is but imperfectly understood; its literature is meagre, and much of that which has been written is vague and unsatisfactory, while many points have remained entirely unexplored. Yet, there are few subjects which present a more fruitful field for histological investigation than the one to which I have now had the honor of calling your attention.

ART. III.—*Experiments*[1] *on Animals, disproving the Theory that Chloral-hydrate acts on the Organism on account of its Decomposition into Chloroform, by the Alkaline Carbonates in the Blood.* By ROBERT AMORY, M. D., Boston, Mass.

CHLORAL-HYDRATE.[2]

THE following experiments were conducted during the autumn and early winter of 1869. The samples of chloral used in these experiments were quite fresh, and, so far as the known tests for purity could be applied, answered every reaction.

[1] These experiments have been previously reported to the Boston Society of Medical Sciences.

[2] When the word chloral is used in this paper, the medicinal agent, chloral-hydrate, is referred to.

None but the experiments in which were used Morson's, Schering's, or a beautiful sample furnished the author by Dr. E. R. Squibb, of Brooklyn, N. Y., are reported. These experiments were selected from a large number conducted, and are fair examples of each class:

EXPERIMENT I. *Warm Fresh Sheep's Blood and Chloral.* —Fresh sheep's blood, of a temperature of 80° Fahr., was placed in a retort, and gently heated over an oil-bath to a temperature of 98° Fahr., and to this was added a solution of chloral-hydrate (grains xxx in aqua ℥ ss). The retort was then secured against the admission of air, and connected with a porcelain combustion-tube, at the outlet of which was a potass.-bulb containing an acidulated solution of nitrate of silver. For an hour the temperature inside the retort was maintained at 98° Fahr., and all this time air was forced over the blood by means of valvular bellows, and the escaping vapor passed through the combustion-tube heated to a red heat, and was then received into the solution of nitrate of silver. No precipitate of chloride of silver appeared in the silver solution[1] contained in the potass.-bulb, thus showing that probably no decomposition into chloroform had occurred in the retort.

Then the combustion-tube was disconnected, and the retort connected with a cold receiver, at the bottom of which was a small quantity of sulphuric acid. This distillation was continued at 95° C., or 203° Fahr., for an hour, and the distilled liquid saved for future experiment, a portion being redistilled; and the last product evaporated over sulphuric acid for twenty-four hours, when the peculiar long, icicular crystals of chloral-hydrate were observed under the microscope, and afterward exhibited to the Boston Society of Medical Sciences.

The principle upon which this experiment was conducted was suggested by M. Duroy's[2] method for the detection of chloroform, viz., that the combustion at a red heat of chloroform in the presence of oxygen would cause its decomposition into chlorine gas, hydrochloric acid, oxide of carbon, and car-

[1] A slight opalescence was detected in this solution, which may have been caused by the volatilization of organic matters. This happens when no nitric acid is added to the substance in the retort.

[2] "Sur l'Empoisonnement." A. Tardieu et Roussin, 1866, p. 841. *Journal de Pharmacie et Chimie*, Avril, 1851.

bonic acid. The hydrochloric acid forms, with the silver in solution, chloride of silver, which is insoluble in acidulated water. The peculiar, irritating, pungent odor of chlorine can easily be perceived at the outlet of the tube, if any of the vapors contain this gas in a free state. The vapor of chloral-hydrate treated in the same manner does not show any products of decomposition, in the experience of the author of the present article. The above test is considered by some physiological chemists as delicate as Marsh's test for arsenic.

The following preliminary experiment was tried merely to correct any mistake in the methods of investigation for chloroform-vapor:

Experiment II.—Some fresh blood was placed in a retort and half a drachm of chloroform was added; the retort was then placed in an oil-bath, and heated to 100° Fahr., subsequently, to 145°; all the vapor that was distilled was condensed and collected in a cold receiver. This distilled liquid was then poured into a test-tube, at the bottom of which a few minute globules coalesced into one larger globule, containing, by estimation, in bulk, the equivalent of about ten drops of water. The supernatant liquid contained an admixture of oily and animal matters; the smell of chloroform was quite marked. A small quantity of alcohol was added to a portion of the liquid and some distilled water, and the whole was then placed in a dialyzer over about four times the bulk of liquid to be tested. The small amount remaining at the bottom of the test-tube was then placed in a watch-glass over a sulphuric-acid bath, and in two hours the evaporation was completely accomplished. The watch-glass was examined under the microscope, and no crystalline substance could be detected. After twenty-four hours the liquid under the parchment was slowly evaporated, at a low temperature, to a very small consistency, and the residue evaporated over a sulphuric-acid drying-dish. The microscope showed no appearance of any crystalline forms. It is unnecessary to remark that any volatile substance would be lost in the process of evaporation, but this was simply an experiment for correction.

This experiment proves that blood treated in the way described does not give the peculiar, icicular crystals which are described in the next and following experiments:

EXPERIMENT III.—To a quart of fresh sheep's blood, when warm, was added a small quantity of a solution of potassa, giving to the blood a decidedly alkaline reaction. This was then placed in a retort, well corked, over an oil-bath, and maintained at a temperature of 100° Fahr.; then was added a solution of chloral, and afterward a small quantity of chloral (making in all about thirty grains). The blood changed from a bright-red to a dark color. The whole mixture was then maintained at 90° to 100° Fahr. for two and a half hours, and at the end of this time a strong evolution of chloroform was detected in the retort.

Then alcohol (about four ounces) was added, to dissolve the chloroform, and the mixture thoroughly agitated and then cooled. When cold, the whole mixture was placed in a dialyzer, over six times its bulk of distilled water, for forty-eight hours. The apparatus was covered with a bell-glass secured to a ground-glass stand. The water below the dialyzer was then slowly distilled at a temperature below 210° Fahr.

The products of distillation, consisting of half a drachm of clear liquid, were covered in a test-tube with a very small quantity of water. There was no deposition of globules, nor could any smell of chloroform be detected. A portion of the distilled liquid was placed in a sulphuric-acid drying-jar, and examined under the microscope; the icicular crystals, resembling in every particular those of chloral-hydrate, were present in large quantity. The other portion was treated with a solution of caustic potassa, and a *faint* smell of chloroform detected, the mixture becoming slightly turbid, but no globules separated at the bottom of the test-tube. This experiment proves that by the above process no large quantity of chloroform was recovered in blood rendered alkaline by caustic potassa, when chloral had been added; and also that not all the chloral was decomposed in the mixture, as crystals of chloral were observed in the ultimate products of distillation; and it will be observed also that it requires the addition of a *caustic* alkali to the blood, for the production of an appreciable amount of chloroform.

EXPERIMENT IV.—The same quantity of fresh sheep's blood, having a faint alkaline reaction by litmus-paper, was treated by the same method as in the preceding experiment, with the same quantity of chloral. At the close of two hours, a distinct odor of chloral, mingled with that of animal matters, was evident; and though, by some, it may have been mistaken for choloroform, this did not so appear to me. This mixture of blood, etc., was then placed in a dialyzer, as in the preceding experiment, and the subnatant liquid distilled at a temperature below 210° Fahr. The product of distillation was yellow, and amounted to twenty minims, which was diluted with distilled water, and half of this placed in a sulphuric-acid drying-jar; the watch-glass examined under the microscope presented distinct icicular and rhombic crystals, in all respects resembling a specimen of chloral-hydrate prepared expressly for comparison, and both specimens were shown to the Boston Society of Medical Sciences, at a meeting held the same evening. The other portion was treated with a solution of potassa, and the odor of chloroform was quite distinct, and about one or two minute globules were perceptible at the bottom of the test-tube. It will be noticed that this experiment demonstrates the fact that chloral, placed in the blood to which no caustic alkali has been added, and the temperature of which is maintained at blood-heat for a given time, does not become decomposed into chloroform, and that the products of distillation contain chloral in solution, and that from this solution chloroform is decomposed by the addition of a caustic alkali.

These last two experiments were repeated twice, and with about the same result.

To remove any doubt about the result of the first experiment, in which a slight opalescence of the silver solution was observed, which had no resemblance to chloride of silver, a modification of it was instituted in the following experiment:

EXPERIMENT V.—About a pint of fresh sheep's blood was warmed over a water-bath to a temperature of 98° Fahr., and in this was immersed a fresh sheep's bladder, which had previously been thoroughly cleaned, containing a solution of chloral (grs. xxx. = ℥ ss). For an hour a blast of air was forced into the retort, allowing the vapors to pass through a combustion-tube placed in a gas-furnace, arranged so that two feet of the tube was heated continuously to a red heat, and were then received into a potass.-bulb containing a solution of nitrate of silver slightly acidulated with nitric acid. No opalescence or turbidity of the silver solution occurred, nor any smell of chlorine gas escaping at the outlet of the tube during the hour that the vapors were forced out of the retort, thus proving distinctly that no decomposition into chloroform or chlorine or hydrochloric acid had occurred. During the second hour the mixture of blood and chloral was heated to a temperature of 215° Fahr., and the vapors condensed in a cold receiver; a portion of the distilled liquid, under the microscope, showed the presence of the same icicular or rhombic crystals mentioned above.

From the above experiments it will be seen that it is not possible to obtain decomposition of chloral into chloroform, when the former is mixed with warm, fresh blood, and it is hardly probable that this decomposition occurs in the organism. The decomposition does not occur if the solution of chloral is separated from the blood by a membranous substance such as a sheep's bladder. If, on the contrary, the blood is made strongly alkaline by the addition of caustic soda, the decomposition suggested, but never proved, so far as the author of this article can determine, by Liebreich, Richardson, and others, does take place. Now, it is hardly possible to suppose that a dose of chloral received into the stomach can pass through the tissues and be absorbed by the blood, any more readily than could have occurred in the fifth experiment related, where a sheep's bladder only lay between the chloral solution and the liquid blood. That chloral-hydrate passes through a colloid substance has been shown in the second, third, and fourth experiments; and it is hardly necessary to add that in the fifth experiment, when the bladder was removed, it contained only a very small quantity of a slightly-red watery substance, occupying less than a quarter as much space as before immersion in the blood.

In order to show that the amount of chloral used in the preceding experiments was sufficient to produce a reaction of chloroform contained in a latent state, I will copy[1] the results

[1] *American Journal of Pharmacy*, October 1, 1871, p. 450.

of some experiments performed by Dr. Rademaker, of Louisville, with a view of determining the amount of chloroform which can be obtained from a given quantity of chloral:

100 grains of	Liebreich's chloral	produced	60	grains of	chloroform.
"	Schering's	"	"	60 "	"
"	De Hass's	"	"	50 "	"

One minim of chloroform in a pint of water can be easily detected by Duroy's test for chloroform.[1] Consequently, with thirty grains of chloral we should expect to find eighteen grains of chloroform, and this would give a very perceptible reaction in the method employed.

Experiments disproving the Presence of Chloroform in the Blood of Animals who have taken Chloral-hydrate.—Experiment VI.—Chloral, grains 40, were placed far back behind the tongue of a dog, and were immediately swallowed. The dog was then allowed to drink a small quantity of water. For ten minutes she has been quietly walking round the room, the hind-legs gradually becoming weaker, and finally giving way under her weight; she is lying quietly, with diminished sensation when the skin is pinched or pricked, though reflex response to irritation is still present. She gradually falls asleep, and in thirty minutes blood is drawn from the vessels of the neck until she dies—125.00 grammes (by measure) of blood have been collected. The whole amount is coagulated by heat (below 300° Fahr.), and then thoroughly mixed with alcohol, strained, and the strained liquid distilled over an oil-bath at a temperature varying from 212° to 400° Fahr. The product of distillation is then treated with strong sulphuric acid, and allowed to stand twenty-four hours.

The coagulum (or portion remaining in the strainer) is digested with ether for forty-eight hours in a closed vessel. At the end of the time mentioned, each portion was dialyzed for twelve hours, and separately distilled. The first products of distillation were placed in a sulphuric-acid drying-jar, and when dried were separately examined under the microscope. In each substance thus treated could be distinctly seen, among other crystalline bodies, the peculiar icicular crystals hereto-

[1] This was the method which was used in these experiments.

fore noticed, and these were compared with specimens of chloral dissolved in water, alcohol, and ether, and then evaporated. The icicular crystals are transparent, and transmit light. A considerable time has been taken up in the study of the polarization of these crystals, but no definite result has been obtained.

EXPERIMENT VII.—A large dog (40 lbs. weight) was selected for the following experiment, and, as it is directly concerned in this portion of my subject, I will relate the details here, and refer to some of them again, when they illustrate still further other portions of the discussion of the action of the drug I am considering:

1st day, at 3 P. M.—Chloral (Morson's) grammes 1.50 wrapped in meat, and swallowed by the dog, without being chewed. In a few moments the peculiar staggering gait, especially of the hind extremities, was observed, but, though the animal lay down afterward, she did not sleep.

2d day, 10 A. M. (2 grammes) and 3 P. M. (2 grammes) of chloral (Schering's), making four grammes in all, were administered in the same way to the same dog, followed by the same symptoms as before noted, and by a short sleep after each dose.

3d day, 2 P. M., 5 grammes of same specimen as on the previous day, administered in the same manner, followed by the same symptoms. The dog has had since the first day an excessive scouring of the contents of intestinal canal.

4th day, 10 A. M., 4 grammes of a specimen furnished by J. T. Brown, of Boston, followed by same symptoms, but sleep lasting only a short time—less than an hour; diarrhœa still continues.

5th day, 1 P. M., 1 gramme of Morson's. This day the staggering was less marked after the use of the drug.

6th day, 10 A. M., 1 gramme of Morson's, and at 2 P. M. 5 grammes of Morson's.

7th day, 9 A. M., 3 grammes of Morson's, followed by sleep.

In every case all effects of the drug passed off in four or five hours after the administration, and the diarrhœa lasted from the first to the tenth day, the dog having some days six to eight loose alvine discharges.[1]

[1] As the drug was administered each time in meat, the diarrhœa was due, probably, to the local irritation produced in the alimentary canal.

7th day, 11.30 A. M., one ounce of blood was drawn from a large vein above the knee, and three and a half ounces from an arterial twig lying near the vein, making in all four and a half ounces of blood. This was then placed in a retort, and kept gently warmed (about 125° Fahr.), and the vapor blown over by bellows into a red-hot combustion-tube and into a test-tube, containing a solution of nitrate of silver, slightly acidulated by a few drops of nitric acid. At the outlet-tube from the test-tube could be distinctly perceived the sweet smell of chloral, but different from that of chloroform. No precipitate formed in the silver solution, though the process was continued steadily for an hour. The addition of a few drops of an alcoholic solution of caustic potassa caused a brown precipitate to be thrown down in the silver solution, and, on the addition of a little more nitric acid, this precipitate became white, and then was dissolved by the addition of aqua ammonia.

At 2 P. M. the dog was forced to exhale into distilled water, which was placed in a retort, and treated in the same manner as detailed above, but no trace of chloroform could be detected. The blood was now treated by distillation at a temperature of 230° Fahr., and the first few drops of distillation yielded, by evaporation over sulphuric acid, a solution which on evaporation showed, under the microscope, icicular and a few rhombic crystals.

The water into which the dog had been exhaling for three quarters of an hour was then distilled, and the distilled liquid was redistilled with quicklime, and found to contain no chloroform until after the addition of a solution of potassa.

No great amount of sleep was noticed after either of the above doses, though the staggering gait was quite marked. Very probably much of the drug was carried off by the discharge from the intestines.

In other experiments when the drug was administered, care was taken that the animal should be fasting for some time before and after the exhibition.

These two experiments are quite illustrative of the failure to detect any signs of decomposition of chloral into chloroform in the blood, and seem quite conclusive in proving that this change does not occur in the living organism. It is to be regretted that no very accurate means of detecting chloral in the presence of animal matters now exists, as this would be a material aid in determining the action of this drug in the organism. The microscopic tests of crystalline bodies are by no means an easy matter, as there are so many bodies which seem to have the same general appearance. The only crystalline bodies which would be present in the substances examined under the microscopes, and by whose form confusion might be caused, are certain salts of potassa and ammonia, but crystals of these bodies would be very much more minute. Crystals of chloral-hydrate can easily be recognized with a small hand-microscope of half an inch focus, and the field is generally quite well filled with crystals. Sometimes they may appear to the eye without the use of a lens.

Appearance of Blood-corpuscles treated with Solution of Chloral.—If a specimen of blood is placed under a high magnifying power ($\frac{1}{10}$) in a microscope, and then an aqueous solution of chloral (grains 10 = ℥j) be added, the blood-corpuscles gradually separate from each other, and become irregular in outline, shrivelled and crenated, somewhat as has been remarked when chloroform or alcohol is added to blood.

The two following experiments were undertaken to prove that the blood of an animal poisoned by chloral does not show the presence of chloroform; while that of an animal poisoned by chloroform does show very distinctly the presence of chloroform.

EXPERIMENT VIII.—A fasting dog was made to swallow two fragments of chloral, which together weighed about 100 grains, and was then allowed to drink a small quantity of water. In the course of ten minutes he vomited twice some frothy mucus, and then fell into a sound sleep. The character of the respiratory movements and the cardiac pulsations were determined by the cardiograph. The temperature, taken by a thermometer placed under the skin, gradually fell, until, during the state of anæsthesia and muscular relaxation, it only indicated 30° C. (or 86° Fahr.). The respiratory movements became gradually more feeble and less frequent, and finally, in about two hours from ingestion of the drug, were almost imperceptible. The pulsations of the heart continued slowly, about 35 or 40 per minute. At this period the animal was killed by transfixion of the medulla, and the heart continued its pulsations for a few moments longer.

The liver and about ℥viij of blood were immediately placed in a closed retort, and tested for chloroform in the manner previously described. Combustion of the volatile vapor, passing through a tube packed with asbestos, was continued for twenty-five minutes, and no chloroform appeared to be present.

The contents of the above were then distilled through an apparatus for fractional distillation, at a temperature of 100° C. (212° Fahr.), and the solution evaporated over sulphuric acid, and the presence of the icicular crystals was determined.

Experiment IX.—A large dog was killed by the slow inhalation of chloroform, care being taken not to poison the dog too rapidly, nor to allow the too rapid absorption of the anæsthetic. Fifteen minutes after the inhalation of the chloroform, the sponge was removed, and the respiratory and cardiac movements determined by the same apparatus as in the last experiment. In about five minutes later the respiratory movements were almost imperceptible, the subcutaneous temperature being 33° C. (91° Fahr.). The heart's pulsations continued for a moment or two longer, and then ceased. A few fragments of the liver and ℥viij of blood were immediately placed in a retort, and, after the application of the previously-described test for chloroform, gave a large amount of chloride of silver in the solution of nitrate of silver, and a large amount of chlorine gas escaped at the outlet tube. The passage of the vapor through the combustion-tube was continued only for seven minutes.

These two experiments are quite convincing proofs that there is no chloroform present either in the liver or in the blood of an animal poisoned by chloral. It should be mentioned in this connection that the liver, according to the experiments with chloroform by MM. Lallemand, Perrin, and Duroy, contains, next to the brain, the largest percentage of chloroform in animals anæsthetized by chloroform. Again, it should be noticed that the test was applied to an animal that had been placed in a state of anæsthesia by a large dose of chloral.

The other experiment (No. VIII.), which was only undertaken to compare with experiment No. IX., showed that my method for determining the presence of chloroform was sufficiently accurate for the purpose. I will also add, in this connection, that I hold to the theory that it is necessary, in producing anæsthesia by chloroform, to cause saturation of the blood by the vapor of this agent.

Experiments disproving Elimination of Chloroform from Animals taking Chloral-hydrate.—The great difficulty experienced in experiments to determine the elimination of chloral, consists in its natural volatility, and the difficulty of recognizing the peculiar odor of this agent in distinction from that of chloroform. A large number of experiments were made by myself, to determine by the sense of smell the presence of chloral or chloroform, when combined with animal matters, either in a state of vapor or in solution. After a very fair trial, I was led to cast aside this method, as being almost valueless. Though a person can become pretty skilful in detecting chloral and chloroform by the nose, yet, if he imagines he has one of these, he is very apt to think he smells the peculiar odor of that one. After numerous and fruitless efforts to separate the strong animal exhalations from the other volatile vapors, I became discouraged, and sought a more convenient as well as reliable means of detecting chloral and chloroform in the presence of animal matter. The result of my investigation induced me to rely altogether upon Duroy's method, which I have previously described; consequently, with one exception only, those experiments conducted after his method will be related, though there are numerous others in which other methods of analysis were carefully tried.

I give merely one experiment, which may serve to illustrate the uselessness of relying upon results of experiments which are founded only upon the sense of smell:

Experiment X.—To a dog were given thirty grains of chloral, and the mouth was then carefully and repeatedly washed with water, to prevent any smell of chloral which might be lingering about the mouth. As soon as the animal was asleep, the exhalations from the nose and mouth were conducted into a (U) tube packed with asbestos, and thence into a wash-bottle containing distilled water. This last water, and a portion of the asbestos, and as much moisture as could be collected from the U-tube, were then placed in a retort and distilled over into a receiver. The liquid distilled had a strong animal smell, mingled with a peculiar, sweetish smell which could not be distinguished as either chloroform or chloral. A solution of potassa was added, and the peculiar, sweetish smell was stronger, but mingled with so strong a smell of animal matters that I could not determine whether this smell was due to chloral or to chloroform. I divided the substance, and into one-half I placed two drops of chloroform; but even then my confusion between the smell from each portion was not relieved. Again, a fragment of chloral was placed in the same retort and distilled through the same tubes unwashed; the same odor was perceived as before, and not materially different, even after the addition of a solution of caustic potassa.

This experiment will suffice to illustrate the futility of relying only upon the sense of smell for the presence of chloroform or chloral, especially when combined with organic volatile substances.

Chloral-hydrate is eliminated as such, and is not decomposed in the System.—We come now to the consideration of an important subject in connection with the effects of the ingestion of chloral: I refer to the elimination of chloral. Liebreich claimed that, though a large portion of chloral may be eliminated undecomposed, yet that the action of the drug was due to the slow formation of chloroform by the decomposition of chloral, when in the presence of the alkaline carbonates in the blood. As anæsthesia cannot be produced by the inhalation of chloroform, until the blood becomes surcharged with its vapor, we should expect to find in the exhalations from the lungs, which is the principal organ of elimination for this anæsthetic agent, signs of the presence of chloroform, provided the action of chloral is due to the evolution of its product (chloroform) of oxidation.

To test this question, the following experiments were devised and carried out:

Experiment XI.—To a dog was given a piece (grains xxx) of Morson's chloral, and she was then allowed to drink about two ounces of water.

Ten minutes after its ingestion, the characteristic symptoms of the action of chloral were observed, such as clumsy walking, and staggering gait on account of weakness of the hind-legs. The dog seemed not to be able to see objects distinctly, as she kept running her head against the chairs and other obstructions which were in her path. In twenty minutes she lay down before the fire and went to sleep. I then securely fastened an air-tight muzzle, provided with a valvular inlet and outlet tube, over her head. The dog was then allowed to inhale air, and her exhalations were conducted through a combustion-tube packed with asbestos, heated to a red heat in a Hoffmann gas-furnace (containing over one hundred Bunsen jets), and then into a Liebig's bulb containing an acidulated aqueous solution of nitrate of silver ($\frac{1}{10}$). The combustion-tube was kept at a red heat for three-quarters of an hour, but no precipitate formed in the potassa-bulb, and no smell of free chlorine could be detected as coming out from the tube.

At the end of this time, an hour and a half from the commencement of the experiment, thirty drops of chloroform were introduced by the hypodermic syringe into the outside portion of the right hind-leg, a few drops of blood following the withdrawal of the point of the syringe. The animal seemed immediately to awake on feeling the puncture, uttered one or two cries, and struggled, attempting to withdraw the leg, but soon after relapsed into the former condition of sleep. In the course of ten minutes the silver solution became clouded with a light precipitate.

The dog was then made to inhale air passing over an open dish containing one drachm of chloroform. Ten inspirations were made, and about one-half of the quantity of chloroform evaporated or was inhaled. Immediately thereafter dense fumes passed from the combustion-tube into the potass.-bulb, leaving a dense white precipitate, a large quantity of the vapor escaping at the outlet in the form of chlorine gas. The white precipitate was insoluble in nitric acid, but soluble in ammonia. I have observed that generally in the daytime the sleep of dogs caused by a dose of twenty or thirty grains of chloral does not continue much longer than two hours, unless the dose be repeated.

EXPERIMENT XII.—A dog was put to sleep by twenty grains of chloral, and her exhalations conducted, as in the previous experiment, through the combustion-tube (porcelain) for fifteen minutes, and into a silver solution, without producing a precipitate. The dog was then allowed to take three inspirations of air impregnated with chloroform, and the exhalations were conducted as before, in five minutes producing a dense white precipitate, insoluble in nitric acid, but soluble in ammonia. The sleep continued for two hours.

EXPERIMENT XIII.—Twenty-five grains of chloral were swallowed by a fasting dog. In a very few moments he went off into a light slumber, which lasted five hours. His exhalations from mouth and nostrils were conducted through a red-hot tube into a potassa-bulb, containing, as before, the acidulated aqueous solution of nitrate of silver. The exhalations were thus consumed in the tube for one hour and twenty minutes, at the end of which time the silver solution was free from any precipitate, and perfectly clear. The dog was removed from the muzzle, and then half a dozen inspirations of chloroform were administered, and his exhalations consumed as before. The silver solution became immediately turbid, the smell of chlorine was very marked, and a dense white precipitate was deposited in the silver solution, which was insoluble in nitric acid, and soluble in ammonia. A portion of the liquid containing the silver solution was boiled, and oxide of silver was deposited at the bottom of the test-tube.

EXPERIMENT XIV.—A dog was made to swallow forty grains of chloral, and in fifteen minutes was sound asleep. His exhalations were conducted through the same apparatus as before described, and thus consumed for three-quarters of an hour, without interfering in the least with the clearness of the silver solution. At the end of this time, I introduced subcutaneously a few drops of chloroform diluted with water, in the outside of the thigh. In five to ten minutes after, a white precipitate appeared in the silver solution, which was insoluble in nitric acid, but soluble in ammonia; a portion of the liquid containing the precipitate was boiled, and a metallic oxide of silver was deposited. The place where the injection was made became inflamed, and an ulcer appeared, which was slow to heal.

These experiments were repeated several times, but invariably with the same result, viz.:

That, unless chloroform was administered by inhalation or by subcutaneous injection, during the sleep caused by chloral, no chloroform is eliminated by the lungs. It remains to be seen whether chloroform is present in the urine of a person taking chloral.

Comparison of Effects of Chloroform, of Chloral-hydrate, and of Morphia.—I do not desire, in this paper, to enter into the discussion with regard to the physiological action of chloroform, as I should not then be able to consider at sufficient length the action of chloral. I have, however, made several experiments upon the mode of action of chloroform as an anæsthetic agent, and, as these results have a special bearing upon the important question whether chloral acts by its slow decomposition in the tissue, I will relate the deductions that I have drawn from my series of experiments, which have never yet been published:

1. The elective action of chloroform for the cerebro-spinal system, upheld by MM. Lallemand, Perrin, and Duroy, by Dr. Anstie, and by others, is not yet proved. The experiments undertaken to disprove this theory, by Gosselin, by Snow, and by Richardson, seem to show that, if the brain of an animal is moistened with chloroform, no anæsthesia follows.

2. The injection of chloroform into arteries or veins of any portion of the body does not cause anæsthesia, unless a sufficient quantity is injected to produce coma or death; the carotid, the femoral, and the brachial arteries, were employed in this method of experimentation. No sleep followed, unless coma and death supervened. It was observed that the chloroform was absorbed by the tissues, and eliminated by the lungs.

3. Blood was directly transfused from one dog, who was in a complete state of anæsthesia from the inhalation of chloroform, to another who had not inhaled chloroform. Five minutes from the commencement of the transfusion, chloroform was exhaled from the lungs of the second dog, though ti

could not be perceived before. The transfusion was continued without interruption for from fifteen to twenty minutes, beyond which time clots were formed in the tube, so as to interfere with the continuation of the experiment. This experiment was repeated several times with the same result. There was not the slightest appearance of abolition of pain, nor any attempt to sleep, on the part of the second dog. The transfusions were conducted from femoral to femoral artery, or from carotid to carotid.

4. Unless the blood is surcharged with chloroform, the vapor of which may most easily be absorbed in the lung-tissue, no sleep or anæsthesia will follow the administration of chloroform. I am well aware that there are some cases in which large doses of chloroform have been swallowed, followed by a comatose condition of the patient; the effects in these cases were not always anæsthetic nor hypnotic, but were in a great measure due to a probable congestion in the nervous centres.

5. The physiological action of chloroform is due to the interference with the process of oxidation of tissue, produced by altering the physical condition of the blood-corpuscles, and rendering them incapable of carrying sufficient oxygen through the tissues.

Now, we will consider the result of clinical experience in the use of chloroform and chloral:

CHLOROFORM.	CHLORAL.
The first symptom, when the vapor is cautiously inhaled, is exhilaration of circulation, and of mental action.	The first symptom is an interference with the power of locomotion and certain heaviness of intellectual effort.
The second symptom is exaggeration of sounds in the room, of the power of sensation, and of disturbed locomotion.	The second symptom is somnolence, and sleep resembling natural sleep, from which the person may be partially aroused and then relapse into the same condition. In small doses, especially in some persons, this drug causes a mental excitement, sometimes delirium; by some authors, exaggeration of sensation occurs after the use of this drug, though others maintain that there is a diminution of sensation.
The third symptom is a light sleep, accompanied with muscular excitement, and followed very soon by sound sleep.	A third symptom, denied by no careful observer, is exaggeration of reflex movements.
The fourth symptom is muscular relaxation, and loss of reflex movements.	A fourth (toxical) effect is anæsthesia and muscular relaxation, most generally followed by death.

Morphia in Small Doses.—The administration, especially hypodermic, causes a soothing of the senses, followed by a gradual falling off to sleep, and the pulsations of the arteries become slower. This sleep may last for from three to ten hours.

In larger doses this drug produces an excitement, in some persons frenzy, and the intelligence is obtuse; the speech is incoherent; there is a muscular excitement, and, if the patient attempts to walk, he staggers. If sleep follows, the slightest noise or jar of the bed produces a sudden muscular tremor, showing an exaggeration of reflex movements. This last effect has been quite clearly demonstrated by Claude Bernard [1] in a series of lectures on anæsthetics, in 1869, before the College de France.

I have merely considered the effects of morphia in a person of ordinary health, and not during the complications of disease; and I might say the same was in my mind in speaking of chloral and chloroform.

The weight of evidence is certainly opposed to the theory of anæsthesia (except as a precursor of fatal result from an overdose) by moderate doses of chloral, while most clearly does it appear that exaggerated reflex movements follow the exhibition of an hypnotic dose. Consequently, the theory, that the symptoms following the use of chloral are the same as we should obtain in chloroformization, cannot at present be admitted. I am at present engaged in studying the physiological action of the drug, and hope, at some future time, to present the result of my investigations.

ART. IV.—*Impetigo Contagiosa: its Parasitic Nature.* By HENRY G. PIFFARD, M. D., Surgeon to the Dispensary for Diseases of the Skin.

ON the 3d of last March I was called upon by Dr. P., a practitioner of this city, with the request that I would see his child, who for some time had been suffering with a cutaneous affection, of which the following is the history:

In April, 1871, the child, then about eight months old, was vaccinated upon the left thigh with humanized virus. The result was a very fair vesicle. About a month subsequent

[1] *Gazette Hebdomadaire*, March and April, 1869.

to the vaccination there appeared upon the vaccinated thigh a few small vesicles, coming out, not simultaneously, but one after the other. The eruption was supposed to be varicella, and little attention was paid to it, and the child was sent into the country to reside with its grandparents. Soon after going there, the eruption became more general, spreading over both lower extremities, and also appearing upon the hands and arms, but not upon the trunk. It was accompanied with considerable itching and irritation. At this time, a neighbor's child, about three years of age, came and played with the baby. Upon this child there subsequently (exact time could not be learned) appeared a vesicular eruption which soon became general. Soon after this, the grandmother, aunt, and three uncles of the baby, residing in the same house, and in the habit of handling the child, became affected. Physicians were now consulted, some of whom diagnosticated the affection to be syphilis, others varicella, and treatment of various kinds was employed.

The baby recovered from the eruption in five or six months, leaving behind it dark-bluish discolorations as the only traces. The other members of the family likewise recovered after a variable period of from six to nine months.

In December the baby was brought back to the city entirely free from all trace of the eruption, except the discolorations referred to. Three months later, a woollen garment, which the child had worn a portion of the time while suffering from the eruption, but which she had not worn for several months past, was put upon her. About ten days subsequently, she was noticed to be suffering from cutaneous irritation, and careful examination revealed one or two vesicles upon the trunk, which in a few days were followed by others.

At this time, I was consulted. Upon examination of the child, the scar of vaccination was distinctly visible near the middle of the inner aspect of the left thigh. There were numerous bluish discolorations upon the limb and upon other portions of the body, and in addition about a dozen small yellowish crusts, two or three upon the thigh, and the rest scattered over the abdomen and back.

The diagnosis of "Impetigo Contagiosa" was made, and the application of *unguentum sulphuris* directed.

Two days later, I saw the child again, and in connection with my colleague Dr. F. L. Satterlee. The appearances were substantially the same as at my first visit, probably owing to the fact that the ointment had been applied upon the crusts and not upon the denuded surface; no new ones, however, had appeared. I removed four of the crusts, and ordered the others to be dealt with in the same manner previous to the application of the ointment. The crusts which I removed were placed in a six-per-cent. solution of *potassa fusa* for the purpose of dissolving as much as possible the animal matter, in order that vegetable organisms, if any were present, might be the more readily discovered.

March 10*th.*—Spots have nearly healed, and no new ones have appeared.

March 15*th.*—The father reports the child to be entirely well.

Microscopical Examination.—The first examination was made March 7th. By this time the potash had effected an almost complete solution of the crusts, with the exception of a few small, whitish masses which remained undissolved. One of these was transferred to a glass slide, a little glycerine added, and a thin covering glass applied. Upon examination with a $\frac{1}{5}$ (Grunow) little was found to attract attention except a few collections of very transparent epidermis, without visible nuclei, many oil-globules, and some granular matter. The first half-hour yielded, with the exception of the above, but negative results. At this time a slight modification of the method of illumination brought to view a minute circular body whose exceeding transparency had hitherto rendered it difficult of detection. Applying a higher power ($\frac{1}{10}$ Grunow), and carefully focussing, the body exhibited a peculiarity of form which was exceedingly striking. The effect produced by depressing the objective was similar to that with which we are all familiar in the case of the red blood-corpuscle, namely, the border first came into focus, as evidenced by the bright outline and dark centre, and then, upon further depression, the border became dark and the centre brighter. Reversal of the movement produced a reversed effect. The first impression was, that the body under examination was in reality a blood corpuscle, so thoroughly was its appearance counterfeited; but its minute size, and the fact that it had lain two days in the potash solution, precluded this possibility. Further search detected six or seven similar bodies in the same field, and prolonged examination of the preparation, and of others upon succeeding days, enabled me to determine the few following facts: The bodies, which were unquestionably vegetable organisms, were generally circular in outline, a few being oval. Their average diameter was $\frac{1}{5000}$ of an inch. Their form, as determined by focussing, *appeared* to be the same as that of the red blood-corpuscle, namely, a biconcave disk. The spores were usually isolated, but occasionally two or three, and in one instance four, were found together. In this latter case, three of them were circular, while the terminal at one extremity of the chain was oval, as shown in the cut Fig. 1.

FIG. 1.

In every instance, whatever the shape or size of the spore, the same appearance as to the relative thickness of the border and centre was observed.

The above observations were recorded, and the drawing and woodcut were made under the expectation that it might be a long time before another case of Impetigo Contagiosa would come under the observation of the writer. A second case, however, appeared sooner than we had anticipated. On the first of April there came to the Dispensary a young girl about fifteen years of age, sent from the Western Dispensary for Women and Children. Two weeks previous there had appeared, upon her left cheek, two or three "mattery blisters," which increased in size, and were followed by others upon different parts of the face. On examination I found upon the left cheek a greenish-yellow, pear-shaped crust, about two inches long and three-quarters of an inch in width near its base, and elevated about a line and a half above the surface. It was dry, and quite lightly attached to the surface beneath. It appeared to be formed by the coalescence of three smaller ones, which, in their development, had encroached upon each other and become confluent. The other crusts upon the face, four in number, were isolated and much smaller. One of them was said to have made its appearance within the last two days. This latter crust was removed, together with portions from the large one. The case was diagnosticated to be "Impetigo Contagiosa."

Microscopical Examination.—The two-days-old crust was placed in a three-per-cent. solution of potassa fusa, while a small fragment of the older crust was macerated for a few minutes in a mixture of liquor potassæ and glycerine, and immediately examined with a $\frac{1}{15}$ inch objective (Wales). The field presented was extremely striking. Not only were the hæmo-discoid bodies, which we had previously encountered, present in abundance, but others exhibiting an almost endless variety of form. This unexpected and puzzling difference of form naturally excited the liveliest curiosity, and led to the most careful examination, with a view to account for it if possible. It seemed improbable that the little hæmo-discoids, which at first we supposed to be the type-form, could have developed into many of the irregular forms presented. This view had to be abandoned, and search was made for some other simpler form or type which would permit of the variations noticed. Among others we observed a body resembling a spermatozoon, that is, with a round or rather oval head, and a thin filamentary tail, slowly *moving* across the field. We say moving in contradistinction to *moved* (by currents in the fluid). This motion was accompanied and probably caused by slow undulatory movements of the tail; nothing like the quick, jerk-ing movement of spermatozoa, but resembling those observed in many other protophytic organisms. In another instance the head was elongated, and in others again the tail consisted of two parallel filaments united below, and directly continuous above with the dilated cephalic extremity (so to speak). In others there were one or more slight dilatations along the course of the tail, as shown in the cut Fig. 2.

FIG. 2.

Evidence of apparently vital movement was not lacking in connection with some other individual spores upon the slide. One of the most striking and unmistakable instances of this was witnessed in the case of a body composed of a chain of three rounded portions, to one extremity of which was attached a filamentary prolongation, itself terminated by a minute rounded body. The whole outline of this curiously-shaped body could be distinctly seen, with the exception of a small portion upon one side, which was partially obscured by some granular matter. At this time, attention was diverted from the microscope for a few minutes, not more than ten, and upon returning to the instrument, instead of finding the object last observed, in its place was one of a somewhat different form (*vide* cut) recognized, however, as the same body by its relations to the granular matter above referred to. Observation was continued, in order to detect further changes, if any should occur. In a few minutes the extremity of the tail curved slowly up, and attached itself to the main body, forming a loop. This movement was distinctly visible. The slide was now temporarily put aside and attention was given to the crust which had been digesting in the potash solution for (by this time) about two hours. The crust was found to be converted into a viscid gelatinous mass, due to the action of the potash upon the pus of which it was in part composed. Portions of this jelly were placed upon slides and immediately sealed with Bell's cement. Examination revealed new shapes, among others an appearance such as would be produced by stretching a thin India-rubber ring (shown in the cut to the left). These elongated bodies varied greatly in size. Their breadth was nearly uniform, being about the $\frac{1}{10000}$ of an inch, but in length they measured from the $\frac{1}{4000}$ to the $\frac{1}{750}$ of an inch, as determined by an eye-piece micrometer.

Another body appeared to be composed of two concentric rings. How these were formed was at first not very clear, but subsequently became easily explainable by the observation of the forms depicted in the drawing (to the right), where their development is shown step by step. They were not, however, observed in the order depicted. The essential portion of the observation was finally completed by the discovery of certain fine filaments, not more than the $\frac{1}{30000}$ of an inch in breadth, but whose length varied from the $\frac{1}{2000}$ to the $\frac{1}{300}$ of an inch. The presence of these filaments readily accounted for the development of the various forms hitherto noticed, from the simple hæmo-discoid to the more complicated varieties. No direct observation of the transformation of the filaments into other forms was witnessed, but it seems to me to be a fair inference that such change occurs.

In addition to the well-marked bodies described, there were present epidermic cells, and a little oil, and besides many granular or rather molecular bodies, some slightly elongated, such as are found in most animal and vegetable infusions, and upon whose exact nature it would be somewhat hazardous to pronounce. These little bodies, as is usually the case, were in rapid motion.

No connection could be traced between the epidermis and the spores; occasionally, to be sure, a spore would be found upon or beneath an epidermic cell, but the occurrence was probably accidental, and nothing like the intimate relationship which exists between the epidermis and the *microsporon furfur* was detectible. No hairs were visible in any of the preparations, and hence nothing can be said concerning them. Observation of the circular spores, especially the larger ones, with a $\frac{1}{16}$ immersion of Powell and Lealand upon their grand binocular stand, the property of my friend Prof. Arnold, leads me to suspect that their form is not that of a section of a cylinder with central depressions, but rather that they are rings. It is possible, however, that in the smaller ones the hole in the ring is occluded by subsequently-deposited material. The difficulty of deciding this point can be readily appreciated when the minute size of the organisms is borne in mind.

The following conclusions seem fairly deducible from the observations above recorded:

The bodies are unquestionably vegetable organisms.

Their primitive form is probably filamentary.

From the filament the others are developed.

Their presence explains the contagiousness of the affection in which they are found, and brings it into the class of the Parasitic affections of the Skin.

The drawings were placed upon the wood from bodies actually under observation. In Fig. 2, their apparently larger size than in Fig. 1 is due to the fact that the former was made while observing with a $\frac{1}{15}$ and the latter with a $\frac{1}{10}$ inch objective. A number of preparations have been permanently mounted, and are subject to the inspection of those interested.

The interesting clinical features of Impetigo Contagiosa we shall not enter upon, but refer the reader for details upon these points to the original papers of Tilbury Fox (*British Medical Journal*, 1864, and *Journal of Cutaneous Medicine*, vol. iv.), and to the recent paper of Taylor (*American Journal of Syphilography*, etc., October, 1871). The observations above recorded have been further confirmed by the examination of some crusts from a case of Impetigo Contagiosa occurring in the practice of Dr. Satterlee, the details of which, together with a consideration of the etiology of the affection, will appear in the July No. of this JOURNAL.

Clinical Records from Private and Hospital Practice.

I.—*Vaccinal Syphilis.* (Extract from Dr. Lanoix's conference at the Sorbonne, Paris, on the 26th of July, 1869. Translated from *La Tribune Médicale.*)

In my opinion, gentlemen, there are two kinds of vaccinal syphilis, and I believe I may say that I was the first to establish this division in a precise manner.

These two varieties are, viz.:

1. Evoked or developed vaccinal syphilis.
2. Inoculated vaccinal syphilis.

Each has its distinct course and characteristics, which enable us to establish their separate diagnosis.

I have had many opportunities of observing *the evoked or developed vaccinal syphilis* in hospitals; examples like the following have also been brought to my notice:

An infant, a few weeks or even several months old, apparently perfectly healthy at the time it was vaccinated, either with human or animal vaccine; the eruption takes place, and is perfectly regular; the appearance of the pustule indicates no irregularity until the tenth or eleventh day, when this pustule, instead of stopping its growth, keeps increasing, and on the twelfth to fifteenth day it attains the size of a fifty centime or one-franc piece; instead of a healthy brown crust, such as that of ordinary vaccine, a greenish crust of a *sui generis* aspect, surrounded with a characteristic copper-colored circle, is formed; when this crust falls, under it will appear a reddish and bloody erosion, indurated at its base, and which, far from healing, grows larger and deeper. This is the outset of syphilis. Secondary symptoms soon show themselves, accompanied by all the other symptoms, which leave no doubt as to the diagnosis.

I now ask you, gentlemen, cannot the most experienced be deceived? Who is it can detect, up to the eleventh or twelfth day, that, under a pustule of regular appearance, an ulcerating process (*travail ulcératif*) is going on, which is the first but still invisible symptom of a reviving (or awakening) syphilis? Who can determine the precise moment when the action of vaccine ends and that of syphilis commences? And behold the terrible consequences which may result: should lymph (*sérosité*) be taken at a late period for the purpose of vaccination, the lancet would be charged with a mixture of the two viruses: syphilis and vaccine would be inoculated at the same time. This, in my opinion, is the cause of transmission of syphilis by vaccination; it explains the positive and negative results (as regards syphilis) obtained by inoculations taken from syphilitic subjects to be transmitted to healthy ones. I believe that at the commencement of its evolution the vaccinal pustule produces, by a sort of dialysis, vaccine alone, however syphilitic the subject may be on which it develops itself; but no sooner has it expended its own vitality, than it participates, like all the other tissues of the human organism, in the qualities or vices inherent to it. It is therefore easily understood that, should vaccine be taken from the pustule at its commencement, vaccine only will be transmitted; but a period of transition comes, when the vaccinal pustule is exhausted, and the *pseudo-chancre* appears, both united at the same point, mingling their secretions in one and the same fluid, which partakes of the properties of vaccine and of syphilis. This mixed liquid is the cause of *inoculated vaccinal syphilis*, of which I will now speak.

Without going into the numerous facts and well-known cases of vaccinal syphilis, such as have been reported by Cerioli, Hubner, Lecoq, Coggiola, Trousseau, Chassaignac, Hérard, and Viennois, and the more recent examples which have occured in the Departments of Lot and Morbihan, the diagnosis of which I shall discuss at another time, I wish only to mention those I have myself witnessed at the Academy of Medicine (Paris).

On the 12th November, 1868, having called at the St. Louis Hospital, I learned by mere chance from M. Hardy that on the preceding day he had observed a case of vaccinal syphilis in an adult who had been vaccinated at the Academy *several months previous.* Thinking that this might be a new case of epidemic vaccinal syphilis, I resolved to make personal inquiries, and I was at last enabled to ascertain the precise day on which this person had been vaccinated at the Academy. It was on the preceding 19th of August; on that same day, ten children and thirty-three soldiers had been vaccinated. Continuing my investigations, I learned the names of the two children from whom the virus had been obtained; their names were Conrad and Rousselot. A few of the soldiers only had been vaccinated from Rousselot; Conrad, on the contrary, had furnished most of the vaccine used on that day. On the 13th of November, the date of my inquiries, that is, nearly three months subsequent to the above vaccinations, the child Rousselot was in perfect health; as to the other child, it was dead, its death occurred two days after having supplied the virus at the Academy. Here is a short narrative of this case: Born of healthy parents, and in good health, it was sent from Paris to a village (in the Department of the Basses-Pyrénées), and confided to the care of a wet-nurse. This woman's bad conduct was notorious in the place. The child soon wasted away; its body became covered with ulcers, and in a few months it appeared so ill that the mayor of the place wrote to the child's mother, stating that it *had caught the disease from its nurse*, and that, being in danger of its life, it had better be taken away at once. The mother went immediately and found her child even worse than she expected. It was so emaciated and weak that she thought it would die in her arms; the genital parts and the anus were covered with ulcerations. She returned to Paris with it, and, after a few weeks' good care, it seemed a good deal better, and she took it to the Academy for vaccination; this was on the 12th of August. On the 19th she took it back. The vaccinal pustules were regularly developed—from these pustules the vaccine for that day's official vaccinations was taken. On the 21st the child died of diarrhœa (*diarrhée colliquative*). The unfortunate mother felt so convinced that her child had taken the contagious malady from its nurse, that, when I called upon her to question her, the first thing she asked me was, whether her poor child had given the disease to any one. Having thus ascertained whence the vaccine had come, I made it my duty to seek out and examine all those who were that day vaccinated. I spare you all the details of this long and painful search. Let it suffice that I succeeded in finding them all. I reported my sad discoveries to M. Depaul, and he accompanied me to see what I had seen. Out of the ten children vaccinated on the 19th of August, two had already died up to the 14th of November; the remainder were all syphilitic; and I assure you, gentlemen, that I did not rely upon my own judgment alone. I submitted my diagnosis to such masters as MM. Depaul, Bouchut, and Ricord, who entirely confirmed it.

Allow me, gentlemen, to state some of these observations; they are characteristic:

1. P., nine months old, born of healthy parents, and vaccinated at the Academy on the 19th of August, 1868. Six punctures producing six vaccinal pustules of good appearance; the crusts having fallen, left the cicatrix peculiar to vaccine, but hardly has this pathologic process (*travail pathologique*) ceased, than two of the cicatrices on the left arm broke out into sores, which alternately crusted and broke out, and did not finally heal until six or seven weeks. When I saw this child, on the 14th of November, it appeared, at first sight, quite healthy; the flesh, however, was soft and flabby, and its complexion yellowish. I thought that it might have perhaps escaped the terrible contamination, but it was not so: on further examination, I found its body covered with a red eruption (*éruption roséolique*), and mucous spots on the margin of the anus. Eight days after I again saw this child; in the interval it had been examined by another physician, who did not hesitate to diagnosticate a case of syphilis, and prescribe a specific treatment. This diagnosis was afterward confirmed by MM. Depaul and Ricord.

2. Emile C., eleven months old, born of healthy parents, who already had two remarkably fine, healthy children; vaccinated on 19th of August, 1868. Six punctures—four pustules, two on the right arm and two on the left. For the first three weeks, as the parents told me, every thing appeared to progress favorably, but at this period two of the cicatrices of the right arm and one on the left broke out into large pimples, which soon ulcerated, and these ulcers soon reached the size of a *franc-piece.* At the time I saw this child it was in a dreadful state, exceedingly emaciated and cachectical; it had the appearance of a little old man (*petit vieillard*), with three

indurated chancres on the arms, the body covered with a syphilitic eruption, mucous spots behind the ears, crusts on the head, and a ganglionic obstruction (*engorgement ganglionnaire*). These poor people were much distressed, and complained that, in spite of all the care they took of it, their poor child seemed to be dying of some unknown malady. I sympathized with them, and, determined to have M. Ricord's advice, I took the child to him that very evening. Here is his diagnosis:

"Vaccinal syphilis; indurated chancre (ulcus elevatum); indolent obstruction of the axillary ganglions; secondary symptoms; roseola, lenticular eruption. (Signed) Ricord."

This child was restored to health after a specific treatment of several months.

3. The following case is, I think, extremely remarkable, as it proves that vaccine, taken from a syphilitic subject to be transmitted to a healthy and non-vaccinated one, may inoculate syphilis without vaccine, which shows that the two viruses are simply contiguous, or side by side on the pimple: D., twenty-one months old. Inoculation from arm to arm had already been attempted three times on this child without success, at the Academy. On the 19th of August it was again brought in; three punctures were made on each arm: vaccination again failed; but four weeks after the attempt a red swelling appeared on two of the spots where the punctures had been made; these soon ulcerated, increased in size, and a greenish crust was formed; the crust still remained, when I saw the child in November. The child was much emaciated, its body covered with an (*papulo-vésiculeuse*) eruption, the axillary and cervical ganglions obstructed. This child died.

Such are some of the facts—the others were all similar. If any one has doubts, gentlemen, I regret he did not accompany me on this sad pilgrimage through the homes thus visited by vaccinal syphilis; he would, I think, have been convinced. These two kinds or sorts of vaccinal syphilis have, therefore, entirely different characteristics.

In the evoked or developed vaccinal syphilis, one single case may occur out of a large number of vaccinations effected on the same day. The first thing that takes place is *the trans-*

formation on the spot, from the twelfth to the fifteenth day, of the vaccinal pustule into a pseudo-chancre, which is immediately followed by all the other symptoms of syphilis. If inquiries are made as to the origin (or source) of the vaccine, the child is said to be healthy; but, should the investigation be followed up, it is found that one or the other of the parents has been syphilitic, and sometimes also that the vaccinated child has had syphilitic symptoms at the time of its birth, but which have since disappeared.

In *inoculated* vaccinal syphilis, on the contrary, when several persons are vaccinated on the same day, all or almost all are affected at the same time; the vaccine follows its regular course, and even reaches so far as cicatrization; but on the twenty-fifth to the thirtieth day one or more of these cicatrices will break out into a pimple (*papule*), which is the starting-point of an indurated chancre. This specific ulceration heals but slowly, and is accompanied by an indolent ganglionary obstruction (*engorgement ganglionnaire*), and in about six weeks, that is to say, two or three months after vaccination, the secondary symptoms of syphilis will appear. Should the origin of this unfortunate operation be sought after, it will appear, like the above facts which took place at the Academy, that the child from whom the vaccine was taken was evidently syphilitic, while those who were vaccinated were perfectly healthy before the operation, and belonged to healthy parents.

For us, gentlemen, who are perfectly convinced of the existence of real danger in Jennerian vaccination, we know of no *certain* and *practical* means of avoiding it; every thing that has been proposed is *uncertain* and *ineffectual*. Animal vaccination alone gives us a logical and practical solution of the difficult problem.

II.—*Conservative Surgery in Minor Operations.* By A. P. Grinnell, M. D.

John Hunter believed that the surgeon who practised amputation of any part of the human body acknowledged his ignorance or want of skill, for he should have possessed the knowledge or power of saving the whole, not destroying a part. It would, indeed, be gratifying to the surgeon who is called to amputate at the thigh for injury below the knee, if he possessed the ability of instituting a course of treatment which would result in perfect restoration of the whole limb. Conservative sugery has accomplished much in checking the wholesale destruction of diseased or injured parts, and taught the surgeon to wait, withhold the knife, and observe the power of Nature to effect a cure. Formerly, the universal treatment followed for inflammation of a joint was amputation above the affected part; now we know a limb is seldom, if ever, lost, even if the knee or ankle-joint has suffered severe injury. In proof of Nature's capability in effecting restoration of injured, even mutilated joints, I would cite two cases which have recently come under my observation:

CASE I.—A lad, aged fourteen, of a vigorous, healthy constitution, received a severe wound on the right hand by a circular saw. Upon examination of the injury, I found that the saw had entirely severed the third finger, with the exception of a little cuticle which held the end suspended between the first and second articulation, had disarticulated the first and second phalanges at the second joint, and, besides, injured the soft parts in different portions of the hand. My first thought was, to finish the amputation of all dependent portions, but, upon reflection, I determined to afford Nature an opportunity of showing her skill in establishing a union of these lacerated tissues. The subsequent treatment involved much care and attention, but the result was sufficiently gratifying to reward the effort of preserving the whole hand. We have been taught that in all injuries of this character, where union has taken place, sensation was lost and the power of motion considerably impaired; but in the above case bone united firmly with bone, tendon with tendon, nerve with nerve, and the circulation of blood was fully restored. Sensation is perfect beyond the point of injury, and, to have secured this result, nerve must have united with nerve, for the amputation of all sensitive portions was complete. The power of motion is also preserved, although not perfect in one finger, owing to anchylosis of a joint.

CASE II.—This case resembles the former in all particulars, except that the instrument inflicting the injury was a scythe instead of a saw, and the seat of injury being the metacarpo-phalangeal articulation of the thumb. All the flexor muscles of the thumb were severed, and the joint fully opened, allowing the disarticulated thumb to rest upon the wrist. The treatment consisted in bringing the thumb to its proper place, securing the cut edges by sutures, and protecting the whole from air by the application of isinglass-plaster. At the expiration of six days, on removing the dressings, I found the wound entirely healed, no inflammation having followed the injury. Sensation and motion perfectly restored, and mobility of joint unimpaired. From the result in these cases, and others similarly treated, I am disposed to think we are justified in making the attempt, at least, to save every and all portions of the hand, however much it may have undergone mutilation. There has been no artificial device or apparatus, nor can there be one discovered, which could be substituted for any part of the human hand; and the surgeon confers a lasting benefit upon his patient by remembering this fact, and with patience, judicious treatment, and attention, the result will be a reward beyond his expectation.

III.—*Surgical Clinic. Burlington Medical College, Burlington, Vt.* By Benjamin Howard, M. D., Professor of Surgery. Reported by A. P. Grinnell, M. D.

Case I. *Adipose Tumor.*—M. W., forty-two years of age, by occupation a farmer, who received an injury of the thigh eighteen years ago, which resulted in the development of a tumor. It had gradually increased in size until the weight, at time of extirpation, exceeded eight pounds. No pain or inconvenience accompanied its development, but the patient suffered annoyance from the weight and friction, which was always increased in the act of walking. Diagnosis, adipose tumor. Treatment, removal. The operation was performed in nine minutes. The usual dressings were applied, and in six days from the time of operation the wound was entirely healed, and the patient allowed to go home.

Case II. *Cyanosis.*—Female child, aged three. The blue and unhealthy appearance of the patient was indicative of an existing organic disease, and upon examination it was pronounced a case of cyanosis. No treatment recommended, beyond attention to diet and general health.

Case III. *Nasal Polypus.*—D. M., aged nineteen, has a fibrous polypus of the nose, which completely fills the posterior nares, but is not discernible anteriorly. Complete removal effected from behind, allowing free passage of air through the nostril.

Case IV. *Multilocular Adipose Tumor.*—J. R., aged forty-four, laborer, in good health, had been troubled for eighteen years by the development of a tumor in the posterior cervical region. Upon examination it was pronounced a multilocular adipose tumor. The patient refused to take an anæsthetic, and submitted without a murmur to its removal, although considerable time was required in the operation, owing to firm attachment of surrounding cellular tissue, making enucleation impossible, and necessitating its entire removal by dissection. Weight of tumor about one pound. The patient has suffered no inconvenience from the operation, and the wound is healing by first intention.

Bibliographical and Literary Notes.

Art. I.—*The Works of Sir James Y. Simpson*, Bart., M. D. New York: D. Appleton & Co., 1872.

Vol. I. Selected Obstetrical and Gynæcological Works, containing the Substance of his Lectures on Midwifery. 8vo, pp. 852.

Vol. II. Anæsthesia, Hospitalism, Hermaphroditism, and a Proposal to stamp out Small-Pox and other Contagious Diseases. 8vo, pp. 562.

Vol. III. Clinical Lectures on the Diseases of Women. 8vo, pp. 789.

A review of the medical works of the late Sir James Y. Simpson is entirely unnecessary at this day. The profession in this country, as well as the world over, are already acquainted with his papers, many of which marked most positive advances in the science and art of medicine, especially in the departments of obstetrics and gynæcology. These advances have been incorporated into our systematic text-books and works of reference, and so have become as familiar as household words; and besides this, the former (but now incomplete) edition of Prof. Simpson's works, edited by Drs. Priestley and Storer, is well known, and has been widely circulated both in this country and Great Britain. Thus an actual experience covering a long series of years has set the stamp of approval upon his writings, and has indicated clearly their worth, so that without hesitation we can now pronounce them among the most valuable contributions ever made to medical literature. Our object, at present, is therefore merely to call attention to the appearance of this superb and complete edition, and to acquaint our readers with the very many new and hitherto uncollected papers which it contains.

The first volume contains the substance of Sir James's course of lectures on midwifery, to which are added the special memoirs and contributions on the subject of obstetrics that were originally published in different periodicals. In the last section of the volume are gathered a number of papers relating to the non-puerperal diseases of women, but all of them have a kindred relation to the general study of obstetrics. References have been made to the original publications by the editor, Dr. J. Watt Black, who for five years was the assistant of Prof. Simpson, living in the same house with him, and who presumably, therefore, may be considered to have had an intimate knowledge of his opinions and modes of practice. Whenever with the lapse of time Sir James had changed such opinions or practice, Dr. Black has indicated the difference; for, although the author was somewhat tenacious of his own views, yet so vast was his experience, and so thorough his acquaintance with the best work of other authorities and laborers in the same field, and withal so liberal and eminently catholic were his opinions, that he did not hesitate to modify his views to accord with the advancing state of medicine. The lecture-notes, which make up the first section of the volume, seem to us out of place in a publication of this sort. These notes are intended only as aids in the lecture-room, and, as every one knows who has had experience in such work, they merely map out the main points to be considered; and, besides this, each teacher's syllabus will convey by his peculiar style of putting things, and by the association of ideas, certain hints to the author, that are not apparent to the cursory reader or student.

Merely to catalogue the titles of the different papers in this volume would extend this notice to an undue length, for there are no less than eighty-eight separate memoirs and shorter articles. We content ourselves, therefore, with saying that among them are found all those contributions to the study of obstetrics which gave their author so famous a reputation in this special branch of medicine, and which resulted in making clear many subjects, up to that time involved in obscurity, and also to indicate many and very positive additions in hitherto unexplored fields of medical research. Throughout them all, as well as in the other volumes of this series, there are displayed that remarkable acquaintance with medical and other literature, and that almost marvellous erudition, which placed the author's name so high upon the scroll of fame. Indeed, the man seemed always to have been a perfect walking encyclopædia of learning, and when occasion demanded he could depart from his purely professional work—as, viz., in his archæological and other studies—with as much ease as he would discuss a clinical problem, and still maintain his unrivalled reputation for brilliant as well as solid acquirements.

More than the half of Volume II. is taken up with the different papers of Prof. Simpson on anæsthesia—a subject in which the author was particularly interested, and in which he extended his already well-established fame. As is well known, he was the discoverer of the anæsthetic properties of chloroform; he was also the first to apply anæsthesia in midwifery practice for the purpose of mitigating the pains of parturition. In 1856, he received the Monthyon prize of two thousand francs awarded by the French Academy of Medicine, "in consideration of his services to humanity by the introduction of anæsthesia into the practice of midwifery," and for the same services he was later created a baronet, besides receiving distinguished honors and recognitions of his work from various other sources.

At its very inception, anæsthesia, particularly in midwifery practice, met with a determined and fierce opposition as well from physicians, from whom better things might have been expected, as from theologians, from whom nothing else might have been anticipated. Convinced of the inestimable value of this procedure, and endowed as was Sir James with somewhat of a combative disposition, this opposition forced upon him the preparation of the numerous papers on the question of anæsthesia which are here collected. They are more in number than would have been demanded had the author been engaged in preparing a systematic work on the subject, and in some instances they partially repeat the material of one another, but they were rendered necessary by the constant attacks and objections to which he was called upon to reply. These papers are arranged by the editor—Prof. Simpson's son, Sir W. G. Simpson—in their logical rather than in their chronological order, and are grouped as follows: I. The History of Anæsthesia. II. Defences of Anæsthesia. III. The Nature and Power of Various Anæsthetic Agents. IV. Applications of Anæsthesia in Surgery and Medicine. V. Applications of Anæsthesia in Midwifery; and VI. Local Anæsthesia.

Under the first head above mentioned are found the masterly replies to the gratuitous and entirely uncalled-for attack upon Sir James by Prof. Jacob Bigelow, of Boston, made but a few months before the former's death. The cause of this attack was, viz.: In October, 1870, the freedom of the city of Edinburgh was presented to Sir James Y. Simpson, and the lord-provost, in the presentation address upon that occasion, saw fit to allude to his distinguished services in connection with the subject of anæsthetics, and spoke of the application of chloroform to the relief of human suffering as "among the greatest medical discoveries of modern times." In his reply, Sir James, as Dr. Bigelow thought, fell very far short of his duty and was severely censurable for not going into the history of anæsthesia, and giving to America her share of credit in the discovery, for it is admitted on all sides that the original discovery of anæsthesia—not of chloroform—was of American origin. If one has still any patriotic misgivings in the matter of this imputation against Prof. Simpson, let him read his papers upon the subject written years before the reply to Dr. Bigelow, and he will learn what Dr. Bigelow ought to have been, and probably was, aware of at the very time he made his attack, viz., that no one ever yielded to America more hearty and handsome acknowledgments for her original discovery and her practical application of anæsthesia than did this same much-abused Sir James Y. Simpson.

As regards the use of ether or chloroform in obstetric practice, there are still some among the older members of the profession who have not yielded their opposition; and, although we cannot ourselves appreciate their reasoning, nor understand the force of their objections, we must in charity admit that they are conscientious and honest in their convictions. We would urge upon all such a careful perusal of these papers of Prof. Simpson, for we feel that they are enough to answer satisfactorily all their objections, and to convince the most skeptical mind, unless the unfortunate possessor thereof has reached a period in life when the mind will not respond to new impressions, or readily admit new ideas. To the beginner in medicine, too, these papers are of equal value in assisting him to a competent knowledge of the whole subject, and enabling him to reply to the absurd prejudices which will occasionally confront him in practice. It is useless, however, for him to argue the point with a patient who conscientiously holds that the practice is unscriptural, and therefore immoral. But, believing, as we do, that anæsthesia is one of the greatest gifts which God ever vouchsafed to man, we have always deemed it our duty to offer to such unbelievers the benefits of the blessing, and, if they reject it, we have generally found in practice that they are speedily brought to their senses after wrestling for a time with the agonizing tortures of parturition.

Next in order in this volume we find collected, and arranged into a unity of plan, the papers upon "Hospitalism," which were among the later contributions of the author to our medical literature. The term itself is, perhaps, as good as any equally concise one that could be suggested, though it does not convey to one not familiar with the subject any clear idea of its meaning. By it is understood the influences which the residence in hospitals has upon the results of disease, and especially of surgical diseases and operations; and the object of the author was, by comparison of the results obtained in the large metropolitan and the smaller provincial hospitals, and in the isolated houses of country practice, to arrive at some conclusion regarding the best methods of providing for public patients. He was satisfied, from his investigations, that the village or cottage system, as it has been styled in Great Britain, is vastly more advantageous for the welfare of the inmates than the colossal and palatial hospitals which have been the tendency in late years, especially in large cities. On this single point, we think there can be no doubt, but ease and economy of administration, and the exigencies of service, and many other points connected with hospitals, cannot be overlooked or evaded, and naturally, therefore, the question still remains an open one, and has arrayed on its two sides very powerful advocates. Our own opinion, derived from some ten years of experience and residence in different hospitals, is, that a very large and many-storied hospital is almost an unmitigated nuisance, and that it needs to be handled as gingerly and cautiously as a powder-magazine, to prevent its becoming a source of destruction to those within its walls. These papers will do good service by the large collections of statistics regarding hospital structures, even if one should dissent from the author's conclusions; and we would recommend their perusal by every unfortunate (we speak advisedly) who may, whether willingly or not, be appointed a member of a hospital-building committee. And, finally, it ought to be said that no one but a well-educated and experienced physician is competent for such a position.

The volume closes with the well-known monograph upon hermaphroditism, in which are manifested, to a wonderful degree, the author's erudition and power of research to which we have already alluded.

Volume III. contains the clinical lectures, fifty in number, of the author, on the diseases of women. Ten of these lectures are now published for the first time. The remainder mostly appeared in the *Medical Times and Gazette*, during the years 1859–'61. All are now edited by Sir James's nephew, and successor in the chair of Midwifery and Diseases of Women, in the University of Edinburgh, Alexander R. Simpson, M. D. Some few of the lectures belong more strictly to the study of obstetrics, and might with propriety have been assigned to the volume devoted to that department. Such are the lectures on spurious pregnancy or pseudocyesis, puerperal mania, and hypochondriasis, cranioclasm, rupture of the perinæum, and phlegmasia dolens; and in return for this transfer an exchange might have been made of some of the papers in the first volume relating to the non-puerperal diseases of women. But the unity of design, in keeping all the clinical lectures and the fugitive essays and systematic memoirs by themselves, would have been lost by this distribution, and the arrangement of the editors must therefore be considered as the wiser course. After an introductory lecture on the diagnosis of diseases of women, which was originally published in the *Edinburgh Monthly Journal of Medical Science*, 1851, the author devotes two lectures to the discussion of the subject of vesico-vaginal fistula, and draws a large share of his material from American sources, Sims, Bozeman, and others, and moreover—will our pugilistic though patriotic friends make a note of this point?—he makes continuous and prominent acknowledgments of his indebtedness to them. The remainder of the lectures cover a discussion of most of the important diseases of women, and they are all treated with great fulness of detail, and with remarkable clearness and conciseness of language. As these lectures are clinical, they of course are not arranged in any logical order of succession, but a copious table of contents and very full indexes are added, so that reference to any point or subject is easy. The same copiousness of indexes is also found in the other two books of this series. The volume is largely and superbly illustrated, giving additional aid in this way to descriptions of the reading-matter.

For the publishers we are bound in duty to say that these books are gotten up in that excellent style which makes all their medical publications so creditable to American mechanical art, while the astonishingly low price at which the volumes are issued should put them in the library of every physician in the land. And we are sure that money could not be placed to better advantage than in acquiring a set of these complete works of one of the master-minds of the profession in modern times, of whom it has been truly said that "no physician was ever more widely known, and no man, since the world was, was ever more greatly loved."

ART. II.—*On the Treatment of Pulmonary Consumption, by Hygiene, Climate, and Medicine, in its Connection with Modern Doctrines.* By JAMES HENRY BENNET, M. D., Member of the Royal College of Physicians, London, etc. New York: D. Appleton & Co., 1872, 8vo, pp. 190.

THE name of the author of this essay heretofore was identified with uterine diseases, his practical study of these diseases having had a very wide-spread and important influence on the opinions and practice of physicians. Of late years he has devoted special attention to pulmonary consumption, and he was led to do this by incentives derived from his personal experience, having himself become affected with tuberculous disease, the progress of which, happily for others, he succeeded in arresting by efficient measures, hygienic, climatic, and medicinal. The knowledge of this fact, together with his previously-acquired reputation, has led patients from all countries to seek his counsel, and thus, probably, there are few practitioners who, during the same period, have had larger opportunities for the clinical study of pulmonary consumption. Whatever Dr. Bennet, under these circumstances, has to say concerning the disease just named, is deserving of careful consideration.

In this little volume, which the reader cannot but wish had been much larger, Dr. Bennet treats of pulmonary consumption from a clinical stand-point. With reference to the importance, at the present time, of recalling attention to this aspect of the disease, we would commend to the reader's attentive perusal the author's preface. We are tempted to quote the greater part of it. To do this, however, would require too much space, and, moreover, we hope it will be rendered superfluous by the fact of the work being in the hands of every reading member of our profession. Suffice it to say that he presents in a strong light the present need of standing by the truths of clinical experience, without yielding to the seductive influence of speculations based on uncertain histological data.

We would by no means disparage histological researches; they have already rendered valuable service to pathology, and it is to be hoped that much more is to be expected therefrom. But when it is attempted to interpret histological observations without regard to, and, indeed, in defiance of views well grounded in clinical experience, let it be considered how discrepant are the doctrines based by different histologists on their own observations. As our author remarks, Virchow and Prof. Bennet, of Edinburgh, represent opposing doctrines concerning the pathology and treatment of pulmonary consumption, and yet both are equally eminent as histologists. But the differences of opinion are not confined to these men and their followers: with respect to each one of the so-called "modern doctrines," histologists of equal eminence are on either side. Let any point be cited as established by the observations of certain histological observers, other not less able and reliable observers have arrived at a different conclusion. Villemin, whose discovery of the inoculability of tubercle as one of the great events in the pathology of to-day, claims that the views of Laennec, which some of our German brethren seem to take a peculiar satisfaction in opposing, are in all respects sustained by histological facts. But this is not the occasion for entering into a discussion of the topics involved in the "new doctrines." We cannot help adding, however, that it were a great advantage if the great work of Louis on Phthisis (great in character and results, although of small bibliographical dimensions) should be more read nowadays. The contrast of Louis's researches with the more pretentious but conflicting, dubious, speculative investigations of some histologists, could not fail to exert a healthful influence on the minds of those who are prone to consider novelty as the criterion of truth.

Lest these desultory remarks should leave on the minds of our readers an erroneous impression of Dr. Bennet's *brochure*, we would state that it is by no means devoted to the discussion of pathological questions; but it presents directly, plainly, tersely, the author's views concerning the treatment of pulmonary consumption. Within the compass of a few pages, Dr. Bennet embraces a great many important practical injunctions, based on his ample experience. Without reproducing any of these here, we conclude by saying to those who have honored this brief notice with a perusal, get the book and read it.

ART. III.—*Vegetable Parasites and the Diseases caused by their Growth upon Man.* By JAMES C. WHITE, M. D., Professor of Dermatology, Harvard University. Pp. 50. Boston: Wright & Potter.

WE believe every dermatologist of note, with the exception of Wilson and Cazenave, admits the parasitic nature of certain cutaneous affections. Those whose parasitic origin is placed beyond question, according to our author, are favus, produced by the *achorion Schönleini*, ringworm, by the *trichophyton tonsurans*, and pityriasis versicolor, by the *microsporon furfur*. The usual seat of favus is upon the scalp, but it may appear upon any portion of the cutaneous surface, and is not infrequent upon some of the lower animals, especially the mouse and the cat, from whom it is sometimes transferred to man. Ringworm may also invade any portion of the body, presenting, however, a somewhat different aspect according to its location. Upon the general surface it presents the usual and long-recognized appearances of ordinary ringworm; upon the scalp it gives rise to a not very frequent but exceedingly intractable form of disease; while upon the hairy portions of the *face* it produces one of the forms of sycosis. Under this latter term our author believes that two distinct diseases have usually been included—one a simple folliculitis, and the other a folliculitis produced by the trichophyton. Pityriasis versicolor invades the general surface, especially the trunk in front and behind. Of the cutaneous affections whose parasitic nature admits of doubt, alopecia areata stands prominent. Most writers deny its fungous origin; but our author, after a careful review of the evidence *pro* and *con*, and the affirmative results of his own investigation, states his decided conviction to be in favor of its parasitic origin in some cases; suggesting, however, that, under the term alopecia areata, two different affections may have possibly been included, the one parasitic and the other not.

The opinion entertained by some authorities that the fungi above noticed are but varieties of a single vegetable organism, and that they are mutually convertible, our author believes to rest upon insufficient evidence, as likewise that they are derivable from the common moulds; with the exception, perhaps, of the aspergillus of the external auditory canal, which appears to be indentical with the aspergillus found elsewhere.

Upon the whole, the author has treated the subject in a very able and thorough manner, and we heartily commend his little book to those interested in parasitology.

ART. IV.—*Memoranda of Poisons.* By the late THOMAS HAWKES TANNER, M. D., F. L. S., etc. Third and completely revised edition. Philadelphia: Lindsay & Blakiston, 1872.

Dr. Rigby's Obstetric Memoranda. Fourth edition, revised and enlarged. By ALFRED MEADOWS, M. D., etc. Philadelphia: Lindsay & Blakiston, 1872.

BOTH these little works are good of their kind, and both have been greatly improved by the careful revision they have undergone. That on poisons has been materially altered, and is now as complete as it is possible to make a treatise of so diminutive a size. These condensed hand-books are useful so long as they are resorted to only as aids to the memory, or for hasty reference.

ART. V.—*A Practical Treatise on the Diseases of Women.* By T. GAILLARD THOMAS, M. D., Professor of Obstetrics and Diseases of Women and Children, in the College of Physicians and Surgeons, New York, etc., etc. Third edition, enlarged and thoroughly revised. With two hundred and forty-six Illustrations on Wood. Philadelphia: Henry C. Lea, 1872, pp. 784.

LITTLE more is necessary, in commending to the profession a new edition of Prof. Thomas's work, than to say that it is greatly enlarged, thoroughly revised, and enriched by the author's latest opinions and experience. Several entirely new chapters have been added, and many of the old ones rewritten and considerably modified. The term "chronic metritis" is discarded, and that of areolar hyperplasia substituted for it. The reason for this change is very ably set forth in the chapter treating of general uterine pathology. The demand thus early for a third edition is sufficient proof of the estimation in which the previous editions are held, and it is quite within the bounds of truth to say that, for the purposes of the general practitioner and student, the present volume is the best treatise on gynæcology in the English language. Prof. Thomas is not only a thorough master of his subject, but he possesses also the rare faculty of presenting his views in a clear, concise, and systematic form, and of so dividing, subdividing, and classifying the various topics as to render his book exceedingly convenient either for reference or study. The treatise is unquestionably one that should be in the hands of every physician and student in the land.

Books and Pamphlets Received.—An Experimental Research on the Antagonism between the Actions of Physostigma and Atropia. By Thomas R. Frazer, M. D., Lecturer on Therapeutics at Surgeons' Hall, Edinburgh. From the Transactions of the Royal Society of Edinburgh, vol. xxvi. Edinburgh: Neill & Co., 1872.

The Treatment of Venereal Diseases: A Monograph on the Method pursued in the Vienna Hospital, under the direction of Prof. Von Sigmund; including all the Formulæ. By M. H. Henry, M. D., Surgeon to the New York Dispensary, Department of Venereal and Skin Diseases, etc. Adapted and arranged from the German. New York: William Wood & Co., 1872.

Worms. A Series of Lectures on Practical Helminthology, delivered at the Medical College of the Middlesex Hospital; with Cases illustrating the Symptoms, Diagnosis, and Treatment of Internal Parasitic Diseases. By T. Spencer Cobbold, M. D., F. R. S., Honorary Correspondent of the Academy of Sciences of Philadelphia. London: J. & A. Churchill, 1872.

A Treatise on the Diseases of Infancy and Childhood. Second edition, enlarged and revised. By J. Lewis Smith, M. D., Curator of the Nursery and Child's Hospital, Clinical Lecturer on Diseases of Children, and Professor in Bellevue Hospital Medical College, etc., etc. Philadelphia: Henry C. Lea, 1872.

Lectures on Aural Catarrh; or, the Commonest Forms of Deafness and their Cure. (Mostly delivered at St. Mary's Hospital.) By Peter Allen, M. D., Fellow of the Royal College of Surgeons, Edinburgh, late Surgeon to the Metropolitan Ear Infirmary, Dublin, etc., etc. New York: William Wood & Co., 1872.

An Investigation concerning the Mechanism of the Ossicles of Hearing, and the Membrane of the Round Window. By Charles H. Burnett, M. D., Fellow of the College of Physicians of Philadelphia, etc. New York: Wm. Wood & Co., 1872, pp. 13.

On the Physiology of Syphilitic Infection. By Fessenden N. Otis, M. D., Clinical Professor of Venereal Diseases at the College of Physicians and Surgeons, etc. Reprinted from the *Medical Gazette.* New York: F. Leypoldt, 1872, pp. 29.

The Origin of Cancer: considered with Reference to the Treatment of the Disease. By Campbell De Morgan, F. R. S., Surgeon to the Middlesex Hospital. Reprinted in part from the *Lancet.* London: J. & A. Churchill, 1872.

Valedictory Address to the Twentieth Graduating Class of the Woman's Medical College of Pennsylvania. By Henry Hartshorne, M. D., Professor of Hygiene and Diseases of Children. Philadelphia: Jas. B. Rodgers & Co., 1872.

Mortality of the United States (by States and Territories), with Distinction of Sex and Percentage of Deaths to Population at the Censuses of 1870, 1860, and 1850. Washington, D. C., Department of the Interior, 1872.

Irrigations of Ice-water as a Means of arresting Hæmorrhage in Cases of Placenta Prævia. By W. O. Baldwin, M. D., Montgomery, Alabama. Louisville: Hull & Brother, 1872, pp. 13.

Catalogue of the Library of the Surgeon-General's Office, United States Army. With an Alphabetical Index of Subjects. Washington, Government Printing-Office, 1872.

Supplement to the above Catalogue, No. I. List of American Medical Journals.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia, from May, 1870, to February, 1872. Philadelphia: Collins, 1872, pp. 411.

Practical Remarks on Vaccination. By Frank P. Foster, M. D., House-Physician to the New York Dispensary. New York: D. Appleton & Co., 1872.

Forty-sixth Annual Report of the Surgeons of the Massachusetts Charitable Eye and Ear Infirmary. Boston: James Campbell, 1872.

Fourth Annual Report, for the Year 1871, of the St. Mary's Hospital of Philadelphia, under the care of the Sisters of St. Francis.

Some Remarks on Cataract. By Edward G. Loring, M. D. Read before the American Ophthalmological Society, July, 1871.

Report of the Medical Superintendent of the Asylum for the Insane, Toronto. Toronto: Hunter, Rose & Co., 1872, pp. 31.

Catalogue of Second-hand and Valuable Medical and Surgical Works. By James Campbell, 18 Tremont Street, Boston, 1872.

Transactions of the American Otological Society; Fourth Annual Meeting, held in Newport, R. I. Boston, 1871.

Twenty-third Annual Report of the Indiana Hospital for the Insane, for the Year ending October 31, 1871.

Annales de Dermatologie et de Syphiligraphie, publiées par le Docteur A. Doyon. Paris: G. Masson, 1872.

Proceedings of the Twenty-first Annual Meeting of the New York State Homœopathic Medical Society, 1872.

Twenty-third Annual Announcement of the Woman's Medical College of Pennsylvania.

Eleventh Annual Report of the Alabama Insane Hospital.

Rhode Island Eighteenth Registration Report, 1870.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, April 22, 1872.

DR. ELLSWORTH ELIOT, Vice-President, in the chair.

NEW MEMBERS.

THE following gentlemen, recommended by the Comitia Minora, were elected to membership: Drs. Thomas K. Cruse, Oscar A. Ruffel, Lewis Tici, Herman G. Klotz, George C. Stubling, Ludwig T. G. Simpect, Allan M. Schaffer, and A. Nelson Beach.

CARBOLIC ACID AS AN ANÆSTHETIC.

Dr. Andrew H. Smith, from the Committee on Intelligence, read a Report on Therapeutics, from which we extract the following:

Dr. Bill, of the United States Army, and Dr. Squibb, have recently called attention to the action of carbolic acid as a local anæsthetic. As their papers have had a very general circulation, I will not give a synopsis of them, but merely state the results of some experiments that I have made upon my own person, which agree entirely with their experience.

In the first experiment I painted a spot on the forearm, about an inch in diameter, with carbolic acid of about the strength of 85 per cent. For about a minute there was a slight burning sensation, after which the integument became entirely insensible, the cuticle being whitened and shrivelled, and the spot slightly elevated. I then with a scalpel made an incision about half an inch in length through the whole thickness of the integument. This was done without even feeling the contact of the knife. The capillary circulation seemed not to be materially interfered with, as the blood flowed as freely as it would from a similar wound under ordinary circumstances. The reparative process was also not impaired, adhesion taking place immediately. Three hours after the application of the acid a needle could be thrust freely into the skin without causing pain.

In the second experiment carbolic acid was applied as before, and ten minutes after a fly-blister was placed upon the spot. The blister remained eight and a half hours without causing any pain, and without producing vesication.

In two instances I have applied the acid previous to incising a whitlow. The operation was almost painless, but, as the whitlow was in each case of the superficial variety, the test was not entirely conclusive.

Inhaled in the form of spray, I have found the acid very useful in allaying irritation of the bronchial mucous membrane; coughs which have resisted all ordinary treatment have been immediately relieved, and in the course of two or three days entirely removed.

I would suggest the use of a strong solution of carbolic acid as a revulsive, in cases in which a continuous impression is desired. While causing but little suffering, it produces an intense hyperæmia of the skin, which persists for eight or ten days, and is followed by desquamation of the cuticle.

METEOROLOGY OF NINE WEEKS.

Dr. D. H. Goodwillie, its chairman, read the Report of the Committee on Meteorology. We give a transcript of the last two reports of this Committee.

From February 4 *to March* 9, 1872.

Barometer: mean, 29.797 inches; maximum, February 8th, 30.398 inches; minimum, February 13th, 29.514 inches.

Thermometer: mean, 29.50° Fahr., maximum, February 24th, 52°, minimum, March 5th, 4°.

Rain on February 4th, 6th, 11th, 13th, and 25th; snow on March 2d and 9th.

From March 10*th to April* 6*th, inclusive.*

Barometer: mean, 29.901 inches; maximum, March 16th, 30.144 inches; minimum, March 31st, 29.255 inches.

Thermometer: mean, 36.75°; maximum, March 29th and April 6th, 59°; minimum, March 21st, 16°; range for the four weeks: 39.8°.

Rain on March 10th, 15th, and 31st; snow on March 12th, 17th, 23d, and 26th. Amount of water 3.92 inches. Prevailing wind was from the northwest.

DISEASES OF THREE MONTHS.

Dr. Charles P. Russel, chairman of the Committee on Diseases, read its report.

The Committee on Diseases have the honor to report as follows:

During the past quarter, comprising January, February, and March, the following *contagious diseases* were reported to the Bureau of Sanitary Inspection of the Health Department: Typhus fever, 32; typhoid fever, 97; scarlatina, 1,043; measles, 241; diphtheria, 181; small-pox, 1,043.

The *mortality* of the three months was excessive, amounting to 7,406 deaths—a weekly average of 570; the weekly average of the corresponding period of 1871 having been 510, and the weekly average of the whole year having been only

522. During the three weeks which have elapsed since the end of the quarter, there have been registered 705, 691, and 677 deaths, respectively. Should a proportionate mortality prevail for the remainder of the year, its aggregate, even in the absence of any serious epidemic, will exceed by at least 7,000 that of any former year, and by 10,000 the annual average of the previous ten years.

The principal *zymotic diseases* of the past quarter were as follows: Small-pox occasioned 320 deaths—a weekly average of 25; its largest quarterly mortality in this city during the present century. Its registered deaths for the past three weeks have been 33, 35, and 40, respectively. It would appear, therefore, to be on the increase. 106 deaths were referred to measles, 301 to scarlatina, 93 to diphtheria, 201 to membranous croup, 247 to whooping-cough—an unusually large number—and 227 to diarrhœal affections. Typhus and typhoid fevers were not very prevalent, the former having caused 37 and the latter 59 deaths. Remittent fever produced 43 deaths, intermittent 10, and typho-malarial 5. 69 deaths were ascribed to cerebro-spinal meningitis, which began to manifest a decided epidemic tendency about the beginning of February, and has since, until very recently, been steadily progressing in fatality. Its registered deaths for each of the three weeks ending last Saturday were 31, 52, and 27, respectively. Puerperal diseases were more than usually fatal, having occasioned 137 deaths, of which 39 were due to puerperal fever attended with septicæmia or pyæmia. At the same time erysipelas prevailed extensively, its deaths for the three months amounting to 64.

The other prominent diseases of the quarter were phthisis pulmonalis, which carried off 1,155 persons—a weekly average of 89; pneumonia, to which 772 deaths were attributed, against 642 in the corresponding period of 1871; and bronchitis, which caused 326 deaths, against 283 in the first quarter of 1871.

THE LABYRINTH OF THE EAR.

Dr. H. Knapp delivered a very interesting lecture upon *the Labyrinth of the Ear, its Structure, Functions, and Diseases*, with black-board illustrations.

After a brief explanation of the nature of sound, as produced by the vibration of ponderable bodies; of the distinction between *noises*, produced by irregular vibrations, and *musical tones*, produced by rhythmically recurring ones; and of the three elements of *pitch*, *intensity* and *quality* (*timbre*), which characterize every tone—the first due to the frequency with which the sound-waves fall upon the ear; the second, to the excursion of the vibrating particles; and the last, to the combination of the higher harmonics, or "over-tones," with the fundamental note, as demonstrated by Helmholtz—the speaker came to the description of the auditory organ as an apparatus adapted to the perception of sound, and to the discrimination of its several characteristics. Modern research had been so active in this field, that the mechanical explanation of audition, only a few years since one of the most difficult and obscure of physiological problems, might now be considered among the clearest and best settled.

Assuming the familiarity of his audience with the anatomy of the external and middle ear, and of the osseous framework of the internal, the doctor confined himself to a minute account of the membranous labyrinth, referring particularly to the late observations of Waldeyer and Gottstein, some of which he had himself confirmed. As we can mention only a few points of his crowded discourse, we must further assume that our readers know so much of the membranous labyrinth as may be learned from recent anatomical text-books, such, for example, as the last English edition of Quain.

The mode of connection between the utricle (common sinus) and the saccule has long been a matter of doubt. Of late, however, what seems to have been an old discovery has been made anew, and it is demonstrated that from either sac there proceeds a slender duct, the two uniting in a single one (after the manner of the cystic and hepatic, in the common choledoch), which passes through the aqueductus vestibuli, to open into the so-called arachnoid cavity. Thus the endolymph is in direct connection with the fluid occupying the space between the arachnoid and the dura mater. Furthermore, the loose areolar tissue surrounding the filaments of the auditory nerve, as they pass through the cribriform plate at the bottom of the meatus internus, permits a tolerably free communication between the perilymph and the subarachnoid fluid. It is not surprising, therefore, that the auditory nerve, expanded on a membrane between the endolymph and the perilymph, should so speedily respond to changes of intra-cranial pressure.

The terminal filaments of the vestibular nerve, being connected with the otoliths of the sacs and the ampullæ—bodies apparently ill adapted to rhythmical vibration—would seem intended for the perception of noises. [This is what we should expect from finding the vestibule, with its suspended otolith, as the most rudimentary form of the ear, serving probably to warn the animal of approaching danger by the noise attending it. The intensity of the sound is no doubt rudely measured by the degree of commotion into which the otoliths are thrown, though in the higher animals this is more perfectly accomplished by the muscles of the tympanum. Is it not possible that the hair-like processes, extending beyond the ears and into the endolymph, may, by vibrating as free rods, also give some perception of pitch, though far less delicate and extended than that furnished by the cochlear apparatus?]

Coming to the more complicated cochlea, it will be remembered that the membrana basilaris, which extends from the edge of the bony lamina spiralis to the cochlear wall, and upon which rests the organ of Corti, is divided into an inner *zona tecta vel arcuata*, covered by the arches formed by the rods of Corti, and an outer *zona pectinata*, characterized by its coarse transverse striæ. Now, these striæ are found to be strings embedded in the substance of the membrane, each being attached, at its outer extremity, to the spiral ligament, and at its inner to the foot of one of the club-shaped ciliated cells which, in a fourfold series, lean upon Corti's rods—the very cells to which have been traced the terminal filaments of the cochlear nerve. As the zona pectinata increases in breadth from base to apex of the cochlea, these strings are of regularly varying length; they appear also to be of differing thickness; and, if we suppose, with Todd and Bowman, that the ligamentum spirale is really muscular, it is conceivable that they may vary in tension, being *tuned* by this "cochlear muscle." The strings are doubtless isolated from each other by a non-elastic "cement," which allows each to vibrate without affecting its neighbors.

Consider that there are some three thousand of these strings, each connected with its own ciliated cell, and communicating its slightest tremor to its special nerve-filament, and we have a resonance-apparatus manifestly delicate enough to discriminate all the shades of tone the ear is able to appreciate. In obedience to the familiar law of resonance, a given note will determine a responsive vibration in that particular string of the whole three thousand which is tuned in unison with it, and so send notice of its pitch to the brain. A chord will throw into vibration several strings, each sending its separate message to its ganglion-cell; and the combination of these independent sensations will give the sense of harmony. This, therefore, requires a kind of intellectual process for its appreciation. Quality, as we have seen, is but another name for harmony, and must be appreciated in the same way, the *timbre* of a tone being determined by the number and relative force of the several harmonics that compose it.

However perfectly an organ may appear adapted to a certain function, still any theory of its function based on structure must be considered merely speculative until verified by experiment. Now, the inner ear is removed, not less by its delicacy than by its inaccessibleness, from the possibility of satisfactory direct experiment; but here, fortunately, we may take advantage of pathological conditions, Nature's experiments, which often guide us as surely as any we might contrive. Some of the following points have been established by *post-mortem* examinations, but the most of them still require this confirmation:

We may have a diminution of hearing-power for noises without a corresponding diminution of the power to distinguish musical tones, and *vice versa*. The watch may be used to determine the one condition, a musical-box to determine the other. It is probable that in the former case the vestibular and ampullar structures are chiefly affected; in the latter, the cochlear.

Again, as some persons are blind to certain colors, so others are deaf to certain notes. These defects in the range of audition are most frequently found in the higher notes, sometimes in the lower, more rarely in the middle of the series. For the defect at either end of the series, the explanation at once suggests itself that either the higher or the lower strings are wanting, or that as a whole the apparatus is tuned to a lower or a higher pitch. For a defect in the middle notes, we must suppose either an arrest of development of the corresponding strings, or their subsequent morbid alteration or destruction. It is clear that a destruction of the nerve-filament connected with a string, or of the brain-cell from which it takes origin, would produce the same effect.

Instead of deafness to certain notes, there may be a painful hyperaudition of them. This must probably be referred to a morbid sensibility of the nerve or its ganglion-cell, unless we suppose the string in question to have one or more others tuned in unison with it, and with a like nervous connection, or to have its vibrations reënforced by the resonance of some other structure.

Perhaps the most curious condition is that in which a certain note is heard not as a single one, but as a discord formed by the association of the note with others immediately above and below it. Here we may imagine that the "cement" between the proper respondent string and its neighbors has become hardened, destroying the isolation, so that it cannot vibrate without compelling them to do so. And, though the vibrations of all may possibly be synchronous, yet the brain will interpret the message from each string as indicating that note to which it was meant to respond. This affection may be termed *diplakousis monauralis*, to distinguish it from another, *diplakousis binauralis*, in which a given note is heard of differing pitch by the two ears.

There are some frightful cases where persons in apparently sound health become totally and hopelessly deaf in a few minutes. They are seized with apoplectic symptoms, in which giddiness and loss of equilibrium are especially prominent; the attack passes off speedily; but permanent deafness remains. This is due to hæmorrhage into the labyrinth, known as *Manière's disease.* The same effect may result from fracture through the bony labyrinth—a frequent situation of fractnre by *contre coup*, since the petrous bone is here weakened by its numerous chambers, while it comes in the line of transmission of force from the vertex to the base of the skull.

The loss of equilibrium, just mentioned as dependent on labyrinthic lesions, points to a function of the semicircular canals as yet imperfectly understood. We only know that, in some unexplained way, these do seem to preside over equilibrium, and that an animal will turn or fall in one way or another according as one or another of them is injured.

Deafness may be produced by syphilitic inflammation of the labyrinth. Many of these cases present all the symptoms of Ménière's disease; but, if the syphilitic origin be established, the prognosis, instead of being hopeless, is quite favorable.

Purulent inflammation of the labyrinth is not infrequent, and it is especially apt to occur as secondary to cerebro-spinal meningitis.

Dr. A. H. Smith drew attention to the fact that in the East-River-bridge caisson, at a depth of one hundred feet below the water-level, and therefore under a pressure of more than three atmospheres above the normal, all sounds, instead of being heightened, were rendered very faint. The men could not hear each other speak across the chamber, and the report of a pistol was no louder than it was said to be at the top of Mont Blanc. This, he thought, must be due to the pressure exerted upon the aural structures interfering in some way with their free vibration.

Dr. Roosa related a case of what he supposed to have been syphilitic periostitis of the labyrinth, simulating Mannière's disease in the suddenness and completeness of the loss of hearing, but cured by anti-syphilitic treatment. The loss of equilibrium, so constant in labyrinth-disease or injury, was by no means uncommon as a consequence of aural catarrh, where the sinking in of the membrana tympani produced abnormal pressure on the stapes and the fluid behind it. He gave a recent case in illustration. It was a prevalent notion that, when a broken-down person grew gradually deaf, the trouble was apt to be with the inner ear; but this was far from being the case: the true "nervous deafness" occurred, perhaps, more frequently in robust subjects. That labyrinth-disease did not oftener result from the very common middle-ear affections, might be in part due to the almost complete want of connection between the vascular supplies of the two regions.

The speaker could not take his seat without saying how much the profession owed to Dr. Knapp for his work in establishing otology on a proper basis in this country. There was now an excellent opportunity for New York surgeons to advance the knowledge of aural pathology by examinations of the inner ear in all fatal cases of cerebro-spinal meningitis. He hoped it would not pass unimproved.

The Chair announced the death of Dr. David A. Smith, after which the meeting adjourned.

Miscellaneous and Scientific Notes.

American Medical Association.—The twenty-third annual meeting of this Association was opened in Philadelphia on Tuesday, May 7th, and lasted until the following Friday. After an appropriate welcome by Prof. R. E. Rogers, M. D., the President, Prof. David W. Yandell, M. D., delivered the annual address. He paid a glowing tribute to the medical fame of Philadelphia, and to the memory of the great physicians who have taught and practised there. The subject of medical education occupied a considerable portion of the address. Dr. Yandell did not favor or deem practicable any attempt to advance the standard of education suddenly to an equality with that of the best European colleges. Improvement was taking place surely, year after year, and with all the defects of our schools they nevertheless turned out good, practical doctors, and that was the desideratum. The refinement of medical education did not necessarily do any actual good, and was often a positive evil. The languages, the sciences, and general literature, were all very well in their way, but they did not supply the knowledge that is indispensable for the diagnosis and cure of disease. The question of admitting women to the study and practice of medicine was touched on rather cautiously, and the address closed by a graceful reference to the good work done by the Association in the past, and a wish that its influence might be more still widely extended in the future. After the formal admission of delegates, it was wisely resolved, on motion of Dr. Askew, to refer all personal questions and complaints at once to the Committee on Ethics. The following papers were then disposed of: Dr. F. W. Hatch, of California, a Report on the Climatology of California; referred to the Section of Meteorology and Epidemics. Dr. W. F. Thoms, of New York, a Report on the Climatology of New York; referred to same. Dr. E. R. Fiske, of Oregon, reported progress on a similar report, and was continued. Dr. G. W. Lawrence, of Arkansas, had, after two years, prepared an elaborate Report on the Climatology of Arkansas, which he presented, but a map was still to be furnished; referred to Section as above. Dr. L. P. Bush, of Delaware, a Report on the Climatology of Delaware; referred to same. Dr. H. Knapp, of New York, a paper on Methods of Extraction of Cataract; referred to Section of Surgery and Anatomy. Dr. E. A. Hildreth, of West Virginia, a Report on the Climatology of West Virginia; referred to Section of Meteorology and Epidemics. Dr. Joseph Jones, of New Orleans, a paper on Yellow Fever; referred to Section of Practice of Medicine and Obstetrics. Dr. S. Fitch, of Portland, Maine, a paper entitled Three Ovariotomists; referred to same. Dr. M. Simmons, of South Carolina, a Report on the Climatology of South Carolina; referred to Section of Meteorology and Epidemics. After the announcement of committees, Dr. Francis Gurney Smith, chairman of the Committee on Publication, presented its Report. It set forth that 750 copies of the Transactions of the Society had been published, at a cost of $1,549.39. Of these, 475 volumes were distributed, including 23 to various medical journals, and 88 copies were still due to members. The work was completed and issued early in November. The report concluded by reminding the members of a resolution passed in 1870 that all members who failed to reply to the committee's circular within one year forfeited their right to a copy of the Transactions. Dr. Caspar Wister, Treasurer, reported $1,005 on hand, some $300 more than last year. Dr. J. S.

Weatherly, of Alabama, presented a report, signed by himself and Dr. J. M. Toner, on Medical Education, which was referred to the Committee on Publication, as was the report of Dr. F. A. Ashford, Librarian. The following reports were presented and referred: Dr. Francis G. Smith, on Prize Essays; Dr. Theophilus Parvin, on Medical Literature; Dr. J. D. Jackson, on Medical Necrology. The Secretary announced the following by title, and they were appropriately referred: By Dr. J. G. Richardson, a paper on the Structure of the White Blood-Corpuscles; to the Section of Medical Jurisprudence, Hygiene, and Physiology. By Dr. Henry Hartshorne, a paper on Vaso-Motor Physiology; to the same. By Dr. J. Pancoast, the Case of George Thomas, a colored man, said to be able to displace his heart, stop its beating at will, and roll his belly in a wonderful way; to the Section of Surgery and Anatomy. By Dr. W. F. Peck, Iowa, a paper on Operation at the Hip-Joint, with details of a case; to the same. By Dr. Harrison Allen, a paper on the Soft Palate in Health and Disease; to the same. By Dr. W. H. Mussey, Cincinnati, a paper on Electrolysis in Cancer; also one on a Case of Vesical Calculus and Polypi of the Prostate; to the same. By Dr. J. S. Packard, a Suspensive Apparatus for Fracture of Leg, and a Splint for Excision of Knee; to the same. By Dr. J. P. Garrish, New York, a paper on the Use of Instruments in Obstetrics; to the Section of Practical Medicines and Obstetrics. By Dr. George M. Beard, New York, a paper on Recent Researches in Electro-Therapeutics; to the same. By Dr. C. F. Perkins, Pennsylvania, a paper on Intemperance; also one on a New Method of Swimming; to the same. By Dr. E. M. Hunt, New Jersey, Report on the Climatology and Epidemics of New Jersey; to the Section of Meteorology, Medical Topography, and Epidemic Diseases. By Dr. P. K. Hoy, Wisconsin, a paper on Four Cases of Fracture of the Femur; to the Section of Surgery and Anatomy. By D. P. Pineo, Massachusetts, Report of the United States Marine Hospital Service; to the Section of Practical Medicine and Obstetrics. The Secretary read the following which had been adopted by the College of Physicians, Philadelphia, May 1, 1872: *Whereas*, cases of accidental poisoning, and of the internal administration of medicines intended only for external use are so frequent; and whereas, every possible safeguard should be employed to prevent such accidents: therefore *Resolved*, That it is recommended to all druggists to place all external remedies in bottles not only colored, so as to appeal to the eye, but also rough upon one side, so that by the sense of touch no mistakes shall be possible even in the dark; and that all bottles containing poisons should not only be labelled "poison," but also with another label indicating the most efficient and convenient antidote. Upon motion of Dr. Sayre, New York, adopted. Dr. A. W. Stein, New York, presented a resolution for the appointment of a committee to devise measures for preventing the extension of diseases of animals transmissible to man. Adopted. Dr. Francis G. Smith reported on the nomenclature of diseases, a minority report being offered by Dr. Woodward, United States Army. The Committee on Nominations made the following report, which was adopted: *President.*—Dr. Thomas M. Logan, California. *First Vice-President.*—Dr. Catlin, Connecticut. *Second Vice-President.*—Dr. McPheeters, Missouri. *Third Vice-President.*—Dr. Pollock, Pennsylvania. *Fourth Vice-President.*—Dr. Briggs, Tennessee. *Treasurer.*—Dr. Caspar Wister, Pennsylvania. *Librarian.*—Dr. William Lee, District of Columbia. *Committee on Library.*—Dr. J. M. Toner, District of Columbia. *Next Place of Meeting.*—St. Louis. *Assistant Secretary.*—Dr. M. A. Pallen, Missouri. The various committees and officers of sections were also reported, and the Association proceeded regularly with the transaction of the business before it. The President appointed the following gentlemen as delegates to the British Medical Association for this year: Drs. J. D. Jackson, Kentucky; I. R. Bronson, Massachusetts; Charles H. Hart, New York; F. Henderson, Alabama; J. F. Johnston, Alabama. The association adjourned on Friday, to meet in St. Louis one year hence. The proceedings were unusually harmonious throughout, and were pleasantly varied by sundry entertainments and excursions.

The Influence of Evacuation of Blood on Temperature. (*Centralbl. Med. Wiss.*, No. 53, 1871.)—Dr. D. Gatzuch instituted a series of experiments, with Prof. J. Dogiel, for the purpose of ascertaining the influence of bloodletting upon the medium rapidity of the circulation, and also upon the medium force of the lateral pressure, and the pulsations of the heart, and upon the temperature of the body. The influence of the abstraction of blood upon the nutrition of the organism has been investigated by Prof. Botkin, and also by Panum and Tohnatshoff. Recently, H. H. Bernstein and Nawrotzky have published reports upon the influence of venesection upon the heart and the blood-pressure. As far as the author knows, however, there have been, until now, no experiments published upon the influence of venesection on the general pressure upon the vascular walls. The experiments were all performed upon dogs. The measurements of the medium rapidity of the current, its medium lateral pressure, and the number of pulsations, were estimated by Prof. Dogiel's process (J. Dogiel, "The Measurement of the Volume of Circulating Blood," Leipsic, 1867). The apparatus which served for the measurement of the volume of the blood (the current-index), and the xymographion, were usually connected with the carotids. The abstraction of blood was practised upon a vein or an artery of the upper or lower extremity. The results obtained by the above means were as follows: 1. Bleeding from an artery retards the medium rapidity in the region of the carotid or crural arteries. 2. A venous or arterial bleeding from the anterior extremities has a greater influence upon the rapidity of the current than an equal emptying of the posterior extremities. 3. The changes of the medium rapidity of the circulation depend upon the amount of blood withdrawn. To prove the above, the author regards the results of four experiments, of which he gives a tabulated report, as sufficient. 4. The medium lateral pressure sinks under the influence of the abstraction of blood, although cases occur where it remains unchanged, or even rises; these variations depend as well upon the quantity as upon the rapidity of the flow of the blood at the bleeding. We have seen, from Experiment III., that in a bleeding of 49.7 grammes the pressure is not changed; before the bleeding, it reached 154 mm. Hg., and soon after remained at about the same height—154; while, in Experiment IV., upon the same dog, and on the same artery, with a bleeding of

279.6 grammes, the medium pressure sank to 58 mm. Hg. Nawrotzky observed, in his experiments on rabbits, a marked increase of the blood-pressure during arterial bleeding, and believes this phenomenon to be explainable through cerebral anæmia. Nawalischen testifies to a marked increase of the blood-pressure in the arteria cruralis of cats as soon as both carotids were compressed; he explains this phenomenon by the irritation of the vaso-motor centre, which produces a contraction of the smaller arteries of the body. The author, in his experiments with dogs, also observed a rising of the medium pressure during and after a copious arterial bloodletting; still this increase was not so marked as in Nawrotzky's cases, and was, besides, of short duration only, and was always followed by a diminution of the pressure. Perhaps this slight and evanescent increase of the blood-pressure, which has been observed in dogs after copious bloodletting, may be explained as well by the amount of blood abstracted, as by the nature of the animal experimented on. 5. After the bleeding the change in the current of blood and the lateral pressure rapidly returns to the normal standard. 6. With the sinking of the current and of the lateral pressure there generally occurs an increase in the rate of the pulse. In very copious and rapid abstraction of blood, there is constantly observed an enfeebling of the heart-sounds, especially of the second sound. 7. In consequence of the abstraction of blood there was observed a depression of the temperature of the body during and after the opening of the artery, which, in the author's experiments, amounted to from 1 to 2° C.

Tic Douloureux and Neuralgia. (*Pesth Med. Chir. Press*, p. 37, 1871.)—Hertzka believes he is making an important contribution to the pathology and treatment of nervous affections, by suggesting the causes leading to the differential diagnosis of the above-named affections, which, in most treatises on pathology, are completely identified, and which, nevertheless, are very different processes in the fifth pair of cerebral nerves. He herein merely reproduces the views of Prof. Benedikt. The most important point in neuralgia is the kind and manner of the pain; the pain increases slowly, then remits slightly, later on becomes more severe, again remits a little to increase again and entirely cease, or to continue in a milder degree. It further attacks entire nerves, and is rapidly and surely healed. In tic douloureux (Fothergill's facial pain), on the other hand, the pain is lancinating, increases rapidly, and sinks again rapidly ; some single points only are attacked (Valleix's points douloureux), especially at the points of exit of the nerves out of the bones, and it is more rarely, and only gradually, cured. Both diseases are to be treated by the galvanic current, although in different ways. In tic the action is directed upon the trigeminus and the single painful points; in neuralgia, upon the vaso-motor nerve-centres, and the sympathetic ganglia in the neck, in order to produce a contraction of the vessels through the vascular nerves ; and further, the current is passed as well along the head, cervical vertebræ, and forehead, as through the head at the cheek-bones.

Appointments, Honors, etc.—Dr. Norton Folsom, of this city, has been appointed Resident Physician in the Massachusetts General Hospital, in place of Dr. Shaw, who has resigned.—It is announced that Dr. Brown-Séquard will lecture in the Harvard Medical School in the autumn. Dr. J. M. DaCosta has been elected to the chair of Theory and Practice of Medicine in Jefferson Medical College, Philadelphia, in place of the late Prof. Samuel H. Dickson.—Mr. Isaac Baker Brown, of London, is completely prostrated by paralysis, and is also in great pecuniary distress. A fund is being raised for his relief.—At the reopening of the Medical School in Paris, on April 15th, the lecture of Prof. Dolbeau passed off without any of the noisy demonstrations with which he was insulted at the close of the session.—Dr. James Tyson has resigned his position as Attending Surgeon to St. Joseph's Hospital; also that as chief of the Medical Clinic at the University of Pennsylvania.—Dr. Richard J. Dunglison has resigned his position as Attending Physician to the Pennsylvania Institution for the Blind, which he has held for the last twelve years, in order to devote himself exclusively to literary labor.—Sir Robert Christison, having been a professor of the University of Edinburgh for the term of fifty years, has been made the subject of a *fête* in honor of the event, and has had presented to him a sword by the University Rifle Club, and a congratulatory address by the Edinburgh University Club of London.

Alumni Association of Bellevue Hospital Medical College.—At the annual meeting of this Association, held recently, the following-named officers were elected for 1872–'73: *President*, Leroy Milton Yale, M. D.; *Vice-Presidents*, Charles A. Leale, M. D., and Frank H. Bosworth, M. D.; *Treasurer*, Henry Raphael, M. D.; *Recording Secretary*, John Winslow, M. D.; *Corresponding Secretary*, Bradford S. Thompson, M. D.; *College Historian*, Frederick A. Castle, M. D.; *Council of Twenty appointed by the President*, William T. Lusk, M. D., Drs. R. M. Wyckoff, of Brooklyn, N. Y., J. F. Ferguson, George G. Needham, Robert Newman, Ira B. Read, of Harlem, George W. Terryberry, of Patterson, N. J., T. M. B. Cross, Jos. D. Bryant, Charles F. Roberts, John W. Pinkham, of Montclair, N. J., A. N. Beach, P. R. Cortelyou, of Brooklyn, N. Y., W. C. Jordan, E. D. Morgan, Jr., J. W. McWhinnie, W. H. Katzenbach, Joseph Cushman, J. Herring Burchard, and Willis J. Estep.

Detection of Arsenic.—Dr. James St. Clair Gray, assistant to the Professor of Medical Jurisprudence in the University of Glasgow, has read a paper to the Chemical Section of the Glasgow Philosophical Society, "On Certain Fallacies in the Means of detecting some Poisons." He pointed out that Reinsch's test for arsenic is liable to fail when the arsenic has undergone oxidation to arsenic acid, or when it exists in the state of sulphuret. He advises the reduction of the arsenic acid by means of sulphite of an alkali; and, in the instance of the sulphuret, he would boil with caustic potash, and dialyze. He notes also that the presence of alcohol, chloroform, or ether, prevents the precipitation of the red iodide of mercury.

Swinging in Phthisis.—The effect of swinging seems to have been observed long ago. George F. Elliot, M. D., states that, in the year 1785, one Dr. Smith, F. R. S., tried the effect of swinging on fourteen consumptive patients, at the Middlesex

Hospital. It was practised twice a day, for half an hour at a time. On two of the patients it seemed to have but little effect; in the remainder the pulse fell from eight to fifteen beats per minute. His conclusion is, that "the motion of swinging has often a very sensible and immediate operation on the heart and lungs, as it reduces the frequency of the pulse, lessens febrile heat, suspends or prevents coughing, and promotes expectoration."

Reimplantation of Teeth.—At a recent meeting of the London Odontological Society, Mr. Steele called attention to this subject, and stated that he recently had a tooth of his own extracted with great care. The tooth was in an exquisitely sensitive condition from the exposure of the pulp, and gave pain on the least change of temperature. It was taken out under the influence of nitrous-oxide gas; the dental canal was cleansed; and caries was removed from the crown. It was stopped in the usual way, and the tooth at once replaced in the socket. The operation lasted about half an hour.

A Hospital for Women in Vienna.—Measures are being taken to establish a special hospital, for diseases of women, in Vienna. The proposal originated with Dr. Hermann Beigel, who for several years practised in London, and within the last few months removed to Vienna. Subscriptions to the amount of twelve thousand florins have been raised toward the establishment of the hospital. Dr. Beigel is to be the directing physician, and Dr. Gustav Lott and Dr. W. Schlesinger, Jr., ordinary physicians.

The Treatment of Syphilis by the Hypodermic Injection of Corrosive Sublimate.—Dr. R. W. Taylor treated fifty syphilitic patients during a period of eighteen months with the hypodermic injections of corrosive sublimate, and has arrived at the following conclusions as a result of his observations:

1. That the use of the bichloride of mercury by hypodermic injections, though a method of treatment possessing certain advantages, is, for various reasons, of limited application.
2. That it is useful in the whole secondary period of syphilis, in roseola, and in the various papular syphilides, and in

that form of pustular syphilide in which there is only slight tendency to the formation of pus.

3. That it very rapidly cures all syphilitic neuroses, and that it is very beneficial in the cachexia of syphilis, whether accompanied or not by perceptible lesion.

4. That it possesses no advantages over other modes of administering mercury in the treatment of mucous patches and condylomata lata; and that these lesions yield more rapidly to a local than to any form of constitutional treatment; and that, in the syphilitic lesions of the nervous system and of bone, particularly if late, its use is not to be commended.

5. That the very early tertiary syphilitic lesions, provided they are not of an ulcerative character, may be very much benefited by it, and that the simultaneous administration of iodide of potassium internally may produce a cure more rapidly than when the two are given internally.

6. That the peculiar advantages of the treatment are: the smallness of the amount of mercury used, the rapidity of action, and the absence of systemic disturbance.

7. That a very minute quantity of mercury, averaging from two to three grains, thus administered, may cause the disappearance of very extensive syphilitic lesions, and the alleviation of very severe symptoms.

8. That, in the greatest number of cases, an injection every second day, of an eighth of a grain of the bichloride of mercury, will produce a cure in rather less than two months, and that, in very urgent cases, they may be pushed, with good effects, to the extent of one or two daily.

9. That the rapidity of cure is the rule, rather than the exception, and that the time required may be stated as varying between three weeks and two months.

10. That, when the injections are given every second day, it is very rare to observe any unpleasant systemic effects of the mercury; and that, even when they are pushed more than this, these effects are never as severe as when mercury is pushed to a similar extent by the mouth.

11. That the relapses after this treatment are equally as frequent, as rapid, and as severe in character, as when mercury is given in other ways.

12. That there are unpleasant local effects of the injections, such as pain of the puncture, pain over the site of injection, induration of the connective tissue, and abscesses.

13. That, in many cases, the pain is very slight, and soon ceases to trouble the patient; but that, in others, it is so severe and persistent as to necessitate a discontinuance of the treatment; and that, in every case, some slightly unpleasant ocal effects are experienced from the use of the injections.

14. That, in exceptional cases, the injections cause a low grade of inflammation in the subcutaneous connective tissue, producing a decided induration in deep portions of the derma; and that, owing to complications which might, perhaps, arise from this condition later on, it is advisable to discontinue the injections in these cases.

15. That this induration may be observed in many cases in which it is only of an ephemeral character.

16. That, if proper care is used in administering the injections, abscesses will rarely, if ever, occur.

17. That it is absolutely necessary that the patient should be both intelligent, and, at the same time, thoroughly impressed with the gravity of his disease, in order that he may comprehend the advantages he is to derive from this mode of treatment; otherwise, he could not subject himself to the inconveniences which are inevitably experienced in the course of the treatment.

18. That while in dispensary and hospital practice the injections may be frequently given, in private practice the smallness of a patient's means may often be an obstacle in the way of the continuance of the treatment.

Finally, that while, in some cases, the treatment may be useful by reason of its rapid action, and in others for the smallness of the dose, the inconveniences which it produces, the objections of the patients, and the presence of lesions which contraindicate its use, confine its sphere of usefulness to very narrow limits.—*Medical Gazette*, May 13, 1871.

The Virtues of Tobacco.—Dr. E. B. Gray, in an article on tobacco, in the *Food Journal*, says: "Against moderate smoking in a healthy person who enjoys it, not a single argument of any weight has yet been advanced. Perhaps the most plausible of them is this—that every smoker daily imbibes a small quantity of tobacco-oil and nicotine; and as these substances taken by themselves in the pure concentrated state and in large doses are highly poisonous, therefore every habitual smoker is slowly poisoning himself. Just as reasonable is it to condemn all alcoholic drinks, such as wine, beer, etc., as pernicious, because a draught of pure alcohol will nearly or quite kill a man; or to condemn tea or coffee as dangerous drinks, because their active principles, theine and caffeine, taken alone, and in large doses, are poisons.

"So much, and often so much nonsense, is prated about the evils of tobacco that its virtues rarely get a hearing, and yet the latter are many and great. To quiet nervous unrest; to soothe a ruffled temper; to favor calm and impartial thought; to steady and clear (*not* to cloud) a confused, overworked brain;

to counteract the effects of physical exhaustion—these are just the things which tobacco does, and if it can effect these ends safely and pleasantly, who shall deny it a place among God's good gifts to men?"

Irish Surgeons in the British Army and Navy.—We can have no desire to depreciate our medical friends on the other side of the Irish Channel—far from it. We allude to "the Irish element" in the united services merely with a view of showing that it preponderates; we shall not inquire into the cause. "According to statistics," says one of our contemporaries, "since the opening of Queen's College, Galway, in 1850, forty army medical, and seventeen naval medical appointments have been made from this college alone."—*Medical Times and Gazette.*

Obituary.

PROF. SAMUEL HENRY DICKSON, whose death occurred in Philadelphia March 31st, was born in Charleston, S. C., in 1798. He was a graduate of the University of Pennsylvania, and as early as 1822 gave a course of medical lectures. His subsequent career as a teacher is well known to the profession, as are likewise his numerous and able contributions to medical literature. It is matter for wonder that Prof. Dickson should have been able to accomplish so much solid work notwithstanding the ill-health against which he struggled nearly all his life. Yet he was a man of wide and liberal culture outside his profession, and possessed of a large fund of scientific and general information. His private character was beyond reproach, and his memory will be tenderly cherished by those who enjoyed his friendship.

DR. ZINA PITCHER, of Detroit, died in that city, April 5th, at the age of seventy-five. He was Emeritus Professor in the University of Michigan, and at one time President of the American Medical Association.

DR. W. W. GERHARD, of Philadelphia, died in that city, April 28th, aged sixty-three years.