OF INDUCTION.

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OF INDUCTION,

WITH ESPECIAL REFERENCE TO

R. J. STUART MILL'S SYSTEM OF LOGIC.

William

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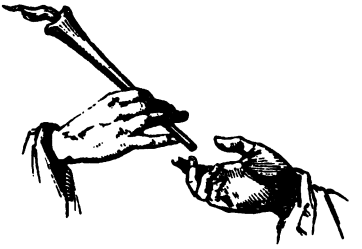

Λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις.

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saying, or thinking, that there is no such thing as Moral and Political Science, or that no method can be suggested for its promotion; but I think that by attempting at present to include the Moral Sciences in the same formulæ with the Physical, we open far more controversies than we close; and that in the moral as in the physical sciences, the first step towards shewing how truth is to be discovered, is to study some portion of it which is assented to so as to be beyond controversy.

4 Confining myself, then, to the material sciences, I shall proceed to offer my remarks on Induction with especial reference to Mr Mill's work. And in order that we may, as I have said, proceed as intelligibly as possible, let us begin by considering what we mean by *Induction*, as a mode of obtaining truth; and let us note whether there is any difference between Mr Mill and me on this subject.

"For the purposes of the present inquiry," Mr Mill says (i. 347*), "Induction may be defined the operation of discovering and forming general propositions:" meaning, as appears by the context, the discovery of them from particular facts. He elsewhere (i. 370) terms it "generalization from experience:" and again

* My references are throughout (except when otherwise expressed) to the volume and the page of Mr. Mill's first edition of his Logic.

he speaks of it with greater precision as the inference of a more general proposition from less general ones.

5 Now to these definitions and descriptions I assent as far as they go; though, as I shall have to remark, they appear to me to leave unnoticed a feature which is very important, and which occurs in all cases of Induction, so far as we are concerned with it. Science, then, consists of general propositions, inferred from particular facts, or from less general propositions, by Induction; and it is our object to discern the nature and laws of *Induction* in this sense. That the propositions are general, or are more general than the facts from which they are inferred, is an indispensable part of the notion of Induction, and is essential to any discussion of the process, as the mode of arriving at Science, that is, at a body of general truths.

6 I am obliged therefore to dissent from Mr Mill when he includes, in his notion of Induction, the process by which we arrive *at individual facts* from other facts *of the same order of particularity*.

Such inference is, at any rate, not Induction *alone;* if it be Induction at all, it is Induction applied to an example.

For instance, it is a general law, obtained by Induction from particular facts, that a body

falling vertically downwards from rest, d
scribes spaces proportional to the squares
the times. But that a particular body wi
fall through 16 feet in one second and 64 fe
in two seconds, is not an induction simply,
is a result obtained by applying the inducti
law to a particular case.

But further, such a process is often n
induction *at all.* That a ball striking anoth
ball directly will communicate to it as mu
momentum as the striking ball itself loses,
a law established by induction: but if, fro
habit or practical skill, I make one billiar
ball strike another, so as to produce the vel
city which I wish, without knowing or thinki
of the general law, the term *Induction* cann
then be rightly applied. If I *know the l*
and act upon it, I have in my mind both t
general induction and its particular appli
tion. But if I act by the ordinary billia
player's skill, without thinking of momentu
or law, there is no Induction in the case.

7 This distinction becomes of impo
ance, in reference to Mr Mill's doctrine, l
cause he has extended his use of the te
Induction, not only to the cases in which t
general induction is consciously applied to
particular instance; but to the cases in whi
the particular instance is dealt with by mea
of experience, in that rude sense in which

perience can be asserted of brutes; and in which, of course, we can in no way imagine that the law is possessed or understood, as a general proposition. He has thus, as I conceive, overlooked the broad and essential difference between speculative knowledge and practical action; and has introduced cases which are quite foreign to the idea of science, alongside with cases from which we may hope to obtain some views of the nature of science and the processes by which it must be formed.

8 Thus (ii. 232) he says, "This inference of one particular fact from another is a case of induction. It is of this sort of induction that brutes are capable." And to the same purpose he had previously said, (i. 251) "He [the burnt child who shuns the fire] is not generalizing: he is inferring a particular from particulars. In the same way also, brutes reason ...not only the burnt child, but the burnt dog, dreads the fire."

9 This confusion, (for such it seems to me,) of knowledge with practical tendencies, is expressed more in detail in other places. Thus he says (i. 118) "I cannot dig the ground unless I have an idea of the ground and of a spade, and of all the other things I am operating upon."

10 This appears to me to be a use of words which can only tend to confuse our idea of

knowledge by obliterating all that is distinctive in *human* knowledge. It seems to me quite false to say that I cannot dig the ground, unless I have an idea of the ground and of my spade. Are we to say that we cannot *walk* the ground, except we have an idea of the ground, and of our feet, and of our shoes, and of the muscles of our legs? Are we to say that a mole cannot dig the ground, except he has an idea of the ground and of the snout and paws with which he digs it? Are we to say that a pholas cannot perforate a rock, except he have an idea of the rock, and of the acid with which he corrodes it?

11 This appears to me, as I have said, to be a line of speculation which can lead to nothing but confusion. The knowledge concerning which I wish to inquire is *human* knowledge. And in order that I may have any chance of success in the inquiry, I find it necessary to single out that kind of knowledge which is especially and distinctively human. Hence, I pass by, in this part of my investigation, all the *knowledge*, if it is to be so called, which man has in no other way than brutes have it;—all that merely shews itself in action. For though action may be modified by habit, and habit by experience, in animals as well as in men, such experience, so long as it retains that merely practical

›rm, is no part of the materials of science. ‹nowledge in a *general* form, is alone know-›dge for that purpose; and to *that*, therefore, must confine my attention; at least till I ave made some progress in ascertaining its ature and laws, and am thus prepared to ompare such knowledge,—*human knowledge* roperly so called,—with mere animal ten-encies to action; or even with practical skill hich does not include, as for the most part ractical skill does not include, speculative nowledge.

12 And thus, I accept Mr Mill's definition f Induction only in its first and largest form; nd reject, as useless and mischievous for our urposes, his extension of the term to the ractical influence which experience of one ct exercises upon a creature dealing with milar facts. Such influence cannot be re-›lved into *ideas* and *induction*, without, as I ›nceive, making all our subsequent investiga-on vague and heterogeneous, indefinite and conclusive. If we must speak of animals as *arning* from experience, we may at least ab-ain from applying to them terms which imply at they learn, in the same way in which men arn astronomy from the stars, and chemistry om the effects of mixture and heat. And ie same may be said of the language which to be used concerning what *men* learn, when

their *learning* merely shews itself in acti
and does not exist as a general thought.
duction must not be applied to such cas
Induction must be confined to cases in whi
we have in our minds general propositions,
order that the sciences, which are our m
instructive examples of the process we have
consider, may be, in any definite and pro
sense, *Inductive* Sciences.

13 Perhaps some persons may be
clined to say that this difference of opini
as to the extent of meaning which is to
given to the term *Induction*, is a questi
merely of words; a matter of definition or
This is a mode in which men in our ti
often seem inclined to dispose of philosophi
questions; thus evading the task of form
an opinion upon such questions, while tl
retain the air of looking at the subject fro
more comprehensive point of view. But a
have elsewhere said, such questions of def
tion are never questions of definition mer
A proposition is always implied along with
definition; and the truth of the proposit
depends upon the settlement of the definiti
This is the case in the present instance.
are speaking of *Induction*, and we mean t
kind of Induction by which the sciences now
isting among men have been constructed.
this account it is, that we cannot include,

ıe meaning of the term, mere practical tenencies or practical habits; for science is not onstructed of these. No accumulation of ıese would make up any of the acknowledged ciences. The elements of such sciences are omething of a kind different from practical abits. The elements of such sciences are rinciples which we *know;* truths which can e contemplated as being *true.* Practical abits, practical skill, instincts and the like ppear in action, and in action only. Such ndowments or acquirements shew themselves hen the occasion for action arrives, and then, hew themselves in the act; without being ut, or being capable of being put, in the orm of truths contemplated by the intellect. ut the elements and materials of Science re necessary truths contemplated by the inellect. It is by consisting of such elements nd such materials, that Science *is* Science. ence a use of the term *Induction* which reuires us to obliterate this distinction, must ake it impossible for us to arrive at any onsistent and intelligible view of the nature of cience, and of the mental process by which ciences come into being. We must, for the urpose which Mr Mill and I have in common, etain his larger and more philosophical denition of Induction, that it is the inference of , more general proposition from less general nes.

14 Perhaps, again, some persons may s
that practical skill and practical experie
lead to science, and may therefore be inclu
in the term *Induction*, which describes
formation of science. But to this we re
that these things lead to science as occasi
only, and do not form part of science; a
that science begins then only when we look
the facts in a general point of view. T
distinction is essential to the philosophy
science. The rope-dancer may, by his p
formances, suggest, to himself or to othe
properties of the center of gravity; but t
is so, because man has a tendency to spe
late and to think of general truths, as well
a tendency to dance on a rope on spe
occasions, and to acquire skill in such danc
by practice. The rope-dancer does not da
by Induction, any more than the dancing d
does. To apply the terms Science and Ind
tion to such cases, carries us into the regi
of metaphor; as when we call birds of p
sage "wise meteorologists," or the bee "a
tural chemist, who turns the flower-dust i
honey." This is very well in poetry: but
our purposes we must avoid recognizing th
cases as really belonging to the sciences
meteorology and chemistry,—as really ca
of Induction. Induction for us is gene
propositions, *contemplated as such*, derived fr
particulars.

Science may result *from* experience and bservation *by* Induction; but Induction is ot therefore the same thing as experience nd observation. Induction is experience or bservation *consciously* looked at in a *general* orm. This consciousness and generality are ecessary parts of that knowledge which is cience. And accordingly, on the other hand, cience cannot result from mere Instinct, as listinguished from Reason; because Instinct y its nature is not conscious and general, but perates blindly and unconsciously in particular cases, the actor not seeing nor thinking f the rule which he obeys.

15 A little further on I shall endeavour o shew that not only a general *thought*, but a eneral *word* or phrase is a requisite element n Induction. This doctrine, of course, still nore decidedly excludes the case of animals, nd of mere practical knowledge in man. A urnt child dreads the fire; but reason must e unfolded before the child learns to understand the words "fire will hurt you." The urnt dog never thus learns to understand ords. And this difference points to an entirely different state of thought in the two ases: or rather, to a difference between a tate of rational thought on the one hand, and f mere practical instinct on the other.

16 Besides this difference of speculative

thought and practical instinct which thus are, as appears to me, confounded in Mr Mill's philosophy, in such a way as tends to destroy all coherent views of human knowledge, there is another set of cases to which Mr Mill applies the term *Induction*, and to which it appears to me to be altogether inapplicable. He employs it to describe the mode in which superstitious men, in ignorant ages, were led to the opinion that striking natural events presaged or accompanied calamities. Thus he says (i. 389) "The opinion so long prevalent that a comet or any other unusual appearance in the heavenly regions was the precursor of calamities to mankind, or at least to those who witnessed it; the belief in the oracles of Delphi and Dodona; the reliance on astrology or on the weather-prophecies in almanacs, were doubtless inductions supposed to be grounded on experience;" and he speaks of these insufficient inductions being extinguished by the stronger inductions subsequently obtained by scientific inquiry. And in like manner, he says in another place (i. 367) "Let us now compare different predictions: the first that eclipses will occur whenever one planet or satellite is so situated as to cast its shadow upon another: the second, that they will occur whenever some great calamity is impending over mankind."

17 Now I cannot see how anything but nfusion can arise from applying the term *In-uction* to superstitious fancies like those here entioned. They are not imperfect truths, it entire falsehoods. Of that, Mr Mill and are agreed: how then can they exemplify ie progress towards truth? They were not llected from the facts by seeking a law of ieir occurrence; but were suggested by an agination of the anger of superior powers iewn by such deviations from the ordinary urse of nature. If we are to speak of *in-uctions* to any purpose, they must be such ductions as represent the facts, in some egree at least. It is not meant, I presume, iat these opinions are in any degree true: to hat purpose then are they adduced? If I ere to hold that my dreams predict or conform the motions of the stars or of the clouds, ould this be an induction? It would be so, much one as those here so denominated: t what but confusion could arise from class-g it among scientific truths? Mr Mill him-lf has explained (ii. 389) the way in which ch delusions as the prophecies of almanac-akers, and the like, obtain credence; namely, the greater effect which the positive in-nces produce on ordinary minds in com-rison of the negative, when the rule has ce taken possession of their thoughts. And

this being, as he says, the recognized ex
planation of such cases, why should we no
leave them to their due place, and not con
found and perplex the whole of our investi
gation by elevating them to the rank of "in
ductions?" The very condemnation of such
opinions is that they are not at all inductiv
When we have made any progress in our
investigation of the nature of science, to at
tempt to drive us back to the wearisom
discussion of such elementary points as thes
is to make progress hopeless.

18 In the cases hitherto noticed, M
Mill extends the term *Induction*, as I think
too widely, and applies it to cases to which i
is not rightly applicable. I have now to notic
a case of an opposite kind, in which he do
not apply it where I do, and condemns m
for using it in such a case. I had spoken o
Kepler's discovery of the Law, that the plane
move round the sun in ellipses, as an exampl
of Induction. The separate facts of a
planet (Mars, for instance,) being in certa
places at certain times, are all included i
the general proposition which Kepler di
covered, that Mars describes an ellipse of
certain form and position. This appears t
me a very simple but a very distinct exampl
of the operation of discovering general pr
positions; general, that is, with reference t

articular facts; which operation Mr Mill, as ell as myself, says is Induction. But Mr Mill lenies this operation in this case to be Induction at all (i. 357). I should not have been prepared for this denial by the previous parts of Mr Mill's book, for he had said just before (i. 350), "Such facts as the magnitudes of the bodies of the solar system, their distances from each other, the figure of the earth and its rotation ... are proved indirectly, by the aid of inductions founded on other facts which we can more easily reach." If the figure of the earth and its rotation are proved by Induction, it seems very strange, and is to me quite incomprehensible, how the figure of the earth's orbit and its revolution (and of course, of the figure of Mars's orbit and his revolution in like manner,) are not also proved by Induction. No, says Mr Mill, Kepler, in putting together a number of places of the planet into one figure, only performed an act of *description*. "This descriptive operation," he adds (i. 359), "Mr Whewell, by an aptly chosen expression, has termed Colligation of Facts." He goes on to commend my observations concerning this process, but says that, according to the old and received meaning of the term, it is not Induction at all.

19 Now I have already shewn that Mr Mill himself, a few pages earlier, had applied

the term *Induction* to cases undistinguishable from this in any essential circumstance. And even in this case, he allows that Kepler did really perform an act of Induction (i. 358), "namely, in concluding that, because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse; and even in concluding that the position of the planet during the time which had intervened between the two observations must have coincided with the intermediate points of the curve." Of course, in Kepler's Induction, of which I speak, I include all this; all this is included in speaking of the *orbit* of Mars: a continuous line, a periodical motion, are implied in the term *orbit*. I am unable to see what would remain of Kepler's discovery, if we take from it these conditions. It would not only not be an induction, but it would not be a description, for it would not recognise that Mars moved in an orbit. Are particular positions to be conceived as points in a curve, without thinking of the intermediate positions as belonging to the same curve? If so, there is no law at all, and the facts are not bound together by any intelligible tie.

In another place (ii. 209) Mr Mill returns to his distinction of Description and Induction;

ut without throwing any additional light upon
:, so far as I can see.

20 The only meaning which I can disco-
er in this attempted distinction of Description
ınd Induction is, that when particular facts
ıre bound together by their relation in *space*,
Mr Mill calls the discovery of the connection
Description, but when they are connected by
)ther general relations, as time, cause and the
ike, Mr Mill terms the discovery of the con-
ıection *Induction*. And this way of making
ı distinction, would fall in with the doctrine of
)ther parts of Mr Mill's book, in which he
ıscribes very peculiar attributes to space and
ts relations, in comparison with other Ideas,
as I should call them). But I cannot see any
;round for this distinction, of connection
.ccording to space and other connections of
acts.

To stand upon such a distinction, appears
o me to be the way to miss the general laws
f the formation of science. For example:
The ancients discovered that the planets re-
olved in recurring periods, and thus connected
he observations of their motions according to
he Idea of *Time*. Kepler discovered that they
evolved in ellipses, and thus connected the
)bservations according to the Idea of *Space*.
Newton discovered that they revolved in virtue
f the Sun's attraction, and thus connected the

motions according to the Idea of *Force*. The first and third of these discoveries are recognised on all hands as processes of Induction. Why is the second to be called by a different name? or what but confusion and perplexity can arise from refusing to class it with the other two? It is, you say, Description. But such Description is a kind of Induction, and must be spoken of as Induction, if we are to speak of Induction as the process by which Science is formed: for the three steps are all, the second in the same sense as the first and third, in co-ordination with them, steps in the formation of astronomical science.

21 But, says Mr Mill, (i. *363*) "it is a fact surely that the planet does describe an ellipse, and a fact which we could see if we had adequate visual organs and a suitable position." To this I should reply: "Let it be so; and it is a fact, surely, that the planet does move periodically: it is a fact, surely, that the planet is attracted by the sun. Still, therefore, the asserted distinction fails to find a ground." Perhaps Mr Mill would remind us that the elliptical form of the orbit is a fact which we could see if we had adequate visual organs and a suitable position: but that force is a thing which we cannot see. But this distinction also will not bear handling. Can we not see a tree blown down by a storm, or a rock

lown up by gunpowder? Do we not here see ɔrce:—see it, that is, by its effects, the only ay in which we need to see it in the case of planet, for the purposes of our argument? ıre not such operations of force, Facts which ıay be the objects of sense? and is not the peration of the sun's Force a Fact of the ame kind, just as much as the elliptical form f orbit which results from the action? If the ıtter be "surely a Fact," the former is a Fact ıo less surely.

22 In truth, as I have repeatedly had ccasion to remark, all attempts to frame an rgument by the exclusive or emphatic appro-riation of the term *Fact* to particular cases, re necessarily illusory and inconclusive. There ı no definite and stable distinction between acts and Theories; Facts and Laws; Facts nd Inductions. Inductions, Laws, Theories, hich are true, *are* Facts. Facts involve Induc-ons. It is a Fact that the moon is attracted y the earth, just as much as it is a Fact that ı apple falls from a tree. That the former ıct is collected by a more distinct and con-ɔious Induction, does not make it the less Fact. That the orbit of Mars is a Fact—a ue Description of the path—does not make the less a case of Induction.

23 There is another argument which [r Mill employs in order to shew that there

is a difference between mere colligation whic
is description, and induction in the more pro
per sense of the term. He notices with com
mendation a remark which I had made (i. 364)
that at different stages of the progress o
science the facts had been successfully con
nected by means of very different conceptions
while yet the later conceptions have not con
tradicted, but included, so far as they were
true, the earlier: thus the ancient Greek re
presentation of the motions of the planets b
means of epicycles and eccentrics, was to
certain degree of accuracy true, and is no
negatived, though superseded, by the moder
representation of the planets as describing
ellipses round the sun. And he then reason
that this, which is thus true of Descriptions
cannot be true of Inductions. He says (i. 367)
"Different descriptions therefore may be al
true: but surely not different explanations."
He then notices the various explanations of th
motions of the planets—the ancient doctrin
that they are moved by an inherent virtue
the Cartesian doctrine that they are moved b
impulse and by vortices; the Newtonian doc
trine that they are governed by a central force
and he adds, "Can it be said of these, as wa
said of the different descriptions, that they
are all true as far as they go? Is it not true
that one only can be true in any degree

and that the other two must be altogether false?"

24 And to this questioning, the history of science compels me to reply very distinctly and positively, in the way which Mr Mill appears to think extravagant and absurd. I am obliged to say, Undoubtedly, all these explanations *may* be true and consistent with each other, and would be so if each had been followed out so as to shew in what manner it could be made consistent with the facts. And this was, in reality, in a great measure done. The doctrine that the heavenly bodies were moved by vortices was successively modified, so that it came to coincide in its results with the doctrine of an inverse-quadratic centripetal force, as I have remarked in the *History*, (B. VII. c. iii. sect. 3). When this point was reached, the vortex was merely a machinery, well or ill devised, for producing such a centripetal force, and therefore did not contradict the doctrine of a centripetal force. Newton himself does not appear to have been averse to explaining gravity by impulse. So little is it true that if one theory be true the other must be false. The attempt to explain gravity by the impulse of streams of particles flowing through the universe in all directions, which I have mentioned in the *Philosophy*, (B. III. c. ix. art. 7), is so far from being in-

consistent with the Newtonian theory, that it is founded entirely upon it. And even with regard to the doctrine, that the heavenly bodies move by an inherent virtue; if this doctrine had been maintained in any such way that it was brought to agree with the facts, the inherent virtue must have had its laws determined; and then, it would have been found that the virtue had a reference to the central body; and so, the "inherent virtue" must have coincided in its effect with the Newtonian force; and then, the two explanations would agree, except so far as the word "inherent" was concerned. And if such a part of an earlier theory as this word *inherent* indicates, is found to be untenable, it is of course rejected in the transition to later and more exact theories, in Inductions of this kind, as well as in what Mr Mill calls Descriptions. There is therefore still no validity discoverable in the distinction which Mr Mill attempts to draw between "descriptions" like Kepler's law of elliptical orbits, and other examples of induction.

25 When Mr Mill goes on to compare what he calls different predictions—the first, the true explanation of eclipses by the shadows which the planets and satellites cast upon one another, and the other, the belief that they will occur whenever some great

calamity is impending over mankind, I must reply, as I have stated already, (Art. 17), that to class such superstitions as the last with cases of Induction, appears to me to confound all use of words, and to prevent, as far as it goes, all profitable exercise of thought. What possible advantage can result from comparing (as if they were alike) the relation of two descriptions of a phenomenon, each to a certain extent true, and therefore both consistent, with the relation of a scientific truth to a false and baseless superstition?

26 But I may make another remark on this example, so strangely introduced. If, under the influence of fear and superstition, men may make such mistakes with regard to laws of nature, as to imagine that eclipses portend calamities, are they quite secure from mistakes in *description?* Do not the very persons who tell us how eclipses predict disasters, also describe to us fiery swords seen in the air, and armies fighting in the sky? So that even in this extreme case, at the very limit of the rational exercise of human powers, there is nothing to distinguish Description from Induction.

I shall now leave the reader to judge whether this feature in the history of science, —that several views which appear at first quite different are yet all true,—which Mr

Mill calls a curious and interesting remark mine, and which he allows to be "strikin true" of the Inductions which he calls *Desc tions*, (i. 364) is, as he says, "unequivoc false" of other Inductions. And I shall c fide in having general assent with me, whe continue to speak of Kepler's *Induction* of elliptical orbits.

I now proceed to another remark.

27 There is a difference between Mr M and me in our view of the essential eleme of this Induction of Kepler, which affects other cases of Induction, and which is, I thi the most extensive and important of the ferences between us. I must therefore t ture to dwell upon it a little in detail.

I conceive that Kepler, in discovering law of Mars's motion, and in asserting t the planet moved in an ellipse, did this;— bound together particular observations of parate places of Mars by the notion, or I have called it, the *conception*, of an *elli* which was supplied by his own mind. Ot persons, and he, before he made this covery, had present to their minds the fi of such separate successive positions of planet; but could not bind them toget rightly, because they did not apply to th this conception of an *ellipse*. To supply conception, required a special preparation,

special activity in the mind of the discoverer. He, and others before him, tried other ways of connecting the special facts, one of which fully succeeded. To discover such a connection, the mind must be conversant with certain relations of space, and with certain kinds of figures. To discover the right figure was a matter requiring research, invention, resource. To hit upon the right conception is a difficult step; and when this step is once made, the facts assume a different aspect from what they had before: what done, they are seen in a new point of view; and the catching this point of view, is a special mental operation, requiring special endowments and habits of thought. Before this, the facts are seen as detached, separate, lawless; afterwards, they are seen as connected, simple, regular; as parts of one general fact, and thereby possessing innumerable new relations before unseen. Kepler, then, I say, bound together the facts by superinducing upon them the *conception* of an *ellipse*, and this was an essential element in his Induction.

28 And there is the same essential element in all Inductive discoveries. In all cases, facts, before detached and lawless, are bound together by a new thought. They are reduced to law, by being seen in a new point of view.

To catch this new point of view, is an act of the mind, springing from its previous preparation and habits. The facts, in other discoveries, are brought together according to other relations, or, as I have called them, *Ideas;*—the Ideas of Time, of Force, of Number, of Resemblance, of Elementary Composition, of Polarity, and the like. But in all cases the mind performs the operation by an apprehension of some such relations; by singling out the one true relation; by combining the apprehension of the true relation with the facts; by applying to them the Conception of such a relation.

29 In previous writings, I have not only stated this view generally, but I have followed it into detail, exemplifying it in the greater part of the History of the principal Inductive Sciences in succession. I have pointed out what are the Conceptions which have been introduced in every prominent discovery in those sciences; and have noted to which of the above Ideas, or of the like Ideas, each belongs. The performance of this task is the office of the greater part of my *Philosophy of the Inductive Sciences.* For that work is, in reality, no less historical than the *History* which preceded it. The *History of the Inductive Sciences* is the history of the discoveries, mainly so far as concerns the *Facts* which were brought to

gether to form sciences. The *Philosophy* is, in the first ten Books, the history of the *Ideas* and *Conceptions*, by means of which the facts were connected, so as to give rise to scientific truths. It would be easy for me to give a long list of the Ideas and Conceptions thus brought into view, but I may refer any reader who wishes to see such a list, to the Tables of Contents of the *History*, and of the first ten Books of the *Philosophy*.

30 That these Ideas and Conceptions are really distinct elements of the scientific truths thus obtained, I conceive to be proved beyond doubt, not only by considering that the discoveries never were made, nor could be made, till the right Conception was obtained, and by seeing how difficult it often was to obtain this element; but also, by seeing that the Idea and the Conception itself, as distinct from the Facts, was, in almost every science, the subject of long and obstinate controversies;—controversies which turned upon the possible relations of Ideas, much more than upon the actual relations of Facts. The first ten Books of the Philosophy to which I have referred, contain the history of a great number of these controversies. These controversies make up a large portion of the history of each science; a portion quite as important as the study of the facts; and a portion, at every

stage of the science, quite as essential to the progress of truth. Men, in seeking and obtaining scientific knowledge, have always shewn that they found the formation of right conceptions in their own minds to be an essential part of the process.

II. Moreover, the presence of a Conception of the mind as a special element of the inductive process, and as the tie by which the particular facts are bound together, is further illustrated by there being some special new term or phrase introduced in every induction, or at least some term or phrase thenceforth steadily applied to the facts, which had not been applied to them before; as when Kepler asserted that Mars moved round the sun in an elliptical orbit, or when Newton asserted that the planets *gravitate* towards the sun; these new terms *elliptical orbit*, and *gravitate*, mark the new conceptions on which the inductions depend. I have in the *Philosophy* (B. I. c. iii.) further illustrated this application of "technical terms," that is, fixed and settled terms, in every inductive discovery; and have spoken of their use in enabling men to proceed from each such discovery to other discoveries more general. But I notice these terms here, for the purpose of shewing the existence of a conception in the discoverer's mind, corresponding to the term thus introduced; which

nception, the term is intended to convey the minds of those to whom the discovery ommunicated.

32 But this element of discovery,—right nceptions supplied by the mind in order to id the facts together,—Mr Mill denies to be element at all. He says, of Kepler's dis-very of the elliptical orbit, (i. 363) "It super-ded nothing to the particular facts which served to bind together;" yet he adds, xcept indeed the knowledge that a resem-nce existed between the planetary orbit l other ellipses;" that is, except the know-ge that it *was* an ellipse;—precisely the cumstance in which the discovery consisted. pler, he says, "asserted as a fact that the net moved in an ellipse. But this fact, ich Kepler did not add to, but found in the tion of the planet...was the very fact, the arate parts of which had been separately erved; it was the sum of the different ob-vations."

33 That the fact of the elliptical motion s not merely the *sum* of the different ob-vations, is plain from this, that other per-s, and Kepler himself before his discovery, not find it by adding together the obser-ions. The fact of the elliptical orbit was the sum of the observations *merely;* it s the sum of the observations, *seen under*

a new point of view, which point of view Kepler's mind supplied. Kepler found it in the facts, because it was there, no doubt, for one reason; but also, for another, because he had, in his mind, those relations of thought which enabled him to find it. We may illustrate this by a familiar analogy. We too find the law in Kepler's book; but if we did not understand Latin, we should not find it there. We must learn Latin in order to find the law in the book. In like manner, a discoverer must know the language of science, as well as look at the book of nature, in order to find scientific truth. All the discussions and controversies respecting Ideas and Conceptions of which I have spoken, may be looked upon as discussions and controversies respecting the grammar of the language in which nature speaks to the scientific mind. Man is the *Interpreter* of Nature; not the Spectator merely, but the Interpreter. The study of the language, as well as the mere sight of the characters, is requisite in order that we may read the inscriptions which are written on the face of the world. And this study of the language of nature, that is of the necessary coherencies and derivations of the relations of phenomena, is to be pursued by examining Ideas, as well as mere phenomena;—by tracing the formation of Conceptions, as well as the accumulation of

Facts. And this is what I have tried to do in the books already referred to.

34 Mr Mill has not noticed, in any considerable degree, what I have said of the formation of the Conceptions which enter into the various sciences; but he has, in general terms, denied that the Conception is anything different from the facts themselves. "If," he says, (i. 301) "the facts are rightly classed under the conceptions, it is because there is in the facts themselves, something of which the conception is a copy." But it is a copy which cannot be made by a person without peculiar endowments; just as a person cannot copy an ill-written inscription, so as to make it convey sense, except he understand the language. "Conceptions," Mr Mill says, (ii. 217) "do not develope themselves from within, but are impressed from without." But what comes from without is not enough: they must have both origins, or they cannot make knowledge. "The conception," he says again, (ii. 221) "is not furnished *by* the mind till it has been furnished *to* the mind." But it is furnished to the mind by its own activity, operating according to its own laws. No doubt, the conception may be formed, and in cases of discovery, must be formed, by the suggestion and excitement which the facts themselves produce; and must be so moulded

as to agree with the facts. But this does not make it superfluous to examine, out of what *materials* such conceptions are formed, and *how* they are capable of being moulded so as to express laws of nature; especially, when we see how large a share this part of discovery—the examination how our ideas can be modified so as to agree with nature,—holds, in the history of science.

35 I have already (Art. 31) given, as evidence that the conception enters as an element in every induction, the constant introduction in such cases, of a new fixed term or phrase. Mr Mill (ii. 282) notices this introduction of a new phrase in such cases as important, though he does not appear willing to allow that it is necessary. Yet the necessity of the conception at least, appears to result from the considerations which he puts forward. "What darkness," he says, "would have been spread over geometrical demonstration, if wherever the word *circle* is used, the definition of a circle was inserted instead of it." "If we want to make a particular combination of ideas permanent in the mind, there is nothing which clenches it like a name specially devoted to express it." In my view, the new conception is the *nail* which connects the previous notions, and the name, as Mr Mill says, *clenches* the junction.

36 I have above (Art. 33) referred to the difficulty of getting hold of the right conception, as a proof, that induction is not a mere juxtaposition of facts. Mr Mill does not dispute that it is often difficult to hit upon the right conception. He says (i. 360) "that a conception of the mind is introduced, is indeed most certain, and Mr Whewell has rightly stated elsewhere, that to hit upon the right conception is often a far more difficult, and more meritorious achievement, than to prove its applicability when obtained. But," he adds, "a conception implies and corresponds to something conceived; and although the conception itself is not in the facts, but in our mind, it must be a conception of something which really is in the facts." But to this I reply, that its being really in the facts, does not help us at all towards knowledge, if we cannot see it there. As the poet says,

> It is the mind that sees: the outward eyes
> Present the object, but the mind descries.

And this is true of the sight which produces knowledge, as well as of the sight which produces pleasure and pain, which is referred to in the Tale.

37 Mr Mill puts his view, as opposed to mine, in various ways, but, as will easily be understood, the answers which I have to offer are in all cases nearly to the same effect.

Thus, he says (ii. 216), "the tardy development of several of the physical sciences, for example, of Optics, Electricity, Magnetism, and the higher generalizations of Chemistry, Mr Whewell ascribes to the fact that mankind had not yet possessed themselves of the idea of Polarity, that is, of opposite properties in opposite directions. But what was there to suggest such an idea, until by a separate examination of several of these different branches of knowledge it was shewn that the facts of each of them did present, in some instances at least, the curious phenomena of opposite properties in opposite directions?" But on this I observe, that these facts did not, nor do yet, present this conception to ordinary minds. The opposition of properties, and even the opposition of directions, which are thus apprehended by profound cultivators of science, are of an abstruse and recondite kind; and to conceive any one kind of polarity in its proper generality, is a process which few persons hitherto appear to have mastered; still less have men in general come to conceive of them all as modifications of a general notion of Polarity. The description which I have given of Polarity in general, "opposite properties in opposite directions," is of itself a very imperfect account of the manner in which corresponding antitheses are involved in the portions

of science into which Polar relations enter. In excuse of its imperfection, I may say, that I believe it is the first attempt to define Polarity in general; but yet, the conception of Polarity has certainly been strongly and effectively present in the minds of many of the sagacious men who have discovered and unravelled polar phenomena. They attempted to convey this conception, each in his own subject, sometimes by various and peculiar expressions, sometimes by imaginary mechanism by which the antithetical results were produced; their mode of expressing themselves being often defective or imperfect, often containing what was superfluous; and their meaning was commonly very imperfectly apprehended by most of their hearers and readers. But still, the conception was there, gradually working itself into clearness and distinctness, and in the mean time, directing their experiments, and forming an essential element of their discoveries. So far would it be from a sufficient statement of the case to say, that they conceived polarity because they saw it; —that they saw it as soon as it came into view; —and that they described it as they saw it.

38 The way in which such conceptions acquire clearness and distinctness is often by means of Discussions of Definitions. To define well a thought which already enters into

trains of discovery, is often a difficult matter. The business of such definition is a part of the business of discovery. These, and other remarks connected with these, which I had made in the *Philosophy*, Mr Mill has quoted and adopted (ii. 242). They appear to me to point very distinctly to the doctrine to which he refuses his assent,—that there is a special process in the mind, in addition to the mere observation of facts, which is necessary at every step in the progress of knowledge. The Conception must be *formed* before it can be *defined*. The Definition gives the last stamp of distinctness to the Conception; and enables us to express, in a compact and lucid form, the new scientific propositions into which the new Conception enters.

39 Since Mr Mill assents to so much of what has been said in the *Philosophy*, with regard to the process of scientific discovery, how, it may be asked, would he express these doctrines so as to exclude that which he thinks erroneous? If he objects to our saying that when we obtain a new inductive truth, we connect phenomena by applying to them a new Conception which fits them, in what terms would he describe the process? If he will not agree to say, that in order to discover the law of the facts, we must find an appropriate Conception, what language would he use instead

this? This is a natural question; and the answer cannot fail to throw light on the relation in which his views and mine stand to each other.

Mr Mill would say, I believe, that when we obtain a new inductive law of facts, we find something in which the facts *resemble each other;* and that the business of making such discoveries is the business of discovering such resemblances. Thus, he says (of me,) (ii. 211), "his Colligation of Facts by means of appropriate Conceptions, is but the ordinary process of finding by a comparison of phenomena, in what consists their agreement or resemblance." And the Methods of experimental Inquiry which he gives (i. 450 &c.), proceed upon the supposition that the business of discovery may be thus more properly described.

40 There is no doubt that when we discover a law of nature by induction, we find some point in which all the particular facts agree. All the orbits of the planets agree in being ellipses, as Kepler discovered; all falling bodies agree in being acted on by a uniform force, as Galileo discovered; all refracted rays agree in having the sines of incidence and refraction in a constant ratio, as Snell discovered; all the bodies in the universe agree in attracting each other, as Newton discovered; all chemical compounds agree in being

constituted of elements in definite proportions, as Dalton discovered. But it appears to me a most scanty, vague, and incomplete account of these steps in science, to say that the authors of them discovered something in which the facts in each case agreed. The point in which the cases agree, is of the most diverse kind in the different cases—in some, a relation of space, in others, the action of a force, in others, the mode of composition of a substance;—and the point of agreement, visible to the discoverer alone, does not come even into his sight, till after the facts have been connected by thoughts of his own, and regarded in points of view in which he, by his mental acts, places them. It would seem to me not much more inappropriate to say, that an officer, who disciplines his men till they move together at the word of command, does so by finding something in which they agree. If the power of consentaneous motion did not exist in the individuals, he could not create it: but that power being there, he finds it and uses it. Of course I am aware that the parallel of the two cases is not exact; but in the one case, as in the other, that in which the particular things are found to agree, is something formed in the mind of him who brings the agreement into view.

41 But Mr Mill has not only thus de

scribed the business of scientific discovery; he has also given rules for it, founded on this description. It may be expected that we should bestow some attention upon the methods of inquiry which he thus proposes. I presume that they are regarded by his admirers as among the most valuable parts of his book; as certainly they cannot fail to be, if they describe methods of scientific inquiry in such a manner as to be of use to the enquirer.

Mr Mill enjoins four methods of experimental inquiry, which he calls *the Method of Agreement, the Method of Difference, the Method of Residues,* and *the Method of Concomitant Variations.* (B. III. c. viii.) They are all described by formulæ of this kind:—Let there be, in the observed facts, combinations of antecedents, *ABC, BC, ADE,* &c. and combinations of corresponding consequents, *abc, bc, ade,* &c.; and let the object of inquiry be, the consequence of some cause *A,* or the cause of some consequence *a.* The Method of Agreement teaches us, that when we find by experiment such facts as *abc* the consequent of *ABC,* and *ade* the consequent of *ADE,* then *a* is the consequent of *A.* The Method of Difference teaches us that when we find such facts as *abc* the consequent of *ABC,* and *bc* the consequent of *BC,* then *a* is the consequent of *A.* The Method of Residues, teaches us, that if *abc* be the

consequent of *ABC*, and if we have already ascertained that the effect of *A* is *a*, and the effect of *B* is *b*, then we may infer that the effect of *C* is *c*. The method of Concomitant Variations teaches us, that if a phenomenon *a* varies according as another phenomenon *A* varies, there is some connection of causation direct or indirect, between *A* and *a*.

42 Upon these methods, the obvious thing to remark is, that they take for granted, the very thing which is most difficult to discover, the reduction of the phenomena to formulæ such as are here presented to us. When we have any set of complex facts offered to us; for instance, those which were offered in the cases of discovery which I have mentioned,—the facts of the planetary paths, of falling bodies, of refracted rays, of cosmical motions, of chemical analysis; and when, in any of these cases, we would discover the law of nature which governs them, or, if any one chooses so to term it, the feature in which all the cases agree, where are we to look for our *A*, *B*, *C* and *a*, *b*, *c*? Nature does not present to us the cases in this form; and how are we to reduce them to this form? You say, *when* we find the combination of *ABC* with *abc* and *ABD* with *abd*, then we may draw our inference. Granted: but when and where are we to find such combinations? Even now that the

discoveries are made, who will point out to us what are the *A*, *B*, *C*, and *a*, *b*, *c* elements of the cases which have just been enumerated? Who will tell us which of the methods of inquiry those historically real and successful inquiries exemplify? Who will carry these formulæ through the history of the sciences, as they have really grown up; and shew us that these four methods have been operative in their formation; or that any light is thrown upon the steps of their progress by reference to these formulæ?

43 Mr Mill's four methods have a great resemblance to Bacon's "Prerogatives of Instances;" for example, the Method of Agreement to the *Instantiæ Ostensivæ;* the Method of Differences to the *Instantiæ Absentiæ in Proximo,* and the *Instantiæ Crucis;* the Method of Concomitant Variations to the *Instantiæ Migrantes.* And with regard to the value of such methods, I believe all study of science will convince us more and more of the wisdom of the remarks which Sir John Herschel has made upon them (*Discourse,* Art. 192).

"It has always appeared to us, we must confess, that the help which the classification of instances under their different titles of prerogative, affords to inductions, however just such classification may be in itself, is yet more apparent than real. The force of the instance

must be felt in the mind before it can b
ferred to its place in the system; and b
it can be either referred or appreciat
must be known; and when it *is* appreci
we are ready enough to weave our web
duction, without greatly troubling ours
whence it derives the weight we acknowl
it to have in our decisions.... No doubt
instances as these are highly instructive;
the difficulty in physics is to find such, n
perceive their force when found."

44 If Mr Mill's four methods had
applied by him in his book to a large bo
conspicuous and undoubted examples of
covery, extending along the whole histo
science, well selected and well analysed
should have been better able to estimat
value of these methods. Mr Mill has cert
offered a number of examples of his meth
but I hope I may say, without offense, that
appear to me to be wanting in the condi
which I have mentioned. As I have to ju
myself for rejecting Mr Mill's criticism of
trines which I have put forward, and e
ples which I have adduced, I may, I trus
allowed to offer some critical remarks i
turn, bearing upon the examples which h
given, in order to illustrate his doctrines
precepts.

45 The first remark which I hav

e is, that a large proportion of his examples
30, &c.) are taken from one favourite au-
; who, however great his merit may be,
o recent a writer to have had his disco-
es confirmed by the corresponding inves-
tions and searching criticisms of other
urers in the same field, and placed
heir proper and permanent relation to
blished truths; these alleged discoveries
g, at the same time, principally such as
with the most complex and slippery por-
s of science, the laws of vital action. Thus
Mill has adduced, as examples of disco-
es, Prof. Liebig's doctrine—that death is
luced by certain metallic poisons through
r forming indecomposable compounds;
the effect of respiration upon the blood
sists in the conversion of peroxide of iron
protoxide—that the antiseptic power of
arises from its attraction for moisture—
t chemical action is contagious; and others.
w supposing that we have no doubt of the
h of these discoveries, we must still ob-
ve that they cannot wisely be referred to,
rder to exemplify the nature of the pro-
ss of knowledge, till they have been ve-
ed by other chemists, and worked into
ir places in the general scheme of chemis-
; especially, since it is tolerably certain
t in the process of verification, they will

be modified and more precisely defined. N can I think it judicious to take so large proportion of our examples from a region science in which, of all parts of our mate knowledge, the conceptions both of ordin persons and of men of science themselves, a most loose and obscure, and the genuine pr ciples most contested; which is the case physiology. It would be easy, I think, point out the vague and indeterminate ch racter of many of the expressions in whi the above examples are propounded, as w as their doubtful position in the scale of ch mical generalization; but I have said enou to shew why I cannot give much weight these, ascardinal examples of the method discovery; and therefore I shall not exam in detail how far they support Mr Mill's r thods of inquiry.

46 Mr Liebig supplies the first and majority of Mr Mill's examples in chapter of his Book on Induction. The second is example for which Mr Mill states himself to indebted to Mr Alexander Bain; the law est lished being this, that (i. 487) electricity can exist in one body without the simultane excitement of the opposite electricity in so neighbouring body, which Mr Mill also c firms by reference to Mr Faraday's exp ments on voltaic wires.

I confess I am quite at a loss to understand hat there is in the doctrine here ascribed to r Bain which was not known to the electrians who, from the time of Franklin, explained ıe phenomena of the Leyden vial. I may obrve also that the mention of an "electrified mosphere" implies a hypothesis long obsote. The essential point in all those explaıtions was, that each electricity produced by ıduction the opposite electricity in neighouring bodies, as I have tried to make aparent in the *History* (B. XI. c. xi.) Faraday as, more recently, illustrated this universal oexistence of opposite electricities with his ısual felicity.

But the conjunction of this fact with volaic phenomena, implies a non-recognition of ome of the simplest doctrines of the subject. "Since," it is said, (i. 488) "common or machine lectricity, and voltaic electricity may be conidered for the present purpose to be identical, Faraday wished to know, &c." I think Mr Faraday would be much astonished to learn hat he considered electricity in equilibrium, nd electricity in the form of a voltaic current, o be, for any purpose, identical. Nor do I onceive that he would assent to the expresion in the next page, that "from the nature f a voltaic charge, the two opposite currents ecessary to the existence of each other are

both accommodated in one wire." Mr Faraday has, as it appears to me, studiously avoided assenting to this hypothesis.

47 The next example is the one already so copiously dwelt upon by Sir John Herschel, Dr Wells' researches on the production of Dew. I have already said (*Phil.* B. XIII. c. ii. Art. 7) that "this investigation, although it has sometimes been praised as an original discovery, was in fact only resolving the phenomenon into principles already discovered;" namely, the doctrine of a *constituent temperature* of vapour, the different conducting power of different bodies, and the like. And this agrees in substance with what Mr Mill says; (i. 497) that the discovery, when made, was corroborated by deduction from the known laws of aqueous vapour, of conduction, and the like. Dr Wells' researches on Dew tended much in this country to draw attention to the general principles of Atmology; and we may see, in this and in other examples which Mr Mill adduces, that the explanation of special phenomena by means of general principles already established, has, for common minds, a greater charm, and is more complacently dwelt on, than the discovery of the general principles themselves.

48 The next example, (i. 502) is given in order to illustrate the Method of Residues

d is the discovery by M. Arago that a disk copper affects the vibrations of the magtic needle. But this apparently detached ɜt affords little instruction compared with the ıgularly sagacious researches by which Mr ıraday discovered the cause of this effect to ɜide in the voltaic currents which the motion the magnetic needle developed in the copper. ıave spoken of this discovery in the *History* . XIII. c. viii.). Mr Mill however is quoting r John Herschel in thus illustrating the Meod of Residues. He rightly gives the Perrbations of the Planets and Satellites as tter examples of the method; (given also in e *Phil. Ind. Sc.* B. XIII. c. vii. § 17.)

49 In the next chapter (c. x.) Mr Mill eaks of Plurality of causes and of the Inmixture of effects, and gives examples of ch cases. He here teaches (i. 517) that che.cal synthesis and analysis, (as when oxyn and hydrogen compose water, and when ıter is resolved into oxygen and hydrogen,) properly *transformation;* but that because ɜ find that the weight of the compound is ual to the sum of the weights of the eleɜnts, we take up the notion of chemical *nposition.* I have endeavoured to shew *hil. Ind. Sc.* B. VI. c. iv.) that the maxim, ıt the sum of the weights of the elements equal to the weight of the compound, was,

historically, not *proved* from experiment, but *assumed* in the reasonings upon experiments.

50 I have now made my remarks upon nearly all the examples which Mr Mill gives of scientific inquiry, so far as they consist of knowledge which has really been obtained. I may mention, as points which appear to me to interfere with the value of Mr Mill's references to examples, expressions which I cannot reconcile with just conceptions of scientific truth; as when he says (i. 523), "some other force which *impinges on* the first force;" and very frequently indeed, of the "tangential *force*," as co-ordinate with the centripetal force.

When he speaks (ii. 20, Note) of "the doctrine now universally received that the earth is a great natural magnet with two poles," he does not recognise the recent theory of Gauss, so remarkably coincident with a vast body of facts. (See *Hist. Ind. Sc.* B. XII. note (D) in the second edition.) Indeed in his statement, he rejects no less the earlier views proposed by Halley, theorized by Euler, and confirmed by Hansteen, which shew that we are compelled to assume at least *four* poles of terrestrial magnetism; which I had given an account of in the first edition of the *History*.

There are several other cases which he puts, in which, the knowledge spoken of not having

een yet acquired, he tells us how he would
t about acquiring it; for instance, if the
uestion were (i. 526) whether mercury be a
ıre for a given disease; or whether the brain
e a voltaic pile (ii. 21); or whether the moon
e inhabited (ii. 100); or whether all crows are
lack (ii. 124); I confess that I have no ex-
ectation of any advantage to philosophy from
iscussions of this kind.

51 I will add also, that I do not think
ny light can be thrown upon scientific me-
ıods, at present, by grouping along with such
hysical inquiries as I have been speaking of,
peculations concerning the human mind, its
ualities and operations. Thus he speaks
. 508) of human characters, as exemplifying
he effect of plurality of causes; of (i. 518)
ıe phenomena of our mental nature, which
re analogous to chemical rather than to
ynamical phenomena; of (i. 518) the reason
hy susceptible persons are imaginative; to
hich I may add, the passage where he says
. 444) "let us take as an example of a phe-
omenon which we have no means of fabricat-
ıg artificially, a human mind." These, and
ther like examples, occur in the part of his
ork in which he is speaking of scientific
ıquiry in general, not in the Book on the
ogic of the Moral Sciences; and are, I
ink, examples more likely to lead us astray

than to help our progress, in discovering the laws of Scientific Inquiry, in the ordinary sense of the term.

52 I will now pass from Mr Mill's methods, illustrated by such examples as those which I have been considering, to the views respecting the conditions of Scientific Induction to which I had been led, by such a survey as I could make, of the whole history of the principal Inductive Sciences; and especially, to those views to which Mr Mill offers his objections*.

* There are some points in my doctrines on the subject of the Classificatory Sciences, to which Mr. Mill objects; (ii. 314, &c.) but there is nothing which I think it necessary to remark here, except one point. After speaking of Classification of organised beings in general, Mr. Mill notices (ii. 321) as an additional subject, the arrangement of natural groups into a Natural Series; and he says, that "all who have attempted a theory of natural arrangement, including among the rest Mr. Whewell, have stopped short of this: all except M. Comte." On this I have to observe, that I stopped short of, or rather passed by, the doctrine of a Series of organized beings, because I thought it bad and narrow philosophy: and that I sufficiently indicated that I did this. In the *History* (B. xvi. c. vi.) I have spoken of the doctrine of Circular Progression propounded by Mr. Macleay, and have said, "so far as this view *negatives* a mere *linear* progression in nature, which would place each genus in contact with the preceding and succeeding ones, and so far as it requires us to attend to the more varied and ramified resemblances, there can be no doubt that it is supported by

Mr Mill thinks that I have been too favourable to the employment of hypotheses, as means of discovering scientific truth; and that I have countenanced a laxness of method, in allowing hypotheses to be established, merely in virtue of the accordance of their results with the phenomena. I believe I should be as cautious as Mr Mill, in accepting mere hypothetical explanations of phenomena, in any case in which we had the phenomena, and their relations, placed before both of us in an equally clear light. I have not accepted the Undulatory theory of Heat, though recommended by so many coincidences and analogies (*Hist. Ind. Sc.* B. x. *Note* (VA) in the second edition). But I see some grave reasons for not giving any great weight to Mr Mill's admonitions;—reasons drawn from the language which he uses on the subject, and which appears to me inconsistent with the conditions of the cases to which he applies it. Thus, when he says (ii. 22) that the condition of a hypothesis accounting for all the known phenomena is "often fulfilled equally well by two conflicting hypotheses," I can only say that I know of no such case in the history of Science,

he result of all the attempts to form natural systems." And ith regard to the difference between Cuvier and M. de lainville, to which Mr. Mill refers (ii. 321), I certainly annot think that M. Comte's suffrage can add any weight the opinion of either of those great naturalists.

where the phenomena are at all numerous and complicated; and that if such a case were to occur, one of the hypotheses might always be resolved into the other. When he says that "this evidence (the agreement of the results of the hypothesis with the phenomena) cannot be of the smallest value, because we cannot have in the case of such an hypothesis the assurance that if the hypothesis be false it must lead to results at variance with the true facts," we must reply, with due submission, that we have, in the case spoken of, the most complete evidence of this; for any change in the hypothesis would make it incapable of accounting for the facts. When he says that "if we give ourselves the license of inventing the causes as well as their laws, a person of fertile imagination might devise a hundred modes of accounting for any given fact;" I reply, that the question is about accounting for a large and complex series of facts, of which the laws have been ascertained: and as a test of Mr Mill's assertion, I would propose as a challenge to any person of fertile imagination to devise any *one* other hypothesis to account for the perturbations of the moon, or the coloured fringes of shadows, besides the hypothesis by which they have actually been explained with such curious completeness. This challenge has been repeatedly offered, but never in any degree accepted; and I

tertain no apprehension that Mr Mill's pposition will ever be verified by such a rformance.

53 I see additional reason for mistrust-g the precision of Mr. Mill's views of that cordance of phenomena with the results of hypothesis, in several others of the expres-ons which he uses (ii. 23). He speaks of hypothesis being a "*plausible* explanation of l or most of the phenomena;" but the case hich we have to consider is where it gives an *act* representation of all the phenomena in hich its results can be traced. He speaks of s being certain that the laws of the pheno-ena are "*in some measure analogous*" to those ven by the hypothesis; the case to be dealt ith being, that they are in every way iden-cal. He speaks of this analogy being certain, om the fact that the hypothesis can be "for moment *tenable*;" as if any one had recom-ended a hypothesis which is tenable only hile a small part of the facts are considered, hen it is inconsistent with others which a ller examination of the case discloses. I have othing to say, and have said nothing, in favour hypotheses which are *not* tenable. He says ere are many such "*harmonies* running rough the laws of phenomena in other re-ects radically distinct;" and he gives as an stance, the laws of light and heat. I have

never alleged such harmonies as grounds o theory, except they should amount to identities; and if they should do this, I have n doubt that the most sober thinkers will suppos the causes to be of the same kind in the tw harmonizing instances. If chlorine, iodine an brome, or sulphur and phosphorus, have, a Mr. Mill says, analogous properties, I shoul call these substances *analogous:* but I can see n temptation to frame an hypothesis that the are *identical* (which he seems to fear), so lon as Chemistry proves them distinct. But an hypothesis of an analogy in the constitution o these elements (suppose, for instance, a resem blance in their atomic form or composition would seem to me to have a fair claim to trial and to be capable of being elevated from one degree of probability to another by the num ber, variety, and exactitude of the explanation of phenomena which it should furnish.

54 These expressions of Mr. Mill hav reference to a way in which hypotheses ma be corroborated, in estimating the value o which, it appears that he and I differ. "I seems to be thought," he says (ii. 23), "th an hypothesis of the sort in question is entitle to a more favourable reception, if, besides a counting for the facts previously known, it h led to the anticipation and prediction of othe which experience afterwards verified." A

ıdds, "Such predictions and their fulfil-
t are indeed well calculated to strike the
rant vulgar;" but it is strange, he says,
any considerable stress should be laid
ı such a coincidence by scientific thinkers.
rever strange it may seem to him, there is
loubt that the most scientific thinkers, far
e than the ignorant vulgar, have allowed
coincidence of results predicted by theory
ı fact afterwards observed, to produce the
ngest effects upon their conviction; and
; all the best-established theories have ob-
ıed their permanent place in general accept-
e in virtue of such coincidences, more than
ıny other evidence. It was not the igno-
t vulgar alone, who were struck by the
ırn of Halley's comet, as an evidence of
Newtonian theory. Nor was it the igno-
t vulgar, who were struck with those facts
ch did so much strike men of science, as
iously felicitous proofs of the undulatory
ory of light,—the production of darkness
two luminous rays interfering in a special
nner; the refraction of a single ray of
ıt into a conical pencil; and other complex
precise results, predicted by the theory
l verified by experiment. It must, one
ıld think, strike all persons in proportion
their thoughtfulness, that when Nature thus
ıs our bidding, she acknowledges that we

have learnt her true language. If we can pre
dict new facts which we have not seen, as we
as explain those which we have seen, it mus
be because our explanation is not a mer
formula of observed facts, but a truth of
deeper kind. Mr Mill says, "If the laws o
the propagation of light agree with those o
the vibrations of an elastic fluid in so many
respects as is necessary to make the hypothe
sis a plausible explanation of all or most of th
phenomena known at the time, it is nothing
strange that they should accord with each
other in one respect more." Nothing strange
if the theory be true; but quite unaccountable
if it be not. If I copy a long series of letters
of which the last half-dozen are concealed, and
if I guess those aright, as is found to be the
case when they are afterwards uncovered, this
must be because I have made out the import
of the inscription. To say, that because I have
copied all that I could see, it is nothing strange
that I should guess those which I cannot see,
would be absurd, without supposing such a
ground for guessing. The notion that the
discovery of the laws and causes of phenomena
is a loose hap-hazard sort of guessing, which
gives "plausible" explanations, accidental coin-
cidences, casual "harmonies," laws, "in some
measure analogous" to the true ones, suppo-
sitions "tenable" for a time, appears to me to

a misapprehension of the whole nature of ience; as it certainly is inapplicable to the se to which it is principally applied by Mr ill.

55 There is another kind of evidence of eories, very closely approaching to the veri- ation of untried predictions, and to which, parently, Mr Mill does not attach much im- rtance, since he has borrowed the term by hich I have described it, *Consilience*, but has plied it in a different manner (ii. 530, 563, 0). I have spoken, in the *Philosophy* (B. xi. v. art. 11), of the *Consilience of Inductions*, as ne of the *Tests of Hypotheses*, and have exem- lified it by many instances; for example, the heory of universal gravitation, obtained by duction from the motions of the planets, was ound to explain also that peculiar motion of he spheroidal earth which produces the Pre- ession of the Equinoxes. This, I have said, as a striking and surprising coincidence which ave the theory a stamp of truth beyond the ower of ingenuity to counterfeit. I may com- are such occurrences to a case of interpreting n unknown character, in which two different nscriptions, deciphered by different persons, hould have given the same alphabet. We hould, in such a case, believe with great con- dence that the alphabet was the true one; nd I will add, that I believe the history of

science offers no example in which a the supported by such consiliences, has been af wards proved to be false.

56 Mr Mill accepts (ii. 21) a rule of Comte's, that we may apply hypotheses, p vided they are capable of being afterwa verified as facts. I have a much hig respect for Mr Mill's opinion than for Comte's*; but I do not think that this r will be found of any value. It appears to to be tainted with the vice which I have

* I have given elsewhere (*Philos. Ind. Sc.* B. XII. c. reasons why I cannot assign to M. Comte's *Philosophie tive* any great value as a contribution to the philosoph science. In this judgment I conceive that I am suppo by the best philosophers of our time. M. Comte owe think, much of the notice which has been given to hi his including, as Mr Mill does, the science of society an human nature in his scheme, and to his boldness in dea with these. He appears to have been received with d ence as a mathematician: but Sir John Herschel has sh that a supposed astronomical discovery of his is a mere sumption. I conceive that I have shewn that his represe tions of the history of science is erroneous, both in its de and in its generalities. His distinction of the three stage sciences, the theological, metaphysical, and positive, is n all supported by the facts of scientific history. Real di veries always involve what he calls *metaphysics;* and doctrine of final causes in physiology, the main elemer science which can properly be called *theological,* is retain the end, as well as the beginning of the science, by all ex a peculiar school.

ady noted, of throwing the whole burthen of
:planation upon the unexplained word *fact*—
ıexplained in any permanent and definite
›position to theory. As I have said, the
ewtonian theory *is* a fact. Every true
.eory is a fact. Nor does the distinction
:come more clear by Mr Mill's examples.
The vortices of Descartes would have been,"
: says, "a perfectly legitimate hypothesis,
it had been possible by any mode of
ɛplanation which we could entertain the
›pe of possessing, to bring the question
hether such vortices exist or not, within the
:ach of our observing faculties." But this
as possible, and was done. The free pas-
ıge of comets through the spaces in which
ıese vortices should have been, convinced
ıen that these vortices did not exist. In like
ıanner Mr Mill rejects the hypothesis of a
ıminiferous ether, "because it can neither be
:en, heard, smelt, tasted, or touched." It is
strange complaint to make of the vehicle of
ght, that it cannot be heard, smelt, or tasted.
ts vibrations *can* be seen. The fringes of
ıadows for instance, shew its vibrations, just
s the visible lines of waves near the shore
ıew the undulations of the sea. Whether
ıis ether can be touched, that is, whether it
:sists motion, is hardly yet clear. I am far
'om saying there are not difficulties on this

point, with regard to *all* theories which su
pose a *medium*. But there are no more dif
culties of this kind in the undulatory theory
light, than there are in Fourier's theory
heat, which M. Comte adopts as a model
scientific investigation; or in the theory
voltaic *currents*, about which Mr Mill appe
to have no doubt; or of electric *atmosphere*
which, though generally obsolete, Mr Mill a
pears to favour; for though it had been sai
that we *feel* such atmospheres, no one had sai
that they have the other attributes of matte

57 Mr Mill conceives (ii. 17) that his ow
rule concerning hypotheses coincides wi
Newton's Rule, that the cause assumed mu
be a *vera causa*. But he allows that "Mr Wh
well... has had little difficulty in shewing tha
his (Newton's) conception was neither preci
nor consistent with itself." He also allow
that "Mr Whewell is clearly right in denyin
it to be necessary that the cause assigne
should be a cause already known; else ho
could we ever become acquainted with ne
causes?" These points being agreed upo
I think that a little further consideration wi
lead to the conviction that Newton's Ru
of philosophizing will best become a valuab
guide, if we understand it as asserting th
when the explanation of two or more differe
kinds of phenomena (as the revolutions of t

anets, the fall of a stone, and the precession
the equinoxes,) lead us to *the same* cause,
ich a coincidence gives a reality to the
use. We have, in fact, in such a case, a
onsilience of Inductions.

58 When Mr Mill condemns me (ii. 24)
sing, however, expressions of civility which
gladly acknowledge,) for having recognized
o mode of Induction except that of trying
ypothesis after hypothesis until one is found
hich fits the phenomena, I must beg to re-
ind the readers of our works, that Mr Mill
imself allows (i. 363) that the process of find-
g a conception which binds together ob-
rved facts "is tentative, that it consists of
succession of guesses, many being rejected
ntil one at last occurs fit to be chosen."
must remind them also that I have given
Section upon the *Tests of Hypotheses*, to
hich I have just referred,—that I have given
rious methods of Induction, as the *Method*
Gradation, the *Method of Natural Classifi-*
tion, the *Method of Curves*, the *Method of*
eans, the *Method of Least Squares*, the *Me-*
od of Residues: all which I have illustrated
conspicuous examples from the History of
ience; besides which, I conceive that what
have said of the Ideas belonging to each
ience, and of the construction and explication
conceptions, will point out in each case, in

what region we are to look for the Inductive Element in order to make new discoveries. I have already ventured to say, elsewhere, that the methods which I have given, are as definite and practical as any others which have been proposed, with the great additional advantage of being the method by which all great discoveries in science have really been made.

59 There is one feature in the construction of science which Mr Mill notices, but to which he does not ascribe, as I conceive, its due importance: I mean, that process by which we not only ascend from particular facts to a general law, but when this is done, ascend from the first general law to others more general; and so on, proceeding to the highest point of generalization. This character of the scientific process was first clearly pointed out by Bacon, and is one of the most noticeable instances of his philosophical sagacity. "There are," he says, "two ways, and can be only two, of seeking and finding truth. The one from sense and particulars, takes a flight to the most general axioms, and from these principles and their truth, settled once for all, invents and judges of intermediate axioms. The other method collects axioms from sense and particulars, ascending *continuously and by degrees*, so that in the end it

rives at the most general axioms:" meaning *axioms*, laws or principles. The structure the most complete sciences consists of seveal such steps,—*floors*, as Bacon calls them, of ıccessive generalization; and thus this strucıre may be exhibited as a kind of scientific yramid. I have constructed this pyramid in ıe case of the science of Astronomy*: (*Phil. nd. Sc.* B. xi. c. vi.) and I am gratified to nd that the illustrious Humboldt approves f the design, and speaks of it as executed ith complete success. (*Cosmos*, Vol. ii. Note 5). The capability of being exhibited in this orm of successive generalizations, arising from articulars upward to some very general law, the condition of all tolerably perfect ciences; and the steps of the successive geeralizations are commonly the most imporant events in the history of the science.

60 Mr Mill does not reject this process f generalization; but he gives it no conpicuous place, making it only one of three ıodes of reducing a law of causation into ther laws. "There is," he says, (i. 555) "the *ubsumption* of one law under another; ... the athering up of several laws into one more

* I have also, in the same place, given the Inductive yramid for the science of Optics. These Pyramids are ecessarily inverted in their form, in order that, in reading the ordinary way, we may proceed *to* the vertex.

general law which includes them all." He adds afterwards, that the general law is the *sum* of the partial ones (i. 557), an expression which appears to me inadequate, for reasons which I have already stated. The general law is not the mere sum of the particular laws. It is, as I have already said, their amount *in a new point of view*. A new conception is introduced; thus, Newton did not merely add together the laws of the motions of the moon and of the planets, and of the satellites, and of the earth; he looked at them altogether as the result of a universal force of mutual gravitation; and therein consisted his generalization. And the like might be pointed out in other cases.

61 I am the more led to speak of Mr Mill as not having given due importance to this process of successive generalization, by the way in which he speaks in another place (ii. 525) of this doctrine of Bacon. He conceives Bacon "to have been radically wrong, when he enunciates, as a universal rule, that induction should proceed from the lowest to the middle principles, and from those to the highest, never reversing that order, and consequently, leaving no room for the discovery of new principles by way of deduction* at all."

* The reader will probably recollect that as *Induction* means the inference of general propositions from particular

62 I conceive that the Inductive Table of stronomy, to which I have already referred, ıews that in that science, the most complete hich has yet existed, the history of the ience has gone on, as to its general move- ıent, in accordance with the view which acon's sagacity enjoined. The successive eneralizations, *so far as they were true*, were ıade by successive generations. I conceive so that the Inductive Table of Optics shews ıe same thing; and this, without taking for ranted the truth of the Undulatory Theory; ır with regard to all the steps of the progress f the science, lower than that highest one, ıere is, I conceive, no controversy.

63 Also, the Science of Mechanics, al- ıough Mr Mill more especially refers to it, a case in which the highest generalizations or example the Laws of Motion) were those ırliest ascertained with any scientific exact- ess, will, I think, on a more careful exami- ıation of its history, be found remarkably to nfirm Bacon's view. For, in that science, e have, in the first place, very conspicuous

ses, *Deduction* means the inference by the application of neral propositions to particular cases, and by combining ch applications; as when from the most general principles Geometry or of Mechanics, we prove some less general eorem; for instance, the number of the possible regular ids, or the principle of *vis viva*.

examples of the vice of the method pursued by the ancients in flying to the highest generalizations first; as when they made their false distinctions of the laws of *natural* and *violent* motions, and of *terrestrial* and *celestial* motions. Many erroneous laws of motion were asserted through neglect of facts or want of experiments. And when Galileo and his school had in some measure succeeded in discovering some of the true laws of the motions of terrestrial bodies, they did not at once assert them as general: for they did not at all apply those laws to the celestial motions. As I have remarked, all Kepler's speculations respecting the causes of the motions of the planets, went upon the supposition that the First Law of terrestrial Motion did not apply to celestial bodies; but that, on the contrary, some continual force was requisite to keep up, as well as to originate, the planetary motions. Nor did Descartes, though he enunciated the Laws of Motion with more generality than his predecessors, (but not with exactness,) venture to trust the planets to those laws; on the contrary, he invented his machinery of Vortices in order to keep up the motions of the heavenly bodies. Newton was the first who extended the laws of terrestrial motion to the celestial spaces; and in doing so, he used all the laws of the celestial motions which had previously

ɔen discovered by more limited inductions. o these instances, I may add the gradual ɔneralization of the third Law of motion by Iuyghens, the Bernoullis, and Herman, which have described in the *History* (B. VI. c. v.) as receding that Period of Deduction, to which ıe succeeding narrative (c. vi.) is appropriated. In Mechanics, then, we have a cardinal xample of the historically gradual and successive ascent of science from particulars to he most general laws.

64 The Science of Hydrostatics may ppear to offer a more favourable example of he ascent to the most general laws, without oing through the intermediate particular laws; nd it is true, with reference to this science, s I have observed (*Hist.* B. VI. c. vi. sect. 13), hat it does exhibit the *peculiarity* of our possessing the most general principles on which he phenomena depend, and from which many ases of special facts are explained by deduction; while other cases cannot be so explained, rom the want of principles intermediate beween the highest and the lowest. And I ave assigned, as the reason of this peculiarity, hat the general principles of the Mechanics f Fluids were not obtained with reference to ıe science itself, but by extension from the ster science of the Mechanics of Solids. The wo sciences are parts of the same Inductive

Pyramid; and having reached the summit of this Pyramid on one side, we are tempted to descend on the other from the highest generality to more narrow laws. Yet even in this science, the best part of our knowledge is mainly composed of inductive laws, obtained by inductive examination of particular classes of facts. The mere mathematical investigations of the laws of waves, for instance, have not led to any results so valuable as the experimental researches of Bremontier, Emy, the Webers, and Mr Scott Russell. And in like manner in Acoustics, the Mechanics of Elastic Fluids, (*Hist. Ind. Sc.* B. VIII.), the deductions of mathematicians made on general principles have not done so much for our knowledge, as the cases of vibrations of plates and pipes examined experimentally by Chladni, Savart, Mr Wheatstone and Mr Willis. We see therefore, even in these sciences, no reason to slight the wisdom which exhorts us to ascend from particulars to intermediate laws, rather than to hope to deduce these latter better from the more general laws obtained once for all.

65 Mr Mill himself indeed, notwithstanding that he slights Bacon's injunction to seek knowledge by proceeding from less general to more general laws, has given a very good reason why this is commonly necessary and

ise. He says (ii. 526), "Before we attempt
explain deductively, from more general
ws, any new class of phenomena, it is de-
rable to have gone as far as is practicable in
certaining the empirical laws of these phe-
omena; so as to compare the results of
eduction, not with one individual instance
fter another, but with general propositions
xpressive of the points of agreement which
ave been found among many instances. For,"
e adds with great justice, "if Newton had
een obliged to verify the theory of gravi-
tion, not by deducing from it Kepler's laws,
ut by deducing all the observed planetary
ositions which had served Kepler to establish
ose laws, the Newtonian theory would pro-
ably never have emerged from the state of an
ypothesis." To which we may add, that it is
ertain, from the history of the subject, that
that case the hypothesis would never have
een framed at all.

66 Mr Mill expresses a hope of the effi-
cy of Deduction, rather than Induction, in
romoting the future progress of Science;
hich hope, so far as the physical sciences are
oncerned, appears to me at variance with all
e lessons of the history of those sciences.
e says (i. 579), "that the advances henceforth
be expected even in physical, and still more
mental and social science, will be chiefly the

result of deduction, is evident from the general considerations already adduced:" these considerations being, that the phenomena to be considered are very complex, and are the result of many known causes, of which we have to disentangle the results.

67 I cannot but take a very different view from this. I think that any one, looking at the state of physical science, will see that there are still a vast mass of cases, in which we do not at all know the causes, at least, in their full generality; and that the knowledge of new causes, and the generalization of the laws of those already known, can only be obtained by new *inductive* discoveries. Except by new Inductions, equal, in their efficacy for grouping together phenomena in new points of view, to any which have yet been performed in the history of science, how are we to solve such questions as those which, in the survey of what we already know, force themselves upon our minds. Such as, to take only a few of the most obvious examples— What is the nature of the connexion of heat and light? How does heat produce the expansion, liquefaction and vaporization of bodies? What is the nature of the connexion between the optical and the chemical properties of light? What is the relation between optical, crystalline and chemical po-

rity? What is the connexion between the tomic constitution and the physical qualities f bodies? What is the tenable definition of mineral species? What is the true relation f the apparently different types of vegetable fe (monocotyledons, dicotyledons, and cryptoamous plants)? What is the relation of the arious types of animal life (vertebrates, artiulates, radiates, &c.)? What is the number, and hat are the distinctions of the Vital Powers? What is the internal constitution of the earth? hese, and many other questions of equal nterest, no one, I suppose, expects to see olved by deduction from principles already nown. But we can, in many of them, see ood hope of progress by a large use of inuction; including, of course, copious and areful experiments and observations.

68 With such questions before us, as have ow been suggested, I can see nothing but a ost mischievous narrowing of the field and nfeebling of the spirit of scientific exertion, o the doctrine that "Deduction is the great cientific work of the present and of future ges;" and that "A revolution is peaceably nd progressively effecting itself in philosophy he reverse of that to which Bacon has atached his name." I trust, on the contrary, hat we have yet many new laws of nature till to discover; and that our race is destined

to obtain a sight of wider truths than any yet discern, including, as cases, the gene laws we now know, and obtained from th known laws as they must be, by Induction.

69 I can see, however, reasons for comparatively greater favour with which Mill looks upon Deduction, in the views which he has mainly directed his attenti The explanation of remarkable phenomena known laws of Nature, has, as I have alre said, a greater charm for many minds t the discovery of the laws themselves. In case of such explanations, the problem p posed is more definite, and the solution m obviously complete. For the process of ind tion includes a mysterious step, by which pass from particulars to generals, of which s the reason always seems to be inadequately r dered by any words which we can use; this step to most minds is not demonstrat as to few is it given to perform it on a gr scale. But the process of explanation of f by known laws is deductive, and has at ev step a force like that of demonstration, p ducing a feeling peculiarly gratifying to clear intellects which are most capable of lowing the process. We may often see instar in which this admiration for deductive s appears in an extravagant measure; as w men compare Laplace with Newton.

should I think it my business to argue against such a preference, except it were likely to leave us too well satisfied with what we know already, to chill our hope of scientific progress, and to prevent our making any further strenuous efforts to ascend, higher than we have yet done, the mountain-chain which limits human knowledge.

70 But there is another reason which, I conceive, operates in leading Mr Mill to look to Deduction as the principal means of future progress in knowledge, and which is a reason of considerable weight, in the subjects of research which, as I conceive, he mainly has in view. In the study of our own minds and of the laws which govern the history of society, I do not think that it is very likely that we shall hereafter arrive at any wider principles than those of which we already possess some considerable knowledge; and this, for a special reason; namely, that our knowledge in such cases is not gathered by mere external observation of a collection of external facts; but acquired by attention to internal facts, our own emotions, thoughts, and springs of action; facts which are connected by ties existing in our own consciousness, and not in mere observed juxta-position, succession, or similitude. How the character, for instance, is influenced by various causes, (an example to which Mr Mill repeatedly refers, ii. 518, &c.), is an enquiry

which may perhaps be best conducted by sidering what we know of the influen education and habit, government and oc tion, hope and fear, vanity and pride, an like, upon men's characters, and by tr the various effects of the intermixture of influences. Yet even here, there seems room for the discovery of laws in the w experimental inquiry: for instance, what race or family has in the formation of ch ter; a question which can hardly be s to any purpose in any other way tha collecting and classing instances. And i same way, many of the principles which late the material wealth of states, are obt if not exclusively, at least most clearly securely, by induction from large surve facts. Still, however, I am quite ready to that in Mental and Social Science, we are less likely than in Physical Science, to c new truths by any process which can b tinctively termed *Induction;* and that in sciences, what may be called *Deductions* principles of thought and action of whi are already conscious, or to which we when they are felicitously picked out c thoughts and put into words, must have a share; and I may add, that this observat Mr Mill appears to me to be important in its present connexion, new.

71 I have made nearly all the re

ıich I now think it of any consequence to ake upon Mr Mill's *Logic*, so far as it bears ıon the doctrines contained in my *History* ıd *Philosophy*. And yet there remains still ıtouched one great question, involving pro-ıbly the widest of all the differences between m and me. I mean the question whether ɔometrical axioms (and, as similar in their ıidence to these, *all* axioms) be truths derived om experience, or be necessary truths in some ɛeper sense. This is one of the fundamental ıestions of philosophy; and all persons who ke an interest in metaphysical discussions, ıow that the two opposite opinions have been aintained with great zeal in all ages of spe-lation. To me it appears that there are *two* ɜtinct elements in our knowledge, Experi-ıce, without, and the Mind, within. Mr Mill ɜrives all our knowledge from Experience *one*. In a question thus going to the root of l knowledge, the opposite arguments must ɛeds cut deep on both sides. Mr Mill cannot ɛny that our knowledge of geometrical axioms ıd the like, *seems* to be *necessary*. I cannot ɛny that our knowledge, axiomatic as well as ther, *never is* acquired *without experience*.

72 Perhaps ordinary readers may despair f following our reasonings, when they find that ıey can only be made intelligible by suppos-ıg, on the one hand, a person who thinks dis-

tinctly, and yet has never seen or felt any
ternal object; and on the other hand, a per
who is transferred, as Mr Mill supposes (ii. 1
to "distant parts of the stellar regions wh
the phenomena may be entirely unlike th
with which we are acquainted," and where e
the axiom, that every effect must have a ca
does not hold good. Nor, in truth, do I th
it necessary here to spend many words
this subject. Probably, for those who
an interest in this discussion, most of
arguments on each side have already been
forwards with sufficient repetition. I h
in an "Essay on the Fundamental Antith
of Philosophy," and in some accompan
"Remarks," printed at the end of the sec
edition of my *Philosophy*, given my repl
what has been said on this subject, both
Mr Mill, and by the author of a very
critique on my *History* and *Philosophy* w
appeared in the *Quarterly Review* in 1
and I will not here attempt to revive
general discussion.

73 Perhaps I may be allowed to no
that in one part of Mr Mill's work where
subject is treated, there is the appearanc
one of the parties to the controversy
nouncing judgment in his own cause.
indeed is a temptation which it is espec
difficult for an author to resist, who writ

atise upon *Fallacies*, the subject of Mr Mill's
h Book. In such a treatise, the writer has
easy way of disposing of adverse opinions
classing them as "Fallacies," and putting
m side by side with opinions universally
nowledged to be false. In this way, Mr
ll has dealt with several points which are
l, as I conceive, matters of controversy.
357, &c.).

74 But undoubtedly, Mr Mill has given
argument against my opinions with great
tinctness in another place (i. 319). In order
shew that it is merely habitual association
ch gives to an experimental truth the
racter of a necessary truth, he quotes the
e of the laws of motion, which were really
covered from experiment, but are now
ked upon as the only conceivable laws;
l especially, what he conceives as "the *re-
tio ad absurdum* of the theory of inconceiv-
eness," an opinion which I had ventured to
ow out, that if we could conceive the Com-
ition of bodies distinctly, we might be able
ee that it is necessary that the modes of
ir composition should be definite. I do not
ık that readers in general will see anything
urd in the opinion, that the laws of Mecha-
s, and even the laws of the chemical Com-
ition of bodies, may depend upon princi-
s as necessary as the properties of space

and number; and that this necessity, not at all perceived by persons wh only the ordinary obscure and confused on such subjects, may be evident to which has, by effort and discipline, r its ideas of Mechanical Causation, Ele Composition and Difference of Kin and precise. It may easily be, I c that while such necessary principles ceived to be necessary only by a fe of highly cultivated insight, such prin the axioms of Geometry and Arithme be perceived to be necessary by *al* which have any habit of abstract tho all: and I conceive also, that thoug axioms are brought into distinct a certain degree of intellectual cul they may still be much better desc conditions of experience, than as r experience:—as laws of the mind an activity, rather than as facts impress a mind merely passive.

75 I will not pursue the subject only, as the question has arisen respec absurdities to which each of the doctrines leads, I will point out opini nected with this subject, which Mr stated in various parts of his book.

He holds (i. 317) that it is mer habit that we are unable to conceive

rint of space or the *last instant* of time. He ılds (ii. 360) that it is strange that any one ıould rely upon the *à priori* evidence that ɔace or extension is infinite, or that nothing ın be made of nothing. He holds (i. 304) ıat the first law of *motion* is *rigorously* true, ıt that the axioms respecting the *lever* are ıly *approximately* true. He holds (ii. 110) ıat there may be sidereal firmaments in hich events succeed each other at random, ithout obeying any laws of causation; al-ıough one might suppose that even if space ıd cause are both to have their limits, still ıey might terminate together: and then, ʻen on this bold supposition, we should no *here* have a world in which events were *ısual.* He holds (ii. 111) that the axiom, that ʻery event must have a cause, is established ʏ means of an "induction by simple enume-ıtion:" and in like manner, that the prin-ples of number and of geometry are proved ʏ this method of simple enumeration alone. ,e ascribes the proof (i. 162) of the axiom, things which are equal to the same are equal ı each other," to the fact that this proposi-on has been perpetually *found* true and never lse. He holds (i. 338) that "In all proposi-ons concerning numbers, a condition is im-ed, without which none of them would be ue; and that condition is an assumption hich *may be false. The condition is that* 1 = 1."

76 Mr Mill further holds (i. 309), that it is a characteristic property of geometrical forms, that they are capable of being painted in the imagination with a distinctness equal to reality:—that our ideas of forms exactly resemble our sensations; which, it is implied, is not the case with regard to any other class of our ideas;—that we thus may have mental pictures of all possible combinations of lines and angles, which are as fit subjects of "geometrical experimentation" as the realities themselves. He says, that "we know that the imaginary lines exactly resemble real ones;" and that we obtain this knowledge respecting the characteristic property of the idea of space by experience; though it does not appear *how* we can compare our *ideas* with the *realities*, since we know the realities only *by* our ideas; or why this property of their resemblance should be confined to *one class* of ideas alone.

77 I have now made such remarks as appear to me to be necessary, on the most important parts of Mr Mill's criticism of my *Philosophy*. I hope I have avoided urging any thing in a contentious manner; as I have certainly written with no desire for controversy, but only with a view to offer to those who may be willing to receive it, some explanation of portions of my previous writings. I have already said, that if this had not been my especial object, I could with pleasure have noted

the passages of Mr Mill's *Logic* which I admire, rather than the points in which we differ. I will in a very few words refer to some of these points, as the most agreeable way of taking leave of the dispute.

I say then that Mr Mill appears to me especially instructive in his discussion of the nature of the proof which is conveyed by the syllogism; and that his doctrine, that the force of the syllogism consists in an *inductive assertion, with an interpretation added to it*, solves very happily the difficulties which baffle the other theories of this subject. I think that this doctrine of his is made still more instructive, by his excepting from it the cases of Scriptural Theology and of Positive Law (i. 260), as cases in which general propositions, not particular facts, are our original data. I consider also that the recognition of *Kinds* (i. 166) as classes in which we have, not a finite but an *inexhaustible* body of resemblances among individuals, and as groups made by nature, not by mere definition, is very valuable, as stopping the inroad to an endless train of false philosophy. I conceive that he takes the right ground in his answer to Hume's argument against miracles (ii. 183): and I admire the acuteness with which he has criticized Laplace's tenets on the Doctrine of Chances, and the candour with which he has, in the second edition, acknow-

ledged oversights on this subject made i
first. I think that much, I may almost sa
which he says on the subject of Langu
is very philosophical; for instance, wha
says (ii. 238) of the way in which words ac
their meaning in common use. I espec
admire the acuteness and force with whic
has shewn (ii. 255) how moral principles
pressed in words degenerate into form
and yet how the formula cannot be reje
without a moral loss. This "perpetual
lation in spiritual truths," as he happily t
it, has never, I think, been noted in the
broad manner, and is a subject of mos
structive contemplation. And though I
myself refrained from associating moral
political with physical science in my stu
the subject, I see a great deal which is fu
promise for the future progress of moral
political knowledge in Mr Mill's sixth B
"On the Logic of the Moral and Pol
Sciences." Even his arrangement of the va
methods which have been or may be foll
in "the Social Science,"—"the Chemic
Experimental Method," "the Geometric
Abstract Method," "the Physical or Con
Deductive Method," "the Inverse Dedu
or Historical Method," though in some d
fanciful and forced, abounds with val
suggestions; and his estimate of "the int

ng philosophy of the Bentham school," the nain example of "the geometrical method," is nteresting and philosophical. On some future ɔccasion, I may, perhaps, venture into the re-ɡion of which Mr Mill has thus essayed to map the high ways: for it is from no despair either of the great progress to be made in such truth as that here referred to, or of the effect of philosophical method in arriving at such truth, that I have, in what I have now written, con-fined myself to the less captivating but more definite part of the subject.

C. C., June 27, 1849.

THE END.

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