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ILLUSTRATIONS OF THE LOGIC OF SCIENCE.

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SIXTH PAPER.—DEDUCTION, INDUCTION, AND HYPOTHESIS.

I.

THE chief business of the logician is to classify arguments; for all testing clearly depends on classification. The classes of the logicians are defined by certain typical forms called syllogisms. For example, the syllogism called *Barbara* is as follows:

S is M; M is P:
Hence, S is P.

Or, to put words for letters—

Enoch and Elijah were men; all men die:
Hence, Enoch and Elijah must have died.

The "is P" of the logicians stands for any verb, active or neuter. It is capable of strict proof (with which, however, I will not trouble the reader) that all arguments whatever can be put into this form; but only under the condition that the is shall mean "*is* for the purposes of the argument" or "is represented by." Thus, an induction will appear in this form something like this:

These beans are two-thirds white;
But, the beans in this bag are (represented by) these beans;
∴The beans in the bag are two-thirds white.

But, because all inference may be reduced in some way to *Barbara,* it does not follow that this is the most appropriate form in which to represent every kind of inference. On the contrary, to show the distinctive characters of different sorts of inference, they must clearly be exhibited in different forms peculiar to each. *Barbara* particularly typifies deductive reasoning; and so long as the is is taken literally, no inductive reasoning can be put into this form. *Barbara* is, in fact, nothing but the application of a rule. The so-called major premise lays down this rule; as, for example, *All men are mortal.* The other or minor premise states a case under the rule; as, *Enoch was a man.*

The conclusion applies the rule to the case and states the result: *Enoch is mortal.* All deduction is of this character; it is merely the application of general rules to particular cases. Sometimes this is not very evident, as in the following:

> All quadrangles are figures,
> But no triangle is a quadrangle;
> Therefore, some figures are not triangles.

But here the reasoning is really this:

> *Rule*.—Every quadrangle is other than a triangle.
> *Case*.—Some figures are quadrangles.
> *Result*.—Some figures are not triangles.

Inductive or synthetic reasoning, being something more than the mere application of a general rule to a particular case, can never be reduced to this form.

If, from a bag of beans of which we know that $\frac{2}{3}$ are white, we take one at random, it is a deductive inference that this bean is probably white, the probability being $\frac{2}{3}$. We have, in effect, the following syllogism:

> *Rule*.—The beans in this bag are white.
> *Case*.—This bean has been drawn in such a way that in the long run the relative number of white beans so drawn would be equal to the relative number in the bag.
> *Result*.—This bean has been drawn in such a way that in the long run it would turn out white $\frac{2}{3}$ of the time.

If instead of drawing one bean we draw a handful at random and conclude that about of the handful are probably white, the reasoning is of the same sort. If, however, not knowing what proportion of white beans there are in the bag, we draw a handful at random and, finding $\frac{2}{3}$ of the beans in the handful white, conclude that about $\frac{2}{3}$ of those in the bag are white, we are rowing up the current of deductive sequence, and are concluding a rule from the observation of a result in a certain case. This is particularly clear when all the handful turn out one color. The induction then is:

> These beans were in this bag.
> These beans are white.
> ∴ All the beans in the bag were white.

Which is but an inversion of the deductive syllogism.

> *Rule*. All the beans in the bag were white.
> *Case*. These beans were in the bag
> *Result*. These beans are white.

So that induction is the inference of the *rule* from the *case* and *result*.

But this is not the only way of inverting a deductive syllogism so as to produce a synthetic inference. Suppose I enter a room and there find a number of bags, containing different kinds of beans. On the table there is a handful of white beans; and, after some searching, I find one of the bags contains white beans only. I at once infer as a probability, or as a fair guess, that this handful was taken out of that bag. This sort of inference is called *making an hypothesis*. It is the inference of a case from a rule and result. We have, then—

Deduction.

Rule.—All the beans from this bag are white.

Case.—These beans are from this bag.

∴ *Result*.—These beans are white.

Induction.

Case.—These beans are from this bag.

Result.—These beans are white.

∴ *Rule*.—All the beans from this bag are white.

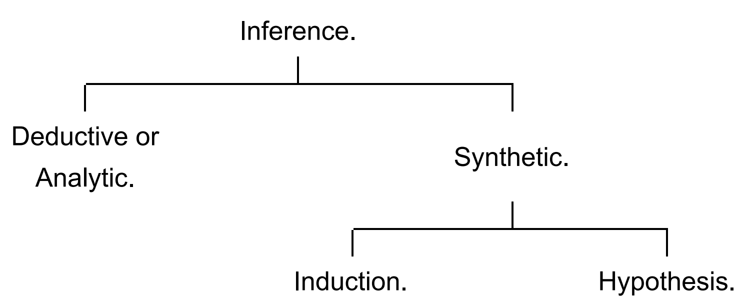
Hypothesis.

Rule.—All the beans from this bag are white.

Result.—These beans are white.

∴ *Case*.—These beans are from this bag.

We, accordingly, classify all inference as follows:



Induction is where we generalize from a number of cases of which something is true, and infer that the same thing is true of a whole class. Or, where we find a certain thing to be true of a certain proportion of cases and infer that it is true of the same proportion of the whole class. Hypothesis is where we find some very curious circumstance, which would be explained by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition. Or, where we find that in certain respects two objects have a strong resemblance, and infer that they resemble one another strongly in other respects.

I once landed at a seaport in a Turkish province; and, as I was walking up to the house which I was to visit, I met a man upon horseback, surrounded by four horsemen holding a canopy over his head. As the governor of the province was the only personage I could think of who would be so greatly honored, I inferred that this was he. This was an hypothesis.

Fossils are found; say, remains like those of fishes, but far in the interior of the country. To explain the phenomenon, we suppose the sea once washed over this land. This is another hypothesis.

Numberless documents and monuments refer to a conqueror called Napoleon Bonaparte. Though we have not seen the man, yet we can not explain what we have seen, namely, all these documents and monuments, without supposing that he really existed. Hypothesis again.

As a general rule, hypothesis is a weak kind of argument. It often inclines our judgment so slightly toward its conclusion that we cannot say that we believe the latter to be true; we only surmise that it may be so. But there is no difference except one of degree between such an inference and that by which we are led to believe that we remember the occurrences of yesterday from our feeling as if we did so.

II.

Besides the way just pointed out of inverting a deductive syllogism to produce an induction or hypothesis, there is another. If from the truth of a certain premise the truth of a certain conclusion would necessarily follow, then from the falsity of the conclusion the falsity of the premise would follow. Thus, take the following syllogism in *Barbara*:

Rule.—All men are mortal.

Case.—Enoch and Elijah were men.

∴ *Result*.—Enoch and Elijah were mortal.

Now, a person who denies this result may admit the rule, and, in that case, he must deny the case. Thus:

Denial of Result.—Enoch and Elijah were not mortal.

Rule.—All men are mortal.

∴ *Denial of Case*.—Enoch and Elijah were not men.

This kind of syllogism is called *Baroco*, which is the typical mood of the second figure. On the other hand, the person who denies the result may admit the case, and in that case he must deny the rule. Thus:

Denial of the Result.—Enoch and Elijah were not mortal.

Case.—Enoch and Elijah were men.

∴ *Denial of the Rule*.—Some men are not mortal.

This kind of syllogism is called *Bocardo*, which is the typical mood of the third figure.

Baroco and *Bocardo* are, of course, deductive syllogisms; but of a very peculiar kind. They are called by logicians indirect moods, because they need some transformation to appear as the application of a rule to a particular case. But if, instead of setting out as we have here done with a necessary deduction in *Barbara*, we take a probable deduction of similar form, the indirect moods which we shall obtain will be—

Corresponding to *Baroco*, an hypothesis;
and, Corresponding to *Bocardo*, an induction.

For example, let us begin with this probable deduction in *Barbara*:

Rule.—Most of the beans in this bag are white.

Case.—This handful of beans are from this bag.

∴ *Result*.—Probably, most of this handful of beans are white.

Now, deny the result, but accept the rule:

Denial of Result.—Few beans of this handful are white.

Rule.—Most beans in this bag are white.

∴ *Denial of Case*.—Probably, these beans were taken from another bag.

This is an hypothetical inference. Next, deny the result, but accept the case:

Denial of Result.—Few beans of this handful are white.

Case.—These beans came from this bag.

∴ *Denial of Rule*.—Probably, few beans in the bag are white.

This is an induction.

The relation thus exhibited between synthetic and deductive reasoning is not without its importance. When we adopt a certain hypothesis, it is not alone because it will explain the observed facts, but also because the contrary hypothesis would probably lead to results contrary to those observed. So, when we make an induction, it is drawn not only because it explains the distribution of characters in the sample, but also because a different rule would probably have led to the sample being other than it is.

But the advantage of this way of considering the subject might easily be overrated. An induction is really the inference of a rule, and to consider it as the denial of a rule is an artificial conception, only admissible because, when statistical or proportional propositions are considered as rules, the denial of a rule is itself a rule. So, an hypothesis is really a subsumption of a case under a class and not the denial of it, except for this, that to deny a subsumption under one class is to admit a subsumption under another.

Bocardo may be considered as an induction, so timid as to lose its amplifiative character entirely. Enoch and Elijah are specimens of a certain kind of men. All that kind of men are shown by these instances to be immortal. But instead of boldly concluding that all very pious men, or all men favorites of the Almighty, etc., are immortal, we refrain from specifying the description of men, and rest in the merely explicative inference that some men are immortal. So *Baroco* might be considered as a very timid hypothesis. Enoch and Elijah are not mortal. Now, we might boldly suppose them to be gods or something of that sort, but instead of that we limit ourselves to the inference that they are of some nature different from that of man.

But, after all, there is an immense difference between the relation of *Baroco* and *Bocardo* to *Barbara* and that of Induction and Hypothesis to Deduction. *Baroco* and *Bocardo* are based upon the fact that if the truth of a conclusion necessarily follows from the truth of a premise, then the falsity of the premise follows from the falsity of the conclusion. This is always true. It is different when the inference is only probable. It by no means follows that, because the truth of a certain premise would render the truth of a conclusion probable, therefore the falsity of the conclusion renders the falsity of the premise probable. At least, this is only true, as we have seen in a former paper, when the word probable is used in one sense in the antecedent and in another in the consequent.

III.

A certain anonymous writing is upon a torn piece of paper. It is suspected that the author is a certain person. His desk, to which only he has had access, is searched, and in it is found a piece of paper, the torn edge of which exactly fits, in all its irregularities, that of the paper in question. It is a fair hypothetic inference that the suspected man was actually the author. The ground of this inference evidently is that two torn pieces of paper are extremely unlikely to fit together by accident. Therefore, of a great number of inferences of this sort, but a very small proportion would be deceptive. The analogy of hypothesis with induction is so strong that some logicians have confounded them. Hypothesis has been called an induction of characters. A number of characters belonging to a certain class are found in a certain object; whence it is inferred that all the characters of that class belong to the object in question. This certainly involves the same principle as induction; yet in a modified form. In the first place, characters are not susceptible of simple enumeration like objects; in the next place, characters run in categories. When we make an hypothesis like that about the piece of paper, we only examine a single line of characters, or perhaps two or three, and we take no specimen at all of others. If the hypothesis were nothing but an induction, all that we should be justified in concluding, in the example above, would be that the two pieces of paper which matched in such irregularities as have been examined would be found to match in other, say slighter, irregularities. The inference from the shape of the paper to its ownership is precisely what distinguishes hypothesis from induction, and makes it a bolder and more perilous step.

The same warnings that have been given against imagining that induction rests upon the uniformity of Nature might be repeated in regard to hypothesis. Here, as there, such a theory not only utterly fails to account for the validity of the inference, but it also gives rise to methods of conducting it which are absolutely vicious. There are, no doubt, certain uniformities in Nature, the knowledge of which will fortify an hypothesis very much. For example, we suppose that iron, titanium, and other metals exist in the sun, because we find in the solar spectrum many lines coincident in position with those which these metals would produce; and this hypothesis is greatly strengthened by our knowledge of the remarkable distinctiveness of the particular line of characters observed. But such a fortification of hypothesis is of a deductive kind, and hypothesis may still be probable when such reinforcement is wanting.

There is no greater nor more frequent mistake in practical logic than to suppose that things which resemble one another strongly in some respects are any the more likely for that to be alike in others. That this is absolutely false, admits of rigid demonstration; but, inasmuch as the reasoning is somewhat severe and complicated (requiring, like all such reasoning, the use of A, B, C, etc., to set it forth), the reader would probably find it distasteful, and I omit it. An example, however, may illustrate the proposition: The comparative mythologists occupy themselves with finding points of resemblance between solar phenomena and the careers of the heroes of all sorts of traditional stories; and upon the basis of such resemblances they infer that these heroes are impersonations of the sun. If there be anything more in their reasonings, it has never been made clear to me. An ingenious logician, to show how futile all that is, wrote a little book, in which he pretended to prove, in the same manner, that Napoleon Bonaparte is only an impersonation of the sun. It was really wonderful to see how many points of resemblance he made out. The truth is, that any two things resemble one another just as strongly as any two others, if recondite resemblances are admitted. But, in order that the process of making an hypothesis should lead to a probable result, the following rules must be followed:

1. The hypothesis should be distinctly put as a question, before making the observations which are to test its truth. In other words, we must try to see what the result of predictions from the hypothesis will be.

2. The respect in regard to which the resemblances are noted must be taken at random. We must not take a particular kind of predictions for which the hypothesis is known to be good.

3. The failures as well as the successes of the predictions must be honestly noted. The whole proceeding must be fair and unbiased.

Some persons fancy that bias and counter-bias are favorable to the extraction of truth—that hot and partisan debate is the way to investigate. This is the theory of our atrocious legal procedure. But Logic puts its heel upon this suggestion. It irrefragably demonstrates that knowledge can only be furthered by the real desire for it, and that the methods of obstinacy, of authority, and every mode of trying to reach a foregone conclusion, are absolutely of no value. These things are proved. The reader is at liberty to think so or not as long as the proof is not set forth, or as long as he refrains from examining it. Just so, he can preserve, if he likes, his freedom of opinion in regard to the propositions of geometry; only, in that case, if he takes a fancy to read Euclid, he will do well to skip whatever he finds with A, B, C, etc., for, if he reads attentively that disagreeable matter, the freedom of his opinion about geometry may unhappily be lost forever.

How many people there are who are incapable of putting to their own consciences this question, "Do I want to know how the fact stands, or not?"

The rules which have thus far been laid down for induction and hypothesis are such as are absolutely essential. There are many other maxims expressing particular contrivances for making synthetic inferences strong, which are extremely valuable and should not be neglected. Such are, for example, Mr. Mill's four methods. Nevertheless, in the total neglect of these, inductions and hypotheses may and sometimes do attain the greatest force.

IV.

Classifications in all cases perfectly satisfactory hardly exist. Even in regard to the great distinction between explicative and ampliative inferences, examples could be found which seem to lie upon the border between the two classes, and to partake in some respects of the characters of either. The same thing is true of the distinction between induction and hypothesis. In the main, it is broad and decided. By induction, we conclude that facts, similar to observed facts, are true in cases not examined. By hypothesis, we conclude the existence of a fact quite different from anything observed, from which, according to known laws, something observed would necessarily result. The former, is reasoning from particulars to the general law; the latter, from effect to cause. The former classifies, the latter explains. It is only in some special cases that there can be more than a momentary doubt to which category a given inference belongs. One exception is where we observe, not facts similar under similar circumstances, but facts different under different circumstances the difference of the former having, however, a definite relation to the difference of the latter. Such inferences, which are really inductions, sometimes present nevertheless some indubitable resemblances to hypotheses.

Knowing that water expands by heat, we make a number of observations of the volume of a constant mass of water at different temperatures. The scrutiny of a few of these suggests a form of algebraical formula which will approximately express the relation of the volume to the temperature. It may be, for instance, that *v* being the relative volume, and *t* the temperature, the few observations examined indicate a relation of the form—

$$v = 1 + a\mathrm{t} + b\mathrm{t}^2 + c\mathrm{t}^3.$$

Upon examining observations at other temperatures taken at random, this idea is confirmed; and we draw the inductive conclusion that all observations within the limits of temperature from which we have drawn our observations could equally be so satisfied. Having once ascertained that such a formula is possible, it is a mere affair of arithmetic to find the values of *a*, *b*, and *c*, which will make the formula satisfy the observations best. This is what physicists call an *empirical formula*, because it rests upon mere induction, and is not explained by any hypothesis.

Such formulæ, though very useful as means of describing in general terms the results of observations, do not take any high rank among scientific discoveries. The induction which they embody, that expansion by heat (or whatever other phenomenon is referred to) takes place in a perfectly gradual manner without sudden leaps or innumerable fluctuations, although really important, attracts no attention, because it is what we naturally anticipate. But the defects of such expressions are very serious. In the first place, as long as the observations are subject to error, as all observations are, the formula cannot be expected to satisfy the observations exactly. But the discrepancies cannot be due solely to the errors of the observations, but must be partly owing to the error of the formula which has been deduced from erroneous observations. Moreover, we have no right to suppose that the real facts, if they could be had free from error, could be expressed by such a formula at all. They might, perhaps, be expressed by a similar formula with an infinite number of terms; but of what use would that be to us, since it would require an infinite number of coefficients to be written down? When one quantity varies with another, if the corresponding values are exactly known, it is a mere matter of mathematical ingenuity to find some way of expressing their relation in a simple manner. If one quantity is of one kind—say, a specific gravity—and the other of another kind—say, a temperature—we do not desire to find an expression for their relation which is wholly free from numerical constants, since if it were free from them when, say, specific gravity as compared with water, and temperature as expressed by the centigrade thermometer, were in question, numbers would have to be introduced when the scales of measurement were changed. We may, however, and do desire to find formulas expressing the relations of physical phenomena which shall contain no more arbitrary numbers than changes in the scales of measurement might require.

When a formula of this kind is discovered, it is no longer called an empirical formula, but a law of Nature; and is sooner or later made the basis of an hypothesis which is to explain it. These simple formulæ are not usually, if ever, exactly true, but they are none the less important for that; and the great triumph of the hypothesis comes when it explains not only the formula, but also the deviations from the formula. In the current language of the physicists, an hypothesis of this importance is called a theory, while the term hypothesis is restricted to suggestions which have little evidence in their favor. There is some justice in the contempt which clings to the word hypothesis. To think that we can strike out of our own minds a true preconception of how Nature acts, is a vain fancy. As Lord Bacon well says: "The subtlety of Nature far exceeds the subtlety of sense and intellect: so that these fine meditations, and speculations, and reasonings of men are a sort of insanity, only there is no one at hand to remark it." The successful theories are not pure guesses, but are guided by reasons.

The kinetical theory of gases is a good example of this. This theory is intended to explain certain simple formulæ, the chief of which is called the law of Boyle. It is, that if air or any other gas be placed in a cylinder with a piston, and if its volume be measured under the pressure of the atmosphere, say fifteen pounds on the square inch, and if then another fifteen pounds per square inch be placed on the piston, the gas will be compressed to one-half its bulk, and in similar inverse ratio for other pressures. The hypothesis which has been adopted to account for this law is that the molecules of a gas are small, solid particles at great distances from each other (relatively to their dimensions), and moving with great velocity, without sensible attractions or repulsions, until they happen to approach one another very closely. Admit this, and it follows that when a gas is under pressure what prevents it from collapsing is not the incompressibility of the separate molecules, which are under no pressure at all, since they do not touch, but the pounding of the molecules against the piston. The more the piston falls, and the more the gas is compressed, the nearer together the molecules will be; the greater number there will be at any moment within a given distance of the piston, the shorter the distance which any one will go before its course is changed by the influence of another, the greater number of new courses of each in a given time, and the oftener each, within a given distance of the piston, will strike it. This explains Boyle's law. The law is not exact; but the hypothesis does not lead us to it exactly. For, in the first place, if the molecules are large, they will strike each other oftener when their mean distances are diminished, and will consequently strike the piston oftener, and will produce more pressure upon it. On the other hand, if the molecules have an attraction for one another, they will remain for a sensible time within one another's influence, and consequently they will not strike the wall so often as they otherwise would, and the pressure will be less increased by compression.

When the kinetical theory of gases was first proposed by Daniel Bernoulli, in 1738, it rested only on the law of Boyle, and was therefore pure hypothesis. It was accordingly quite naturally and deservedly neglected. But, at present, the theory presents quite another aspect; for, not to speak of the considerable number of observed facts of different kinds with which it has been brought into relation, it is supported by the mechanical theory of heat. That bringing together bodies which attract one another, or separating bodies which repel one another, when sensible motion is not produced nor destroyed, is always accompanied by the evolution of heat, is little more than an induction. Now, it has been shown by experiment that, when a gas is allowed to expand without doing work, a very small amount of heat disappears. This proves that the particles of the gas attract one another slightly, and but very slightly. It follows that, when a gas is under pressure, what prevents it from collapsing is not any repulsion between the particles, since there is none. Now, there are only two modes of force known to us, force of position or attractions and repulsions, and force of motion. Since, therefore, it is not the force of position which gives a gas its expansive force, it must be the force of motion. In this point of view, the kinetical theory of gases appears as a deduction from the mechanical theory of heat. It is to be observed, however, that it supposes the same law of mechanics (that there are only those two modes of force) which holds in regard to bodies such as we can see and examine, to hold also for what are very different, the molecules of bodies. Such a supposition has but a slender support from induction. Our belief in it is greatly strengthened by its connection with the law of Boyle, and it is, therefore, to be considered as an hypothetical inference. Yet it must be admitted that the kinetical theory of gases would deserve little credence if it had not been connected with the principles of mechanics.

The great difference between induction and hypothesis is, that the former infers the existence of phenomena such as we have observed in cases which are similar, while hypothesis supposes something of a different kind from what we have directly observed, and frequently something which it would be impossible for us to observe directly. Accordingly, when we stretch an induction quite beyond the limits of our observation, the inference partakes of the nature of hypothesis. It would be absurd to say that we have no inductive warrant for a generalization extending a little beyond the limits of experience, and there is no line to be drawn beyond which we cannot push our inference; only it becomes weaker the further it is pushed. Yet, if an induction be pushed very far, we cannot give it much credence unless we find that such an extension explains some fact which we can and do observe. Here, then, we have a kind of mixture of induction and hypothesis supporting one another; and of this kind are most of the theories of physics.

V.

That synthetic inferences may be divided into induction and hypothesis in the manner here proposed,[1] admits of no question. The utility and value of the distinction are to be tested by their applications.

Induction is, plainly, a much stronger kind of inference than hypothesis; and this is the first reason for distinguishing between them. Hypotheses are sometimes regarded as provisional resorts, which in the progress of science are to be replaced by inductions. But this is a false view of the subject. Hypothetic reasoning infers very frequently a fact not capable of direct observation. It is an hypothesis that Napoleon Bonaparte once existed. How is that hypothesis ever to be replaced by an induction? It may be said that from the premise that such facts as we have observed are as they would be if Napoleon existed, we are to infer by induction that all facts that are hereafter to be observed will be of the same character. There is no doubt that every hypothetic inference may be distorted into the appearance of an induction in this way. But the essence of an induction is that it infers from one set of facts another set of similar facts, whereas hypothesis infers from facts of one kind to facts of another. Now, the facts which serve as grounds for our belief in the historic reality of Napoleon are not by any means necessarily the only kind of facts which are explained by his existence. It may be that, at the time of his career, events were being recorded in some way not now dreamed of, that some ingenious creature on a neighboring planet was photographing the earth, and that these pictures on a sufficiently large scale may some time come into our posession, or that some mirror upon a distant star will, when the light reaches it, reflect the whole story back to earth. Never mind how improbable these suppositions are; everything which happens is infinitely improbable. I am not saying that *these* things are likely to occur, but that *some* effect of Napoleon's existence which now seems impossible is certain nevertheless to be brought about. The hypothesis asserts that such facts, when they do occur, will be of a nature to confirm, and not to refute, the existence of the man. We have, in the impossibility of inductively inferring hypothetical conclusions, a second reason for distinguishing between the two kinds of inference.

A third merit of the distinction is, that it is associated with an important psychological or rather physiological difference in the mode of apprehending facts. Induction infers a rule. Now, the belief of a rule is a habit. That a habit is a rule active in us, is evident. That every belief is of the nature of a habit, in so far as it is of a general character, has been shown in the earlier papers of this series. Induction, therefore, is the logical formula which expresses the physiological process of formation of a habit. Hypothesis substitutes, for a complicated tangle of predicates attached to one subject, a single conception. Now, there is a peculiar sensation belonging to the act of thinking that each of these predicates inheres in the subject. In hypothetic inference this complicated feeling so produced is replaced by a single feeling of greater intensity, that belonging to the act of thinking the hypothetic conclusion. Now, when our nervous system is excited in a complicated way, there being a relation between the elements of the excitation, the result is a single harmonious disturbance which I call an emotion. Thus, the various sounds made by the instruments of an orchestra strike upon the ear, and the result is a peculiar musical emotion, quite distinct from the sounds themselves. This emotion is essentially the same thing as an hypothetic inference, and every hypothetic inference involves the formation of such an emotion. We may say, therefore, that hypothesis produces the *sensuous* element of thought, and induction the *habitual* element. As for deduction, which adds nothing to the premises, but only out of the various facts represented in the premises selects one and brings the attention down to it, this may be considered as the logical formula for paying attention, which is the *volitional* element of thought, and corresponds to nervous discharge in the sphere of physiology.

Another merit of the distinction between induction and hypothesis is, that it leads to a very natural classification of the sciences and of the minds which prosecute them. What must separate different kinds of scientific men more than anything else are the differences of their *techniques*. We cannot expect men who work with books chiefly to have much in common with men whose lives are passed in laboratories. But, after differences of this kind, the next most important are differences in the modes of reasoning. Of the natural sciences, we have, first, the classificatory sciences, which are purely inductive—systematic botany and zoölogy, mineralogy, and chemistry. Then, we have the sciences of theory, as above explained—astronomy, pure physics, etc. Then, we have sciences of hypothesis—geology, biology, etc.

There are many other advantages of the distinction in question which I shall leave the reader to find out by experience. If he will only take the custom of considering whether a given inference belongs to one or other of the two forms of synthetic inference given on page 472, I can promise him that he will find his advantage in it, in various ways.

1. This division was first made in a course of lectures by the author before the Lowell Institute, Boston, in 1866, and was printed in the "Proceedings of the American Academy of Arts and Sciences," for April 9, 1867.