Charles S. Peirce

From the Harvard Class Album of 1859

COLLECTED PAPERS OF CHARLES SANDERS PEIRCE

EDITED BY

CHARLES HARTSHORNE

AND

PAUL WEISS

VOLUME II

ELEMENTS OF LOGIC

CAMBRIDGE
HARVARD UNIVERSITY PRESS
1932

B. AMPLIATIVE REASONING

CHAPTER 5

*DEDUCTION, INDUCTION, AND HYPOTHESIS**

§1. RULE, CASE, AND RESULT[E]

619. 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.

620. 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

* *Popular Science Monthly*, vol. 13, pp. 470–82 (1878); intended as Essay XIII of the *Search for a Method* (1893). It is the sixth and last of a series of papers on the "Illustrations of the Logic of Science," which appeared in the *Popular Science Monthly*. For the first and second papers, see vol. 5, bk. II, chs. 4 and 5; the third and fourth constitute chapters 6 and 7 of the present book; for the fifth paper, see vol. 6, bk. II, ch. 1.

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 premiss lays down this rule; as, for example, *All men are mortal.* The other or minor premiss 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.

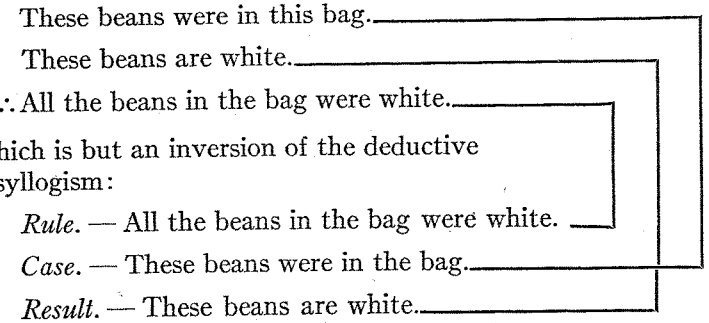
Inductive or synthetic reasoning, being something more than the mere application of a general rule to a particular case, can never be reduced to this form.

621. 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 $\frac{2}{3}$ 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.

622. If instead of drawing one bean we draw a handful at random and conclude that about $\frac{2}{3}$ 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:



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*.

623. 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 a *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:

Inference.

Deductive or Analytic. Synthetic.

Induction. Hypothesis.

624. 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.

625. 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 cannot 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.

§2. BAROCO AND BOCARDO; HYPOTHESIS AND INDUCTION[E]

626. 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 premiss the truth of a certain conclusion would necessarily follow, then from the falsity of the conclusion the falsity of the premiss 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.

627. *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.

628. 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.

629. 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.

630. *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.

631. 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 premiss, then the falsity of the premiss 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 premiss would render the truth of a conclusion probable, therefore the falsity of the conclusion renders the falsity of the premiss 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.

§3. RULES FOR INDUCTION AND HYPOTHESES[E]

632. 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

* See, e.g., 515.

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.

633. 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 reënforcement is wanting.

634. 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 prob-

ably 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.

635. 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

* See vol. 5, bk. II, ch. 4, §5.

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.

§4. EMPIRICAL FORMULÆ AND THEORIES[E]

636. 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.

637. 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, a few observations examined indicate a relation of the form —

$$v = 1 + at + bt^2 + ct^3.$$

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 formulæ expressing the relations of physical phenomena which shall contain no more arbitrary numbers than changes in the scales of measurement might require.

638. 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.

639. 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

* *Novum Organum*, bk. I, Aphorism X.

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 or destroyed,

* In his *Hydrodynamica*.

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.

640. 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 sup-

* Cf. 511n.

porting one another; and of this kind are most of the theories of physics.

§5. ON THE DIFFERENCE BETWEEN INDUCTION AND HYPOTHESIS[E]

641. 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.

642. 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 premiss 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 possession, 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; every-

[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. [See 508–12.]

thing 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.

643. 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 premisses, but only out of the various facts represented in the premisses 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

* See, e.g., the first paper, vol. 5, bk. II, ch. 4.

of thought, and corresponds to nervous discharge in the sphere of physiology.*

644. 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 in 623, I can promise him that he will find his advantage in it, in various ways.

* Cf. 712.

† Cf. vol. 1, bk. II, ch. 2.

CHAPTER 6

*THE DOCTRINE OF CHANCES**

§1. CONTINUITY AND THE FORMATION OF CONCEPTS[E]

645. It is a common observation that a science first begins to be exact when it is quantitatively treated. What are called the exact sciences are no others than the mathematical ones. Chemists reasoned vaguely until Lavoisier showed them how to apply the balance to the verification of their theories, when chemistry leaped suddenly into the position of the most perfect of the classificatory sciences. It has thus become so precise and certain that we usually think of it along with optics, thermotics, and electrics. But these are studies of general laws, while chemistry considers merely the relations and classification of certain objects; and belongs, in reality, in the same category as systematic botany and zoölogy. Compare it with these last, however, and the advantage that it derives from its quantitative treatment is very evident.[1]

646. The rudest numerical scales, such as that by which the mineralogists distinguish the different degrees of hardness, are found useful. The mere counting of pistils and stamens sufficed to bring botany out of total chaos into some kind of form. It is not, however, so much from *counting* as from *measuring*, not so much from the conception of number as from

* *Popular Science Monthly*, vol. 12, pp. 604–15 (1878) with corrections of 1893 and a note of 1910; intended as ch. 18 of the *Grand Logic* (1893), and as Essay X of the *Search for a Method* (1893), the third of a series of papers on "Illustrations of the Logic of Science." See notes to ch. 5 and 6.410.

[1] This characterization of chemistry now sounds antiquated indeed; and yet it was justified by the general state of mind of chemists at that day, as is shown by the fact that only a few months before, van't Hoff had put forth a statement of the law of mass-action as something absolutely new to science. I am satisfied by considerable search after pertinent facts that no distinction between different allied sciences can represent any truth of fact other than a difference between what habitually passes in the minds, and moves the investigations of the two general bodies of the cultivators of those sciences at the time to which the distinction refers. — 1910.

that of continuous quantity, that the advantage of mathematical treatment comes. Number, after all, only serves to pin us down to a precision in our thoughts which, however beneficial, can seldom lead to lofty conceptions, and frequently descends to pettiness. Of those two faculties of which Bacon speaks,* that which marks differences and that which notes resemblances, the employment of number can only aid the lesser one; and the excessive use of it must tend to narrow the powers of the mind. But the conception of continuous quantity has a great office to fulfill, independently of any attempt at precision. Far from tending to the exaggeration of differences, it is the direct instrument of the finest generalizations. When a naturalist wishes to study a species, he collects a considerable number of specimens more or less similar. In contemplating them, he observes certain ones which are more or less alike in some particular respect. They all have, for instance, a certain S-shaped marking. He observes that they are not *precisely* alike, in this respect; the S has not precisely the same shape, but the differences are such as to lead him to believe that forms could be found intermediate between any two of those he possesses. He, now, finds other forms apparently quite dissimilar — say a marking in the form of a C — and the question is, whether he can find intermediate ones which will connect these latter with the others. This he often succeeds in doing in cases where it would at first be thought impossible; whereas, he sometimes finds those which differ, at first glance, much less, to be separated in Nature by the non-occurrence of intermediaries. In this way, he builds up from the study of Nature a new general conception of the character in question. He obtains, for example, an idea of a leaf which includes every part of the flower, and an idea of a vertebra which includes the skull. I surely need not say much to show what a logical engine is here. It is the essence of the method of the naturalist. How he applies it first to one character, and then to another, and finally obtains a notion of a species of animals, the differences between whose members, however great, are confined within limits, is a matter which does not here concern us. The whole method of classification must be considered later; but, at present, I only desire to point out that it is by taking advan-

* *Novum Organum*, bk. II, Aphorism XXVII.

tage of the idea of continuity, or the passage from one form to another by insensible degrees,[1] that the naturalist builds his conceptions. Now, the naturalists are the great builders of conceptions; there is no other branch of science where so much of this work is done as in theirs; and we must, in great measure, take them for our teachers in this important part of logic. And it will be found everywhere that the idea of continuity[2] is a powerful aid to the formation of true and fruitful conceptions. By means of it, the greatest differences are broken down and resolved into differences of degree, and the incessant application of it is of the greatest value in broadening our conceptions. I propose to make a great use of this idea[3] in the present series of papers; and the particular series of important fallacies, which, arising from a neglect of it,[4] have desolated philosophy, must further on be closely studied. At present, I simply call the reader's attention to the utility of this conception.

In studies of numbers, the idea of continuity is so indispensable, that it is perpetually introduced even where there is no continuity in fact, as where we say that there are in the United States 10.7 inhabitants per square mile, or that in New York 14.72 persons live in the average house.[5] Another example is that law of the distribution of errors which Quetelet, Galton, and others, have applied with so much success to the study of biological and social matters. This application of continuity to cases where it does not really exist illustrates, also, another point which will hereafter demand a separate study, namely, the great utility which fictions sometimes have in science.*

[1] "Or rather of an idea that continuity suggests — that of limitless intermediation; i.e., of a series between every two members of which there is another member of it"— to be substituted for the phrase "or . . . degrees."— 1893.

[2] For "continuity" substitute "limitless intermediation, the business of reasoning."— 1893.

[3] "And others that are involved in that of continuity."— 1893.

[4] For "neglect of" substitute "want of close study of these concepts."— 1893.

[5] This mode of thought is so familiarly associated with all exact numerical consideration, that the phrase appropriate to it is imitated by shallow writers in order to produce the appearance of exactitude where none exists. Certain newspapers, which affect a learned tone, talk of "the average man," when they simply mean *most men*, and have no idea of striking an average.

* See, e.g., 1.383.

§2. THE PROBLEM OF PROBABILITY[E]

647. The theory of probabilities is simply the science of logic quantitatively treated. There are two conceivable certainties with reference to any hypothesis, the certainty of its truth and the certainty of its falsity. The numbers *one* and *zero* are appropriated, in this calculus, to marking these extremes of knowledge; while fractions having values intermediate between them indicate, as we may vaguely say, the degrees in which the evidence leans toward one or the other. The general problem of probabilities is, from a given state of facts, to determine the numerical probability of a possible fact. This is the same as to inquire how much the given facts are worth, considered as evidence to prove the possible fact. Thus the problem of probabilities is simply the general problem of logic.

648. Probability is a continuous quantity, so that great advantages may be expected from this mode of studying logic. Some writers have gone so far as to maintain that, by means of the calculus of chances, every solid inference may be represented by legitimate arithmetical operations upon the numbers given in the premisses. If this be, indeed, true, the great problem of logic, how it is that the observation of one fact can give us knowledge of another independent fact, is reduced to a mere question of arithmetic. It seems proper to examine this pretension before undertaking any more recondite solution of the paradox.

But, unfortunately, writers on probabilities are not agreed in regard to this result. This branch of mathematics is the only one, I believe, in which good writers frequently get results entirely erroneous. In elementary geometry the reasoning is frequently fallacious, but erroneous conclusions are avoided; but it may be doubted if there is a single extensive treatise on probabilities in existence which does not contain solutions absolutely indefensible. This is partly owing to the want of any regular method of procedure; for the subject involves too many subtilities to make it easy to put its problems into equations without such an aid. But, beyond this, the fundamental principles of its calculus are more or less in dispute. In regard to that class of questions to which it is chiefly applied for practical purposes, there is comparatively little doubt; but in

regard to others to which it has been sought to extend it, opinion is somewhat unsettled.

This last class of difficulties can only be entirely overcome by making the idea of probability perfectly clear in our minds in the way set forth in our last paper.*

§3. ON DEGREES OF PROBABILITY[E]

649. To get a clear idea of what we mean by probability, we have to consider what real and sensible difference there is between one degree of probability and another.

The character of probability belongs primarily, without doubt, to certain inferences. Locke† explains it as follows: After remarking that the mathematician positively knows that the sum of the three angles of a triangle is equal to two right angles because he apprehends the geometrical proof, he thus continues: "But another man who never took the pains to observe the demonstration, hearing a mathematician, a man of credit, affirm the three angles of a triangle to be equal to two right ones, *assents* to it; *i.e.*, receives it for true. In which case the foundation of his assent is the probability of the thing, the proof being such as, for the most part, carries truth with it; the man on whose testimony he receives it not being wont to affirm anything contrary to, or besides his knowledge, especially in matters of this kind." The celebrated *Essay Concerning Humane Understanding* contains many passages which, like this one, make the first steps in profound analyses which are not further developed. It was shown‡ in the first of these papers that the validity of an inference does not depend on any tendency of the mind to accept it, however strong such tendency may be; but consists in the real fact that, when premisses like those of the argument in question are true, conclusions related to them like that of this argument are also true. It was remarked that in a logical mind an argument is always conceived as a member of a *genus* of arguments all constructed in the same way, and such that, when their premisses are real facts, their conclusions are so also. If the argument is demonstrative, then this is always so; if it is only

* See vol. 5, bk. II, ch. 5.

† *Essay*, bk. IV, ch. 15, §1.

‡ See vol. 5, bk. II, ch. 4, §2.

probable, then it is for the most part so. As Locke says, the probable argument is "*such as* for the most part carries truth with it."

650. According to this, that real and sensible difference between one degree of probability and another, in which the meaning of the distinction lies, is that in the frequent employment of two different modes of inference, one will carry truth with it oftener than the other. It is evident that this is the only difference there is in the existing fact. Having certain premisses, a man draws a certain conclusion, and as far as this inference alone is concerned the only possible practical question is whether that conclusion is true or not, and between existence and non-existence there is no middle term. "Being only is and nothing is altogether not," said Parmenides; and this is in strict accordance with the analysis of the conception of reality given in the last paper.* For we found that the distinction of reality and fiction depends on the supposition that sufficient investigation would cause one opinion to be universally received and all others to be rejected. That presupposition, involved in the very conceptions of reality and figment, involves a complete sundering of the two. It is the heaven-and-hell idea in the domain of thought. But, in the long run, there is a real fact which corresponds to the idea of probability, and it is that a given mode of inference sometimes proves successful and sometimes not, and that in a ratio ultimately fixed. As we go on drawing inference after inference of the given kind, during the first ten or hundred cases the ratio of successes may be expected to show considerable fluctuations; but when we come into the thousands and millions, these fluctuations become less and less; and if we continue long enough, the ratio will approximate toward a fixed limit. We may, therefore, define the probability of a mode of argument as the proportion of cases in which it carries truth with it.

651. The inference from the premiss, A, to the conclusion, B, depends, as we have seen, on the guiding principle, that if a fact of the class A is true, a fact of the class B is true. The probability consists of the fraction whose numerator is the number of times in which both A and B are true, and whose denominator is the total number of times in which A is true,

* See vol. 5, bk. II, ch. 5, §4.

whether B is so or not. Instead of speaking of this as the probability of the inference, there is not the slightest objection to calling it the probability that, if A happens, B happens. But to speak of the probability of the event B, without naming the condition, really has no meaning at all. It is true that when it is perfectly obvious what condition is meant, the ellipsis may be permitted. But we should avoid contracting the habit of using language in this way (universal as the habit is), because it gives rise to a vague way of thinking, as if the action of causation might either determine an event to happen or determine it not to happen, or leave it more or less free to happen or not, so as to give rise to an *inherent* chance in regard to its occurrence. It is quite clear to me that some of the worst and most persistent errors in the use of the doctrine of chances have arisen from this vicious mode of expression.[1]

§4. THREE LOGICAL SENTIMENTS[E]

652. But there remains an important point to be cleared up. According to what has been said, the idea of probability essentially belongs to a kind of inference which is repeated indefinitely. An individual inference must be either true or false, and can show no effect of probability; and, therefore, in reference to a single case considered in itself, probability can have no meaning. Yet if a man had to choose between drawing a card from a pack containing twenty-five red cards and a black one, or from a pack containing twenty-five black cards and a red one, and if the drawing of a red card were destined to transport him to eternal felicity, and that of a black one to consign him to everlasting woe, it would be folly to deny that he ought to prefer the pack containing the larger proportion of red cards, although, from the nature of the risk, it could not be repeated. It is not easy to reconcile this with our analysis of the conception of chance. But suppose he should choose the red pack, and should draw the wrong card, what consolation would he have? He might say that he had acted in accordance with reason, but that would only show that his reason was

[1] The conception of probability here set forth is substantially that first developed by Mr. Venn, in his *Logic of Chance*. Of course, a vague apprehension of the idea had always existed, but the problem was to make it perfectly clear, and to him belongs the credit of first doing this.

absolutely worthless. And if he should choose the right card, how could he regard it as anything but a happy accident? He could not say that if he had drawn from the other pack, he might have drawn the wrong one, because an hypothetical proposition such as, "if A, then B," means nothing with reference to a single case. Truth consists in the existence of a real fact corresponding to the true proposition. Corresponding to the proposition, "if A, then B," there may be the fact that *whenever* such an event as A happens such an event as B happens. But in the case supposed, which has no parallel as far as this man is concerned, there would be no real fact whose existence could give any truth to the statement that, if he had drawn from the other pack, he might have drawn a black card. Indeed, since the validity of an inference consists in the truth of the hypothetical proposition that *if* the premisses be true the conclusion will also be true, and since the only real fact which can correspond to such a proposition is that whenever the antecedent is true the consequent is so also, it follows that there can be no sense in reasoning in an isolated case, at all.

653. These considerations appear, at first sight, to dispose of the difficulty mentioned. Yet the case of the other side is not yet exhausted. Although probability will probably manifest its effect in, say, a thousand risks, by a certain proportion between the numbers of successes and failures, yet this, as we have seen, is only to say that it certainly will, at length, do so. Now the number of risks, the number of probable inferences, which a man draws in his whole life, is a finite one, and he cannot be absolutely *certain* that the mean result will accord with the probabilities at all. Taking all his risks collectively, then, it cannot be certain that they will not fail, and his case does not differ, except in degree, from the one last supposed. It is an indubitable result of the theory of probabilities that every gambler, if he continues long enough, must ultimately be ruined. Suppose he tries the martingale, which some believe infallible, and which is, as I am informed, disallowed in the gambling-houses. In this method of playing, he first bets say \$1; if he loses it he bets \$2; if he loses that he bets \$4; if he loses that he bets \$8; if he then gains he has lost $1+2+4=7$, and he has gained \$1 more; and no matter how many bets he loses, the first one he gains will make him \$1 richer than he

was in the beginning. In that way, he will probably gain at first; but, at last, the time will come when the run of luck is so against him that he will not have money enough to double, and must, therefore, let his bet go. This will *probably* happen before he has won as much as he had in the first place, so that this run against him will leave him poorer than he began; some time or other it will be sure to happen. It is true that there is always a possibility of his winning any sum the bank can pay, and we thus come upon a celebrated paradox that, though he is certain to be ruined, the value of his expectation calculated according to the usual rules (which omit this consideration) is large. But, whether a gambler plays in this way or any other, the same thing is true, namely, that if [he] plays long enough he will be sure some time to have such a run against him as to exhaust his entire fortune. The same thing is true of an insurance company. Let the directors take the utmost pains to be independent of great conflagrations and pestilences, their actuaries can tell them that, according to the doctrine of chances, the time must come, at last, when their losses will bring them to a stop. They may tide over such a crisis by extraordinary means, but then they will start again in a weakened state, and the same thing will happen again all the sooner. An actuary might be inclined to deny this, because he knows that the expectation of his company is large, or perhaps (neglecting the interest upon money) is infinite. But calculations of expectations leave out of account the circumstance now under consideration, which reverses the whole thing. However, I must not be understood as saying that insurance is on this account unsound, more than other kinds of business. All human affairs rest upon probabilities, and the same thing is true everywhere. If man were immortal he could be perfectly sure of seeing the day when everything in which he had trusted should betray his trust, and, in short, of coming eventually to hopeless misery. He would break down, at last, as every great fortune, as every dynasty, as every civilization does. In place of this we have death.

654. But what, without death, would happen to every man, with death must happen to some man. At the same time, death makes the number of our risks, of our inferences, finite, and so makes their mean result uncertain. The very idea of

probability and of reasoning rests on the assumption that this number is indefinitely great. We are thus landed in the same difficulty as before, and I can see but one solution of it. It seems to me that we are driven to this, that logicality inexorably requires that our interests shall *not* be limited. They must not stop at our own fate, but must embrace the whole community. This community, again, must not be limited, but must extend to all races of beings with whom we can come into immediate or mediate intellectual relation. It must reach, however vaguely, beyond this geological epoch, beyond all bounds. He who would not sacrifice his own soul to save the whole world, is, as it seems to me, illogical in all his inferences, collectively. Logic is rooted in the social principle.

To be logical men should not be selfish; and, in point of fact, they are not so selfish as they are thought. The willful prosecution of one's desires is a different thing from selfishness. The miser is not selfish; his money does him no good, and he cares for what shall become of it after his death. We are constantly speaking of *our* possessions on the Pacific, and of *our* destiny as a republic, where no personal interests are involved, in a way which shows that we have wider ones. We discuss with anxiety the possible exhaustion of coal in some hundreds of years, or the cooling-off of the sun in some millions, and show in the most popular of all religious tenets that we can conceive the possibility of a man's descending into hell for the salvation of his fellows.

Now, it is not necessary for logicality that a m͡an should himself be capable of the heroism of self-sacrifice. It is sufficient that he should recognize the possibility of it, should perceive that only that man's inferences who has it are really logical, and should consequently regard his own as being only so far valid as they would be accepted by the hero. So far as he thus refers his inferences to that standard, he becomes identified with such a mind.

This makes logicality attainable enough. Sometimes we can personally attain to heroism. The soldier who runs to scale a wall knows that he will probably be shot, but that is not all he cares for. He also knows that if all the regiment, with whom in feeling he identifies himself, rush forward at once, the fort will be taken. In other cases we can only imitate the virtue.

The man whom we have supposed as having to draw from the two packs, who if he is not a logician will draw from the red pack from mere habit, will see, if he is logician enough, that he cannot be logical so long as he is concerned only with his own fate, but that that man who should care equally for what was to happen in all possible cases of the sort could act logically, and would draw from the pack with the most red cards, and thus, though incapable himself of such sublimity, our logician would imitate the effect of that man's courage in order to share his logicality.

But all this requires a conceived identification of one's interests with those of an unlimited community. Now, there exist no reasons, and a later discussion will show that there can be no reasons, for thinking that the human race, or any intellectual race, will exist forever. On the other hand, there can be no reason against it;[1] and, fortunately, as the whole requirement is that we should have certain sentiments, there is nothing in the facts to forbid our having a *hope*, or calm and cheerful wish, that the community may last beyond any assignable date.

655. It may seem strange that I should put forward three sentiments, namely, interest in an indefinite community, recognition of the possibility of this interest being made supreme, and hope in the unlimited continuance of intellectual activity, as indispensable requirements of logic. Yet, when we consider that logic depends on a mere struggle to escape doubt, which, as it terminates in action, must begin in emotion, and that, furthermore, the only cause of our planting ourselves on reason is that other methods of escaping doubt fail on account of the social impulse, why should we wonder to find social sentiment presupposed in reasoning? As for the other two sentiments which I find necessary, they are so only as supports and accessories of that. It interests me to notice that these three sentiments seem to be pretty much the same as that famous trio of Charity, Faith, and Hope, which, in the estimation of St. Paul, are the finest and greatest of spiritual gifts. Neither

[1] I do not here admit an absolutely unknowable. Evidence could show us what would probably be the case after any given lapse of time; and though a subsequent time might be assigned which that evidence might not cover, yet further evidence would cover it.

Old nor New Testament is a textbook of the logic of science, but the latter is certainly the highest existing authority in regard to the dispositions of heart which a man ought to have.

§5. FUNDAMENTAL RULES FOR THE CALCULATION OF CHANCES[E]

656. Such average statistical numbers as the number of inhabitants per square mile, the average number of deaths per week, the number of convictions per indictment, or, generally speaking, the numbers of x's per y, where the x's are a class of things some or all of which are connected with another class of things, their y's, I term *relative numbers*. Of the two classes of things to which a relative number refers, that one of which it is a number may be called its *relate*, and that one *per* which the numeration is made may be called its *correlate*.

657. Probability is a kind of relative number; namely, it is the ratio of the number of arguments of a certain genus which carry truth with them to the total number of arguments of that genus, and the rules for the calculation of probabilities are very easily derived from this consideration. They may all be given here, since they are extremely simple, and it is sometimes convenient to know something of the elementary rules of calculation of chances.

658. Rule I. *Direct Calculation.* — To calculate, directly, any relative number, say for instance the number of passengers in the average trip of a street-car, we must proceed as follows:

Count the number of passengers for each trip; add all these numbers, and divide by the number of trips. There are cases in which this rule may be simplified. Suppose we wish to know the number of inhabitants to a dwelling in New York. The same person cannot inhabit two dwellings. If he divide his time between two dwellings he ought to be counted a half-inhabitant of each. In this case we have only to divide the total number of the inhabitants of New York by the number of their dwellings, without the necessity of counting separately those which inhabit each one. A similar proceeding will apply wherever each individual relate belongs to one individual correlate exclusively. If we want the number of x's per y, and no x belongs to more than one y, we have only to divide the whole

number of x's of y's by the number of y's. Such a method would, of course, fail if applied to finding the average number of street-car passengers per trip. We could not divide the total number of travelers by the number of trips, since many of them would have made many passages.

To find the probability that from a given class of premisses, A, a given class of conclusions, B, follows, it is simply necessary to ascertain what proportion of the times in which premisses of that class are true, the appropriate conclusions are also true. In other words, it is the number of cases of the occurrence of both the events A and B, divided by the total number of cases of the occurrence of the event A.

659. Rule II. *Addition of Relative Numbers.* — Given two relative numbers having the same correlate, say the number of x's per y, and the number of z's per y, it is required to find the number of x's and z's together per y. If there is nothing which is at once an x and a z to the same y, the sum of the two given numbers would give the required number. Suppose, for example, that we had given the average number of friends that men have, and the average number of enemies, the sum of these two is the average number of persons interested in a man. On the other hand, it plainly would not do to add the average number of persons having constitutional diseases to the average number over military age, and to the average number exempted by each special cause from military service, in order to get the average number exempt in any way, since many are exempt in two or more ways at once.

This rule applies directly to probabilities, given the probability that two different and mutually exclusive events will happen under the same supposed set of circumstances. Given, for instance, the probability that if A then B, and also the probability that if A then C, then the sum of these two probabilities is the probability that if A then either B or C, so long as there is no event which belongs at once to the two classes B and C.

660. Rule III. *Multiplication of Relative Numbers.* — Suppose that we have given the relative number of x's per y; also the relative number of z's per x of y; or, to take a concrete example, suppose that we have given, first, the average number of children in families living in New York; and, second, the

average number of teeth in the head of a New York child — then the product of these two numbers would give the average number of children's teeth in a New York family. But this mode of reckoning will only apply in general under two restrictions. In the first place, it would not be true if the same child could belong to different families, for in that case those children who belonged to several different families might have an exceptionally large or small number of teeth, which would affect the average number of children's teeth in a family more than it would affect the average number of teeth in a child's head. In the second place, the rule would not be true if different children could share the same teeth, the average number of children's teeth being in that case evidently something different from the average number of teeth belonging to a child.

In order to apply this rule to probabilities, we must proceed as follows: Suppose that we have given the probability that the conclusion B follows from the premiss A, B and A representing as usual certain classes of propositions. Suppose that we also knew the probability of an inference in which B should be the premiss, and a proposition of a third kind, C, the conclusion. Here, then, we have the materials for the application of this rule. We have, first, the relative number of B's per A. We next should have the relative number of C's per B following from A. But the classes of propositions being so selected that the probability of C following from any B in general is just the same as the probability of C's following from one of those B's which is deducible from an A, the two probabilities may be multiplied together, in order to give the probability of C following from A. The same restrictions exist as before. It might happen that the probability that B follows from A was affected by certain propositions of the class B following from several different propositions of the class A. But, practically speaking, all these restrictions are of very little consequence, and it is usually recognized as a principle universally true that the probability that, if A is true, B is, multiplied by the probability that, if B is true, C is, gives the probability that, if A is true, C is.

There is a rule supplementary to this, of which great use is made. It is not universally valid, and the greatest caution has to be exercised in making use of it — a double care, first,

never to use it when it will involve serious error; and, second, never to fail to take advantage of it in cases in which it can be employed. This rule depends upon the fact that in very many cases the probability that C is true if B is, is substantially the same as the probability that C is true if A is. Suppose, for example, we have the average number of males among the children born in New York; suppose that we also have the average number of children born in the winter months among those born in New York. Now, we may assume without doubt, at least as a closely approximate proposition (and no very nice calculation would be in place in regard to probabilities), that the proportion of males among all the children born in New York is the same as the proportion of males born in summer in New York; and, therefore, if the names of all the children born during a year were put into an urn, we might multiply the probability that any name drawn would be the name of a male child by the probability that it would be the name of a child born in summer, in order to obtain the probability that it would be the name of a male child born in summer. The questions of probability, in the treatises upon the subject, have usually been such as relate to balls drawn from urns, and games of cards, and so on, in which the question of the *independence* of events, as it is called — that is to say, the question of whether the probability of C, under the hypothesis B, is the same as its probability under the hypothesis A — has been very simple; but, in the application of probabilities to the ordinary questions of life, it is often an exceedingly nice question whether two events may be considered as independent with sufficient accuracy or not. In all calculations about cards it is assumed that the cards are thoroughly shuffled, which makes one deal quite independent of another. In point of fact the cards seldom are, in practice, shuffled sufficiently to make this true; thus, in a game of whist, in which the cards have fallen in sets of four of the same suit, and are so gathered up, they will lie more or less in sets of four of the same suit, and this will be true even after they are shuffled. At least some traces of this arrangement will remain, in consequence of which the number of "short suits," as they are called — that is to say, the number of hands in which the cards are very unequally divided in regard to suits — is smaller than the calculation

would make it to be; so that, when there is a misdeal, where the cards, being thrown about the table, get very thoroughly shuffled, it is a common saying that in the hands next dealt out there are generally short suits. A few years ago a friend of mine, who plays whist a great deal, was so good as to count the number of spades dealt to him in 165 hands, in which the cards had been, if anything, shuffled better than usual. According to calculation, there should have been 85 of these hands in which my friend held either three or four spades, but in point of fact there were 94, showing the influence of imperfect shuffling.

According to the view here taken, these are the only fundamental rules for the calculation of chances. An additional one, derived from a different conception of probability, is given in some treatises, which if it be sound might be made the basis of a theory of reasoning. Being, as I believe it is, absolutely absurd, the consideration of it serves to bring us to the true theory; and it is for the sake of this discussion, which must be postponed to the next number,* that I have brought the doctrine of chances to the reader's attention at this early stage of our studies of the logic of science.

§6. NOTES ON THE DOCTRINE OF CHANCES†

661. On reperusing this article after the lapse of a full generation, it strikes me as making two points that were worth making. The better made of the two had been still better made ten years before in my three articles in the [*Journal of Speculative Philosophy*] Vol. 2.‡ This point is that no man can be logical whose supreme desire is the well-being of himself or of any other existing person or collection of persons. The other good point is that probability never properly refers immediately to a single event, but exclusively to the happening of a given kind of event on any occasion of a given kind. So far all is well. But when I come to define probability, I repeatedly say that it is the quotient of the *number* of occurrences of the event divided by the *number* of occurrences of the occasion. Now this is manifestly wrong, for probability relates to the

* Ch. 7.

† 1910.

‡ See vol. 5, bk. II, chs. 1, 2, 3, particularly 5.355.

future; and how can I say how many times a given die will be thrown in the future? To be sure I might, immediately after my throw, put the die in strong nitric acid, and dissolve it, but this suggestion only puts the preposterous character of the definition in a still stronger light. For it is plain that, if probability be the ratio of the occurrences of the specific event to the occurrences of the generic occasion, it is the ratio that there *would be* in the long run, and has nothing to do with any supposed cessation of the occasions. This long run can be nothing but an endlessly long run; and even if it be correct to speak of an infinite "number," yet $\frac{\infty}{\infty}$ (infinity divided by infinity) has certainly, *in itself*, no definite value.

But we have not yet come to the end of the flaws in the definition, since no notice whatever has been taken of two conditions which require the strictest precautions in all experiments to determine the probability of a specific event on a generic occasion. Namely, in the first place we must limit our endeavors strictly to counting occurrences of the right genus of occasion and carefully resist all other motives for counting them, and strive to take them just as they would ordinarily occur. In the next place, it must be known that the occurrence of the specific event on one occasion will have no tendency to produce or to prevent the occurrence of the same event upon any other of the occurrences of the generic occasion. In the third place, after the probability has been ascertained, we must remember that this probability cannot be relied upon at any future time unless we have adequate grounds for believing that it has not too much changed in the interval.

662. I will now give over jeering at my former inaccuracies, committed when I had been a student of logic for only about a quarter of a century, and was naturally not so well-versed in it as now, and will proceed to define probability. I must premiss that we, all of us, use this word with a degree of laxity which corrupts and rots our reasoning to a degree that very few of us are at all awake to. When I say our "reasoning," I mean not formal reasonings only but our thoughts in general, so far as they are concerned with any of those approaches toward knowledge which we confound with probability. The result is that we not only fall into the falsest ways of thinking, but, what is often still worse, we give up sundry

problems as beyond our powers — problems of gravest concern, too — when, in fact, we should find they were not a bit so, if we only rightly discriminated between the different kinds of imperfection of certitude, and if we had only once acquainted ourselves with their different natures. I shall in these notes endeavor to mark the three ways of falling short of certainty by the three terms *probability*, *verisimilitude* or *likelihood*, and *plausibility*. Just at present I propose to deal only with Probability; but I will so far characterize *verisimilitude* and *plausibility* as to mark them off as being entirely different from Probability. Beginning with Plausibility,* I will first endeavor to give an example of an idea which shall be strikingly marked by its very low degree of this quality. Suppose a particularly symmetrical larch tree near the house of a great lover of such trees had been struck by lightning and badly broken, and that as he was looking sorrowfully out of the window at it, he should have happened to say, "I wonder why that particular tree should have been struck, when there are so many about the place that seem more exposed!" Suppose, then, his wife should reply, "Perhaps there may be an eagle's eyrie on some of the hills in the neighborhood, and perhaps the male bird in building it may have used some stick that had a nail in it; and one of the eaglets may have scratched itself against the nail; so that the mother may have reproached the male for using such a dangerous stick; and he, being vexed with her teasing, may have determined to carry the piece to a great distance; it may have been while he was doing this that the explosion of lightning took place, and the electricity may have been deflected by the iron in such a way as to strike this tree. Mind, I do not say that this is what did happen; but if you want to find out why that tree was struck, I think you had better search for an eyrie, and see whether any of the eaglets have been scratched." This is an example of as unplausible a theory as I can think of. We should commonly say it was highly improbable; and I suppose it would be so. But were it ever so probable in all its elements, it would still deserve no attention, because it is perfectly gratuitous to suppose that the lightning was deflected at all; and this supposition does not help to explain the phenomenon.

* Cf. 111, 269, 756f.

Eusapia Palladino had been proved to be a very clever prestigiateuse and cheat, and was visited by a Mr. Carrington,* whom I suppose to be so clever in finding out how tricks are done, that it is highly improbable that any given trick should long baffle him. In point of fact he has often caught the Palladino creature in acts of fraud. Some of her performances, however, he cannot explain; and thereupon he urges the theory that these are supernatural, or, as he prefers to phrase it, "supernormal." Well, I know how it is that when a man has been long intensely exercised and over-fatigued by an enigma, his common-sense will sometimes desert him; but it seems to me that the Palladino has simply been too clever for him, as no doubt she would be for me. The theory that there is anything "supernormal," or *super* anything but *superchérie* in the case, seems to me as needless as any theory I ever came across. That is to say, granted that it is not yet *proved* that women who deceive for gain receive aid from the spiritual world, I think it more plausible that there are tricks that can deceive Mr. Carrington than that the Palladino woman has received such aid. By Plausible, I mean that a theory that has not yet been subjected to any test, although more or less surprising phenomena have occurred which it would explain if it were true, is in itself of such a character as to recommend it for further examination or, if it be *highly* plausible, justify us in seriously inclining toward belief in it, as long as the phenomena be inexplicable otherwise.

663. I will now give an idea of what I mean by *likely* or *verisimilar*. It is to be understood that I am only endeavouring so far to explain the meanings I attach to "plausible" and to "likely," as this may be an assistance to the reader in understanding the meaning I attach to *probable*. I call that theory *likely* which is not yet proved but is supported by such evidence that if the rest of the conceivably possible evidence should turn out upon examination to be of a *similar* character, the theory would be conclusively proved. Strictly speaking, matters of fact never can be demonstrably proved, since it will always remain conceivable that there should be some mistake about it. For instance, I regard it as *sufficiently* proved that my name is Charles Peirce and that I was born in Cambridge,

* See Carrington's *Eusapia Palladino*, B. W. Dodge & Co., New York (1909).

Massachusetts, in a stone-colored wooden house in Mason Street. But even of the part of this of which I am most assured — of my name — there is a certain small probability that I am in an abnormal condition and have got it wrong. I am conscious myself of occasional lapses of memory about other things; and though I well remember — or think I do — living in that house at a tender age, I do not in the least remember being born there, impressive as such a first experience might be expected to be. Indeed, I cannot specify any date on which any certain person informed me I had been born there; and it certainly would have been easy to deceive me in the matter had there been any serious reason for doing so; and how can I be so sure as I surely am that no such reason did exist? It would be a theory without plausibility; that is all.

The history of science, particularly physical science, in contradistinction to natural science — or, as I usually, though inadequately, phrase the distinction, the history of nomological in contradistinction to classificatory sciences — this history ever since I first seriously set myself, at the age of thirteen, in 1852, to the study of logic,* shows only too grievously how great a boon would be any way [of] determining and expressing by numbers the degree of likelihood that a theory had attained — any general recognition, even among leading men of science, of the true degree of significance of a given fact, and of the proper method of determining it. I hope my writings may, at any rate, awaken a few to the enormous waste of effort it would save. But any numerical determination of likelihood is more than I can expect.

664. The only kind of reasoning which can render our conclusions certain — and even this kind can do so only under the proviso that no blunder has been committed in the process — attains this certainty by limiting the conclusion (as Kant virtually said, and others before him), to facts already expressed and accepted in the premisses. This is called necessary, or syllogistic reasoning. Syllogism, not confined to the kind that Aristotle and Theophrastus studied, is merely an artificial form in which it may be expressed, and it is not its best form, from any point of view. But the kind of reasoning which creates likelihoods by virtue of observations may render a likeli-

* Peirce read *Whately's Logic* at this time.

hood *practically* certain — as certain as that a stone let loose from the clutch will, under circumstances not obviously exceptional, fall to the ground — and this conclusion may be that under a certain general condition, easily verified, a certain actuality will be *probable*, that is to say, will come to pass once in so often in the long run. One such familiar conclusion, for example, is that a die thrown from a dice box will with a *probability* of one-third, that is, once in three times in the long run, turn up a number (either *tray* or *size*) that is divisible by three. But this can be affirmed with practical certainty only if by a "long run" be meant an endless series of trials, and (as just said) infinity divided by infinity gives of itself an entirely indefinite quotient. It is therefore necessary to define the phrase. I might give the definition with reference to the probability, p, where p is any vulgar fraction, and in reference to a generic condition, m, and a specific kind of event n. But I think the reader will follow me more readily, if in place of the letter, m (which in itself is but a certain letter, to which is attached a peculiar meaning, that of the fulfillment of some generic condition) I put instead the supposition that a die is thrown from a dice box; and this special supposition will be as readily understood by the reader to be replaceable by any other general condition along with a simultaneous replacement of the *event*, that a number divisible by three is turned up, and at the same time with the replacement of one third by whatever other vulgar fraction may be called for when some different example of a probability is before us. I am, then, to define the meanings of the statement that the *probability*, that if a die be thrown from a dice box it will turn up a number divisible by three, is one-third. The statement means that the die has a certain "would-be"; and to say that a die has a "would-be" is to say that it has a property, quite analogous to any *habit* that a man might have. Only the "would-be" of the die is presumably as much simpler and more definite than the man's habit as the die's homogeneous composition and cubical shape is simpler than the nature of the man's nervous system and soul; and just as it would be necessary, in order to define a man's habit, to describe how it would lead him to behave and upon what sort of occasion — albeit this statement would by no means imply that the habit *consists* in that

action — so to define the die's "would-be," it is necessary to say how it would lead the die to behave on an occasion that would bring out the full consequence of the "would-be"; and this statement will not of itself imply that the "would-be" of the die *consists* in such behavior.

665. Now in order that the full effect of the die's "would-be" may find expression, it is necessary that the die should undergo an endless series of throws from the dice box, the result of no throw having the slightest influence upon the result of any other throw, or, as we express it, the throws must be *independent* each of every other.

666. It will be no objection to our considering the consequences of the supposition that the die is thrown an endless succession of times, and that with a finite pause after each throw, that such an endless series of events is impossible, for the reason that the impossibility is merely a physical, and not a logical, impossibility, as was well illustrated in that famous sporting event in which Achilles succeeded in overtaking the champion tortoise, in spite of his giving the latter the start of a whole *stadion*. For it having been ascertained, by delicate measurements between a mathematical point between the shoulder-blades of Achilles (marked [by] a limit between a red, a green, and a violet sector of a stained disk) and a similar point on the carapace of the tortoise, that when Achilles arrived where the tortoise started, the latter was just 60 feet 8 inches and $\frac{1}{10}$ inch further on, which is just one tenth of a stadion, and that when Achilles reached that point the tortoise was still 6 feet and $8\frac{1}{100}$ inch in advance of him, and finally that, both advancing at a perfectly uniform rate, the tortoise had run just 67 feet 5 inches when he was overtaken by Achilles, it follows that the tortoise progressed at just one tenth the speed of Achilles, the latter running a distance in *stadia* of $1.111111111\dot{1}$, so that he had to traverse the sum of an infinite multitude of finite distances, each in a finite time, and yet covered the *stadion* and one ninth in a finite time. No contradiction, therefore, is involved in the idea of an endless series of finite times or spaces having but a finite sum, provided there is no *fixed* finite quality which every member of an endless part of that series must each and every one *exceed*.

The reader must pardon me for occupying any of his time

with such puerile stuff as that $0.1111 = \frac{1}{9}$; for astounding as it seems, it has more than once happened to me that men have come to me — every one of them not merely educated men, but highly accomplished — men who might well enough be famous over the civilized world, if fame were anything to the purpose, but men whose studies had been such that one would have expected to find each of them an adept in the accurate statement of arguments, and yet each has come and has undertaken to prove to me that the old catch of Achilles and the tortoise is a sound argument. If I tell you what after listening to them by the hour, I have always ended by saying — it may serve your turn on a similar occasion — I have said, "I suppose you do not mean to say that you really believe that a fast runner cannot, as a matter of fact, overtake a slow one. I therefore conclude that the argument which you have been unable to state, either syllogistically or in any other intelligible form, is intended to show that Zeno's reasoning about Achilles and the tortoise is sound according to some system of logic which admits that sound necessary reasoning may lead from true premisses to a false conclusion. But in my system of logic what I mean by bad necessary reasoning is precisely an argument which might lead from true premisses to a false conclusion — just that and nothing else. If you prefer to call such reasoning a sound necessary argument, I have no objection in the world to your doing so; and you will kindly allow me to employ my different nomenclature. For I am such a plain, uncultured soul that when I reason I aim at nothing else than just to find out the truth." To get back, then, to the die and its habit — its "would-be" — I really know no other way of defining a habit than by describing the kind of behavior in which the habit becomes actualized. So I am obliged to define the statement that there is a probability of one-third that the die when thrown will turn up either a three or a six by stating how the numbers will run when the die is thrown.

667. But my purpose in doing so is to explain what *probability*, as I use the word, consists in. Now it would be no explanation at all to say that it consists in something being *probable*. So I must avoid using that word or any synonym of it. If I were to use such an expression, you would very properly turn upon me and say, "I either know what it is to

be *probable*, in your sense of the term, or I do not. If I don't, how can I be expected to understand you until you have explained yourself; and if I do, what is the use of the explanation?" But the fact [is] that the probability of the die turning up a three or a six is not *sure* to produce any determination [of] the run of the numbers thrown in any *finite* series of throws. It is only when the series is endless that we can be *sure* that it will have a particular character. Even when there is an endless series of throws, there is no syllogistic certainty, no "mathematical" certainty (if you are more familiar with this latter phrase) — that the die will not turn up a six obstinately at every single throw. It might be that if in the course of the endless series, some friends should borrow the die to make a pair for a game of backgammon, there might be nothing unusual in the behavior of the lent die, and yet when it was returned and our experimental series was resumed where it had been interrupted, the die might return to turning up nothing but six every time. I say it *might*, in the sense that it would not violate the principle of contradiction if it did. It sanely *would not*, however, unless a miracle were performed; and moreover if such miracle *were* worked, I should say (since it is my use of the term "probability" that we have supposed to be in question) that during this experimental series of throws, the die took on an abnormal, a miraculous, habit. For I should think that the performance of a certain line of behavior, throughout an endless succession of occasions, without exception, very decidedly *constituted* a habit. There may be some doubt about this, for owing to our not being accustomed to reason in this way about successions of events which are endless *in the sequence* and yet are completed *in time*, it is hard for me quite to satisfy myself what I ought to say in such a case. But I have reflected seriously on it, and though I am not perfectly sure of my ground (and I am a cautious reasoner), yet I am more that what you would understand by "pretty confident," that supposing one to be in a condition to assert what *would surely be* the behavior, *in any single determinate respect*, of any subject throughout an endless series of occasions of a stated kind, he *ipso facto* knows a "would-be," or habit, of that subject. It is very true, mind you, that *no* collection whatever of single acts, though it were ever so many grades

greater than a simple endless series, can constitute a would-be, nor can the knowledge of single acts, whatever their multitude, tell us for *sure* of a would-be. But there are two remarks to be made; first, that in the case under consideration a person is supposed to be in a condition to assert what surely *would be* the behavior of the subject throughout the endless series of occasions — a knowledge which cannot have been derived from reasoning from its behavior on the single occasions; and second, that that which in our case renders it true, as stated, that the person supposed "*ipso facto* knows a would-be of that subject," is not the occurrence of the single acts, but the fact that the person supposed "was in condition to assert what *would surely be* the behavior of the subject throughout an endless series of occasions."[1]

668. I will now describe the behavior of the die during the endless series of throws, in respect to turning up numbers divisible by three. It would be perfectly possible to construct a machine that would automatically throw the die and pick it up, and continue doing so as long as it was supplied with energy. It would further be still easier to design the plan of an arrangement whereby a hand should after each throw move over an arc graduated so as to indicate the value of the quotient of the number of throws of three or six that had been known since the beginning of the experiment, divided by the total number of throws since the beginning. It is true that the mechanical difficulties would become quite insuperable before the die had been thrown many times; but fortunately a general description of the way the hand would move will answer our purpose much better than would the actual machine, were it ever so perfect.

After the first throw, the hand will go either to $0=\frac{0}{1}$ or $1=\frac{1}{1}$; and there it may stay for several throws. But when it once moves, it will move after every throw, without exception, since the denominator of the fraction at whose value it points will always increase by 1, and consequently the value

[1] Meantime it may be remarked that, though an endless series of acts is not a habit, nor a would-be, it does present the first of an endless series of steps toward the full nature of a would-be. Compare what I wrote nineteen[thirteen!] years ago, in an article on the logic of relatives[3.526ff].

of the fraction will be diminished if the numerator remains unchanged, as it will be increased in case the numerator is increased by 1, these two being the only possible cases. The behavior of the hand may be described as an excessively irregular oscillation, back and forth, from one side of ⅓ to the other. . . .

CHAPTER 7

*THE PROBABILITY OF INDUCTION**

§1. RULES FOR THE ADDITION AND MULTIPLICATION OF PROBABILITIES[E]

669. We have found that every argument derives its force from the general truth of the class of inferences to which it belongs; and that probability is the proportion of arguments carrying truth with them among those of any *genus*. This is most conveniently expressed in the nomenclature of the mediæval logicians. They called the fact expressed by a premiss an *antecedent*, and that which follows from it its *consequent;* while the leading principle, that every (or almost every) such antecedent is followed by such a consequent, they termed the *consequence*. Using this language, we may say that probability belongs exclusively to *consequences*, and the probability of any consequence is the number of times in which antecedent and consequent both occur divided by the number of all the times in which the antecedent occurs. From this definition are deduced the following rules for the addition and multiplication of probabilities:

670. *Rule for the Addition of Probabilities.* — Given the separate probabilities of two consequences having the same antecedent and incompatible consequents. Then the sum of these two numbers is the probability of the consequence, that from the same antecedent one or other of those consequents follows.

671. *Rule for the Multiplication of Probabilities.* — Given the separate probabilities of the two consequences, "If A then B," and "If both A and B, then C." Then the product of the these two numbers is the probability of the consequence, "If A, then both B and C."

672. *Special Rule for the Multiplication of Independent*

* *Popular Science Monthly*, vol. 12, pp. 705–18 (1878), the fourth of a series of papers on "Illustrations of the Logic of Science." See 612n. Intended as Essay XI of the *Search for a Method* (1893).

Probabilities. — Given the separate probabilities of two consequences having the same antecedents, "If A, then B," and "If A, then C." Suppose that these consequences are such that the probability of the second is equal to the probability of the consequence, "If both A and B, then C." Then the product of the two given numbers is equal to the probability of the consequence, "If A, then both B and C."

To show the working of these rules we may examine the probabilities in regard to throwing dice. What is the probability of throwing a six with one die? The antecedent here is the event of throwing a die; the consequent, its turning up a six. As the die has six sides, all of which are turned up with equal frequency, the probability of turning up any one is $\frac{1}{6}$. Suppose two dice are thrown, what is the probability of throwing sixes? The probability of either coming up six is obviously the same when both are thrown as when one is thrown — namely, $\frac{1}{6}$. The probability that either will come up six when the other does is also the same as that of its coming up six whether the other does or not. The probabilities are, therefore, independent; and, by our rule, the probability that both events will happen together is the product of their several probabilities, or $\frac{1}{6}\times\frac{1}{6}$. What is the probability of throwing deuce-ace? The probability that the first die will turn up ace and the second deuce is the same as the probability that both will turn up sixes — namely, $\frac{1}{36}$; the probability that the *second* will turn up ace and the *first* deuce is likewise $\frac{1}{36}$; these two events — first, ace; second, deuce; and, second, ace; first, deuce — are incompatible. Hence the rule for addition holds, and the probability that either will come up ace and the other deuce is $\frac{1}{36}+\frac{1}{36}$, or $\frac{1}{18}$.

In this way all problems about dice, etc., may be solved. When the number of dice thrown is supposed very large, mathematics (which may be defined as the art of making groups to facilitate numeration) comes to our aid with certain devices to reduce the difficulties.

§2. MATERIALISTIC AND CONCEPTUALISTIC VIEWS OF PROBABILITY[E]

673. The conception of probability as a matter of *fact*, i.e., as the proportion of times in which an occurrence of one kind

is accompanied by an occurrence of another kind, is termed by Mr. Venn the materialistic view of the subject. But probability has often been regarded as being simply the degree of belief which ought to attach to a proposition, and this mode of explaining the idea is termed by Venn the conceptualistic view. Most writers have mixed the two conceptions together. They, first, define the probability of an event as the reason we have to believe that it has taken place, which is conceptualistic; but shortly after they state that it is the ratio of the number of cases favorable to the event to the total number of cases favorable or contrary, and all equally possible. Except that this introduces the thoroughly unclear idea of cases equally possible in place of cases equally frequent, this is a tolerable statement of the materialistic view. The pure conceptualistic theory has been best expounded by Mr. De Morgan in his *Formal Logic: or, the Calculus of Inference, Necessary and Probable.*

674. The great difference between the two analyses is, that the conceptualists refer probability to an event, while the materialists make it the ratio of frequency of events of a *species* to those of a *genus* over that *species*, thus *giving it two terms instead of one.* The opposition may be made to appear as follows:*

Suppose that we have two rules of inference, such that, of all the questions to the solution of which both can be applied, the first yields correct answers to $\frac{81}{100}$, and incorrect answers to the remaining $\frac{19}{100}$; while the second yields correct answers to $\frac{93}{100}$, and incorrect answers to the remaining $\frac{7}{100}$. Suppose, further, that the two rules are entirely independent as to their truth, so that the second answers correctly $\frac{93}{100}$ of the questions which the first answers correctly, and also $\frac{93}{100}$ of the questions which the first answers incorrectly, and answers incorrectly the remaining $\frac{7}{100}$ of the questions which the first answers correctly, and also the remaining $\frac{7}{100}$ of the questions which the first answers incorrectly. Then, of all the questions to the solution of which both rules can be applied —

$$\text{both answer correctly } \frac{93}{100}\text{ of }\frac{81}{100}\text{, or }\frac{93\times81}{100\times100};$$

* Cf. 3.17.

the second answers correctly and the first incorrectly

$$\frac{93}{100} \text{ of } \frac{19}{100}, \text{ or } \frac{93\times19}{100\times100};$$

the second answers incorrectly and the first correctly

$$\frac{7}{100} \text{ of } \frac{81}{100}, \text{ or } \frac{7\times81}{100\times100};$$

and both answer incorrectly

$$\frac{7}{100} \text{ of } \frac{19}{100}, \text{ or } \frac{7\times19}{100\times100};$$

Suppose, now, that, in reference to any question, both give the same answer. Then (the questions being always such as are to be answered by *yes* or *no*), those in reference to which their answers agree are the same as those which both answer correctly together with those which both answer falsely, or $\frac{93\times81}{100\times100}+\frac{7\times19}{100\times100}$ of all. The proportion of those which both answer correctly out of those their answers to which agree is, therefore —

$$\frac{\frac{93\times81}{100\times100}}{\frac{93\times81}{100\times100}+\frac{7\times19}{100\times100}} \text{ or } \frac{93\times81}{(93\times81)+(7\times19)}.$$

675. This is, therefore, the probability that, if both modes of inference yield the same result, that result is correct. We may here conveniently make use of another mode of expression. *Probability* is the ratio of the favorable cases to all the cases. Instead of expressing our result in terms of this ratio, we may make use of another — the ratio of favorable to unfavorable cases. This last ratio may be called the *chance* of an event. Then the chance of a true answer by the first mode of inference is $\frac{81}{19}$ and by the second is $\frac{93}{7}$; and the chance of a correct answer from both, when they agree, is —

$$\frac{81\times93}{19\times7}, \text{ or } \frac{81}{19}\times\frac{93}{7},$$

or the product of the chances of each singly yielding a true answer.

It will be seen that a chance is a quantity which may have any magnitude, however great. An event in whose favor there is an even chance, or $\frac{1}{1}$, has a probability of $\frac{1}{2}$. An argument having an even chance can do nothing toward reënforcing others, since according to the rule its combination with another would only multiply the chance of the latter by 1.

676. Probability and chance undoubtedly belong primarily to consequences, and are relative to premisses; but we may, nevertheless, speak of the chance of an event absolutely, meaning by that the chance of the combination of all arguments in reference to it which exist for us in the given state of our knowledge. Taken in this sense it is incontestable that the chance of an event has an intimate connection with the degree of our belief in it. Belief is certainly something more than a mere feeling; yet there is a feeling of believing, and this feeling does and ought to vary with the chance of the thing believed, as deduced from all the arguments. Any quantity which varies with the chance might, therefore, it would seem, serve as a thermometer for the proper intensity of belief. Among all such quantities there is one which is peculiarly appropriate. When there is a very great chance, the feeling of belief ought to be very intense. Absolute certainty, or an infinite chance, can never be attained by mortals, and this may be represented appropriately by an infinite belief. As the chance diminishes the feeling of believing should diminish, until an even chance is reached, where it should completely vanish and not incline either toward or away from the proposition. When the chance becomes less, then a contrary belief should spring up and should increase in intensity as the chance diminishes, and as the chance almost vanishes (which it can never quite do) the contrary belief should tend toward an infinite intensity. Now, there is one quantity which, more simply than any other, fulfills these conditions; it is the *logarithm* of the chance. But there is another consideration which must, if admitted, fix us to this choice for our thermometer. It is that our belief ought to be proportional to the weight of evidence, in this sense, that two arguments which are entirely independent, neither weakening nor strengthening each other, ought, when they concur, to produce a belief equal to the sum of the intensities of belief which either would produce separately. Now, we have seen that the

chances of independent concurrent arguments are to be multiplied together to get the chance of their combination, and, therefore, the quantities which best express the intensities of belief should be such that they are to be *added* when the *chances* are multiplied in order to produce the quantity which corresponds to the combined chance. Now, the logarithm is the only quantity which fulfills this condition. There is a general law of sensibility, called Fechner's psychophysical law. It is that the intensity of any sensation is proportional to the logarithm of the external force which produces it. It is entirely in harmony with this law that the feeling of belief should be as the logarithm of the chance, this latter being the expression of the state of facts which produces the belief.

The rule for the combination of independent concurrent arguments takes a very simple form when expressed in terms of the intensity of belief, measured in the proposed way. It is this: Take the sum of all the feelings of belief which would be produced separately by all the arguments *pro*, subtract from that the similar sum for arguments *con*, and the remainder is the feeling of belief which we ought to have on the whole. This is a proceeding which men often resort to, under the name of *balancing reasons*.

These considerations constitute an argument in favor of the conceptualistic view. The kernel of it is that the conjoint probability of all the arguments in our possession, with reference to any fact, must be intimately connected with the just degree of our belief in that fact; and this point is supplemented by various others showing the consistency of the theory with itself and with the rest of our knowledge.

677. But probability, to have any value at all, must express a fact. It is, therefore, a thing to be inferred upon evidence. Let us, then, consider for a moment the formation of a belief of probability. Suppose we have a large bag of beans from which one has been secretly taken at random and hidden under a thimble. We are now to form a probable judgment of the color of that bean, by drawing others singly from the bag and looking at them, each one to be thrown back, and the whole well mixed up after each drawing. Suppose the first drawing is white and the next black. We conclude that there is not an immense preponderance of either color, and that there

is something like an even chance that the bean under the thimble is black. But this judgment may be altered by the next few drawings. When we have drawn ten times, if 4, 5, or 6, are white, we have more confidence that the chance is even. When we have drawn a thousand times, if about half have been white, we have great confidence in this result. We now feel pretty sure that, if we were to make a large number of bets upon the color of single beans drawn from the bag, we could approximately insure ourselves in the long run by betting each time upon the white, a confidence which would be entirely wanting if, instead of sampling the bag by 1,000 drawings, we had done so by only two. Now, as the whole utility of probability is to insure us in the long run, and as that assurance depends, not merely on the value of the chance, but also on the accuracy of the evaluation, it follows that we ought not to have the same feeling of belief in reference to all events of which the chance is even. In short, to express the proper state of our belief, not *one* number but *two* are requisite, the first depending on the inferred probability, the second on the amount of knowledge on which that probability is based.[1] It is true that when our knowledge is very precise, when we have made many drawings from the bag, or, as in most of the examples in the books, when the total contents of the bag are absolutely known, the number which expresses the uncertainty of the assumed probability and its liability to be changed by further experience may become insignificant, or utterly vanish. But, when our knowledge is very slight, this number may be even more important than the probability itself; and when we have no knowledge at all this completely overwhelms the other, so that there is no sense in saying that the chance of the totally unknown event is even (for what expresses absolutely no fact has absolutely no meaning), and what ought to be said is that the chance is entirely indefinite. We thus perceive that the conceptualistic view, though answering well enough in some cases, is quite inadequate.

678. Suppose that the first bean which we drew from our bag was black. That would constitute an argument, no matter how slender, that the bean under the thimble was also

[1] Strictly we should need an infinite series of numbers each depending on the probable error of the last.

black. If the second bean was also to turn out black, that would be a second independent argument reënforcing the first. If the whole of the first twenty beans drawn should prove black, our confidence that the hidden bean was black would justly attain considerable strength. But suppose the twenty-first bean was to be white and that we were to go on drawing until we found that we had drawn 1,010 black beans and 990 white ones. We should conclude that our first twenty beans being black was simply an extraordinary accident, and that in fact the proportion of white beans to black was sensibly equal, and that it was an even chance that the hidden bean was black. Yet according to the rule of *balancing reasons*, since all the drawings of black beans are so many independent arguments in favor of the one under the thimble being black, and all the white drawings so many against it, an excess of twenty black beans ought to produce the same degree of belief that the hidden bean was black, whatever the total number drawn.

679. In the conceptualistic view of probability, complete ignorance, where the judgment ought not to swerve either toward or away from the hypothesis, is represented by the probability $\frac{1}{2}$.[1]

But let us suppose that we are totally ignorant what colored hair the inhabitants of Saturn have. Let us, then, take a color-chart in which all possible colors are shown shading into one another by imperceptible degrees. In such a chart the relative areas occupied by different classes of colors are perfectly arbitrary. Let us inclose such an area with a closed line, and ask what is the chance on conceptualistic principles that the color of the hair of the inhabitants of Saturn falls within that area? The answer cannot be indeterminate because we must be in some state of belief; and, indeed, conceptualistic writers do not admit indeterminate probabilities. As there is no certainty in the matter, the answer lies between *zero* and *unity*. As no numerical value is afforded by the data, the number must be determined by the nature of the scale of probability itself, and not by calculation from the data. The answer can, therefore, only be one-half, since the judgment should neither favor nor oppose the hypothesis. What is true of this area is true of any

[1] "Perfect indecision, belief inclining neither way, an even chance." — De Morgan, p. 182.

other one; and it will equally be true of a third area which embraces the other two. But the probability for each of the smaller areas being one-half, that for the larger should be at least unity, which is absurd.

§3. ON THE CHANCE OF UNKNOWN EVENTS[E]

680. All our reasonings are of two kinds: 1. *Explicative, analytic,* or *deductive;* 2. *Amplifiative, synthetic,* or (loosely speaking) *inductive.* In explicative reasoning, certain facts are first laid down in the premisses. These facts are, in every case, an inexhaustible multitude, but they may often be summed up in one simple proposition by means of some regularity which runs through them all. Thus, take the proposition that Socrates was a man; this implies (to go no further) that during every fraction of a second of his whole life (or, if you please, during the greater part of them) he was a man. He did not at one instant appear as a tree and at another as a dog; he did not flow into water, or appear in two places at once; you could not put your finger through him as if he were an optical image, etc. Now, the facts being thus laid down, some order among some of them, not particularly made use of for the purpose of stating them, may perhaps be discovered; and this will enable us to throw part or all of them into a new statement, the possibility of which might have escaped attention. Such a statement will be the conclusion of an analytic inference. Of this sort are all mathematical demonstrations. But synthetic reasoning is of another kind. In this case the facts summed up in the conclusion are not among those stated in the premisses. They are different facts, as when one sees that the tide rises m times and concludes that it will rise the next time. These are the only inferences which increase our real knowledge, however useful the others may be.

681. In any problem in probabilities, we have given the relative frequency of certain events, and we perceive that in these facts the relative frequency of another event is given in a hidden way. This being stated makes the solution. This is, therefore, mere explicative reasoning, and is evidently entirely inadequate to the representation of synthetic reasoning, which goes out beyond the facts given in the premisses. There is,

therefore, a manifest impossibility in so tracing out any probability for a synthetic conclusion.

682. Most treatises on probability contain a very different doctrine. They state, for example, that if one of the ancient denizens of the shores of the Mediterranean, who had never heard of tides, had gone to the bay of Biscay, and had there seen the tide rise, say m times, he could know that there was a probability equal to

$$\frac{m+1}{m+2}$$

that it would rise the next time. In a well-known work by Quetelet,* much stress is laid on this, and it is made the foundation of a theory of inductive reasoning.

683. But this solution betrays its origin if we apply it to the case in which the man has never seen the tide rise at all; that is, if we put $m=0$. In this case, the probability that it will rise the next time comes out $\frac{1}{2}$, or, in other words, the solution involves the conceptualistic principle that there is an even chance of a totally unknown event. The manner in which it has been reached has been by considering a number of urns all containing the same number of balls, part white and part black. One urn contains all white balls, another one black and the rest white, a third two black and the rest white, and so on, one urn for each proportion, until an urn is reached containing only black balls. But the only possible reason for drawing any analogy between such an arrangement and that of Nature is the principle that alternatives of which we know nothing must be considered as equally probable. But this principle is absurd. There is an indefinite variety of ways of enumerating the different possibilities, which, on the application of this principle, would give different results. If there be any way of enumerating the possibilities so as to make them all equal, it is not that from which this solution is derived, but is the following: Suppose we had an immense granary filled with black and white balls well mixed up; and suppose each urn were filled by taking a fixed number of balls from this granary quite at random. The relative number of white balls in the granary might be anything, say one in three. Then in one-third of the

* *Théorie des Probabilités*, deuxième partie, §1.

urns the first ball would be white, and in two-thirds black. In one-third of those urns of which the first ball was white, and also in one-third of those in which the first ball was black, the second ball would be white. In this way, we should have a distribution like that shown in the following table, where w stands for a white ball and b for a black one. The reader can, if he chooses, verify the table for himself.

wwww.

wwwb.	wwbw.	wbww.	bwww.		
wwwb.	wwbw.	wbww.	bwww.		
wwbb.	wbwb.	bwwb.	wbbw.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbw.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbw.	bwbw.	bbww.
wwbb.	wbwb.	bwwb.	wbbw.	bwbw.	bbww.
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		
wbbb.	bwbb.	bbwb.	bbbw.		

bbbb. In the second group, where there is one b, there are bbbb. two sets just alike; in the third there are 4, in the bbbb. fourth 8, and in the fifth 16, doubling every time. bbbb. This is because we have supposed twice as many bbbb. black balls in the granary as white ones; had we sup- bbbb. posed 10 times as many, instead of

bbbb.

bbbb. 1, 2, 4, 8, 16

bbbb.

bbbb. sets we should have had

bbbb.

bbbb. 1, 10, 100, 1000, 10000

bbbb.

bbbb. sets; on the other hand, had the numbers of black and bbbb. white balls in the granary been even, there would have bbbb. been but one set in each group. Now suppose two balls

were drawn from one of these urns and were found to be both white, what would be the probability of the next one being white? If the two drawn out were the first two put into the urns, and the next to be drawn out were the third put in, then the probability of this third being white would be the same whatever the colors of the first two, for it has been supposed that just the same proportion of urns has the third ball white among those which have the first two *white-white*, *white-black*, *black-white*, and *black-black*. Thus, in this case, the chance of the third ball being white would be the same whatever the first two were. But, by inspecting the table, the reader can see that in each group all orders of the balls occur with equal frequency, so that it makes no difference whether they are drawn out in the order they were put in or not. Hence the colors of the balls already drawn have no influence on the probability of any other being white or black.

684. Now, if there be any way of enumerating the possibilities of Nature so as to make them equally probable, it is clearly one which should make one arrangement or combination of the elements of Nature as probable as another, that is, a distribution like that we have supposed, and it, therefore, appears that the assumption that any such thing can be done, leads simply to the conclusion that reasoning from past to future experience is absolutely worthless.* In fact, the moment that you assume that the chances in favor of that of which we are totally ignorant are even, the problem about the tides does not differ, in any arithmetical particular, from the case in which a penny (known to be equally likely to come up heads or tails) should turn up heads m times successively. In short, it would be to assume that Nature is a pure chaos, or chance combination of independent elements, in which reasoning from one fact to another would be impossible; and since, as we shall hereafter see,† there is no judgment of pure observation without reasoning, it would be to suppose all human cognition illusory and no real knowledge possible. It would be to suppose that if we have found the order of Nature more or less regular in the past, this has been by a pure run of luck which we may expect is now at an end. Now, it may be we have no scintilla

* Cf. vol. 6, bk. II, ch. 1, §2.

† See 692.

of proof to the contrary, but reason is unnecessary in reference to that belief which is of all the most settled, which nobody doubts or can doubt, and which he who should deny would stultify himself in so doing.

The relative probability of this or that arrangement of Nature is something which we should have a right to talk about if universes were as plenty as blackberries, if we could put a quantity of them in a bag, shake them well up, draw out a sample, and examine them to see what proportion of them had one arrangement and what proportion another. But, even in that case, a higher universe would contain us, in regard to whose arrangements the conception of probability could have no applicability.

§4. ON THE PROBABILITY OF SYNTHETIC INFERENCES[E]

685. We have examined the problem proposed by the conceptualists, which, translated into clear language, is this: Given a synthetic conclusion; required to know out of all possible states of things how many will accord, to any assigned extent, with this conclusion; and we have found that it is only an absurd attempt to reduce synthetic to analytic reason, and that no definite solution is possible.

686. But there is another problem in connection with this subject. It is this: Given a certain state of things, required to know what proportion of all synthetic inferences relating to it will be true within a given degree of approximation. Now, there is no difficulty about this problem (except for its mathematical complication); it has been much studied, and the answer is perfectly well known. And is not this, after all, what we want to know much rather than the other? Why should we want to know the probability that the fact will accord with our conclusion? That implies that we are interested in all possible worlds, and not merely the one in which we find ourselves placed. Why is it not much more to the purpose to know the probability that our conclusion will accord with the fact? One of these questions is the first above stated and the other the second, and I ask the reader whether, if people, instead of using the word probability without any clear apprehension of their own meaning, had always spoken of relative frequency,

they could have failed to see that what they wanted was not to follow along the synthetic procedure with an analytic one, in order to find the probability of the conclusion; but, on the contrary, to begin with the fact at which the synthetic inference aims, and follow back to the facts it uses for premisses in order to see the probability of their being such as will yield the truth.

687. As we cannot have an urn with an infinite number of balls to represent the inexhaustibleness of Nature, let us suppose one with a finite number, each ball being thrown back into the urn after being drawn out, so that there is no exhaustion of them. Suppose one ball out of three is white and the rest black, and that four balls are drawn. Then the table in 683 represents the relative frequency of the different ways in which these balls might be drawn. It will be seen that if we should judge by these four balls of the proportion in the urn, 32 times out of 81 we should find it $\frac{1}{4}$, and 24 times out of 81 we should find it $\frac{1}{2}$, the truth being $\frac{1}{3}$. To extend this table to high numbers would be great labor, but the mathematicians have found some ingenious ways of reckoning what the numbers would be. It is found that, if the true proportion of white balls is p, and s balls are drawn, then the error of the proportion obtained by the induction will be —

half the time within	$0.477\sqrt{\frac{2p(1-p)}{s}}$
9 times out of 10 within	$1.163\sqrt{\frac{2p(1-p)}{s}}$
99 times out of 100 within	$1.821\sqrt{\frac{2p(1-p)}{s}}$
999 times out of 1,000 within	$2.328\sqrt{\frac{2p(1-p)}{s}}$
9,999 times out of 10,000 within	$2.751\sqrt{\frac{2p(1-p)}{s}}$
9,999,999,999 times out of 10,000,000,000 within	$4.77\sqrt{\frac{2p(1-p)}{s}}$

The use of this may be illustrated by an example. By the census of 1870, it appears that the proportion of males among

native white children under one year old was 0.5082, while among colored children of the same age the proportion was only 0.4977. The difference between these is 0.0105, or about one in 100. Can this be attributed to chance, or would the difference always exist among a great number of white and colored children under like circumstances? Here p may be taken at $\frac{1}{2}$; hence $2p(1-p)$ is also $\frac{1}{2}$. The number of white children counted was near 1,000,000; hence the fraction whose square-root is to be taken is about $\frac{1}{2000000}$. The root is about $\frac{1}{1400}$, and this multiplied by 0.477 gives about 0.0003 as the probable error in the ratio of males among the whites as obtained from the induction. The number of black children was about 150,000, which gives 0.0008 for the probable error. We see that the actual discrepancy is ten times the sum of these, and such a result would happen, according to our table, only once out of 10,000,000,000 censuses, in the long run.

688. It may be remarked that when the real value of the probability sought inductively is either very large or very small, the reasoning is more secure. Thus, suppose there were in reality one white ball in 100 in a certain urn, and we were to judge of the number by 100 drawings. The probability of drawing no white ball would be $\frac{366}{10000}$; that of drawing one white ball would be $\frac{370}{10000}$; that of drawing two would be $\frac{185}{10000}$; that of drawing three would be $\frac{61}{10000}$; that of drawing four would be $\frac{15}{10000}$; that of drawing five would be only $\frac{3}{10000}$, etc. Thus we should be tolerably certain of not being in error by more than one ball in 100.

689. It appears, then, that in one sense we can, and in another we cannot, determine the probability of synthetic inference. When I reason in this way:

> Ninety-nine Cretans in a hundred are liars,
> But Epimenides is a Cretan;
> Therefore, Epimenides is a liar;

I know that reasoning similar to that would carry truth 99 times in 100. But when I reason in the opposite direction:

> Minos, Sarpedon, Rhadamanthus, Deucalion, and Epimenides, are all the Cretans I can think of,
> But these were all atrocious liars;
> Therefore, pretty much all Cretans must have been liars;

I do not in the least know how often such reasoning would carry me right. On the other hand, what I do know is that some definite proportion of Cretans must have been liars, and that this proportion can be probably approximated to by an induction from five or six instances. Even in the worst case for the probability of such an inference, that in which about half the Cretans are liars, the ratio so obtained would probably not be in error by more than $\frac{1}{6}$. So much I know; but, then, in the present case the inference is that pretty much all Cretans are liars, and whether there may not be a special improbability in that I do not know.

§5. THE RATIONALE OF SYNTHETIC INFERENCE[E]

690. Late in the last century, Immanuel Kant asked the question, "How are synthetical judgments *a priori* possible?" By synthetical judgments he meant such as assert positive fact and are not mere affairs of arrangement; in short, judgments of the kind which synthetical reasoning produces, and which analytic reasoning cannot yield. By *a priori* judgments he meant such as that all outward objects are in space, every event has a cause, etc., propositions which according to him can never be inferred from experience. Not so much by his answer to this question as by the mere asking of it, the current philosophy of that time was shattered and destroyed, and a new epoch in its history was begun. But before asking *that* question he ought to have asked the more general one, "How are any synthetical judgments at all possible?" How is it that a man can observe one fact and straightway pronounce judgment concerning another different fact not involved in the first? Such reasoning, as we have seen, has, at least in the usual sense of the phrase, no definite probability; how, then, can it add to our knowledge? This is a strange paradox; the Abbé Gratry says it is a miracle, and that every true induction is an immediate inspiration from on high.[1] I respect this explanation far more than many a pedantic attempt to solve the question by some juggle with probabilities, with the forms of

[1] *Logique*. The same is true, according to him, of every performance of a differentiation, but not of integration. He does not tell us whether it is the supernatural assistance which makes the former process so much the easier.

syllogism, or what not. I respect it because it shows an appreciation of the depth of the problem, because it assigns an adequate cause, and because it is intimately connected — as the true account should be — with a general philosophy of the universe. At the same time, I do not accept this explanation, because an explanation should tell *how* a thing is done, and to assert a perpetual miracle seems to be an abandonment of all hope of doing that, without sufficient justification.

691. It will be interesting to see how the answer which Kant gave to his question about synthetical judgments *a priori* will appear if extended to the question of synthetical judgments in general. That answer is, that synthetical judgments *a priori* are possible because whatever is universally true is involved in the conditions of experience. Let us apply this to a general synthetical reasoning. I take from a bag a handful of beans; they are all purple, and I infer that all the beans in the bag are purple. How can I do that? Why, upon the principle that whatever is universally true of my experience (which is here the appearance of these different beans) is involved in the condition of experience. The condition of this special experience is that all these beans were taken from that bag. According to Kant's principle, then, whatever is found true of all the beans drawn from the bag must find its explanation in some peculiarity of the contents of the bag. This is a satisfactory statement of the principle of induction.

692. When we draw a deductive or analytic conclusion, our rule of inference is that facts of a certain general character are either invariably or in a certain proportion of cases accompanied by facts of another general character. Then our premiss being a fact of the former class, we infer with certainty or with the appropriate degree of probability the existence of a fact of the second class. But the rule for synthetic inference is of a different kind. When we sample a bag of beans we do not in the least assume that the fact of some beans being purple involves the necessity or even the probability of other beans being so. On the contrary, the conceptualistic method of treating probabilities, which really amounts simply to the deductive treatment of them, when rightly carried out leads to the result that a synthetic inference has just an even chance in its favor, or in other words is absolutely worthless. The color

of one bean is entirely independent of that of another. But synthetic inference is founded upon a classification of facts, not according to their characters, but according to the manner of obtaining them. Its rule is, that a number of facts obtained in a given way will in general more or less resemble other facts obtained in the same way; or, *experiences whose conditions are the same will have the same general characters.*

693. In the former case, we know that premisses precisely similar in form to those of the given ones will yield true conclusions, just once in a calculable number of times. In the latter case, we only know that premisses obtained under circumstances similar to the given ones (though perhaps themselves very different) will yield true conclusions, at least once in a calculable number of times. We may express this by saying that in the case of analytic inference we know the probability of our conclusion (if the premisses are true), but in the case of synthetic inferences we only know the degree of trustworthiness of our proceeding. As all knowledge comes from synthetic inference, we must equally infer that all human certainty consists merely in our knowing that the processes by which our knowledge has been derived are such as must generally have led to true conclusions.

Though a synthetic inference cannot by any means be reduced to deduction, yet that the rule of induction will hold good in the long run may be deduced from the principle that reality is only the object of the final opinion to which sufficient investigation would lead. That belief gradually tends to fix itself under the influence of inquiry is, indeed, one of the facts with which logic sets out.

CHAPTER 8

*A THEORY OF PROBABLE INFERENCE**

§1. PROBABLE DEDUCTION AND PROBABILITY IN GENERAL†

694. The following is an example of the simplest kind of probable inference:

About two per cent of persons wounded in the liver recover,
This man has been wounded in the liver;
Therefore, there are two chances out of a hundred that he will recover.

Compare this with the simplest of syllogisms, say the following:

Every man dies,
Enoch was a man;
Hence, Enoch must have died.

The latter argument consists in the application of a general rule to a particular case. The former applies to a particular case a rule not absolutely universal, but subject to a known proportion of exceptions. Both may alike be termed deductions, because they bring information about the uniform or usual course of things to bear upon the solution of special questions; and the probable argument may approximate indefinitely to demonstration as the ratio named in the first premiss approaches to unity or to zero.

695. Let us set forth the general formulæ of the two kinds of inference in the manner of formal logic.

* The Johns Hopkins *Studies in Logic*, edited by C. S. Peirce, Little Brown and Co., Boston (1883), pp. 126–181; intended as Essay XIV of the *Search for a Method* (1893).

† The headings of these sections were made by Peirce in his own copy of the Johns Hopkins *Studies*.

FORM I.

Singular Syllogism in Barbara.

Every M is a P,
S is an M;
Hence, S is a P.

FORM II.

Simple Probable Deduction.

The proportion ρ of the M's are P's;
S is an M;
It follows, with probability ρ, that S is a P.

It is to be observed that the ratio ρ need not be exactly specified. We may reason from the premiss that not more than two per cent of persons wounded in the liver recover, or from "not less than a certain proportion of the M's are P's," or from "no very large nor very small proportion," etc. In short, ρ is subject to every kind of indeterminacy; it simply excludes some ratios and admits the possibility of the rest.

696. The analogy between syllogism and what is here called probable deduction is certainly genuine and important; yet how wide the differences between the two modes of inference are, will appear from the following considerations:

(1) The logic of probability is related to ordinary syllogistic as the quantitative to the qualitative branch of the same science. Necessary syllogism recognizes only the inclusion or non-inclusion of one class under another; but probable inference takes account of the proportion of one class which is contained under a second. It is like the distinction between projective geometry, which asks whether points coincide or not, and metric geometry, which determines their distances.

(2) For the existence of ordinary syllogism, all that is requisite is that we should be able to say, in some sense, that one term is contained in another, or that one object stands to a second in one of those relations: "better than," "equivalent to," etc., which are termed *transitive* because if A is in any such relation to B, and B is in the same relation to C, then A is in that relation to C. The universe might be all so fluid and variable that nothing should preserve its individual identity,

and that no measurement should be conceivable; and still one portion might remain inclosed within a second, itself inclosed within a third, so that a syllogism would be possible. But probable inference could not be made in such a universe, because no signification would attach to the words "quantitative ratio." For that there must be counting; and consequently units must exist, preserving their identity and variously grouped together.

(3) A cardinal distinction between the two kinds of inference is, that in demonstrative reasoning the conclusion follows from the existence of the objective facts laid down in the premisses; while in probable reasoning these facts in themselves do not even render the conclusion probable, but account has to be taken of various subjective circumstances — of the manner in which the premisses have been obtained, of there being no countervailing considerations, etc.; in short, good faith and honesty are essential to good logic in probable reasoning.

When the partial rule that the proportion ρ of the M's are P's is applied to show with probability ρ that S is a P, it is requisite, not merely that S should *be* an M, but also that it should be an instance drawn *at random* from among the M's. Thus, there being four aces in a piquet pack of thirty-two cards, the chance is one-eighth that a given card not looked at is an ace; but this is only on the supposition that the card has been drawn at random from the whole pack. If, for instance, it had been drawn from the cards discarded by the players at piquet or euchre, the probability would be quite different. The instance must be drawn at random. Here is a maxim of conduct. The volition of the reasoner (using what machinery it may) has to choose S so that it shall be an M; but he ought to restrain himself from all further preference, and not allow his will to act in any way that might tend to settle what particular M is taken, but should leave that to the operation of chance. Willing and wishing, like other operations of the mind, are *general* and imperfectly determinate. I wish for a horse — for some particular kind of horse perhaps, but not usually for any individual one. I will to act in a way of which I have a general conception; but so long as my action conforms to that general description, how it is further determined I do not care. Now in choosing the instance S, the gen-

eral intention (including the whole plan of action) should be to select an M, but beyond that there should be no preference; and the act of choice should be such that if it were repeated many enough times with the same intention, the result would be that among the totality of selections the different sorts of M's would occur with the same relative frequencies as in experiences in which volition does not intermeddle at all. In cases in which it is found difficult thus to restrain the will by a direct effort, the apparatus of games of chance — a lottery-wheel, a roulette, cards, or dice — may be called to our aid. Usually, however, in making a simple probable deduction, we take that instance in which we happen at the time to be interested. In such a case, it is our interest that fulfills the function of an apparatus for random selection; and no better need be desired, so long as we have reason to deem the premiss "the proportion ρ of the M's are P's" to be equally true in regard to that part of the M's which are alone likely ever to excite our interest.

Nor is it a matter of indifference in what manner the other premiss has been obtained. A card being drawn at random from a piquet pack, the chance is one-eighth that it is an ace, if we have no other knowledge of it. But after we have looked at the card, we can no longer reason in that way. That the conclusion must be drawn in advance of any other knowledge on the subject is a rule that, however elementary, will be found in the sequel to have great importance.

(4) The conclusions of the two modes of inference likewise differ. One is necessary; the other only probable. Locke, in the *Essay Concerning Human Understanding*, hints at the correct analysis of the nature of probability. After remarking that the mathematician positively knows that the sum of the three angles of a triangle is equal to two right angles because he apprehends the geometrical proof, he then continues:* "But another man who never took the pains to observe the demonstration, hearing a mathematician, a man of credit, affirm the three angles of a triangle to be equal to two right ones, *assents* to it, that is, receives it for true. In which case, the foundation of his assent is the probability of the thing, the proof being such as, for the most part, carries truth with it; the man on

* Bk. IV, ch. 15, §1.

whose testimony he receives it not being wont to affirm anything contrary to or beside his knowledge, especially in matters of this kind." Those who know Locke are accustomed to look for more meaning in his words than appears at first glance. There is an allusion in this passage to the fact that a probable argument is always regarded as belonging to a *genus* of arguments. This is, in fact, true of any kind of argument. For the belief expressed by the conclusion is determined or caused by the belief expressed by the premisses. There is, therefore, some general rule according to which the one succeeds the other. But, further, the reasoner is conscious of there being such a rule, for otherwise he would not know he was reasoning, and could exercise no attention or control; and to such an involuntary operation the name "reasoning" is very properly not applied. In all cases, then, we are conscious that our inference belongs to a general class of logical forms, although we are not necessarily able to describe the general class. The difference between necessary and probable reasoning is that in the one case we conceive that such facts as are expressed by the premisses are never, in the whole range of possibility, true, without another fact, related to them as our conclusion is to our premisses, being true likewise; while in the other case we merely conceive that, in reasoning as we do, we are following a general maxim that will usually lead us to the truth.

697. So long as there are exceptions to the rule that all men wounded in the liver die, it does not necessarily follow that because a given man is wounded in the liver he cannot recover. Still, we know that if we were to reason in that way, we should be following a mode of inference which would only lead us wrong, in the long run, once in fifty times; and this is what we mean when we say that the probability is one out of fifty that the man will recover. To say, then, that a proposition has the probability ρ means that to infer it to be true would be to follow an argument such as would carry truth with it in the ratio of frequency ρ.

It is plainly useful that we should have a stronger feeling of confidence about a sort of inference which will oftener lead us to the truth than about an inference that will less often prove right — and such a sensation we do have. The celebrated law of Fechner is that as the force acting upon an organ of sense

increases in geometrical progression, the intensity of the sensation increases in arithmetical progression. In this case the odds (that is, the ratio of the chances in favor of a conclusion to the chances against it) take the place of the exciting cause, while the sensation itself is the feeling of confidence. When two arguments tend to the same conclusion, our confidence in the latter is equal to the sum of what the two arguments separately would produce; the *odds* are the product of the *odds* in favor of the two arguments separately. When the value of the *odds* reduces to unity, our confidence is null; when the *odds* are less than unity, we have more or less confidence in the negative of the conclusion.

§2. STATISTICAL DEDUCTION

698. The principle of probable deduction still applies when S, instead of being a single M, is a set of M's — n in number. The reasoning then takes the following form:

FORM III.

Complex Probable Deduction.

Among all sets of n M's, the proportion q consist each of m P's and of $n-m$ not-P's,

S', S'', S''', etc.; form a set of n objects drawn at random from among the M's;

Hence, the probability is q that among S, S', S'', etc. there are m P's and $n-m$ not-P's.

In saying that S, S', S'', etc., form a set drawn at random, we here mean that not only are the different individuals drawn at random, but also that they are so drawn that the qualities which may belong to one have no influence upon the selection of any other. In other words, the individual drawings are independent, and the set as a whole is taken at random from among all possible sets of n M's. In strictness, this supposes that the same individual may be drawn several times in the same set, although if the number of M's is large compared with n, it makes no appreciable difference whether this is the case or not.

699. The following formula expresses the proportion,

among all sets of n M's, of those which consist of m P's and $n-m$ not-P's. The letter r denotes the proportion of P's among the M's, and the sign of admiration is used to express the continued product of all integer numbers from 1 to the number after which it is placed. Thus, $4!=1\cdot2\cdot3\cdot4=24$, etc. The formula is:

$$q=n!\times\frac{r^m}{m!}\times\frac{(1-r)^{n-m}}{(n-m)!}$$

As an example, let us assume the proportion $r=\frac{2}{3}$ and the number of M's in a set $n=15$. Then the values of the probability q for different numbers, m, of P's, are fractions having for their common denominator 14,348,907, and for their numerators as follows:

m	Numerator of q.
0	1
1	30
2	420
3	3640
4	21840
5	96096
6	320320
7	823680

m	Numerator of q.
8	1667360
9	2562560
10	3075072
11	2795520
12	1863680
13	860160
14	122880
15	32768

A very little mathematics would suffice to show that, r and n being fixed, q always reaches its maximum value with that value of m that is next less than $(n+1)r$, [1] and that q is very small unless m has nearly this value.

700. Upon these facts is based another form of inference to which I give the name of statistical deduction. Its general formula is as follows:

[1] In case $(n+1)r$ is a whole number, q has equal values for $m=(n+1)r$ and for $m=(n+1)r-1$.

FORM IV

Statistical Deduction.

The proportion r of the M's are P's,

S', S'', S''', etc. are a *numerous* set, taken at random from among the M's;

Hence, *probably* and *approximately*, the proportion r of the S's are P's.

As an example, take this:

A little more than half of all human births are males;

Hence, probably a little over half of all the births in New York during any one year are males.

We have now no longer to deal with a mere probable inference, but with a *probable approximate* inference. This conception is a somewhat complicated one, meaning that the probability is greater according as the limits of approximation are wider, conformably to the mathematical expression for the values of q.

701. This conclusion has no meaning at all unless there be more than one instance; and it has hardly any meaning unless the instances are somewhat numerous. When this is the case, there is a more convenient way of obtaining (not exactly, but quite near enough for all practical purposes) either a single value of q or the sum of successive values from $m=m_1$ to $m=m_2$ inclusive. The rule is first to calculate two quantities which may conveniently be called t_1 and t_2 according to these formulæ:

$$t_1=\frac{m_1-(n+1)r}{\sqrt{2nr(1-r)}} \qquad t_2=\frac{1+m_2-(n+1)r}{\sqrt{2nr(1-r)}}$$

where $m_2>m_1$. Either or both the quantities t_1 and t_2 may be negative. Next with each of these quantities enter the table below, and take out $\frac{1}{2}\Theta t_1$ and $\frac{1}{2}\Theta t_2$ and give each the same sign as the t from which it is derived. Then

$$\Sigma\, q=\tfrac{1}{2}\,\Theta t_2-\tfrac{1}{2}\Theta t_1.$$

Table of $\Theta t=\frac{2}{\sqrt{\pi}}\int_0^t e^{-t^2}dt$.

t	Θt
0.0	0.000
0.1	0.112
0.2	0.223
0.3	0.329
0.4	0.428
0.5	0.520
0.6	0.604
0.7	0.678
0.8	0.742
0.9	0.797
1.0	0.843

t	Θt
1.0	0.843
1.1	0.880
1.2	0.910
1.3	0.934
1.4	0.952
1.5	0.966
1.6	0.976
1.7	0.984
1.8	0.989
1.9	0.993
2.0	0.995

t	Θt
2.0	0.99532
2.1	0.99702
2.2	0.99814
2.3	0.99886
2.4	0.99931
2.5	0.99959
2.6	0.99976
2.7	0.99987
2.8	0.99992
2.9	0.99996
3.0	0.99998

t	Θ
4	0.999999989
5	0.9999999999984
6	0.99999999999999982
7	0.9999999999999999999999958

In rough calculations we may take Θt equal to t for t less than 0.7, and as equal to *unity* for any value above $t=1.4$.

§3. INDUCTION*

702. The principle of statistical deduction is that these† two proportions — namely, that of the P's among the M's, and that of the P's among the S's — are probably and approximately equal. If, then, this principle justifies our inferring the value of the second proportion from the known value of the first, it equally justifies our inferring the value of the first

* There was no §3 in the original, and the present section formed part of §2.

† "these" is deleted in Peirce's own copy.

from that of the second, if the first is unknown but the second has been observed. We thus obtain the following form of inference:

FORM V

Induction.

S', S'', S''', etc. form a numerous set taken at random from among the M's,

S', S'', S''', etc. are found to be — the proportion ρ of them — P's;

Hence, *probably* and *approximately* the same proportion, ρ, of the M's are P's.

The following are examples. From a bag of coffee a handful is taken out, and found to have nine-tenths of the beans perfect; whence it is inferred that about nine-tenths of all the beans in the bag are probably perfect. The United States Census of 1870 shows that of native white children under one year old, there were 478,774 males to 463,320 females; while of colored children of the same age there were 75,985 males to 76,637 females. We infer that generally there is a larger proportion of female births among negroes than among whites.

703. When the ratio ρ is *unity* or *zero*, the inference is an ordinary induction; and I ask leave to extend the term "induction" to all such inference, whatever be the value of ρ. It is, in fact, inferring from a sample to the whole lot sampled. These two forms of inference, statistical deduction and induction, plainly depend upon the same principle of equality of ratios, so that their validity is the same. Yet the nature of the probability in the two cases is very different. In the statistical deduction, we know that among the whole body of M's the proportion of P's is ρ; we say, then, that the S's being random drawings of M's are probably P's in about the same proportion — and though this may happen not to be so, yet at any rate, on continuing the drawing sufficiently, our prediction of the ratio will be vindicated at last. On the other hand, in induction we say that the proportion ρ of the sample being P's, probably there is about the same proportion in the whole lot; or at least, if this happens not to be so, then on continuing the drawings the inference will be, not *vindicated* as

in the other case, but *modified* so as to become true. The deduction, then, is probable in this sense, that though its conclusion may in a particular case be falsified, yet similar conclusions (with the same ratio ρ) would generally prove approximately true; while the induction is probable in this sense, that though it may happen to give a false conclusion, yet in most cases in which the same precept of inference was followed, a different and approximately true inference (with the right value of ρ) would be drawn.

§4. HYPOTHETIC INFERENCE

704. Before going any further with the study of Form V, I wish to join to it another extremely analogous form.

We often speak of one thing being very much like another, and thus apply a vague quantity to resemblance. Even if qualities are not subject to exact numeration, we may conceive them to be approximately measurable. We may then measure resemblance by a scale of numbers from zero up to unity. To say that S has a 1-likeness to a P will mean that it has every character of a P, and consequently *is* a P. To say that it has a 0-likeness will imply total dissimilarity. We shall then be able to reason as follows:

FORM II (*bis*).

Simple probable deduction in depth.

Every M has the simple mark P,
The S's have an r-likeness to the M's;
Hence, the probability is r that every S is P.

It would be difficult, perhaps impossible, to adduce an example of such kind of inference, for the reason that *simple marks* are not known to us. We may, however, illustrate the complex probable deduction in depth (the general form of which it is not worth while to set down) as follows: I forget whether, in the ritualistic churches, a bell is tinkled at the elevation of the Host or not. Knowing, however, that the services resemble somewhat decidedly those of the Roman Mass, I think that it is not unlikely that the bell is used in the ritualistic, as in the Roman, churches.

705. We shall also have the following:

FORM IV (*bis*).

Statistical deduction in depth.

Every M has, for example, the numerous marks P', P'', P''', etc.,

S has an r-likeness to the M's;

Hence, probably and approximately, S has the proportion r of the marks P', P'', P''', etc.

For example, we know that the French and Italians are a good deal alike in their ideas, characters, temperaments, genius, customs, institutions, etc., while they also differ very markedly in all these respects. Suppose, then, that I know a boy who is going to make a short trip through France and Italy; I can safely predict that among the really numerous though relatively few respects in which he will be able to compare the two people, about the same degree of resemblance will be found.

Both these modes of inference are clearly deductive. When $r=1$, they reduce to Barbara.[1]

706. Corresponding to induction, we have the following mode of inference:

FORM V (*bis*).

Hypothesis.

M has, for example, the numerous marks P', P'', P''', etc.,

S has the proportion r of the marks P', P'', P''', etc.;

Hence, probably and approximately, S has an r-likeness to M.

[1] When $r=0$, the last form becomes

M has all the marks P,
S has no mark of M;
Hence, S has none of the marks P.

When the universe of marks is unlimited (see a note appended to this paper for an explanation of this expression [519]), the only way in which two terms can fail to have a common mark is by their together filling the universe of things; and consequently this form then becomes

M is P,
Every non-S is M;
Hence, every non-S is P.

This is one of De Morgan's syllogisms.

In putting $r=0$ in Form II (*bis*) it must be noted that, since P is simple in depth, to say that S is not P is to say that it has no mark of P.

Thus, we know, that the ancient Mound-builders of North America present, in all those respects in which we have been able to make the comparison, a limited degree of resemblance with the Pueblo Indians. The inference is, then, that in all respects there is about the same degree of resemblance between these races.

If I am permitted the extended sense which I have given to the word "induction," this argument is simply an induction respecting qualities instead of respecting things. In point of fact P', P'', P''', etc., constitute a random sample of the characters of M, and the ratio r of them being found to belong to S, the same ratio of all the characters of M are concluded to belong to S. This kind of argument, however, as it actually occurs, differs very much from induction, owing to the impossibility of simply counting qualities as individual things are counted. Characters have to be weighed rather than counted. Thus, antimony is bluish-gray: that is a character. Bismuth is a sort of rose-gray; it is decidedly different from antimony in color, and yet not so very different as gold, silver, copper, and tin are.

707. I call this induction of characters *hypothetic inference*,* or, briefly, *hypothesis*. This is perhaps not a very happy designation, yet it is difficult to find a better. The term "hypothesis" has many well established and distinct meanings. Among these is that of a proposition believed in because its consequences agree with experience. This is the sense in which Newton used the word when he said, *Hypotheses non fingo*. He meant that he was merely giving a general formula for the motions of the heavenly bodies, but was not undertaking to mount to the causes of the acceleration they exhibit. The inferences of Kepler, on the other hand, were hypotheses in this sense; for he traced out the miscellaneous consequences of the supposition that Mars moved in an ellipse, with the sun at the focus, and showed that both the longitudes and the latitudes resulting from this theory were such as agreed with observation. These two components of the motion were observed; the third, that of approach to or regression from the earth, was supposed. Now, if in Form V (*bis*) we put $r=1$, the inference is the drawing of a hypothesis in this sense. I

* Cf. 102.

take the liberty of extending the use of the word by permitting r to have any value from zero to unity. The term is certainly not all that could be desired; for the word hypothesis, as ordinarily used, carries with it a suggestion of uncertainty, and of something to be superseded, which does not belong at all to my use of it. But we must use existing language as best we may, balancing the reasons for and against any mode of expression, for none is perfect; at least the term is not so utterly misleading as "analogy" would be, and with proper explanation it will, I hope, be understood.

§5. GENERAL CHARACTERS OF DEDUCTION, INDUCTION, AND HYPOTHESIS

708. The following examples will illustrate the distinction between statistical deduction, induction, and hypothesis. If I wished to order a font of type expressly for the printing of this book, knowing, as I do, that in all English writing the letter *e* occurs oftener than any other letter, I should want more *e*'s in my font than other letters. For what is true of all other English writing is no doubt true of these papers. This is a statistical deduction. But then the words used in logical writings are rather peculiar, and a good deal of use is made of single letters. I might, then, count the number of occurrences of the different letters upon a dozen or so pages of the manuscript, and thence conclude the relative amounts of the different kinds of type required in the font. That would be inductive inference. If now I were to order the font, and if, after some days, I were to receive a box containing a large number of little paper parcels of very different sizes, I should naturally infer that this was the font of types I had ordered; and this would be hypothetic inference. Again, if a dispatch in cipher is captured, and it is found to be written with twenty-six characters, one of which occurs much more frequently than any of the others, we are at once led to suppose that each character represents a letter, and that the one occurring so frequently stands for *e*. This is also hypothetic inference.

709. We are thus led to divide all probable reasoning into deductive and ampliative, and further to divide ampliative reasoning into induction and hypothesis. In deductive reasoning, though the predicted ratio may be wrong in a limited number of drawings, yet it will be approximately verified in a larger number. In ampliative reasoning the ratio may be wrong, because the inference is based on but a limited number of instances; but on enlarging the sample the ratio will be changed till it becomes approximately correct. In induction, the instances drawn at random are numerable things; in hypothesis they are characters, which are not capable of strict enumeration, but have to be otherwise estimated.

710. This classification of probable inference is connected with a preference for the copula of inclusion over those used by Miss Ladd [Mrs. Christine Ladd-Franklin] and by Mr. Mitchell.[1] De Morgan established eight forms of simple propositions; and from a purely formal point of view no one of these has a right to be considered as more fundamental than any other. But formal logic must not be too purely formal; it must represent a fact of psychology, or else it is in danger of degenerating into a mathematical recreation. The categorical proposition, "every man is mortal," is but a modification of the hypothetical proposition, "if humanity, then mortality"; and since the very first conception from which logic springs is that one proposition follows from another, I hold that "if A, then B" should be taken as the typical form of judgment. Time flows; and, in time, from one state of belief (represented by the premisses of an argument) another (represented by its conclusion) is developed. Logic arises from this circumstance, without which we could not learn anything nor correct any opinion. To say that an inference is correct is to say that if the premisses are true the conclusion is also true; or that every possible state of things in which the premisses should be true would be included among the possible states of things in which the conclusion would be true. We are thus led to the copula of inclusion. But the main characteristic of the relation of inclusion is that it is transitive — that is, that what is included in something included in anything is itself included in that thing; or, that if A is B and B is C, then A is C. We thus get *Barbara* as the primitive type of inference. Now in *Barbara*

[1] I do not here speak of Mr. Jevons, because my objection to the copula of identity is of a somewhat different kind. [See *Studies in Logic*, pp. 17–69 and 72–106 for Miss Ladd's and Mr. Mitchell's papers.]

we have a *Rule*, a *Case* under the *Rule*, and the inference of the *Result* of that rule in that case. For example:

Rule. All men are mortal,
Case. Enoch was a man;
Result. ∴ Enoch was mortal.

711. The cognition of a rule is not necessarily conscious, but is of the nature of a habit, acquired or congenital. The cognition of a case is of the general nature of a sensation; that is to say, it is something which comes up into present consciousness. The cognition of a result is of the nature of a decision to act in a particular way on a given occasion.[1] In point of fact, a syllogism in *Barbara* virtually takes place when we irritate the foot of a decapitated frog. The connection between the afferent and efferent nerve, whatever it may be, constitutes a nervous habit, a rule of action, which is the physiological analogue of the major premiss. The disturbance of the ganglionic equilibrium, owing to the irritation, is the physiological form of that which, psychologically considered, is a sensation; and, logically considered, is the occurrence of a case. The explosion through the efferent nerve is the physiological form of that which psychologically is a volition, and logically the inference of a result. When we pass from the lowest to the highest forms of inervation, the physiological equivalents escape our observation; but, psychologically, we still have, first, habit — which in its highest form is understanding, and which corresponds to the major premiss of *Barbara;* we have, second, feeling, or present consciousness, corresponding to the minor premiss of *Barbara;* and we have, third, volition, corresponding to the conclusion of the same mode of syllogism. Although these analogies, like all very broad generalizations, may seem very fanciful at first sight, yet the more the reader reflects upon them the more profoundly true I am confident they will appear. They give a significance to the ancient system of formal logic which no other can at all share.

712. Deduction proceeds from Rule and Case to Result; it is the formula of Volition. Induction proceeds from Case and Result to Rule; it is the formula of the formation of a

[1] See my paper on "How to make our ideas clear." [Vol. 5, bk. II, ch. 5.]

habit or general conception — a process which, psychologically as well as logically, depends on the repetition of instances or sensations. Hypothesis proceeds from Rule and Result to Case; it is the formula of the acquirement of secondary sensation — a process by which a confused concatenation of predicates is brought into order under a synthetizing predicate.*

713. We usually conceive Nature to be perpetually making deductions in *Barbara*. This is our natural and anthropomorphic metaphysics. We conceive that there are Laws of Nature, which are her Rules or major premisses. We conceive that Cases arise under these laws; these cases consist in the predication, or occurrence, of *causes*, which are the middle terms of the syllogisms. And, finally, we conceive that the occurrence of these causes, by virtue of the laws of Nature, results in effects which are the conclusions of the syllogisms. Conceiving of nature in this way, we naturally conceive of science as having three tasks — (1) the discovery of Laws, which is accomplished by induction; (2) the discovery of Causes, which is accomplished by hypothetic inference; and (3) the predictio of Effects, which is accomplished by deduction. It appears to me to be highly useful to select a system of logic which shall preserve all these natural conceptions.

714. It may be added that, generally speaking, the conclusions of Hypothetic Inference cannot be arrived at inductively, because their truth is not susceptible of direct observation in single cases. Nor can the conclusions of Inductions, on account of their generality, be reached by hypothetic inference. For instance, any historical fact, as that Napoleon Bonaparte once lived, is a hypothesis; we believe the fact, because its effects — I mean current tradition, the histories, the monuments, etc. — are observed. But no mere generalization of observed facts could ever teach us that Napoleon lived. So we inductively infer that every particle of matter gravitates toward every other. Hypothesis might lead to this result for any given pair of particles, but it never could show that the law was universal.

* Cf. 643.

§6. INDUCTION AND HYPOTHESIS

INDIRECT STATISTICAL INFERENCES; GENERAL RULE FOR THEIR VALIDITY

715. We now come to the consideration of the Rules which have to be followed in order to make valid and strong Inductions and Hypotheses. These rules can all be reduced to a single one; namely, that the statistical deduction of which the Induction or Hypothesis is the inversion, must be valid and strong.

716. We have seen that Inductions and Hypotheses are inferences from the conclusion and one premiss of a statistical syllogism to the other premiss. In the case of hypothesis, this syllogism is called the *explanation*. Thus in one of the examples used above, we suppose the cryptograph to be an English cipher, because, as we say, this *explains* the observed phenomena that there are about two dozen characters, that one occurs more frequently than the rest, especially at the end of words, etc. The explanation is —

Simple English ciphers have certain peculiarities,
This is a simple English cipher;
Hence, this necessarily has these peculiarities.

717. This explanation is present to the mind of the reasoner, too; so much so, that we commonly say that the hypothesis is adopted *for the sake of* the explanation. Of induction we do not, in ordinary language, say that it explains phenomena; still, the statistical deduction, of which it is the inversion, plays, in a general way, the same part as the explanation in hypothesis. From a barrel of apples, that I am thinking of buying, I draw out three or four as a sample. If I find the sample somewhat decayed, I ask myself, in ordinary language, not "Why is this?" but "How is this?" And I answer that it probably comes from nearly all the apples in the barrel being in bad condition. The distinction between the "Why" of hypothesis and the "How" of induction is not very great; both ask for a statistical syllogism, of which the observed fact shall be the conclusion, the known conditions of the observation one premiss, and the inductive or hypothetic inference the other. This statistical syllogism may be conveniently termed the explanatory syllogism.

718. In order that an induction or hypothesis should have

any validity at all, it is requisite that the explanatory syllogism should be a valid statistical deduction. Its conclusion must not merely follow from the premisses, but follow from them upon the principle of probability. The inversion of *ordinary* syllogism does not give rise to an induction or hypothesis. The statistical syllogism of Form IV is invertible, because it proceeds upon the principle of an approximate *equality* between the ratio of P's in the whole class and the ratio in a well-drawn sample, and because equality is a convertible relation. But ordinary syllogism is based upon the property of the relation of containing and contained, and that is not a convertible relation. There is, however, a way in which ordinary syllogism may be inverted; namely, the conclusion and either of the premisses may be interchanged by negativing each of them. This is the way in which the indirect, or apagogical,[1] figures of syllogism are derived from the first, and in which the *modus tollens* is derived from the *modus ponens*. The following schemes show this:

First Figure.

Rule. All M is P,
Case. S is M;
Result. S is P.

Second Figure.		*Third Figure.*	
Rule.	All M is P,	*Denial of Result.*	S is not P,
Denial of Result.	S is not P;	*Case.*	S is M;
Denial of Case.	$\therefore S$ is not M.	*Denial of Rule.*	$\therefore$ Some M is not P.

Modus Ponens.

Rule. If A is true, C is true,
Case. In a certain case A is true;
Result. $\therefore$ In that case C is true.

Modus Tollens.	*Modus Innominatus.*
Rule. If A is true, C is true,	*Case.* In a certain case A is true,
Denial of Result. In a certain case C is not true;	*Denial of Result.* In that case, C is not true;
Denial of Case. $\therefore$ In that case A is not true.	*Denial of Rule.* $\therefore$ If A is true, C is not necessarily true.

[1] From *apagoge*, ἀπαγωγὴ εἰς τὸ ἀδύνατον, Aristotle's name for the *reductio ad absurdum*.

719. Now suppose we ask ourselves what would be the result of thus apagogically inverting a statistical deduction. Let us take, for example, Form IV:

The S's are a numerous random sample of the M's,

The proportion r of the M's are P's;

Hence, probably about the proportion r of the S's are P's.

720. The ratio r, as we have already noticed, is not necessarily perfectly definite; it may be only known to have a certain maximum or minimum; in fact, it may have any kind of indeterminacy. Of all possible values between 0 and 1, it admits of some and excludes others. The logical negative of the ratio r is, therefore, itself a ratio, which we may name ρ; it admits of every value which r excludes, and excludes every value of which r admits. Transposing, then, the major premiss and conclusion of our statistical deduction, and at the same time denying both, we obtain the following inverted form:

The S's are a numerous random sample of the M's,

The proportion ρ of the S's are P's;

Hence, probably about the proportion ρ of the M's are P's.[1]

721. But this coincides with the formula of Induction. Again, let us apagogically invert the statistical deduction of Form IV (*bis*). This form is —

Every M has, for example, the numerous marks P', P'', P''', etc.,

S has an r-likeness to the M's;

Hence, probably and approximately, S has the proportion r of the marks P', P'', P''', etc.

Transposing the minor premiss and conclusion, at the same time denying both, we get the inverted form —

Every M has, for example, the numerous marks P', P'', P''', etc.,

S has the proportion ρ of the marks P', P'', P''', etc.;

Hence, probably and approximately, S has a ρ-likeness to the class of M's.

[1] The conclusion of the statistical deduction is here regarded as being "the proportion r of the S's are P's," and the words "probably about" as indicating the modality with which this conclusion is drawn and held for true. It would be equally true to consider the "probably about" as forming part of the contents of the conclusion; only from that point of view the inference ceases to be probable, and becomes rigidly necessary, and its apagogical inversion is also a necessary inference presenting no particular interest.

722. This coincides with the formula of Hypothesis. Thus we see that Induction and Hypothesis are nothing but the apagogical inversions of statistical deductions. Accordingly, when r is taken as 1, so that ρ is "less than 1," or when r is taken as 0, so that ρ is "more than 0," the induction degenerates into a syllogism of the third figure and the hypothesis into a syllogism of the second figure. In these special cases, there is no very essential difference between the mode of reasoning in the direct and in the apagogical form. But, in general, while the probability of the two forms is precisely the same — in this sense, that for any fixed proportion of P's among the M's (or of marks of S's among the marks of the M's) the probability of any given error in the concluded value is precisely the same in the indirect as it is in the direct form — yet there is this striking difference, that a multiplication of instances will in the one case *confirm*, and in the other *modify*, the concluded value of the ratio.

723. We are thus led to another form for our rule of validity of ampliative inference; namely, instead of saying that the *explanatory* syllogism must be a good probable deduction, we may say that the syllogism of which the induction or hypothesis is the apagogical modification (in the traditional language of logic, the *reduction*) must be valid.

724. Probable inferences, though valid, may still differ in their strength. A probable deduction has a greater or less probable error in the concluded ratio. When r is a definite number the probable error is also definite; but as a general rule we can only assign maximum and minimum values of the probable error. The probable error is, in fact —

$$0.477\sqrt{\frac{2r(1-r)}{n}}$$

where n is the number of independent instances. The same formula gives the probable error of an induction or hypothesis; only that in these cases, r being wholly indeterminate, the minimum value is *zero*, and the maximum is obtained by putting $r=\frac{1}{2}$.

§7. FIRST SPECIAL RULE FOR SYNTHETIC INFERENCE.

SAMPLING MUST BE FAIR. ANALOGY

725. Although the rule given above really contains all the conditions to which Inductions and Hypotheses need to conform, yet inasmuch as there are many delicate questions in regard to the application of it, and particularly since it is of that nature that a violation of it, if not too gross, may not absolutely destroy the virtue of the reasoning, a somewhat detailed study of its requirements in regard to each of the premisses of the argument is still needed.

726. The first premiss of a scientific inference is that certain things (in the case of induction) or certain characters (in the case of hypothesis) constitute a fairly chosen *sample* of the class of things or the run of characters from which they have been drawn.

The rule requires that the sample should be drawn at random and independently from the whole lot sampled. That is to say, the sample must be taken according to a precept or method which, being applied over and over again indefinitely, would in the long run result in the drawing of any one set of instances as often as any other set of the same number.

727. The needfulness of this rule is obvious; the difficulty is to know how we are to carry it out. The usual method is mentally to run over the lot of objects or characters to be sampled, abstracting our attention from their peculiarities, and arresting ourselves at this one or that one from motives wholly unconnected with those peculiarities. But this abstention from a further determination of our choice often demands an effort of the will that is beyond our strength; and in that case a mechanical contrivance may be called to our aid. We may, for example, number all the objects of the lot, and then draw numbers by means of a roulette, or other such instrument. We may even go so far as to say that this method is the type of all random drawing; for when we abstract our attention from the peculiarities of objects, the psychologists tell us that what we do is to substitute for the images of sense certain mental signs, and when we proceed to a random and arbitrary choice among these abstract objects we are governed by fortuitous determinations of the nervous system, which in this case serves the purpose of a roulette.

The drawing of objects at random is an act in which honesty is called for; and it is often hard enough to be sure that we have dealt honestly with ourselves in the matter, and still more hard to be satisfied of the honesty of another. Accordingly, one method of sampling has come to be preferred in argumentation; namely, to take of the class to be sampled all the objects of which we have a sufficient knowledge. Sampling is, however, a real art, well deserving an extended study by itself: to enlarge upon it here would lead us aside from our main purpose.

728. Let us rather ask what will be the effect upon inductive inference of an imperfection in the strictly random character of the sampling. Suppose that, instead of using such a precept of selection that any one M would in the long run be chosen as often as any other, we used a precept which would give a preference to a certain half of the M's, so that they would be drawn twice as often as the rest. If we were to draw a numerous sample by such a precept, and if we were to find that the proportion ρ of the sample consisted of P's, the inference that we should be regularly entitled to make would be, that among all the M's, counting the preferred half for two each, the proportion ρ would be P's. But this regular inductive inference being granted, from it we could deduce by arithmetic the further conclusion that, counting the M's for one each, the proportion of P's among them must (ρ being over $\frac{2}{3}$) lie between $\frac{3}{4}\rho+\frac{1}{4}$ and $\frac{3}{2}\rho-\frac{1}{2}$. Hence, if more than two thirds of the instances drawn by the use of the false precept were found to be P's, we should be entitled to conclude that more than half of all the M's were P's. Thus, without allowing ourselves to be led away into a mathematical discussion, we can easily see that, in general, an imperfection of that kind in the random character of the sampling will only weaken the inductive conclusion, and render the concluded ratio less determinate, but will not necessarily destroy the force of the argument completely. In particular, when ρ approximates towards 1 or 0, the effect of the imperfect sampling will be but slight.

729. Nor must we lose sight of the constant tendency of

the inductive process to correct itself. This is of its essence. This is the marvel of it. The probability of its conclusion only consists in the fact that if the true value of the ratio sought has not been reached, an extension of the inductive process will lead to a closer approximation. Thus, even though doubts may be entertained whether one selection of instances is a random one, yet a different selection, made by a different method, will be likely to vary from the normal in a different way, and if the ratios derived from such different selections are nearly equal, they may be presumed to be near the truth. This consideration makes it extremely advantageous in all ampliative reasoning to fortify one method of investigation by another.[1] Still we must not allow ourselves to trust so much to this virtue of induction as to relax our efforts towards making our drawings of instances as random and independent as we can. For if we infer a ratio from a number of different inductions, the magnitude of its probable error will depend very much more on the worst than on the best inductions used.

730. We have, thus far, supposed that although the selection of instances is not exactly regular, yet the precept followed is such that every unit of the lot would eventually get drawn. But very often it is impracticable so to draw our instances, for the reason that a part of the lot to be sampled is absolutely inaccessible to our powers of observation. If we want to know whether it will be profitable to open a mine, we sample the ore; but in advance of our mining operations, we can obtain only what ore lies near the surface. Then, simple induction becomes worthless, and another method must be resorted to. Suppose we wish to make an induction regarding a series of events extending from the distant past to the distant future; only those events of the series which occur within the period of time over which available history extends can be taken as instances. Within this period we may find that the events of the class in question present some uniform character; yet how do we know but this uniformity was suddenly established a little while before the history commenced, or will suddenly break up a little while after it terminates? Now, whether the uniformity observed consists (1) in a mere resemblance between all the phenomena, or (2) in their consisting of a disorderly mixture of two kinds in a certain constant proportion, or (3) in the character of the events being a mathematical function of the time of occurrence — in any of these cases we can make use of an apagoge from the following probable deduction:

Within the period of time M, a certain event P occurs,

S is a period of time taken at random from M, and more than half as long;

Hence, probably the event P will occur within the time S.

Inverting this deduction, we have the following ampliative inference:

S is a period of time taken at random from M, and more than half as long,

The event P does not happen in the time S;

Hence, probably the event P does not happen in the period M.

The probability of the conclusion consists in this, that we here follow a precept of inference, which, if it is very often applied will more than half the time lead us right. Analogous reasoning would obviously apply to any portion of an unidimensional continuum, which might be similar to periods of time. This is a sort of logic which is often applied by physicists in what is called *extrapolation* of an empirical law. As compared with a typical induction, it is obviously an excessively weak kind of inference. Although indispensable in almost every branch of science, it can lead to no solid conclusions in regard to what is remote from the field of direct perception, unless it be bolstered up in certain ways to which we shall have occasion to refer further on.

731. Let us now consider another class of difficulties in regard to the rule that the samples must be drawn at random

[1] This I conceive to be all the truth there is in the doctrine of Bacon and Mill regarding different Methods of Experimental Inquiry. The main proposition of Bacon's and Mill's doctrine is, that in order to prove that all M's are P's, we should not only take random instances of the M's and examine them to see that they are P's, but we should also take instances of not-P's and examine them to see that they are not-M's. This is an excellent way of fortifying one induction by another, when it is applicable; but it is entirely inapplicable when r has any other value than 1 or 0. For, in general, there is no connection between the proportion of M's that are P's and the proportion of non-P's that are non-M's. A very small proportion of calves may be monstrosities, and yet a very large proportion of monstrosities may be calves.

and independently. In the first place, what if the lot to be sampled be infinite in number? In what sense could a random sample be taken from a lot like that? A random sample is one taken according to a method that would, in the long run, draw any one object as often as any other. In what sense can such drawing be made from an infinite class? The answer is not far to seek. Conceive a cardboard disk revolving in its own plane about its centre, and pretty accurately balanced, so that when put into rotation it shall be about[1] as likely to come to rest in any one position as in any other; and let a fixed pointer indicate a position on the disk: the number of points on the circumference is infinite, and on rotating the disk repeatedly the pointer enables us to make a selection from this infinite number. This means merely that although the points are innumerable, yet there is a certain order among them that enables us to run them through and pick from them as from a very numerous collection. In such a case, and in no other, can an infinite lot be sampled. But it would be equally true to say that a finite lot can be sampled only on condition that it can be regarded as equivalent to an infinite lot. For the random sampling of a finite class supposes the possibility of drawing out an object, throwing it back, and continuing this process indefinitely; so that what is really sampled is not the finite collection of things, but the unlimited number of possible drawings.

732. But though there is thus no insuperable difficulty in sampling an infinite lot, yet it must be remembered that the conclusion of inductive reasoning only consists in the approximate evaluation of a *ratio*, so that it never can authorize us to conclude that in an infinite lot sampled there exists no single exception to a rule. Although all the planets are found to gravitate toward one another, this affords not the slightest direct reason for denying that among the innumerable orbs of heaven there may be some which exert no such force. Although at no point of space where we have yet been have we found any possibility of motion in a fourth dimension, yet this does not tend to show (by simple induction, at least) that space has absolutely but three dimensions. Although all the bodies

[1] I say *about*, because the doctrine of probability only deals with approximate evaluations.

we have had the opportunity of examining appear to obey the law of inertia, this does not prove that atoms and atomicules are subject to the same law. Such conclusions must be reached, if at all, in some other way than by simple induction. This latter may show that it is unlikely that, in my lifetime or yours, things so extraordinary should be found, but [does] not warrant extending the prediction into the indefinite future. And experience shows it is not safe to predict that such and such a fact will *never* be met with.

733. If the different instances of the lot sampled are to be drawn independently, as the rule requires, then the fact that an instance has been drawn once must not prevent its being drawn again. It is true that if the objects remaining unchosen are very much more numerous than those selected, it makes practically no difference whether they have a chance of being drawn again or not, since that chance is in any case very small. Probability is wholly an affair of approximate, not at all of exact, measurement; so that when the class sampled is very large, there is no need of considering whether objects can be drawn more than once or not. But in what is known as "reasoning from analogy," the class sampled is small, and no instance is taken twice. For example: we know that of the major planets the Earth, Mars, Jupiter, and Saturn revolve on their axes, and we conclude that the remaining four, Mercury, Venus, Uranus, and Neptune, probably do the like. This is essentially different from an inference from what has been found in drawings made hitherto, to what will be found in indefinitely numerous drawings to be made hereafter. Our premisses here are that the Earth, Mars, Jupiter, and Saturn are a random sample of a natural class of major planets — a class which, though (so far as we know) it is very small, yet *may* be very extensive, comprising whatever there may be that revolves in a circular orbit around a great sun, is nearly spherical, shines with reflected light, is very large, etc. Now the examples of major planets that we can examine all rotate on their axes; whence we suppose that Mercury, Venus, Uranus, and Neptune, since they possess, so far as we know, all the properties common to the natural class to which the Earth, Mars, Jupiter, and Saturn belong, possess this property likewise. The points to be observed are, first, that any small class

of things may be regarded as a mere sample of an actual or possible large class having the same properties and subject to the same conditions; second, that while we do not know what all these properties and conditions are, we do know some of them, which some may be considered as a random sample of all; third, that a random selection without replacement from a small class may be regarded as a true random selection from that infinite class of which the finite class is a random selection. The formula of the analogical inference presents, therefore, three premisses, thus:

S', S'', S''', are a random sample of some undefined class X, of whose characters P', P'', P''', are samples,
Q is P', P'', P''';
S', S'', S''', are R's;
Hence, Q is an R.

We have evidently here an induction and an hypothesis followed by a deduction; thus:

Every X is, for example, P', P'', P''', etc.,	S', S'', S''', etc., are samples of the X's,
Q is found to be P', P'', P''', etc.;	S', S'', S''', etc., are found to be R's;
Hence, hypothetically, Q is an X.	Hence, inductively, every X is an R.

Hence, deductively, Q is an R.[1]

[1] That this is really a correct analysis of the reasoning can be shown by the theory of probabilities. For the expression

$$\frac{(p+q)!}{p!q!}\,\frac{(\pi+\rho)!}{\pi!\rho!}\cdot\frac{(p+\pi)!(q+\rho)!}{(p+\pi+q+\rho)!}$$

expresses at once the probability of two events; namely, it expresses first the probability that of $p+q$ objects drawn without replacement from a lot consisting of $p+\pi$ objects having the character R together with $q+\rho$ not having this character, the number of those drawn having this character will be p; and second, the same expression denotes the probability that if among $p+\pi+q+\rho$ objects drawn at random from an infinite class (containing no matter what proportion of R's to non-R's), it happens that $p+\pi$ have the character R, then among any $p+q$ of them, designated at random, p will have the same character. Thus we see that the chances in reference to drawing without replacement from a finite class are precisely the same as those in reference to a class which has been drawn at random from an infinite class.

734. An argument from analogy may be strengthened by the addition of instance after instance to the premisses, until it loses its ampliative character by the exhaustion of the class and becomes a mere deduction of that kind called *complete induction*, in which, however, some shadow of the inductive character remains, as this name implies.

§8. *SECOND SPECIAL RULE FOR SYNTHETIC INFERENCE, THAT OF PREDESIGNATION

735. Take any human being, at random — say Queen Elizabeth. Now a little more than half of all the human beings who have ever existed have been males; but it does not follow that it is a little more likely than not that Queen Elizabeth was a male, since we know she was a woman. Nor, if we had selected Julius Cæsar, would it be only a little more likely than not that he was a male. It is true that if we were to go on drawing at random an indefinite number of instances of human beings, a slight excess over one-half would be males. But that which constitutes the probability of an inference is the proportion of true conclusions among all those which could be derived *from the same precept.* Now a precept of inference, being a rule which the mind is to follow, changes its character and becomes different when the case presented to the mind is essentially different. When, knowing that the proportion r of all M's are P's, I draw an instance, S, of an M, without any other knowledge of whether it is a P or not, and infer with probability, r, that it is P, the case presented to my mind is very different from what it is if I have such other knowledge. In short, I cannot make a valid probable inference without taking into account whatever knowledge I have (or, at least, whatever occurs to my mind) that bears upon the question.

736. The same principle may be applied to the statistical deduction of Form IV. If the major premiss, that the proportion r of the M's are P's be laid down first, before the instances of M's are drawn, we really draw our inference concerning those instances (that the proportion r of them will be P's) in advance of the drawing, and therefore before we know whether they are P's or not. But if we draw the instances of the M's first, and after the examination of them decide what we will select for the predicate of our major premiss, the

inference will generally be completely fallacious. In short, we have the rule that the major term P must be decided upon in advance of the examination of the sample; and in like manner in Form IV (*bis*) the minor term S must be decided upon in advance of the drawing.

737. The same rule follows us into the logic of induction and hypothesis. If in sampling any class, say the M's, we first decide what the character P is for which we propose to sample that class, and also how many instances we propose to draw, our inference is really made before these latter are drawn, that the proportion of P's in the whole class is probably about the same as among the instances that are to be drawn, and the only thing we have to do is to draw them and observe the ratio. But suppose we were to draw our inferences without the predesignation of the character P; then we might in every case find some recondite character in which those instances would all agree. That, by the exercise of sufficient ingenuity, we should be sure to be able to do this, even if not a single other object of the class M possessed that character, is a matter of demonstration. For in geometry a curve may be drawn through any given series of points, without passing through any one of another given series of points, and this irrespective of the number of dimensions. Now, all the qualities of objects may be conceived to result from variations of a number of continuous variables; hence any lot of objects possesses some character in common, not possessed by any other. It is true that if the universe of quality is limited, this is not altogether true; but it remains true that unless we have some special premiss from which to infer the contrary, it always *may* be possible to assign some common character of the instances S', S'', S''', etc., drawn at random from among the M's, which does not belong to the M's generally. So that if the character P were not predesignate, the deduction of which our induction is the apagogical inversion would not be valid; that is to say, we could not reason that if the M's did not generally possess the character P, it would not be likely that the S's should all possess this character.

738. I take from a biographical dictionary* the first five names of poets, with their ages at death. They are,

* Wheeler's *Biographical Dictionary*.

Aagard, died at 48.
Abeille, died at 76.
Abulola, died at 84.
Abunowas, died at 48.
Accords, died at 45.

These five ages have the following characters in common:

1. The difference of the two digits composing the number, divided by three, leaves a remainder of *one*.

2. The first digit raised to the power indicated by the second, and then divided by three, leaves a remainder of *one*.

3. The sum of the prime factors of each age, including *one* as a prime factor, is divisible by *three*.

Yet there is not the smallest reason to believe that the next poet's age would possess these characters.

Here we have a *conditio sine qua non* of valid induction which has been singularly overlooked by those who have treated of the logic of the subject, and is very frequently violated by those who draw inductions. So accomplished a reasoner as Dr. Lyon Playfair, for instance, has written a paper of which the following is an abstract. He first takes the specific gravities of the three allotropic forms of carbon, as follows:

Diamond, 3.48.
Graphite, 2.29.
Charcoal, 1.88.

He now seeks to find a uniformity connecting these three instances; and he discovers that the atomic weight of carbon, being 12,

$$\text{Sp. gr. diamond nearly} = 3.46 = \sqrt[3]{12}$$
$$\text{Sp. gr. graphite nearly} = 2.29 = \sqrt[3]{12}$$
$$\text{Sp. gr. charcoal nearly} = 1.86 = \sqrt[4]{12}$$

This, he thinks, renders it probable that the specific gravities of the allotropic forms of other elements would, if we knew them, be found to equal the different roots of their atomic weight. But so far, the character in which the instances agree not having been predesignated, the induction can serve only to suggest a question, and ought not to create any belief. To test the proposed law, he selects the instance of silicon, which

like carbon exists in a diamond and in a graphitoidal condition. He finds for the specific gravities —

Diamond silicon, 2.47
Graphite silicon, 2.33.[1]

Now, the atomic weight of silicon, that of carbon being 12, can only be taken as 28. But 2.47 does not approximate to any root of 28. It is, however, nearly the cube root of 14, ($\sqrt[3]{\frac{1}{2}\times 28}=2.41$), while 2.33 is nearly the fourth root of 28 ($\sqrt[4]{28}=2.30$). Dr. Playfair claims that silicon is an instance satisfying his formula. But in fact this instance requires the formula to be modified; and the modification not being predesignate, the instance cannot count. Boron also exists in a diamond and a graphitoidal form; and accordingly Dr. Playfair takes this as his next example. Its atomic weight is 10.9, and its specific gravity is 2.68; which is the square root of $\frac{2}{3}\times 10.9$. There seems to be here a further modification of the formula not predesignated, and therefore this instance can hardly be reckoned as confirmatory. The next instances which would occur to the mind of any chemist would be phosphorus and sulphur, which exist in familiarly known allotropic forms. Dr. Playfair admits that the specific gravities of phosphorus have no relations to its atomic weight at all analogous to those of carbon. The different forms of sulphur have nearly the same specific gravity, being approximately the fifth root of the atomic weight 32. Selenium also has two allotropic forms, whose specific gravities are 4.8 and 4.3; one of these follows the law, while the other does not. For tellurium the law fails altogether; but for bromine and iodine it holds. Thus the number of specific gravities for which the law was predesignate are 8; namely, 2 for phosphorus, 1 for sulphur, 2 for selenium, 1 for tellurium, 1 for bromine, and 1 for iodine. The law holds for 4 of these, and the proper inference is that about half the specific gravities of metalloids are roots of some simple ratio of their atomic weights.

[1] The author ought to have noted that this number is open to some doubt, since the specific gravity of this form of silicon appears to vary largely. If a different value had suited the theory better, he might have been able to find reasons for preferring that other value. But I do not mean to imply that Dr. Playfair has not dealt with perfect fairness with his facts, except as to the fallacy which I point out.

Having thus determined this ratio, we proceed to inquire whether an agreement half the time with the formula constitutes any special connection between the specific gravity and the atomic weight of a metalloid. As a test of this, let us arrange the elements in the order of their atomic weights, and compare the specific gravity of the first with the atomic weight of the last, that of the second with the atomic weight of the last but one, and so on. The atomic weights are —

Boron,	10.9	Tellurium,	128.1
Carbon,	12.0	Iodine,	126.9
Silicon,	28.0	Bromine,	80.0
Phosphorus,	31.0	Selenium,	79.1

Sulphur, 32.

There are three specific gravities given for carbon, and two each for silicon, phosphorus, and selenium. The question, therefore, is, whether of the fourteen specific gravities as many as seven are in Playfair's relation with the atomic weights, not of the same element, but of the one paired with it. Now, taking the original formula of Playfair we find

Sp. gr. boron	$=2.68$	$\sqrt[5]{\text{Te}}=2.64$
3d Sp. gr. carbon	$=1.88$	$\sqrt[5]{\text{I}}=1.84$
2d Sp. gr. carbon	$=2.29$	$\sqrt[6]{\text{I}}=2.24$
1st Sp. gr. phosphorus	$=1.83$	$\sqrt[7]{\text{Se}}=1.87$
2d Sp. gr. phosphorus	$=2.10$	$\sqrt[8]{\text{Se}}=2.07$

or five such relations without counting that of sulphur to itself. Next, with the modification introduced by Playfair, we have

1st Sp. gr. silicon	$=2.47$	$\sqrt[4]{\frac{1}{2}\times\text{Br}}=2.51$
2d Sp. gr. silicon	$=2.33$	$\sqrt[6]{2\times\text{Br}}=2.33$
Sp. gr. iodine	$=4.95$	$\sqrt[2]{2\times\text{C}}=4.90$
1st Sp. gr. carbon	$=3.48$	$\sqrt[3]{\frac{1}{3}\times\text{I}}=3.48$

It thus appears that there is no more frequent agreement with Playfair's proposed law than what is due to chance.[1]

[1] As the relations of the different powers of the specific gravity would be entirely different if any other substance than water were assumed as the standard, the law is antecedently in the highest degree improbable. This makes it likely that some fallacy was committed, but does not show what it was.

739. Another example of this fallacy was "Bode's law" of the relative distances of the planets, which was shattered by the first discovery of a true planet after its enunciation. In fact, this false kind of induction is extremely common in science and in medicine.[1] In the case of hypothesis, the correct rule has often been laid down; namely, that a hypothesis can only be received upon the ground of its having been *verified* by successful *prediction*. The term *predesignation* used in this paper appears to be more exact, inasmuch as it is not at all requisite that the ratio ρ should be given in advance of the examination of the samples. Still, since ρ is equal to 1 in all ordinary hypotheses, there can be no doubt that the rule of prediction, so far as it goes, coincides with that here laid down.

740. We have now to consider an important modification of the rule. Suppose that, before sampling a class of objects, we have predesignated not a single character but n characters, for which we propose to examine the samples. This is equivalent to making n different inductions from the same instances. The probable error in this case is that error whose probability for a simple induction is only $(\frac{1}{2})^n$, and the theory of probabilities shows that it increases but slowly with n; in fact, for $n=1000$ it is only about five times as great as for $n=1$, so that with only 25 times as many instances the inference would be as secure for the former value of n as with the latter; with 100 times as many instances an induction in which $n=$ 10,000,000,000 would be equally secure. Now the whole universe of characters will never contain such a number as the last; and the same may be said of the universe of objects in the case of hypothesis. So that, without any voluntary predesignation, the limitation of our imagination and experience amounts to a predesignation far within those limits; and we thus see that if the number of instances be very great indeed, the failure to predesignate is not an important fault. Of characters at all striking, or of objects at all familiar, the number will seldom reach 1,000; and of very striking characters or very familiar objects the number is still less. So that if a large number of samples of a class are found to have some

[1] The physicians seem to use the maxim that you cannot reason from *post hoc* to *propter hoc* to mean (rather obscurely) that cases must not be used to prove a proposition that has only been suggested by these cases themselves.

very striking character in common, or if a large number of characters of one object are found to be possessed by a very familiar object, we need not hesitate to infer, in the first case, that the same characters belong to the whole class, or, in the second case, that the two objects are practically identical; remembering only that the inference is less to be relied upon than it would be had a deliberate predesignation been made. This is no doubt the precise significance of the rule sometimes laid down, that a hypothesis ought to be *simple* — simple here being taken in the sense of familiar.

This modification of the rule shows that, even in the absence of voluntary predesignation, *some* slight weight is to be attached to an induction or hypothesis. And perhaps when the number of instances is not very small, it is enough to make it worth while to subject the inference to a regular test. But our natural tendency will be to attach too much importance to such suggestions, and we shall avoid waste of time in passing them by without notice until some stronger plausibility presents itself.

§9. UNIFORMITIES

741. In almost every case in which we make an induction or a hypothesis, we have some knowledge which renders our conclusion antecedently likely or unlikely. The effect of such knowledge is very obvious, and needs no remark. But what also very often happens is that we have some knowledge, which, though not of itself bearing upon the conclusion of the scientific argument, yet serves to render our inference more or less probable, or even to alter the terms of it. Suppose, for example, that we antecedently know that all the M's strongly resemble one another in regard to characters of a certain order. Then, if we find that a moderate number of M's taken at random have a certain character, P, of that order, we shall attach a greater weight to the induction than we should do if we had not that antecedent knowledge. Thus, if we find that a certain sample of gold has a certain chemical character — since we have very strong reason for thinking that all gold is alike in its chemical characters — we shall have no hesitation in extending the proposition from the one sample to gold in general. Or if we know that among a certain people — say the Icelanders — an extreme uniformity prevails in regard to all their

ideas, then, if we find that two or three individuals taken at random from among them have all any particular superstition, we shall be the more ready to infer that it belongs to the whole people from what we know of their uniformity. The influence of this sort of uniformity upon inductive conclusions was strongly insisted upon by Philodemus,* and some very exact conceptions in regard to it may be gathered from the writings of Mr. Galton. Again, suppose we know of a certain character, P, that in whatever classes of a certain description it is found at all, to those it usually belongs as a universal character; then any induction which goes toward showing that all the M's are P will be greatly strengthened. Thus it is enough to find that two or three individuals taken at random from a genus of animals have three toes on each foot, to prove that the same is true of the whole genus; for we know that this is a *generic* character. On the other hand, we shall be slow to infer that all the animals of a genus have the same color, because color varies in almost every genus. This kind of uniformity seemed to J. S. Mill to have so controlling an influence upon inductions, that he has taken it as the centre of his whole theory of the subject.

742. Analogous considerations modify our hypothetic inferences. The sight of two or three words will be sufficient to convince me that a certain manuscript was written by myself, because I know a certain look is peculiar to it. So an analytical chemist, who wishes to know whether a solution contains gold, will be completely satisfied if it gives a precipitate of the purple of cassius with chloride of tin; because this proves that either gold or some hitherto unknown substance is present. These are examples of characteristic tests. Again, we may know of a certain person, that whatever opinions he holds he carries out with uncompromising rigor to their utmost logical consequences; then, if we find his views bear some of the marks of any ultra school of thought, we shall readily conclude that he fully adheres to that school.

743. There are thus four different kinds of uniformity and non-uniformity which may influence our ampliative inferences:

(1) The members of a class may present a greater or less general resemblance as regards a certain line of characters.

* See Theodor Gomperz, *Herculanische Studien*, pt. I (1865). Cf. 761.

(2) A character may have a greater or less tendency to be present or absent throughout the whole of whatever classes of certain kinds.

(3) A certain set of characters may be more or less intimately connected, so as to be probably either present or absent together in certain kinds of objects.

(4) An object may have more or less tendency to possess the whole of certain sets of characters when it possesses any of them.

A consideration of this sort may be so strong as to amount to demonstration of the conclusion. In this case, the inference is mere deduction — that is, the application of a general rule already established. In other cases, the consideration of uniformities will not wholly destroy the inductive or hypothetic character of the inference, but will only strengthen or weaken it by the addition of a new argument of a deductive kind.

§10. CONSTITUTION OF THE UNIVERSE

744. We have thus seen how, in a general way, the processes of inductive and hypothetic inference are able to afford answers to our questions, though these may relate to matters beyond our immediate ken. In short, a theory of the logic of verification has been sketched out. This theory will have to meet the objections of two opposing schools of logic.

The first of these explains induction by what is called the doctrine of Inverse Probabilities, of which the following is an example: Suppose an ancient denizen of the Mediterranean coast, who had never heard of the tides, had wandered to the shore of the Atlantic Ocean, and there, on a certain number m of successive days had witnessed the rise of the sea. Then, says Quetelet, he would have been entitled to conclude that there was a probability equal to $\frac{m+1}{m+2}$ that the sea would rise on the next following day.[1] Putting $m=0$, it is seen that this view assumes that the probability of a totally unknown event is $\frac{1}{2}$; or that of all theories proposed for examination one half are true. In point of fact, we know that although theories are not proposed unless they present some decided plausibility, nothing like one half turn out to be true. But to apply correctly

[1] See Laplace, *Théorie Analitique des Probabilités*, [1812], livre ii, ch. vi.

the doctrine of inverse probabilities, it is necessary to know the antecedent probability of the event whose probability is in question. Now, in pure hypothesis or induction, we know nothing of the conclusion antecedently to the inference in hand. Mere ignorance, however, cannot advance us toward any knowledge; therefore it is impossible that the theory of inverse probabilities should rightly give a value for the probability of a pure inductive or hypothetic conclusion. For it cannot do this without assigning an antecedent probability to this conclusion; so that if this antecedent probability represents mere ignorance (which never aids us), it cannot do it at all.

745. The principle which is usually assumed by those who seek to reduce inductive reasoning to a problem in inverse probabilities is, that if nothing whatever is known about the frequency of occurrence of an event, then any one frequency is as probable as any other. But Boole has shown that there is no reason whatever to prefer this assumption, to saying that any one "constitution of the universe" is as probable as any other. Suppose, for instance, there were four possible occasions upon which an event might occur. Then there would be 16 "constitutions of the universe," or possible distributions of occurrences and non-occurrences. They are shown in the following table, where Y stands for an occurrence and N for a non-occurrence.

4 *occurrences.*	3 *occurrences.*	2 *occurrences.*	1 *occurrence.*	0 *occurrence.*
$Y\ Y\ Y\ Y$	$Y\ Y\ Y\ N$	$Y\ Y\ N\ N$	$Y\ N\ N\ N$	$N\ N\ N\ N$
	$Y\ Y\ N\ Y$	$Y\ N\ Y\ N$	$N\ Y\ N\ N$	
	$Y\ N\ Y\ Y$	$Y\ N\ N\ Y$	$N\ N\ Y\ N$	
	$N\ Y\ Y\ Y$	$N\ Y\ Y\ N$	$N\ N\ N\ Y$	
		$N\ Y\ N\ Y$		
		$N\ N\ Y\ Y$		

It will be seen that different frequencies result some from more and some from fewer different "constitutions of the universe," so that it is a very different thing to assume that all frequencies are equally probable from what it is to assume that all constitutions of the universe are equally probable.

746. Boole says that one assumption is as good as the other. But I will go further, and say that the assumption that

all constitutions of the universe are equally probable is far better than the assumption that all frequencies are equally probable. For the latter proposition, though it may be applied to any one unknown event, cannot be applied to all unknown events without inconsistency. Thus, suppose all frequencies of the event whose occurrence is represented by Y in the above table are equally probable. Then consider the event which consists in a Y following a Y or an N following an N. The possible ways in which *this* event may occur or not are shown in the following table:

3 *occurrences.*	2 *occurrences.*	1 *occurrence.*	0 *occurrence.*
$Y\ Y\ Y\ Y$	$Y\ Y\ Y\ N$	$Y\ Y\ N\ Y$	$Y\ N\ Y\ N$
$N\ N\ N\ N$	$N\ N\ N\ Y$	$N\ N\ Y\ N$	$N\ Y\ N\ Y$
	$Y\ Y\ N\ N$	$Y\ N\ N\ Y$	
	$N\ N\ Y\ Y$	$N\ Y\ Y\ N$	
	$N\ Y\ Y\ Y$	$Y\ N\ Y\ Y$	
	$Y\ N\ N\ N$	$N\ Y\ N\ N$	

It will be found that assuming the different frequencies of the first event to be equally probable, those of this new event are not so — the probability of three occurrences being half as large again as that of two, or one. On the other hand, if all constitutions of the universe are equally probable in the one case, they are so in the other; and this latter assumption, in regard to perfectly unknown events, never gives rise to any inconsistency.

Suppose, then, that we adopt the assumption that any one constitution of the universe is as probable as any other; how will the inductive inference then appear, considered as a problem in probabilities? The answer is extremely easy;[1] namely, the occurrences or non-occurrences of an event in the past in no way affect the probability of its occurrence in the future.

747. Boole frequently finds a problem in probabilities to be indeterminate. There are those to whom the idea of an unknown probability seems an absurdity. Probability, they say, measures the state of our knowledge, and ignorance is denoted by the probability $\frac{1}{2}$. But I apprehend that the expression "the probability of an event" is an incomplete one.

[1] See Boole, *Laws of Thought*, p. 370.

A probability is a fraction whose numerator is the frequency of a specific kind of event, while its denominator is the frequency of a genus embracing that species. Now the expression in question names the numerator of the fraction, but omits to name the denominator. There is a sense in which it is true that the probability of a perfectly unknown event is one half; namely, the assertion of its occurrence is the answer to a possible question answerable by "yes" or "no," and of all such questions just half the possible answers are true. But if attention be paid to the denominators of the fractions, it will be found that this value of $\frac{1}{2}$ is one of which no possible use can be made in the calculation of probabilities.

748. The theory here proposed does not assign any probability to the inductive or hypothetic conclusion, in the sense of undertaking to say how frequently *that conclusion* would be found true. It does not propose to look through all the possible universes, and say in what proportion of them a certain uniformity occurs; such a proceeding, were it possible, would be quite idle. The theory here presented only says how frequently, in this universe, the special form of induction or hypothesis would lead us right. The probability given by this theory is in every way different — in meaning, numerical value, and form — from that of those who would apply to ampliative inference the doctrine of inverse chances.

749. Other logicians hold that if inductive and hypothetic premisses lead to true oftener than to false conclusions, it is only because the universe happens to have a certain constitution. Mill and his followers maintain that there is a general tendency toward uniformity in the universe, as well as special uniformities such as those which we have considered. The Abbé Gratry believes that the tendency toward the truth in induction is due to a miraculous intervention of Almighty God, whereby we are led to make such inductions as happen to be true, and are prevented from making those which are false.* Others have supposed that there is a special adaptation of the mind to the universe, so that we are more apt to make true theories than we otherwise should be. Now, to say that a theory such as these is *necessary* to explaining the validity of induction and hypothesis is to say that these modes of inference are not in themselves valid, but that their conclusions are rendered probable by being probable deductive inferences from a suppressed (and originally unknown) premiss. But I maintain that it has been shown that the modes of inference in question are necessarily valid, whatever the constitution of the universe, so long as it admits of the premisses being true. Yet I am willing to concede, in order to concede as much as possible, that when a man draws instances at random, all that he knows is that he *tries* to follow a certain precept; so that the sampling process might be rendered generally fallacious by the existence of a mysterious and malign connection between the mind and the universe, such that the possession by an object of an *unperceived* character might influence the will toward choosing it or rejecting it. Such a circumstance would, however, be as fatal to deductive as to ampliative inference. Suppose, for example, that I were to enter a great hall where people were playing *rouge et noir* at many tables; and suppose that I knew that the red and black were turned up with equal frequency. Then, if I were to make a large number of mental bets with myself, at this table and at that, I might, by statistical deduction, expect to win about half of them — precisely as I might expect, from the results of these samples, to infer by induction the probable ratio of frequency of the turnings of red and black in the long run, if I did not know it. But could some devil look at each card before it was turned, and then influence me mentally to bet upon it or to refrain therefrom, the observed ratio in the cases upon which I had bet might be quite different from the observed ratio in those cases upon which I had not bet. I grant, then, that even upon my theory some fact has to be supposed to make induction and hypothesis valid processes; namely, it is supposed that the supernal powers withhold their hands and let me alone, and that no mysterious uniformity or adaptation interferes with the action of chance. But then this negative fact supposed by my theory plays a totally different part from the facts supposed to be requisite by the logicians of whom I have been speaking. So far as facts like those they suppose can have any bearing, they serve as major premisses from which the fact inferred by induction or hypothesis might be deduced; while the negative fact supposed by me is merely the denial of

* See *La Logique*, Paris (1855), vol. 2, pp. 196–97.

any major premiss from which the falsity of the inductive or hypothetic conclusion could in general be deduced. Nor is it necessary to deny altogether the existence of mysterious influences adverse to the validity of the inductive and hypothetic processes. So long as their influence were not too overwhelming, the wonderful self-correcting nature of the ampliative inference would enable us, even if they did exist, to detect and make allowance for them.

750. Although the universe need have no peculiar constitution to render ampliative inference valid, yet it is worth while to inquire whether or not it has such a constitution; for if it has, that circumstance must have its effect upon all our inferences. It cannot any longer be denied that the human intellect is peculiarly adapted to the comprehension of the laws and facts of nature, or at least of some of them; and the effect of this adaptation upon our reasoning will be briefly considered in the next section. Of any miraculous interference by the higher powers, we know absolutely nothing; and it seems in the present state of science altogether improbable. The effect of a knowledge of special uniformities upon ampliative inferences has already been touched upon. That there is a general tendency toward uniformity in nature is not merely an unfounded, it is an absolutely absurd, idea in any other sense than that man is adapted to his surroundings. For the universe of marks is only limited by the limitation of human interests and powers of observation. Except for that limitation, every lot of objects in the universe would have (as I have elsewhere shown)* some character in common and peculiar to it. Consequently, there is but one possible arrangement of characters among objects as they exist, and there is no room for a greater or less degree of uniformity in nature. If nature seems highly uniform to us, it is only because our powers are adapted to our desires.

§11. FURTHER PROBLEMS

751. The questions discussed in this essay relate to but a small part of the Logic of Scientific Investigation. Let us just glance at a few of the others.

752. Suppose a being from some remote part of the uni-

* See vol. 6, bk. II, ch. 1, §2.

verse, where the conditions of existence are inconceivably different from ours, to be presented with a United States Census Report — which is for us a mine of valuable inductions, so vast as almost to give that epithet a new signification. He begins, perhaps, by comparing the ratio of indebtedness to deaths by consumption in counties whose names begin with the different letters of the alphabet. It is safe to say that he would find the ratio everywhere the same, and thus his inquiry would lead to nothing. For an induction is wholly unimportant unless the proportions of P's among the M's and among the non-M's differ; and a hypothetic inference is unimportant unless it be found that S has either a greater or a less proportion of the characters of M than it has of other characters. The stranger to this planet might go on for some time asking inductive questions that the Census would faithfully answer, without learning anything except that certain conditions were independent of others. At length, it might occur to him to compare the January rainfall with the illiteracy. What he would find is given in the following table[1]:

REGION	January Rainfall	Illiteracy
	Inches	Per cent
Atlantic seacoast, Portland to Washington	0.92	11
Vermont, Northern and Western New York	0.78	7
Upper Mississippi River	0.52	3
Ohio River Valley	0.74	8
Lower Mississippi, Red River, and Kentucky	1.08	50
Mississippi Delta and Northern Gulf Coast	1.09	57
Southeastern Coast	0.68	40

[1] The different regions with the January rainfall are taken from Mr. Schott's work. [*Tables and Results of the Precipitation in Rain and Snow in the United States*, 1872.] The percentage of illiteracy is roughly estimated from the numbers given in the Report of the 1870 Census. [The maps originally published with this paper have not been considered worth reproducing.]

He would infer that in places that are drier in January there is, not always but generally, less illiteracy than in wetter places. A detailed comparison between Mr. Schott's map of the winter rainfall with the map of illiteracy in the general census, would confirm the result that these two conditions have a partial connection. This is a very good example of an induction in which the proportion of P's among the M's is different, but not very different, from the proportion among the non-M's. It is unsatisfactory; it provokes further inquiry; we desire to replace the M by some different class, so that the two proportions may be more widely separated. Now we, knowing as much as we do of the effects of winter rainfall upon agriculture, upon wealth, etc., and of the causes of illiteracy, should come to such an inquiry furnished with a large number of appropriate conceptions; so that we should be able to ask intelligent questions not unlikely to furnish the desired key to the problem. But the strange being we have imagined could only make his inquiries haphazard, and could hardly hope ever to find the induction of which he was in search.

753. Nature is a far vaster and less clearly arranged repertory of facts than a census report; and if men had not come to it with special aptitudes for guessing right, it may well be doubted whether in the ten or twenty thousand years that they may have existed their greatest mind would have attained the amount of knowledge which is actually possessed by the lowest idiot. But, in point of fact, not man merely, but all animals derive by inheritance (presumably by natural selection) two classes of ideas which adapt them to their environment. In the first place, they all have from birth some notions, however crude and concrete, of force, matter, space, and time; and, in the next place, they have some notion of what sort of objects their fellow-beings are, and of how they will act on given occasions. Our innate mechanical ideas were so nearly correct that they needed but slight correction. The fundamental principles of statics were made out by Archimedes. Centuries later Galileo began to understand the laws of dynamics, which in our times have been at length, perhaps, completely mastered. The other physical sciences are the results of inquiry based on guesses suggested by the ideas of mechanics. The moral sciences, so far as they can be called sciences, are equally

developed out of our instinctive ideas about human nature. Man has thus far not attained to any knowledge that is not in a wide sense either mechanical or anthropological in its nature, and it may be reasonably presumed that he never will.*

754. Side by side, then, with the well established proposition that all knowledge is based on experience, and that science is only advanced by the experimental verifications of theories, we have to place this other equally important truth, that all human knowledge, up to the highest flights of science, is but the development of our inborn animal instincts.

* Cf. 1.118.