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The Proper Treatment of Hypotheses:
a Preliminary Chapter, toward an Examination of Hume's Argument against Miracles, in its Logic and in its History.

By C. S. Peirce.

The psychological instructors of my college days used to tell me that when a dog is observed to act as if he had reasoned, he was really acting, not from reason, but from "the association of ideas". But more advanced study taught me that that was a shocking abuse of a phrase which was invented to mark the greatest discovery ever made in the science of mind, namely, that all the operations of the soul take place according to one general formula which

applies to reasoning and instinctive action alike.*
Then in 1863 came Wundt's Lectures on the Minds of
Men and ~~Bst~~ Brutes, which so emphasized the analogy
between the dog's process of thought and that of the
philosopher, that I, for one, lost sight, for a time,
of the distinction my old professors had made, —
a distinction of substantial importance, notwithstanding
their vicious way of expressing it.

Certainly, dogs do, occasionally, really reason. Once,

* Hamilton (Reid's Works, Note D**) shows that
Aristotle had carefully formulated the Law of association.
But Aristotle failed to perceive that it governs all the operations
of the mind. The great discovery was made by Gay alone (1733
and 1747), to whom so little justice has been done that even his first
name, though that at least would easily be ascertained from the books
of Sidney Sussex College, Cambridge. The attempt of Germans to claim
the discovery, under a modified name, for their own people, I will not
trust myself to qualify.

The latter the date of an anonymous treatise,
which Dr. Samuel Parr, in a short hand
note in my possession, attributes to its
unquestionable author

Perhaps it is
in the Dict. of
National Biog.

I saw a bicyclist & dog racing over an unknown road after his master, who was miles ahead of him. The dog came to a fork in the road, and stopped, perplexed. After first examining one road and then the other, he came back to the fork, sat down, and waited. After some time another bicyclist passed over the road in the same direction, and I satisfied myself by inquiring that he was entirely unknown to the dog. Second bicyclist than first But as soon as the dog saw which road he took, he raced off on that same road at the very top of his speed, quite distancing from whom he had gained his information. the bicyclist. That dog had certainly reasoned. On the other hand, I am equally satisfied that men often think they have acted from reason, and will tell themselves what their reasoning was, when they have not reasoned at all, and their fancied process having been all made up afterward.

I refuse to call a mental process reasoning, unless

the conclusion is deliberately approved, and the whole proceeding is under conscious control, so as to be open to self-criticism. For if it is not so, while it may be satisfactory or otherwise, just as the action of a man's heart is, yet it cannot be the subject of praise or blame. Now the distinction between ~~operations~~ actions which ought to be performed in one way and movements of the mind that cannot be directly influenced is too important to be left unsignalized by appropriate words. What I mean by reasoning is either right reasoning or wrong reasoning. Logic, indeed, may be regarded as a branch of ethics. The logical reasoner puts a restraint upon his natural tendencies of thinking upon principle, just the moral man does upon all his actions.

Every time a man really reasons, in that sense, he is clearly or obscurely conscious that his present inference belongs to a general class of cases in which an analogous conclusion might be drawn; and his approval of his reasoning consists in a

belief that by acting on the same principle in all cases he will on the whole be advancing his knowledge more than by not drawing such conclusions. If this be true, as the reader's self-observation may satisfy him that it is, a man cannot truly reason without having some notions about the classification of arguments. But the classification of arguments is the chief business of the science of Logic; so that every man who reasons (in the above sense) has necessarily a rudimentary science of Logic, good or bad. The slang of the medieval universities called this his logica utens, — his "logic in possession", — in contradistinction to logica docens, or the legitimate doctrine that is to be learned by study.

A thoroughly satisfactory science of Logic would be one of the most vitally important of all possible knowledges in reference to all vexed and perplexing questions, such as is that of the proper method of dealing with historical documents. Unfortunately, however, the science still remains in that stage of development in which its first principles are in dispute among those who have consecrated

~~devoted~~ their whole lives to its elucidation. It is desirable to ascertain the causes of this ~~lamentable~~ state of things, in order that it may be cured. Some of them appear to me to be manifest. I may mention, first, the fact that practice must, to some extent, precede theory. ~~One could not~~ Could one, for example, expect a good logic of scientific reasoning in the middle ages, when nobody cared to practise scientific reasoning? Very well; it Now it is only ~~act~~ since Weierstrass, who died but a few years ago, that even mathematicians have taken pains to reason accurately; and as long as even the most accurate thinkers, as mathematicians certainly are, in fact, would not be at the trouble of reasoning accurately, how could any ~~no~~ accurate science of logic ~~could~~ be expected to ~~arise~~ grow up? In the second place, a man cannot cultivate a purely theoretical science unless he either happens to be rich or is placed in a position in which he will receive food and raiment ~~be paid for~~

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~~for~~ in exchange for his time spent in that pursuit.

No matter how devoted he may be to the study, he will find, as I know to my cost, that the material obstacles are almost insuperable, especially when almost every man on this planet has the conceit that he is the one individual who never reasons wrong. Now chairs of logic have usually been filled from the theological seminaries, — I won't say from their dregs, — at any rate, writers on logic mostly breathe the atmosphere of the seminary, where the idea of, and feeling for, truth are in a backward state of development. It requires for example, no great discernment to see that the work of the great mathematical logician, Boole, suffered from his theological notions. Both the causes I have mentioned have been, but only two real and influential; yet they have been feeble in comparison with a third.

If we ask why it was that the vast intelligence of Aristotle made such an utter failure in physics as

he did, in comparison with his successes in all the other varied departments of his activity, his physical books are before us to show us very plainly what the reason was. It was that he shared with almost all the Greeks the ~~notion~~ opinion that physics was a science in which it was peculiarly desirable to take broad views, and not descend to minutiæ. That this was the secret of the matter is shown by the fact that the few Greeks who ~~did~~ were not inoculated with this notion, Archimedes, Eratosthenes, Hipparchus, Posidonius, and Ptolemy, had great success in physical inquiries. Galileo and the others who founded modern physics, though they ~~rose~~ aimed to rise to general laws as quickly as they could, always based their ~~reasoning~~ conclusions upon minute observation and minute reasoning. Bishop Berkeley intended it for sarcasm when he called ~~mathematicians~~ Royal Society men "minute philosophers"; but they, ~~were always entirely~~ on their side, were entirely content

with the designation. Now the opinion which the Greeks had about physics is precisely the general opinion of moderns about logic. To me it is most amazing to find even scientists, even physicists, whose reasonings are most exact and minute in their own departments, not only reasoning loosely about logic, but evidently impressed with the idea that ~~that~~ loose and broad reasoning is meritorious in logic. That, in my opinion, is the chief reason why the great mass of what has been written upon ~~logic~~ the subject in modern times is as worthless as Aristotle's physics. The logic of the mediæval theologians, narrow as it was, is at any rate more solid, as far as it goes, than the bulk of the trash that the XIXth century has condemned the student to go through. Of the English books, ~~what has been written on the subject in our times~~ I may acknowledge that if they are wanting in scientific accuracy, they are at least marked by good sense and by literary taste; but those of Germany ~~especially in Germany~~ have not one of the moderns agreed with one another, it would, no doubt, appear presumptuous in me to pass such a judgment; but since there are about

a dozen schools, and the adherents of each declare all the others to be utterly wrong, I accept with implicit faith their authoritative declaration upon the sole point in which they all unanimously agree, — the little worth of which any one of them that can be mentioned

Meantime, there have been, since the early days of modern science, a few individuals who have believed in investigating logic minutely and exactly. In the past, there were, for example, Pascal (1623-62), Nicolas Bernoulli (1687-1759), Euler (1708-83), Plouquet (1716-90), Lambert (1728-77), La Place (1749-1827), De Morgan (1806-71), Boole (1815-64); and a few men in different countries still continue, under every possible discouragement, the same methods of study. Few as they have been, they have achieved some advances, among which may be mentioned the origination and development of the theory of probabilities (which is now in continual use in the exact sciences, as it is in the business of insurance), the logic of relatives (which has thrown a new

light upon every part of logic) an exact theory of inductive reasoning, a ~~new~~ previously unknown form of inference called the syllogism of transposed quantity, the theory of the Fermatian inference ~~considerable steps toward~~ an analysis of number, and infinite multitude, and of the logic of continuity, considerable steps in topical geometry (which underlies projective geometry as this underlies metrical geometry,) contributions toward several branches of pure mathematics, systems of representing ~~intuitively~~ in intuitional forms the relations of premisses to conclusions, and other things of like nature.

Candor compels me to admit that the great bulk of university professors of logic pooh-pooh all this, even more strenuously than they do one another; and as long as this state of things continues, as it will for another generation, it cannot be claimed that the principles of exact and minute logic are established. Nevertheless, it may perhaps yet turn out that the opinion

of a man who has ~~studied the prin~~ devoted years to a minute and arduous study of the principles upon which historical testimony ought to be judged are, after all, not much further from the truth of the matter than are those which have resulted from occasional idle ~~thought~~ reflections and loose reasonings; and in that view, the reader may deem them worth listening to, ~~as~~ (if merely as a matter of curiosity. The proofs of his allegations, which the exact logician would be quite ~~is~~ prepared to adduce, must here be suppressed, because they would be found too mathematical and fatiguing for the reader. Only a few quite secondary considerations can alone here be mentioned. After this preface and acknowledgment, I cannot be misunderstood in expressing positively those positions concerning which I take the responsibility of saying that critical examination has made me, ~~I am~~ confident that they are destined to be generally accepted, in time.

In old times, the majority of philosophers used to declare that the material of our knowledge is partly the gift of our senses and partly that of the eye of reason. Others said that it comes exclusively from the senses; and a few, that true knowledge comes only from an inborn power of knowing. Today it appears that the first opinion is wrong and the other two right in different senses. In what sense all knowledge is a development of an inward power of knowing will appear below. That all our knowledge is founded upon observation is true in this sense, that it all depends upon percepts, that is, direct knowledge of things perceived, and that behind the percepts logical criticism cannot go. The psychologists prove to us that percepts are themselves products of mental operations and are far different from the first impressions of sense. But those operations are beyond our control, and can only be criticized in the sense in which the optical performance of an eye can be criticized. For the purposes of logic, then, percepts are the first data of knowledge.

Now our percepts and direct observations relate exclusively to the

circumstances that happened to exist when they were made, and not to any future occasions in which we might be in doubt how to act. Consequently, observed facts do not, in themselves, contain any practical knowledge; and in order to attain such knowledge, additions must be made to the ~~facts observed~~ data of perception. Any proposition added to ~~observed facts~~ the percepts, tending to make those ~~facts~~ data illuminate other circumstances than those under which they were observed, may be called a hypothesis. For instance, it is a hypothesis that thirteen of the present United States were formerly colonies of Great Britain. For it cannot be directly observed. All that we can observe is that it is so asserted in books and tradition, and that a few movements of divers kinds ~~agree with~~ support the ~~supposition~~ assertion.

Let us, then, begin with ~~inquiring~~ examining the principles upon which hypotheses in general ought

to be treated; after which we can inquire how far there is anything peculiar about the hypotheses of history demanding a different treatment, and so come down to the consideration of Hume's famous argumentation.

Now, in an inquiry concerning a hypothesis in general, three distinct stages have to be recognized, these stages being governed by entirely different logical principles. The first stage consists in the invention, selection, and entertainment of the hypothesis. This I call the abduction. The second stage consists in the application to the hypothesis of facts which, simply as facts, irrespective of how they came to present themselves, tend to strengthen or weaken the hypothesis. I call this the deduction. The third stage consists mainly in basing predictions on the hypothesis, in testing these predictions by experiment, and, so far as they are successful, in bestowing upon the hypothesis a certain measure of belief. I call this the induction. It differs from deduction in this, that if the results of

When I speak of predictions as hypothetical predictions, I mean that they are for since we have never yet satisfactorily tested the truth of the hypothesis, we ought never absolutely to assert the predictions based upon it.

the experiments had not been predicted, or (at least virtually predicted, they would not have had the same probative force, perhaps none at all. For example, when Mendeléef published the periodic law of the properties of the chemical elements, he predicted the principal properties of three unknown elements, Gallium, ~~Germanium~~ Scandium, and Germanium, ~~and Scandium~~, which were, shortly afterward, actually met with. These (remarkably successful predictions rightly induced all chemists to believe in the law. But if it had (afterward been found out that Mendeléef had had a secret knowledge of these elements, and had slily adapted his law to their properties, (This law it would have reverted to the state of an ingenious but unverified theory. I need not not say that no such trick was, in fact, played by the great ~~chemist~~ discoverer. A man may ~~unduly~~ shape a theory to facts that he knows; but if it fits facts that he does not know, it can only be nature, not he, that has done.

(Speaking more generally, the force of this third kind of reasoning essentially depends, in part, upon the fact that the experimenter has pursued a certain line of conduct. Now, deductively it could not in any way affect the argument about an outward fact that the argument has chosen to behave in one way rather than another. If it does affect an induction (because the conduct of the usual course of experience and the conduct of the experimenter has been such as to furnish him with a sample of the course of experience.

the shaping.
The precise service which induction renders, in every case, is to show us what value a quantity takes in the average present course of experience. It does that, and it does no more. It does not furnish us with any generalization nor any new idea of any kind. It is true that the quantity ~~we appeal~~ for the evaluation of which we appeal to induction may be the ratio of occurrences in which a hypothetical law of nature is verified, and the answer of induction may be that it is always verified, — or rather, strictly speaking, approximately always, for no value ascertained by induction can be assumed to be more than an approximation. In such a case, there is a sense in which the induction may be said to furnish us with a generalization. Still, the possibility that the law might always be verified was virtually latent in the question "how often is it verified?" which is the question we put to the test; and induction has only assured us of the reality

of that which we virtually tested as possible.

Before leaving the subject of induction, it will be well to point out that ~~&~~ inductions separate themselves into two classes, which are distinguished by their very different degrees of definiteness and trustworthiness. All induction is reasoning from a sample, the conclusion being that the whole ~~exp~~ class, as it presents itself in experience, will be similar to the sample drawn from it under conditions as nearly like those of the ordinary course of experience as we can draw it. Now the two cases are those in which the sample consists of units which can be counted or measured (and measurement ~~is~~ but a device ~~useless~~ ~~counting~~) for making numeration applicable), and those in which nothing of the sort is possible. ~~The above~~ A testing of the periodic law ~~was~~ would be an induction of the first class; ~~because~~ the matter ~~was~~ could be controlled by numbers.

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But suppose that, while I am travelling upon a railway, somebody draws my attention to a man near us, and asks me whether he is not something ~~of the ger~~ allied to a catholic priest. I thereupon begin to run over in my mind the observable characteristics of ordinary catholic priests, in order to see what proportion of them this man displays. Characteristics are not capable of being counted or measured: their relative ~~importance~~ significance in reference to the question put can only be vaguely estimated. Indeed, the question itself admits of no precise answer. Nevertheless, if the man's style of dress, boots, trousers, coat, and hat, — are such as are seen on the majority of American catholic priests, if his movements are such as are characteristic of them, betraying a similar mental state of nerves, and if the expression of countenance,

which results from a certain long discipline, is also characteristic of a priest, while there is a single circumstance very unlike a Roman priest, such as his wearing a masonic emblem, I may say, he is not a priest, but he has been, or has been near becoming, a catholic priest. This sort of vague induction, I term an abductory induction.

Let us now briefly consider deduction. If it had turned out that the properties of gallium, scandium, and germanium were in hopeless conflict with Mendeléef's proposed law, that fact would at once have refuted the theory. Nor would the circumstance that he had actually predicted other characters have made the refutation any more or less complete. The facts in themselves (of Mendeléef's conduct) apart from all prediction or other circumstance at-

tending their discovery, would have settled the question. That, then, would have been a deductive inference, although it would have been a deduction occurring as an incident, or breaking off, of an inductive inquiry. Deductive reasoning is, however, frequently not conclusive. Thus, suppose the question to be whether ~~a man~~ a person who writes to me believes in the infallibility of the pope, or not. If I ascertain, no matter how, that he believes in prayers for the dead, in the confessional, in the celebacy of the clergy, and in the sacrament of marriage, these circumstances will not absolutely close the inquiry, since he may be an 'Old Catholic'; but they will render it his belief in the pope's infallibility extremely probable. Or again, if I ascertain that the man in question is

a violent partizan in politics and in other directions, ~~subjects~~, and further, that he has given money toward a catholic institution, I may fairly ~~reason~~ argue that it is unlikely that a man of that character would do such a thing ~~that~~ unless he embraced catholicism wholly. Yet this would not preclude the desirability of ~~further evidence~~ testing the hypothesis. Or again, if I ascertain that the man in question is one of a triplet of brothers, who are almost indistinguishable physically and mentally; and further that the other two accept the pope's infallibility, this would be a strong, yet inconclusive, reason for thinking that the third brother shares the same opinion. This again would be strengthened if, in a fourth way, I learn that ~~members of~~ brothers brought up together, although they very frequently differ about politics and other subjects, are, for the most part, either all protestants or all catholics.

All these ways of reasoning are deductive, since they rest exclusively upon objective facts, and not at all upon the fact that the reasoner has pursued any particular line of conduct, such as predicting the consequence of a hypothesis or drawing a sample at random.

I already owe the reader an apology for forcing him to gulp all this so hurriedly; and now the third course, which is abduction, has to be dispatched with even greater haste. A singular salad is abduction, whose chief elements are its groundlessness, its ubiquity, and its trustworthiness. We will first see what sort of a mixture they make, and then this traveller's repast must be done.

Abduction is that kind of operation which suggests a statement in no wise contained in the data from which it sets out. There is a more familiar name for it than abduction; for it is neither more nor less than guessing. A given object presents an extraordinary

combination of characters of which we should like to have an explanation. That there is any explanation of them is a pure assumption; and if there be, it is some one hidden fact which explains them; while there are, perhaps, a million other possible ways of explaining them, if they were not all, unfortunately, false. A man is found in the streets of New York stabbed in the back. The chief of police might open a directory and put his finger on any name and guess that that is the name of the murderer. How much would such a guess be worth? But the number of names in the directory does not approach the multitude of possible laws (of attraction) which would have accounted for Kepler's laws of planetary motion and in advance of verification by predications (of perturbations etc., would have accounted) for them to perfection. Newton, you will say, assumed that the law would be a simple one. But what was that but piling guess on guess? Surely, vastly more

phenomena in nature are complex than simple. By its very definition abduction leads to a hypothesis which is entirely foreign to the data. To assert the truth of its conclusion even ever so dubiously would be too much. There is no warrant for doing more than putting it as an interrogation. To do that would seem to be innocent; yet if the interrogation means anything, it means that the hypothesis is to be tested. Now testing by experiment is a very expensive business, involving great outlay of money, time, and energy; so that comparatively few hypotheses can be tested. Thus, even the admission of an abductive conclusion to the rank of an active interrogation is a concession not to be too lightly accorded.

Any novice in logic may well be surprised at my calling a guess an inference. It is equally easy to define inference so as to exclude or include abduction. But all the objects of logical study have to be classified;

and it is found that there is no other good class in abduction
which to put ~~it~~ but that of inferences. Many logicians,
however, ~~leaving~~ leave it unclassed, a sort of logical
supernumerary, as if its importance were too small to
entitle it to any regular place. They evidently forget
that neither deduction nor induction can ever add the
~~one~~ smallest item to the data of perception; and, as we
have already noticed, mere percepts do not constitute
any knowledge applicable to any practical or theoretical
use. All that makes knowledge applicable comes ~~by~~ the way
to us ~~by the~~ via abduction. Looking out of my window
this lovely spring morning I see an azalea in full bloom.
No, no! I do not see that; though that is the only way I
can describe what I see. That is a proposition, a sentence,
a fact; but what I perceive is not proposition, sentence,
fact, but only an image, which I make intelligible in
part by means of a statement of fact. This statement which is abstract;

but
~~though~~ what I see is concrete. I perform an abduction
anything I see.
when I do [illegible] as expressed ~~what I see~~ in a sentence. The
the whole fabric of our
truth is that ~~our whole~~ knowledge is one matted felt of
pure hypothesis confirmed and refined by induction.
Not the smallest
~~one~~ advance can be made in knowledge beyond
the stage of vacant staring, without making an abduction
at every step. ~~Hama~~

When a chicken first emerges from the shell, it does not
try
~~tries~~ fifty random ways of appeasing its hunger, but
within five minutes is picking up food, and choosing as
it picks, and picking what it aims to pick. That is not
reasoning, because it is not done deliberately; but in every
respect but that, it is just like abductive inference. In man,
two broad instincts common to all animals, the instinct
for getting food, and the instinct for reproduction, are
developed into some degree of rational insight into nature.
The instincts ~~for~~ connected with getting food require that every animal should

have some ~~correct~~ just ideas of the action of mechanical forces. In man these ideas become abstract and general. Archimedes and Galileo make right guesses about mechanics almost ~~as~~ at once. Only a few of their notions have to be rejected, because they know how to do their guessing piece-meal and in an orderly sequence. Out of their guesses, corrected by induction and deduction, the Science of dynamics has been built. Guided by the ideas of dynamics, physicists have guessed at the constitution of gases, the nature of heat and of sound, and experiment has only corrected errors and measured quantities. ~~But~~ By analogous processes, one science suggesting ideas ~~to~~ for another, the whole physical side of our theoretical knowledge has grown up ~~out~~ of the original seed of the food instincts.

The instincts ~~of~~ connected with reproduction require that every animal should have some tact and judgment as to how another animal will feel and act under different circum-

stances. These ideas likewise take more abstract forms in man, and enable us to make our initial hypotheses successfully in the ~~department of the~~ psychical side of science, — in such studies, for example, as psychology, linguistics, ethnology, history, economics, etc.

It is evident that unless man had had some inward light ~~or~~ tending to make his guesses on these subjects much more often true than they would be by mere chance, the human race would long ago have been extirpated for its utter incapacity in the struggle for existence; or, if some protection had kept it ~~still~~ continually multiplying, the time from the tertiary epoch to our own would be altogether too short to expect that the human race could yet have made its first happy guess in any science. The mind of man has been formed under the action of the laws of nature, and

therefore it is not so very surprising to find that its constitution is such that, when we can get rid of caprices, idiosyncrasies, and other perturbations, its thoughts naturally show a tendency to agree with the laws of nature.

But it is one thing to say that the human mind has a sufficient magnetic turning toward the truth to cause the right guess to be made in the course of centuries during which a hundred good guessers have been unceasingly occupied in endeavoring to make such a guess, and a far different thing to say that the first guess that may happen to possess Tom, Dick, or Harry has any appreciably greater probability of being true than false.

It is necessary to remember that among the swarms who have covered the globe, there have not been above three individuals, Archimedes, Galileo, and Thomas Young,

whose mechanical and physical guesses were mostly correct in the first instance.

It is necessary to remember that even those unparalleled intelligences would certainly not have guessed right if they had not all possessed a great art of so subdividing their guesses as to give to each one almost the character of self-evidence. Thus, the proof by Archimedes of the properties of the lever, which makes the foundation of the whole science of mechanics, is composed of a series of abductions, or guesses. But look at the character of these guesses. He begins by saying that equal weights freely hanging from the extremities of an equal-armed balance will be in equilibrium. That was a matter of familiar knowledge; at least when the two weights were suspended at equal distances from the balance. But

Archimedes guessed that the length of the suspending thread would make no difference, otherwise than by its own weight. It is not recorded that he assured himself of this truth of this by experiment before proceeding further. We must hope he did, because logic required it. He next guessed that so long as the suspension was freely flexible, it would make no difference how the weight in either pan were distributed; whether, for example, it were piled up in a column in the middle of the pan, or divided into two equal parts placed equally distant from the middle. No doubt, Archimedes would have tried that experiment; but the classical style of writing forbade the ~~introduction~~ statement of all these intermediate steps of the process of thought. It followed, then, from the verification which preambly was made, that if any weights assumed as units

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should be by single threads
~~as~~ hung from the extremities of an equal armed balance, they would be in equilibrium, although one of the threads were indefinitely short; and further, that the weight hung from this short thread might be divided into three parts, of which one, reduced as near to zero as possible, formed a balance-beam of the same length as the original balance-beam, while the other two were equal and suspended from the extremities of the equal arms of this second balance. But this would bring one of them directly under the point of suspension of the first balance, while the other would be distant from that point by twice the length of the arms of the first balance. Still, the whole ~~they~~ must be in equilibrium. Archimedes next guessed that if any jointed apparatus was in equilibrium, so that there was no motion at any joint, it would not

be thrown out of equilibrium, though that joint were made stiff. This was a highly rational guess, yet certainly one requiring verification by experiment. Archimedes must ~~certainly~~ probably have practically made such an experiment, since the Roman steelyard ~~would certainly~~ could hardly fail to have been more or less in use in Sicily, where he lived, and it must afford him the means of subjecting his guess to an easy test. That guess having been ~~found~~ correct, he followed ~~them~~ that if a balance had one arm twice the length of the other, but were so constructed as to be in equilibrium by itself, and if from the short end of this balance any weight were suspended, and a half that weight from the long end, while a second half weight was hung from the point of support of the balance, the whole would be in equilibrium. Finally, Archimedes guessed that a weight hung from the point of support of a balance would not affect its equilibrium.

This having been verified, all the rest of his reasoning was purely deductive and need not detain us now. Such is the sort of guesses which a mighty intellect may trust itself to make about nature, provided it goes no further than a single step without applying the check of experimentation. [By the way, will those who doubt whether guessing is inference see why to say that Archimedes, Galileo, almost the intellectual peer of Archimedes, when he had to guess at what rate the speed of a falling body would increase during its fall, guessed wrong at first; and Keppler, one of the most extraordinary unravellers of phenomena that ever lived, made many false hypotheses which he had to correct, in the course of his great work on Mars.

It is plain, therefore, that our only safe expectation will be that any guess which we may adopt on probation will prove but the first of a whole series of guesses which will have to be examined and rejected. Unless a hypothesis is supported by some deductive evidence, its likelihood that it

will be the last we have to try, is about as well worth considering as the probability that the present year may be your last on earth. That is to say, it is a thing to be considered as possible, and provided for, but a thing that it would be downright folly to build upon.

Even if a hypothesis be supported deductively, unless the deductive argument amount to almost conclusive proof, one ought to be exceedingly wary of allowing it to influence the abductive proceedure. There are Three reasons for this may be mentioned. The first is that a probability is either an objective statistical fact, such as those which guide the insurance companies, in which case it becomes of great value provided one has an enormous number of closely analogous cases to deal with, but otherwise is of no value nor significance; or else it is merely a subjective likelihood, which is a mere

expression of our preconceived ideas, which are the grand source of deceptions ~~and expense~~ in forming hypotheses. The second reason is, that the deductive argument will receive due consideration at a later stage of the inquiry when it will come up naturally while to consider it so early inevitably makes a disorderly proceedure very unfavorable to sound guessing. The third reason is, that in view of the absence of all probative force in the operation of guessing, the governing consideration in dealing with it should be that of economy, especially when the serious expense of induction is considered.

The first thing that economy prescribes is that every guess be broken up into its elements and taken piece-meal. For instance, suppose a phenomenon so far as known can be explained by any one of a million hypotheses. To test these, each one in its entirety would probably require 500000 experimental investigations, — enough to cramp an empire. But

if they can be so broken up that a certain hypothetic statement capable of experimental test agrees with 500,000 of them, and is in conflict with the other 500000, then one induction will reduce the number of admissible hypotheses by one half. It will suffice to repeat this preceding nineteen times more to reduce the hypotheses to a single one. That is, this method will cost only 1/25000 as much as the other.

There is a whole systematic code of ~~systematic~~ maxims of economy which ought to be methodically applied in every abductive operation. But our general consideration of the subject must here be brought to a close. In the next chapter, we shall see how these principles of minute logic are to be applied in dealing with historical documents.

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On the Principles which ought to Guide us in Accepting or Rejecting Historical Testimony.

By C. S. Peirce.

The pre-Wundtian psychologists of my youth used to say that when a dog is observed to act as if he had reasoned, he really was not acting from reason but from the association of ideas. It has since been made clear that all reasoning, as well as all other intellectual operations, come under the one general law which, rather narrowly conceived, used to be called the association of ideas. ~~The~~ attention ~~is~~ of most men is now so much directed to the analogy between the dog's mental operation and that of a philosopher, that they are, perhaps, in danger of overlooking the important distinction that those earlier psychologists emphasized.

I am satisfied that dogs sometimes really do reason. I once

The old scheme for the examination of historical testimony was that the text of each document was first to be made accurate by "textual criticism"; secondly, that the ~~authenticity~~ authorship of the whole was to be determined by "higher criticism"; thirdly, that the meaning was to be ascertained by "exegesis"; and fourthly, that the historic truth was to be concluded from the consideration of the ~~credibility~~ veracity and competence of the witness and of the admissibility of the events narrated. But as the business is at present understood, the "higher critic" is not to stop with inquiring who was the final author of the writing which has come to us, but must ask where he obtained his materials, who the witnesses nearer the facts were, and by what process ~~he has made~~ the report which reaches us was made up; and the admissibility of the events narrated is one of the factors which, it is held, should guide the critic in this examination. The result is that substantially

the whole business of the historian, as far as the particular events narrated are concerned, is practically thrown upon the ~~[illegible]~~ critic; and thus it happens that by "higher criticism" is now commonly meant ~~[illegible]~~ the style of higher criticism which has been in vogue during the last two generations, - a style of criticism of which the characteristic is the frequent, not to say continual, rejection of the entire mass of testimony in favor of a totally different account of the events which has been made up by the critic without any effort to keep as close to the testimony as he can, this account being accepted as appearing most likely in itself. This method is said to have originated in 1722 when Lévesque de Pouilly, at his entry into the Académie des Inscriptions, read a dissertation on the uncertitude ~~of the history~~ of the first four centuries of Roman history. There was nothing at all new in doubting ancient historians, but only in sounding those doubts

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upon a definitely stated code of principles. Although the method originated in France, it was in the German brain that it found a congenial soil. In our times, its results have been many times put to the test by archaeological exploration, and have in almost every single instance come to grief most signally.

All our knowledge may be said to be founded upon observed facts, in this sense, that logical criticism of the operations of our minds cannot be carried back further than to our perceptions. For whatever mental operations may have gone to the formation of percepts were beyond our control, and is only subject to criticism in the sense in which the optical performance of an eye may be criticised. We cannot blame ourselves for not having performed such operations differently. Now our percepts and direct observations relate exclusively to particular circum-

the shaping.

In order to show precisely what the nature of the service which induction renders us is, I will make a little test of Mendeléef's law as far as it applies to atomic weights. The law is not expressed in numerically exact form; but we may suppose that there is some simple ~~def~~ numerical determination of it which will more or less approximately express the atomic weights. Now in order to get an idea of how nearly it would do so, ~~we may~~ it will be sufficient to compare the ascertained atomic weights, as given in Richards's list of 1900 with a very rough law. From Hydrogen, whose atomic weight is 1, to Helium, whose atomic weight is 4, there are 7 intervals, or say ~~extra~~ half a unit per interval. From Lithium, (at. wt. 7.03) to Chromium (at. wt. 35.46) there are 14 intervals, or say two units per interval.

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Potassium

From K (At. wt. 39.14) to Caesium (At. Wt. 132.9), there are 36 intervals, or say 2½ units per interval. I will, therefore, compare with the observed atomic weights, the hypothesis that the atomic weights increase by half-units from Hydrogen to Helium, and thence by double units to Argon, and by 2½ units above the atomic weight of Argon.

The results are exhibited in the table. An inspection of this table will show that no very simple determination of the periodic law would satisfy the ascertained atomic weights much better than the rough hypothesis employed. We may, therefore, say that the atomic weights agree pretty well with Mendeléef's law in about five cases out of every six; and it would be a waste of time, in the present state of our knowledge, to attempt to go further.

We see here that what induction does for us is simply

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But now suppose that while I am ~~ride~~ travelling in a railway ~~street car~~ the question arises in my mind whether or not a man seated near me believes in the infallibility of the Pope.

1.000
10
75
9925

7.03 1 09975
5
6.982 19950
3 29925
4 39900
5 49875
6 59850
7 69825
8 79800
9 89775
99750

8.9825
22
09925
19850
29775
39700
49625
59550
69475
79400
89325
99250

6.9475
298
6.98
8.93
10
9.03
9.92
99
10.91
09.92
198
11.90
09.925
397
40
13.93
15.8840

09.925
89325
496
18.907
19.85
19.850
2977½
49!
22.877
19.850
3970
298
59
24.18

7.03
6
6.97

9.1
1
9.0

11.0
1
10.9

12.08
8
11.912

14.04
10
13.94

16.00
12
15.88

19.05
14
18.91

20.15
19.8

1.0075
745
6

23.05
17
22.88

24.36
18
24.18

27.1
2
26.9

28.4
2
28.2

31.0
2
30.8

32.06
24
31.82

35.46
26
35.20

39.9
3
39.6

39.14
29
38.85

40.1
3
39.8

44
44

48.17
36
37.81

51.4
4
51.0

52.14
39
51.75

55.02
41
54.61

55.9
4
55.5

59.00
44
58.56

58.70
44
58.26

63.60
47
63.13

65.40
119
64.91

70.0
5
69.5

72.5
5
72.0

75.0
6
74.4

79.2
6
78.6

79.96
59
79.37

82
1
81

85.44
64
84.80

87.68
65
87.03

89.0
7
88.3

90.5
7
89.8

94
1
93

96.0
4
95.3

101.7
8
100.9

103.0
8
102.2

106.5
8
105.7

107.93
80
107.13

112.3
8
111.5

114
1
113

119.0
9
118.1

120.0
9
119.1

127.5
9
126.6

126.85
94
125.91

I will now pass to the principal subject of this paper, that of <u>abduction</u>, by which I mean the first adoption of a hypothesis, — its invention and selection. Evidently it ought to be an adoption on probation merely; for what is a hypothesis, at its first coming into the world, but the merest guess? ~~Guesses are~~ We are driven to guessing by the circumstance that no other procedure can furnish us with a new idea. Now much truth ~~is~~ requires new ideas for its apprehension. We begin, and must begin, almost every inquiry by making a guess. To that we apply our deductive and more effectively our inductive machinery, and ascertain, perhaps, that our guess will answer approximately, but that it requires correction. We again set our guessing mill to work, to turn out a correction, which has to be subjected to a new examination. Thus, it is that the whole fabric of science is a matted felt of pure guess —

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work, confirmed (and purified) by experiment.

But (to explain any given phen) there is only one ~~true~~ guess

work, confirmed and purified by experiment.

But here an apparent difficulty looms up. To explain a given phenomenon, there is just one possible guess that is the truth; and how many false ones are possible? Often millions; not infrequently many millions, if not an endless variety. What chance have we, then, of guessing right? Had we ~~to do~~ nothing to depend upon but the principles of probability, the human race, (one might wager,) would not yet have had time to make, ~~never yet have made~~ have made its first correct surmise. Indeed, it would have been extirpated for its incapacity in the struggle for existence. But just as a chicken just out of the shell does not guess at a hundred different ways of appeasing the sensation of hunger, but begins to pick up food in a very few minutes, so man has inherited ~~a~~ less save but ~~a~~ broader instincts for guessing right about certain matters.

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What are these matters? In the first place, out of the instinct for guessing how to obtain ~~their~~ food, which belongs to all animals, man has developed an imperfect instinct for guessing correctly about the action of mechanical forces. This is proved by examining the action of minds of high intelligence when they are confronted with ~~before~~ such problems. Archimedes established the theory of the lever by a well-ordered series of pure guesses. He set out from ~~with~~ the proposition ~~began by guessing~~ that an equal-armed lever equally weighted at its two extremities would remain in equilibrium, a proposition well-known to him by experience, a not a new guess of his. But when he assumed that if the weights were suspended from the balance by perfectly flexible threads, it would make no difference how ~~these~~ the weights were shaped, since there could only be a downward pull upon the threads, his guess-work may be said to have begun; for it

was conceivable that ~~weights~~ a mass horizontally spread out should weigh more than the same mass moulded into ~~in the form~~ a long column. But the guess of Archimedes was right. It followed that one of the two equal weights at the extremities of an equal-armed balance, might be ~~replaced by~~ divided into halves, themselves suspended from the extremities of a weightless balance having arms of the same length as ~~the first~~ those of the original balance, and this balance might hang by a thread from ~~the~~ one extremity of the original balance; and still there would be equilibrium. We further guessed that the length of the string could make no difference, so long as it was weightless; so that it might be indefinitely short. We next guessed that, as long as a balance was in equilibrium, so that all its parts were, in fact, at rest, it could make no difference whether they were free to move or not. Consequently, the two

balances could be melded together. He next guessed that a weight placed upon a balance at it fulcrum, or point of support, could make no difference; so that one of the two half-weights could be taken away, and when the result would be that a half-weight at the extremity of an arm of double length would balance a whole weight at the extremity of an arm of single length; and so he reasoned throughout. This is all guess-work; and yet it is all perfect proof. Dr. Ernst Mach, indeed, in his admirable Science of Mechanics (Open Court Co.) with which every student of science ought to familiarize himself, says that the reasoning of Archimedes is bad. But, on the contrary, nothing could be better; only it is not reasoning of the deductive kind, - a kind of reasoning that could not by any possibility have answered the purpose of

Archimedes, by solving the problem before him.

Notwithstanding the vast genius of Archimedes, neither he nor anybody else was able to make a good guess about bodies in motion before Galileo. The reason was that their guesses were awkward contortions of their minds. Every inventor knows that it is the idea which seems, when it is once gained, the most natural and obvious, which is the most difficult to discover, and requires the most genius to find it out. It is just so in dancing, writing, riding, etc. The beginner fails to make the movements which are, in reality, made with most facility. Galileo himself began by making a wrong guess about falling bodies, which he was able to refute by deduction without any experiment. When he finally made the right guess, which was his second one, he was unable to test it directly, on account

of the speed with which bodies fall. He therefore enlarged his guess, and supposed that balls rolling down an inclined plane would follow the same law. & When that turned out to be true, which he ascertained by excellent experiments demanding no apparatus beyond what Archimedes had at his hand, Galileo had only to find how the general law was varied for planes of different inclination; and having done that, deduction could show him how the ball would descend a plane indefinitely near to being vertical, and a plane indefinitely near to being horizontal. His discovery of the law for planes of different inclination was pure deduction from the guess that on no arrangement of inclined planes could a body left to itself in a state of rest mount by its mere weight indefinitely high while

gaining, at the same time, an indefinitely great rate of speed.

These instances, with many similar ones, suffice to prove inductively that man does have an imperfect but ~~~~ important instinct for guessing right about the actions of forces. In the second place, the instincts connected with reproduction, which are found in all animals, are developed, in man, into an instinct for divining how other beings than himself will feel under different circumstances, and how they will behave. This instinct is so well-known to everybody, that it would be idle ~~to~~ for me to address any evidences of its existence. I do not know that man has any other important instincts than these two. Out of ~~the~~ guesses furnished by one of them have been built up the whole

series of physical sciences: ~~on the~~ out of guesses furnished by the other have been built up the whole series of psychical sciences, such as psychology, linguistics, ethnology, history, economics, etc.

It may reasonably be feared that, as we penetrate more and more deeply into the abstruser parts of science, we may find ourselves in a realm where nature no longer speaks a language that we naturally understand, and where our guesses have a weaker and weaker magnetism pointing them toward the truth. It is certain that scientific men of today feel far less confidence in their theories than the pioneers did. It is right that full weight should be given to this danger. But we must conquer the truth by guessing, or not at all. Here, in the atmosphere of America, the moment for despair does not seem

to have come, however it may seem in France; so that the only effect of the danger upon us will be to make us more wary ~~than~~ and artful than ever in the ordering of our guesses.

The art of ordering guesses is reducible to a moderate (systematic) code of maxims resting upon a small number of theorems. I regret that want of space forbids my developing this important subject here. The governing consideration of the whole matter is economy. One (good) reason which may given for this is, that, notwithstanding man's aptitude for guessing right, the only safe calculation must be that any given hypothesis will, after a time, have to be superseded or, at least, modified. We must calculate upon having to test several, and induction is expensive business. The question of how we are to plan our proceedings so as to keep down our

~~expidures~~ expenditures of money, time, and energy, is a vital one. Indeed, it embraces every other.

The likelihood of a hypothesis ought not to be considered in abduction, except so far as it affects the question of economy; and it is as often good economy to begin with an unlikely hypothesis as with a likely one. It is true that we must rely upon a general likelihood in our hypotheses; but to trust to the likelihood of any special one is quite a different thing. Any given likelihood may be illusory; the likelihood is that it is so; so that likihood warns us against its own deceptions. Moreover, when our abduction has been made and the investigation has passed into the deductive stage, every likelihood will come to be properly weighed by appropriate methods, the use of which could only confuse the abductive process; and such confused proceeding

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commonly results in an enormous enhancement of expense.

This having been verified, all the rest of his reasoning was purely deductive and requires no notice here. Such is the sort of guesses which a mighty intellect may trust itself to make about nature, provided it ~~the~~ goes no further than a single step without the check of experimentation. Galileo was almost the intellectual peer of Archimedes; yet ~~he~~ when he had to guess ~~the~~ at what rate the speed of a falling body would increase, a question it was too difficult for him to guess right at first. His first guess was that with every inch of fall the velocity was increased by the same amount. Mere deduction sufficed to refute that; for unless it never would start ~~fall~~ from a state of rest, which was contrary to ordinary experience, it would after having once started to fall, fall with an infinite velocity, which equally was contrary to the ordinary experience. Although Galileo does not tell us so, it is quite likely that he next considered the effect of everywhere transposing time and space in his first

We here find ourselves plunged into a corner of the fray which has long been raging all over the field of historical criticism between instinct and systematic logic. "Your instinctive judgments are subjective" say the logicians, "and your pretentions to be able to detect what others cannot detect is proved to be characteristic by your disputes among one another." "Your wooden rules never penetrate the skin of the facts," say the Leaders of instinct "and your notion that one can form any just judgment or idea of human actions without the exercise of tact and subtlety of feeling, is unworthy of the savours of Lapsta."

We are bound to suppose that a treaty will be patched up between the two parties, after the field has been well fought over, and perhaps we can divine what some of its articles will be.

hypothesis. That is to say, perhaps he guessed that with every second of fall, the time it would take to fall an inch would be diminished by the same amount. But this would evidently lead to the absurd result that after a fixed time the body would fall an inch in no time at all. There now remained two equally simple hypotheses that he might make. That is to say, he could either suppose that with every inch of fall the time that it took to fall an inch would be diminished by any minute fraction of an amount, a hypothesis very readily refuted, since the body must then begin to fall with a finite velocity and after falling twice the distance that would double its velocity it would stop falling altogether, or he could suppose, what he was driven to suppose, that with every second of fall the distance fallen per second would be increased by a constant amount. If mathematics had been in a more advanced state in Galileo's time he would have reasoned that the fall is toward the centre of the earth and that any distances of fall which we can observe in

Adds his weight

Dr Frazer takes the logical side of the discussion of that branch of the science of diplomatics which seeks to identify individual handwritings. The general mob of experts have endeavored to reduce the instinctive methods to such rules that they may convince juries. But instinct itself, which is the sole foundation for such a procedure, as has often expressed in public opinion and in judicial dicta professes distrust of anything of the sort. This seems that style of too much like the methods of historical "higher criticism," which we mean the fantastical style of higher criticism which professes to base its conclusions on the instinct for likelihood among German professors, which has in our time received so many hard knocks from archeological explorations. Both are alike based on individual impressions of what is likely.