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CHARLES PEIRCE AT JOHNS HOPKINS

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In the company of scholars and investigators which Daniel Coit Gilman gathered at Baltimore in the seventies and eighties was Charles S. S. Peirce, son of the Harvard mathematician, Benjamin Peirce, and considered by Sylvester (surely a capable judge) "a far greater mathematician than his father." But great though his mathematical powers were, it is not they alone which chiefly distinguished him among the scientists of his generation. Mathematics was simply one of the many fields of thought tilled by his active brain. At Harvard it was in chemistry that he had chiefly distinguished himself, but it might as well have been in physics. While lecturing at the Johns Hopkins, he was also conducting experiments for the U. S. Coast Survey. But it is perhaps for his researches in symbolic logic that he is chiefly known, being, indeed, the inventor of the logic of relatives, since developed by Russel into the logic of relations. He himself considered more important his theory of inductive reasoning; which certainly is more understandable and usable, for the logic of relatives is the acme of technicality and abstraction. As an indication of the power of this logic, let it suffice to say that in terms of it all of the two hundred and sixteen varieties of algebra classified by his father can be expressed, among which ordinary algebra, the algebra of imaginaries, and quaternions are special cases. Further, any algebra whatever can be brought under the domain of this system.

As for his theory of induction it is presented in a series of six articles in the *Popular Science Monthly* for 1878, though there is a briefer, but more technical exposition, in the *Johns Hopkins Studies in Logic*; while Baldwin's *Dictionary of Philosophy and Psychology* contains a number of Peirce's contributions bearing upon his theory. His lectures in advanced logic, delivered at Baltimore the second semester of 1880-1881, were wholly devoted to an examination of the logical foundations of the leading scientific theories of the time. They were based on his theory of induction, the gist of which is that induction is a safe process because it is a self-corrective one. Truth in science, as truth everywhere, is that conclusion concern-

ing any matter toward which tend, in the long run, the opinions of competent investigators. If science lead us astray, more science will set us right. But at any given time may not common sense be right and science wrong ? In matters where the judgment of common sense is based on intimate experience this may certainly be the case, even though those who pass the judgment cannot always give an account of the manner in which they have arrived at it. Those judgments that are the result of race-experience most of us would be unable to distinguish from intuitions. We believe in them, but cannot tell how we have come to believe in them. Yet if they relate to matters of constant experience they may be trusted. For constant experience, by trial and error and correction, even though these be half unconscious, leads to true ideas. Even the lower animals have such ideas. The human animal, however, not infrequently holds to ideas that are altogether false. When this is so, it will in most cases be found: (1) that there has been no experience; or (2) that the experience has occurred under conditions that have ceased to exist; or (3) that in some other way the experience was not typical but unique and peculiar .I t is conceivable that one might be led to have very strong convictions about the politics of the inhabitants of Mars; but neither the strength of one's convictions, nor the fact that he had got a number of people to agree with him, nor even that he had founded a society perpetuating his ideas from generation to generation would give the least validity to any conclusions reached by his possibly intensely interesting speculations. The probability that one shall be right in a matter concerning which he knows nothing and has no experience—direct, indirect, or remote—is zero. A blindfolded marksman cannot be expected to hit the bull's-eye.

With most of us true ideas and false are mingled in inextricable confusion. Is there a way in which they may be disentangled? Pascal seemed to think that clearness is the touchstone of truth, -as it certainly is a help. One service of Peirce is that by his maxim of pragmatism he has shown us how to clarify our ideas somewhat. This maxim, as stated in the *Popular Science Monthly* for January, 1878, is as follows: "Consider what effects, that might conceivably have practical bearings, we conceive the object of our conception to have. Then our conception of these effects is the whole of our conception of the object." How completely this criterion brushes aside such speculations as those concerning Martian politics! How many interminable discussions are cut short by its application! Two men are hotly contending as to whether the first life on earth is due to spontaneous generation or to special creation; they apply to a third to decide between them. "Tell me," he says to them, "just what your spontaneous generation is; that is, how I am to know it, supposing a case to occur before me; and tell me also just how I am to distinguish a special creation. Of course, if the phenomena are all the same, there is really nothing to dispute about." To be sure, the pragmatic maxim, like many another, can be misapplied; in the opinion of its author, it has been misapplied. In fact, he later admitted that his own statement had been inadequate, that, besides immediate practical consequences, one should have in view consequences in the long run, fitting into a general reasonable view of the universe.

A belief is a habit of thought formed, as are all habits, mainly by repetition. To say that the belief is based on experience, is merely to say that the occasions calling for the exercise of the correct habit have been sufficiently numerous to lead to its formation. To draw conclusions under the dominance of a belief is strictly analogous, likewise, to action in accordance with habit. Deduction is the correlative of willing; induction, of habit-learning; reasoning by analogy or hypothesis is the correlative of feeling. If a habit be strong, then, given the compelling circumstances, the resulting action can be predicted. We say of a man that he always acts in such or such away. If his habit be less strong, he may still be *liable* to act in the characteristic way; and we may perhaps say that the chances are three to one that he will. But knowing that there were certain circumstances tending to form a habit, we should find it very difficult, wellnigh impossible, to tell how strong a habit a man placed in those circumstances would form. Still one could safely wager that, taken as they come, men would be unlikely to form habits leading to disastrous results in the given circumstances. In the long run, most of them would certainly learn better; a belief that mankind can walk on water would not be likely to spring up among sailors.

Again, knowing that under certain circumstances a friend acts in a certain way, can we from the action assume the circumstances? Can we calculate the probability that these circumstances, rather than some other circumstances, are the cause of the action? Does the fact that this friend carries a valise tell us

whether he is going to a train or coming from it? When one feels that he is singing correctly, is he really singing correctly ? Is feeling that we know anything, more than knowing that we feel? Were those who let Sully escape at the massacre of St. Bartholomew justified in assuming that he who carried a missal was a Catholic? Can we in any of these cases calculate the numerical probability of the conclusion? This point is worth dwelling upon; for it may enable us to see why Peirce, though trained as a mathematician, was not misled by that school of mathematicians, with Laplace at the head, who pretended to calculate the probability of an induction; as though, forsooth, one could put a lot of universes in a hat and wager seven to three, say, that our sort of universe would be drawn out. Does this look absurd? Well, Laplace calculated that the odds against the then received mass of Jupiter being in error by two per cent. was 999,308 to 1. The perturbations of Juno proved, nevertheless, that it was two per cent. in error. Neither did Peirce assume, as did Mill, any such principle as the "uniformity of nature, " but merely that, in observing a series of events we were more likely, in the long run, to observe what did characterise the series than what did not.

Peirce's lectures were illustrated, as has been intimated, by important theories from the principal sciences. At the very beginning it was necessary to emphasise the distinction between mathematics and the other sciences, though these others may, at least in certain of their branches, tend to become mathematical. It is true, as Sylvester pointed out in his controversy with Huxley, that mathematicians use induction; but that does not make of mathematics an inductive science. The final proofs, and no true mathematician rests content until he gets these,—the final proofs are always deductive. This is one characteristic of mathematical reasoning; but another, quite as obvious, is the intricacy of the reasoning. Whatever the subject matter may be,—number, space, time, force, groups, relations of any sort,—if the final proofs are obtained by intricate deductive reasoning, then we are thinking mathematically. In the Century Dictionary, to which he was an extensive contributor, Peirce defined mathematics as "the study of ideal construction& (often applicable to real objects) and the discovery thereby of relations between the parts of these constructions before unknown." This conception supplements the earlier idea, as indeed it was supplemented in his lectures, by pointing out that by deduction, as well as by induction, one is led to discover truths before unknown. For that matter, it is hardly likely that any considerable part of mathematics could ever have been developed except by the help of deduction; by this process the mathematician has been led into ideal fields that had else remained wholly unexplored; reason has transcended imagination. Cantor would even replace the terms *pure* and *applied* mathematics by the terms *free* and *constrained* mathematics. The physical sciences may become, and have become in some degree, mathematical; but free they cannot become and yet remain physical. The service of mathematics in these sciences is that one is enabled by it to make long-range predictions; when these predictions are found, by observation and experiment, to be in consonance with the facts, then there is reason to believe that a consistent, well-ordered theory is growing up. The discovery of Uranus verified not only the prediction thereof, but also the previous observations and the reasoning based on these observations which led to the prediction; it verified the scientific method of astronomy. Similarly the discovery of Galium was a verification of Mendelieff's law and of the science of chemistry; while the discovery of the Hertzian waves rendered a like service to physics. Thus, there is need for the verification of mathematical deductions. Without this verification, they are dangerous conclusions; and the farther the deductions are carried, the greater the danger. It is precisely the most far-reaching speculations of science, those that get farthest away from observed facts, that must be suspected. It is a characteristic of many theories, for example, the wave-theory of light, that while up to a certain point they triumphantly explain the facts, yet, as one pushes the theory farther and farther, a realm is reached that is mainly a realm of conjecture. Hence the need for crucial experiments.

The matter was neatly illustrated one evening by Peirce before the Johns Hopkins Scientific Society. He reviewed Priestley's investigation concerning atomic weights and specific heats. On comparing the specific heats and the atomic weights of each of the monad elements, Priestley found that he could write a formula expressing each specific heat in terms of the corresponding atomic weight. This is easy; any small set of numbers can thus be paired off with another small set. But when Priestley tried his formula upon the dyad elements, it did not work. He found, however, that by a modification he could get a formula to hold

for both monads and dyads. This formula did not hold, however, for the triads. So there was another modification, and after that still another. When in this way he got a formula holding, say, for half the elements, would there be any probability of its holding for the others? If not, why not? The method taken to show the fallacy of the assumption of such a probability was to assign the specific heats at random among the elements and then show that Priestley's method would work just as well as ever. Priestley's predictions had uniformly failed; thus no confidence could be placed in his final prediction. Again, of all the conics that pass through four points, only one, in general, passes through an arbitrary fifth point. But when an astronomer calculates an orbit from a few observations, and then finds that the planet is always to be found following that orbit with reasonable approximation, his computation and the underlying theory are alike verified. Why is not this an exact verification? In brief, because observations are not exact, neither those used for the basis of calculation nor those used to test the motion of the planet in the orbit. Nor are we exactly given all the forces acting upon the planet; some of them may be both unknown and unsuspected. Our observations are discordant and have to be reduced and smoothed by methods based on the theory of chance. Law, if law there be in these variations, has not manifested itself, and to us it is as though non-existent. But a study of the conception chance has in the very negation of law revealed a reign of law. Laws of chance, however, are never exactly kept, else would they cease to be laws of chance and freedom would succumb to habit.

May not the laws of the universe be the acquired habits of the universe ? May there not still be a possibility of the modification of these habits? May there not be the possibility, forever, of the formation of new habits, new laws? May not law be evolved from a primordial chaos, a universe of chance? In the play of chance still apparent may we not see the continual renewal of the life of the universe, a continual renewal of the capacity for habit-forming and growth? Tracing the evolution backwards we are led to the conception of absolute chance. This is of course a limiting conception; and we need not assume its actual existence, for there are countless ways in which a limit may be forever approached and yet never reached. It is not wise, certainly it is not pragmatic wisdom, to go too far from facts. But does not the universe now behave as if in transition from less of law to more? Does it not behave as if there were a never failing source of new life, new energy, which is to be, in turn, brought under the reign of law? There is a certain likeness, in all this, to the "creative evolution" of Bergson, though to me it seems a clearer and more definite conception than Bergson's, and one more in touch with science, more in harmony with experience and observation. For it does not require us to reject the supremacy of intellect and reason; certainly it would not use the triumphs of science as an argument against science, or say that science is successful because of its limitations, because blind to everything except the grossly material. Surely it is sufficient to admit, as does Peirce, that many matters, and vitally important matters, have not as yet been brought under the domain of reason; to admit that many matters never can be reduced to reason; and again to admit that action is demanded and always will be demanded, in matters irrational. This is merely saying that where we have not knowledge, we are compelled to go ahead as well as we can without it. Who shall set bounds to observation, to experiment, to the reasoning, inductive and deductive, based thereon? Surely, there is an infinite possibility for the formation and development of ever new bodies of knowledge—that is, new sciences. The conception of the universe in transition but increases this possibility; even the laws of the universe are evolving and so have themselves laws of evolution. In the words of Peirce, "in spite of the general shipwreck of ideas, the flotilla of knowledge is constantly increasing."

It would require great technicality to present any large number of Peirce's criticisms of scientific theory, but I will indicate a few of these. Mechanics expresses all motion and force in terms of five units, three of space, one of time, one of mass. What are the masses of bodies? Numbers which we attach to them in order that our equations of motion may be satisfied. Can there be action at a distance? That is, can a body act where it is not? What do we mean by the presence of a body except that it has certain effects, exerts a certain force? The table is there, because, when we press there, it presses back. Very well; if repulsive forces prove the existence of a body, so do attractive: that is how Uranus was discovered. If, then, a body is where it acts and the law of gravitation is true, each and every body exists throughout the universe, and action at a distance is not action where a body is not, but merely where its attraction prevails over its repulsion. The boundary of a body is the place where vanish certain differential coefficients. The reasoning by which chemists were in the first instance led to the atomic theory was very bad. The law of simple proportions was not helped by the conception of atoms. The chemists did not then, or for a long while after, give a reason why atoms should not combine in very complicated proportions, say 3,872 atoms of hydrogen to 11,916 of oxygen. The real proof of the existence of molecules and atoms was furnished by physics in connection with one of its great triumphs, the kinetic theory of gases. The triumph of chemistry was Mendelieff's law. The establishment of this law was remarkable because it is difficult either to establish or disestablish laws of periodicity; waves so melt into waves. For example, it has so far baffled observers, whether there may not be some connection between lunar movements and earthly weather. The progress of chemistry will lie in the explanation of valency, which includes why elements combine in simple proportions. Many of the recondite phenomena of physics, chemistry, and especially biology may be explained by supposing the atoms composed of atomicules. To-day we should substitute "ions" for Peirce's "atomicules"; but his idea was prophetic. One of his most striking lectures had to do with the application of this conception to the phenomena of growth and reproduction. As to whether biologic phenomena can be explained mechanically, he remarked that the processes to which the conservation of energy is applicable are reversible, but life is an irreversible process. He held, further, that without catastrophes from time to time the accumulation of minute chance variations by the survival of the fittest was insufficient for the development of a new species. His reason was that chance variations, in accordance with Bernoulli's law, would always preserve the dominance of the original type. He admitted that, assuming (in accordance with his philosophy) the habit-acquiring property in the universe, the development of a new species by minute variations is possible; but he insisted that the biologists had never called attention to this fact. Remember that this was thirty years ago.

In such theories as the scientists were then developing, Peirce saw the germ of the philosophy of the future, the foundations of an edifice yet to be erected. The attempts of the Germans to erect a philosophic edifice in the air, without such foundations, merely caused him to smile. Had not the German philosophers proven each other false? Why give them ear? Kant, instead of asking how are synthetic judgments *a priori* possible, should have asked the simpler and more profitable question, how are synthetic judgments possible. Perhaps he had then been led to a correct theory of induction. As for a *Ding an sich,* it could be only a thing out of relation-that is, out of existence. Mill's *Logic* he characterised as a great philosophic work, embodying the philosophy of ordinary mankind. But Mill did not know what was important in science. To such a degree was this true that most of the instances of scientific induction which he gave, in the first edition of his book, afterwards turned out to be bad inductions. Mill should have concluded from this fact that there was something wrong with his theory. Moreover, though on first reading seemingly clear, Mill is really not so; study of his work brings out ambiguities and contradictions. During the beginning of one's study of logic, we were told—that is, during the first ten years—one should devote oneself entirely to learning the exact meaning of words. Mill had neglected this.

Perhaps I have, in some measure, reproduced the atmosphere of Peirce's Hopkins lectures. To complete the conception of the man, it would be necessary to exhibit him in his talks before the Logic Club, necessary to speak of his papers in the old *Journal of Speculative Philosophy* and in the *Monist*, besides more technical papers elsewhere. But this would require profound and long-continued study. I may say merely that the deeper one enters into the spirit of Peirce's teachings the more logically and philosophically satisfying, the more complete and harmonious and inclusive they seem to be.

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