The Basic Writings of Josiah Royce, Volume II
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Inductive scientific generalizations, in the logically simplest cases, depend upon what Mr. Charles Peirce has defined as the method of taking a "fair sample" of a chosen type of facts. Thus one who samples, to use Mr. Peirce's typical example, a cargo of wheat, by taking samples from various parts of the cargo, carefully selecting the samples so that they shall not tend to represent one part of the cargo only, but any part chosen at random, employs essentially the same inductive method which, as I gather from inquiry, Vir- chow used in reaching the main fundamental generalizations of his cellular pathology. Samples chosen from investigation from a great variety of growths show, both in the case of normal and in the case of morbid tissues, that in the observed samples there is sufficient evidence of the origin of each cell from a previous cell, and evidence too that the tissue is formed of generations of cells whose beginnings, both in the normal and in the morbid growths, lead back to parent cells of certain definable types. This outcome of observation, repeatedly confirmed by samples fairly chosen, that is, by samples chosen from various organisms, from various tissues, and chosen not merely to illustrate the theory, but to represent as well as may be all sorts of growth—this, I say, leads to the
probable assertion that this kind of origin of tissues is universal, and that one is dealing with a genuine law of nature. The probability of such a generalization can be tested in a more or less exact way, as Peirce has shown, by the principles of the mathematical theory of probabilities. Inductions of this type we may call statistical inductions. They presuppose nothing at the outset as to what laws are present in the world of the facts which are to be sampled. The technique of induction here consists wholly in learning, (1) how to take fair samples of the facts in question, and (2) how to observe these facts accurately and adequately. This kind of induction seems to be especially prominent in the organic sciences. Its logical theory is reducible to the general theory of probability, since fair samples, chosen at random from a collection of objects, tend to agree in their constitution with the average constitution of the whole collection.

But now, as you well know, a great deal of scientific work consists of the forming and testing of hypotheses. In such cases the inductive process is more complex. Peirce defines it first as the process of taking a fair sample from amongst the totality of those consequences which will be true if the hypothesis to be tested is true, and secondly as the process of observing how far these chosen consequences agree with experience. If a given hypothesis, in case it is true, demands, as often happens, countless consequences, you of course cannot test all of these consequences, to see if every one of them is true. But you select a fair sample from amongst these consequences, and test each of these selected consequences of the hypothesis. If they agree with experience, the hypothesis is thereby rendered in some degree probable. The technique of induction now involves at least four distinct processes: (1) The choice of a good hypothesis; (2) the computation of certain consequences, all of which must be true if the hypothesis is true; (3) the choice of a fair sample of these consequences for a test; and (4) the actual test of each of these chosen consequences. So far as you make use of this method of induction, you need what is called training in the theory of your topic, that is, training in the art of deducing the consequences of a given hypothesis. This may involve computations of all degrees of complexity. You also need training in the art of taking a fair sample of consequences for your test; for a given hypothesis may involve numerous consequences that are already known, from previous experience, to be true. And such conse-
quences furnish you with no crucial tests. In case of success, your hypothesis may become very highly probable. But induction never renders it altogether certain.

Classic instances of this method of induction exist in the physical sciences. In the organic sciences the process of testing hypotheses is frequent, but is less highly organized, and generally less exact than in the great cases that occur in the inorganic sciences. No theory of the consequences of any hypothesis in the organic sciences has ever yet reached the degree of precision attained by the kinetic theory of gases, or by the theory of gravitation.

So much for the two great inductive methods, as Peirce defines them. But now does successful scientific method wholly reduce to these two processes, viz., (1) sampling the constitution of classes of phenomena; and (2) sampling the theoretical consequences of hypotheses? Many students of the subject seem to think so. I think that the history of science shows us otherwise.

As a fact, I think that the progress of science largely depends upon still another factor, viz., upon the more or less provisional choice and use of what I have already called, in this paper, leading ideas.

A leading idea is, of course, in any given natural science, an hypothesis. But it is an hypothesis which decidedly differs from those hypotheses that you directly test by the observations and experiments of the particular research wherein you are engaged. Unlike them, it is an hypothesis that you use as a guide, or in Kant's phrase, as a regulative principle of your research, even although you do not in general intend directly to test it by your present scientific work. It is usually of too general a nature to be tested by the means at the disposal of your special investigation. Yet it does determine the direction of your labors, and may be highly momentous for you.

Such a leading idea, for instance, is the ordinary hypothesis that even in the most confused or puzzling regions of the natural world law actually reigns, and awaits the coming of the discoverer. We can not say that our science has already so fairly sampled natural phenomena as to have empirically verified this assumption, so as to give it a definite inductive probability. For as a fact, science usually pays small attention to phenomena unless there appears to be a definable prospect of reducing them to some sort of law within a reasonable time; and chaotic natural facts, if there were
such, would probably be pretty stubbornly neglected by science, so far as such neglect was possible. On the other hand, the leading idea that law is to be found if you look for it long enough and carefully enough is one of the great motive powers not only of science but of civilization.

It may interest you to know that the modern study of the so-called axioms of geometry, as pursued by the mathematicians themselves, has shown that such principles as the ordinary postulate about the properties of parallel lines (as Euclid defines that postulate) are simply leading ideas. What the text-books of geometry usually assert to be true about the fundamental properties of parallel lines is a principle that is neither self-evident, nor necessarily true, nor even an inductively assured truth of experience. It turns out, in the light of modern logical mathematical analysis, to be, I say, simply a leading idea,—that is, a principle which we can neither confirm nor refute by any experience now within our range, but which we use and need in geometry precisely because it is so serviceable in simplifying the geometry of the plane.

If I may venture to cite an example from your own science, I should suggest the following: That fundamental principle of Virchow's "Cellular Pathology" which asserted the origin of every cell from a cell was, as I already said, a perfectly straightforward induction, of Peirce's first type, that is, it was a probable assertion of a certain constitution as holding for a whole type of cases—an assertion made simply because this constitution had been observed to hold for a sufficient number of fairly selected samples of the type. But, on the other hand, consider another principle which Virchow asserted already in 1847 or earlier, and which, as I have long been told, has been of the first importance for the whole later development of your science: "We have learned to recognize," says Virchow, "that diseases are not autonomous organisms, that they are no entities that have entered into the body, that they are no parasites which take root in the body, but that they merely show us the course of the vital processes under altered conditions" ("das sie nur den Ablauf der Lebenserscheinungen unter veränderten Bedingungen darstellen").

Now of course I have nothing to suggest regarding the objective truth of this assertion. But I venture to point out that, logically regarded, it is not an hypothesis to be definitely tested by any observation, but is rather an hypothesis of the type of Euclid's postulate about the parallel lines, that is, it is a leading idea. For, on
the one hand, how could Virchow regard this principle as one that had been definitely tested, and already confirmed by direct observation and experience at a time when, as in 1847, he was not yet possessed even of his own general principle of a cellular pathology, and when he regarded the whole science of pathology as in its infancy, and the causation of disease as very largely unknown? On the other hand, what experience could one look for that would definitely refute the principle if it were false? Would the experience of such facts as those of your modern bacteriology refute that principle? No, at least so far as I understand the sense of the principle as Virchow stated it in 1847. For when bacteria, or when any of their products or accompaniments came to be recognized either as causing disease, or as affecting the course of disease in any way, it was still open to Virchow to say that the causes thus defined simply constitute these very veränderte Bedingungen under which the Ablauf der Lebenserscheinungen takes place. In other words, the principle, if understood with sufficient generality, simply asserts that a disease can not occur in an organism without the processes of the disease being themselves alterations of the processes of the organism, and such alterations as the altered conditions, whatever they are, determine. Such a principle, so understood, seems tolerably safe from empirical refutation. It would remain unrefuted, and empirically irrefutable, so far as I can see, even if the devil caused disease. For the devil would then simply be one of the veränderte Bedingungen. Thus when the devils on a famous occasion entered, in the tale, into the Gaderene swine, the Ablauf of the Lebenserscheinungen of the swine was such, under the veränderte Bedingungen, that, as we are told, they ran down a steep place into the sea. But I do not see that this just stated pathological postulate of Virchow’s need have suffered shipwreck, or need even have received any damage, even on this occasion. The devils are indeed represented in the tale as entities that from without entered into the swine, as bullets might have done. But the running down into the sea is nur der Ablauf der Lebenserscheinungen of the swine themselves. Let bullets or bacteria, poisons or compressed air, be the Bedingungen, the postulate that Virchow states will remain irrefutable, if only it be interpreted to meet the case. For the principle merely says that whatever entity it may be, fire or air or bullet or poison or devil, that affects the organism, the disease is not that entity, but is the changed process of the organism. What then is this hypothesis, this rejection of every ex-
ternal-entity-theory of disease, as the hypothesis appears when Virchow writes these words in 1847? I reply, this is no hypothesis in the stricter sense; that is, it is no trial proposition to be submitted to precise empirical tests. It is, on the contrary, a very precious leading idea. It is equivalent to a resolution to search for the concrete connection between the processes of any disease and the normal process of the organism, so as to find the true unity of the pathological and the normal process through such a search. Without some such leading idea, the cellular pathology itself could never have resulted; because the facts in question would never have been observed. And I suppose that some equivalent leading idea, if not precisely that which Virchow stated in 1847, is just as precious to you to-day in your own pathological work.

The value of such leading ideas for a science lies in the sorts of research that they lead men to undertake, and also in the sorts of work that they discourage. They are, I repeat, regulative principles. Observation does not, at least for the time, either confirm or refute them. But, on the other hand, they awaken interest in vast ranges of observation and experiment, and sustain the patience and enthusiasm of workers through long and baffling investigations. They organize science, keep it in touch with the spirit of the age, keep alive in it the sense of the universal, and assure its service to humanity. Specialism, without leading ideas, remains but a sounding brass and a tinkling cymbal.

The sources of useful leading ideas seem to me to be various. Social, and in particular industrial interests, suggest some of them, as the perennial need of paying the coal-bills for the steam engines suggested, as we have seen, one of the leading ideas which pointed the way towards the modern theory of energy. The comparison of the results of various sciences awakens such leading ideas in various minds. Schleiden set Schwamm searching for the basis of the cell theory in animal tissues. That was the suggestion of an hypothesis in the narrower sense, to be tested. But when the physical sciences set the students of organic science to the work of conceiving organic processes as mechanical in their inmost nature, that was the suggestion of a leading idea.

But another source of such leading ideas has been, upon occasion, philosophy. Philosophy itself might be defined as a systematic scrutiny of leading ideas. It has also proved to be often an inventory and interpreter of such ideas.