The Popular Science Monthly vol. 83 no. 4, October 1913

SOME PSYCHOLOGICAL PROBLEMS EMPHASIZED BY PRAGMATISM

By Professor JOSIAH ROYCE
HARVARD UNIVERSITY

IT is not my purpose upon this occasion to enter into the philosophical aspect of the discussion regarding pragmatism, excepting in so far as may be necessary to call attention to the psychological problems that I now have in mind. I presuppose, of course, a familiarity on the part of all of you with the main outlines of the recent discussions concerning the problem of truth. But I shall not try with any exactness to define what the term pragmatism means. I recognize that the word as now used refers to a considerable variety of opinions, and that the comparison of a "holding company," which Professor Dewey has, I believe, on one occasion employed, is not altogether inapt. It is enough for our present purpose that by the name pragmatism we all of us mean a certain set of tendencies in recent discussion which lay stress upon the importance of defining the truth of propositions or judgments or ideas in terms of those empirical facts and relations of facts which are said to constitute the "workings" of the propositions, or judgments, or ideas which are in question. The favorite summary of pragmatism, to the effect that from the point of view of pragmatism a proposition, or judgment, or idea is true "if it works," is sufficient to serve as a general indication of the tendencies of opinion which are here in question.

What pragmatism asserts about truth may be considered from the point of view of a general theory of knowledge, or of a metaphysic. But pragmatism itself especially emphasizes its relation to psychology, on the one hand, and to the recognized methods of empirical science, on the other. As Mr. Schiller said to me during the Philosophical Congress of 1908 at Heidelberg, "What is most essential to pragmatism is that it insists that the relations and values of the thinking process must be estimated in psychological terms. Success tests truth, and success is itself a matter of experience that can best be understood when it is defined psychologically." Another way of stating the essence of pragmatism is to insist, as Professor Dewey has so often done, upon the fact that the method which pragmatism proposes to apply to all problems is the method already used by the various sciences of experience. They employ "working hypotheses." They test these working hypotheses by

comparison with experience. Pragmatism consists in the assertion that all propositions should be tested as the hypotheses of science are tested.

These two points of view are, of course, very closely connected. Students of the physical sciences often take little account of the psychology of their own processes. But the processes of science obviously have their psychology, and this psychology conforms in its types to those laws of mental activity which have interested psychologists ever since the apperceptive process was formulated by Herbart,—yes, ever since the doctrine of association was so widely applied by the English psychologists, and still more since the modern so-called "functional" psychology has connected the apperceptive processes and the associative linkages with the physical processes whereby an organism is adjusted to its environment. The psychology of the apperceptive process, and the work of the scientific finding and testing of hypotheses, have a close relation, and since common sense also is interested in successful verifications, although this interest is less precise than is the interest of the student of the more exact sciences of observation in the criteria to which they submit their more careful tests,—common sense and science and psychology join in contributing their various shares to the modern general theory of truth.

But now to come to the matter to which I wish especially to attract your attention. Since pragmatism is thus especially interested in the psychology of the thinking process, it has emphasized this psychological problem in recent literature. A general psychology of thought, on a pragmatic basis, has been worked out by Professor Pillsbury. The psychological text-books of the Chicago school, and in particular the contributions of Professor Dewey, have familiarized us with other accounts of the psychology of thought. The psychological problems to which attention is thus especially attracted may be, of course, studied apart from their relations to the theory of truth. These problems are threefold. (1) There are the problems regarding the processes whereby hypotheses are invented, or, in common-sense terms, the processes whereby people get their ideas; (2) there are problems regarding the processes whereby ideas, once in hand, are made sufficiently clear to be a proper subject for testing; and (3) a psychological problem arises as to what happens when an idea is tested. To all these problems the pragmatists as psychologists have contributed. I wish to illustrate in the course of my discussion a certain dissatisfaction which I feel with the present state of some of their contributions to these purely psychological issues, when viewed apart from the other issues of the pragmatist philosophy.

Yet I admit that when you hear me you will say that my psychological dissatisfactions are due to certain philosophical dissatisfactions, and that the pragmatist psychology appears to me inadequate partly

because I do not wholly accept the pragmatist theory of truth. I recognize the connection in my own mind between the two dissatisfactions. I do not wish to pretend that I can wholly dissociate my interest in psychology from my interest in the other aspects of the pragmatist controversy and in the very nature of pragmatism itself. My pragmatist friends least of all would desire me to do this. And so I shall aid you in following my further inquiries if I first briefly state one ground of my dissatisfaction as a student of logic and of general philosophy, with the pragamtist's theory of truth. This statement I make not as if it were here important for the psychological purpose of this discussion, but because by confessing my own state of mind I may help you to fix your attention upon matters that will more directly interest you. For my dissatisfaction as a student of logic, with the pragmatist theory regarding truth, will call attention to the way in which I should approach precisely those psychological problems which pragmatism has most emphasized.

I

When pragmatism asserts that the truth of a proposition is tested by the "workings" of the ideas that the proposition expresses, a student of logic very naturally raises the question as to *what* workings are meant. A man who hears a proposition and who more or less completely understands its meaning, and who hereupon more or less believes the proposition, has certain mental attitudes aroused in him, and these mental attitudes have their physical expression. They tend to lead to action. The action may well be accompained by expectations of various sorts, and the expectations may remain throughout more or less identical with those that the utterance of the proposition first arouses in the mind of the inquirer. Thus ideas may lead to actions. These actions may gratify or in a measure satisfy expectations. In this case the ideas which the proposition expressed are said to "work." But now consider the contrast between what this decidedly general statement expresses and what a student of any more exact empirical science is likely to have in mind when he thinks of testing the truth of a definite assertion. One familiar process of testing the truth of hypotheses in scientific regions is to trace the consequences that must be true if those hypotheses are true, and then to see whether these consequences can be found verified by particular experiences. An essential part of this process is the deduction of certain consequences from one's hypothesis. How extensive this deductive process may be, a glance at any textbook of theoretical physics, in particular of theoretical astronomy of the classic type, will show.

Now what happens when one deduces the consequences of an hypothesis? Does one simply let one's ideas work? Are the consequences of hypotheses simply ideas that are as a fact aroused in the mind of a

man who begins with the hypothesis itself in mind? Certainly the usual taste and purpose of the student of any more exact science is *not* sufficiently expressed by saying that he looks merely for such "workings" as may happen to be suggested to his mind. *He is looking for what must be true if the hypothesis is true.* He is making use of those mental processes whose nature is sometimes discussed in text-books of deductive logic. But when we are dealing merely with the world of common sense, ideas that have been suggested or propositions that have been uttered, frequently arouse in our minds expectations which are not directly called for by the logical meaning of the propositions, but which through various processes of association or various reminders of past experience are more or less casually aroused. Such resulting expectations may be said in a given mind to follow from the utterance of the proposition that arouses them. But they are not logical consequences of the proposition in the sense which the student of the more exact sciences has in mind.

Again, when the student of an exact science has made his deductions and proceeds to verify them, he may find his hypothesis confirmed or refuted by the results of experience. In this case his hypothesis may be said to "work," but what sort of working is in question in the more exact sciences? I answer, in any more exact science the confirmations or refutations which experience is able to furnish to the hypothesis whose logical consequences are to be tested, are required to be so exact that we can define them in definite *affirmations* and *denials.* The very essence of precise confirmation or refutation is that if it is as successful as the requirements of an exact science demand, we are able to say as the result of our process of confirmation, "So and so, thus and thus defined, happens or does not happen, is found or is not found." Or again, we sometimes say of the failing hypothesis, "It is *contradicted* by the facts." Somewhat different however is the situation in the world of common sense, where one's expectation may often be met or disappointed with very various degrees of definiteness. In the world of common sense a man may say, "This more or less meets my expectations." In the world of an exact science the investigator is interested in seeing within what precise limits a definite experienced measurement agrees with the prediction. *Precision,* in other words, characterizes the confirmations or refutations which experience furnishes in the more advanced sciences. And the concept of precision has characters which are studied in text-books of logic, although, as I admit, in most text-books of logic the concept of precision is very inadequately studied.

In consequence of all this the student of logic is likely to object to the ordinary formulation of pragmatism, (1) that pragmatism seems not to be interested in the distinction between merely arousing an expectation, and deducing a consequence; (2) that it takes compara-

tively little account of the distinction between feeling more or less personal satisfaction in partial agreements between experience and our expectations, and precisely confirming or refuting a determinate hypothesis, in so far as that can be done by a certain group of experiences.

And finally the student of logic finds a certain difficulty in the usual statements of pragmatism regarding the sense in which empirical verifications are said to lead to a certain "belief" that the ideas which we have been trying to verify are true. In ordinary life beliefs exist in all sorts of vague degrees of intensity. But when the student of a science formulates his results, considerable stress is laid upon the assertion that, by virtue of a given group of confirmations, a hypothesis has received a somewhat determinate degree of what is called *objective probability*. Now into the theory of probability this is no place to go. But most of you who have dealt with statistical probabilities will admit that our subjective confidence is somewhat different in its nature from that objective or statistically estimated probability which most students of an inductive science prefer to be able to define if they can. The whole modern development of the theory of probability has been in the direction of separating the concept of objective probability from the concept of subjective belief or confidence. From the objective point of view a proposition has a certain probability P in case it belongs to the class of propositions of which in the long run a proportion P are true. The difficulty of defining such probability with genuine exactness is great, and the whole subject of probability is too complex to be here discussed, but the student of logic feels dissatisfied with the fact that his pragmatist brethren take little interest in defining the difference between the vague confidence which in the world of common sense confirmed expectations may arouse, and the scientific measure of probability in exact and relatively objective terms which the students of an inductive science are commonly seeking.

II

So much for my general statement of logical scruples concerning the adequacy of pragmatistic formulations. Into the merits of these logical scruples I have no wish to go on this occasion. I have stated them merely in order to formulate the problems of a psychological nature to which I wish to attract attention. Pragmatism has emphasized these problems, has undertaken to solve them, has contributed a great deal to their study, but in my opinion has failed to satisfy all the requirements that it might satisfy, just because it is not sufficiently interested in the very logical problems that I have just outlined. These logical problems, however, have their psychological aspects. If one does not deal with them in an exact fashion from the logical point of view, one is not likely to have one's attention attracted to their psychological

complexity. On the other hand, if I can do anything to awaken your interest in the psychological character of these problems, perhaps I may indirectly help to awaken interest in their logical aspect.

Let me repeat the list of the problems to which I have called attention, emphasizing the sense in which each one of them is a psychological problem. I mention the fact that a science which is testing hypotheses *deduces the logical consequences of these hypotheses.* The process may be an extended one. *What is the psychology of deduction?* What happens when a process of deduction takes place? In some respects this problem has indeed been repeatedly dealt with from the psychological point of view. I do not wish in the least to deny that the analyses of Professor Pillsbury and others have contributed to this problem, but just because these psychologists have been so little interested in the logic of the deductive process they have failed, in my opinion, to emphasize some of its most important psychological aspects. Yet their own discussions emphasize the need of such a psychology. Here lies the first of the problems to which I now call attention.

Secondly, the pragmatists in speaking of the working hypothesis have emphasized, as Professor Moore has recently done, the fact that the agreement of an assertion or idea with its expected workings constitutes the test of its truth so far as up to a certain point in our investigations we may happen to have gone. I have called attention to the difference between an expectation and an assertion or a denial. One goes to the play expecting amusement. At the end of the play, have his expectations been met or not? The question may be unanswerable in any definite way. The play was amusing, and yet perhaps not so very amusing, or not so amusing as one could have wished it to be. One goes away partially disappointed, partially pleased. One is not so disappointed but that one continues to go to plays over and over again. One is not so pleased that he expresses himself very enthusiastically. What ideas with regard to the amusing character of plays have been precisely tested, so long as one remains in this state of mind? On the other hand, through a deductive process the occurrence of an eclipse is precisely predicted. The eclipse is observed, its beginning is noted with an accuracy as great as the errors of observation permit. A precise assertion is made as to the agreement between the prediction and the observation. When the assertion has received its proper qualifications with regard to probable error and the rest, the assertion appears in the records as true or false. In this case an issue is met and something is tested, yes or no. I now ask, what is the psychology of assertion and denial, of the difference between yes and no? In what way does this difference, namely that between yes and no, differ from any other kind of difference? This problem I mention, without hoping in this paper to do more than mention it. I called attention to this psycho-

logical problem some years since in an address that I was permitted to give before the Psychological Association. I venture to emphasize this problem afresh and to declare that it is a problem which the whole pragmatist controversy has itself especially emphasized and has not yet adequately solved.

Again I have called attention to the difference between vaguely estimated confidence and objective probability. Here is a problem that once more presents psychological aspects. I shall have no time to discuss them upon this occasion. The psychology of probability is, however, to my mind one full of very interesting problems.

III

I have thus enumerated three of the psychological problems which to my mind are emphasized by the course of the pragmatistic discussion. That these problems come to my mind with a special force because of my logical interest, you see. It is now my purpose to appeal to you as psychologists or as students interested in the subject, to follow for a few moments some further characterization especially of the first of these psychological problems. I am dissatisfied in the recent discussions of the psychology of reasoning with what seems to be a failure to understand what takes place in exact deductive procedure. The current prejudices as well as the hoary traditions on this subject seem to conspire to call the attention of students away from the center of the problem. Without attempting to give any adequate summary of Professor Pillsbury's account of the reasoning process in his recent "Psychology of Reasoning," I may attempt by a few references to indicate how inadequate some current views are to take account of what the deductive process actually is. In Professor Pillsbury's "Psychology of Reasoning," he distinguishes pretty sharply between the two processes of *inference* and *proof*. By inference, if I understand him, he means the process whereby a conclusion is suggested in such wise as to arouse more or less belief. By proof he means a process whereby this belief is more or less adequately tested. Now logicians are accustomed to use the word inference in a somewhat different way from that which Professor Pillsbury emphasizes. And what this way is I shall try to point out in a moment. But laying inference aside for the moment, and passing to the other side of the reasoning process as Professor Pillsbury defines it, namely to the process of proof, the only form of deductive proof which Professor Pillsbury seems to emphasize is the one that has received its traditional description in the doctrine of the syllogism. The essence of the traditional syllogism is, according to Professor Pillsbury, that the general major premise is supposed to aid us in testing our belief in the conclusion, by virtue of the fact that in the minor premise something

has been brought under this major premise as a particular case of the principle. Professor Pillsbury consequently points out how comparatively insignificant both from the logical and from the psychological point of view the syllogism is as an expression of the nature of the concrete process of reasoning. To bring cases under major premises is to do little to confirm our belief except in so far as one thus emphasizes in a somewhat formal way the tendency of every new or questionable fact to find its place by being brought into conformity with the habits, or better with the principles of action, which have been formulated on the basis of previous experience. Deductive proof appears therefore to be, as Professor Pillsbury says, not so different from induction as is usually supposed, and to be in any case of comparatively minor importance. The business of proof, according to Professor Pillsbury, is to produce belief. Belief in general is not produced by formulating major premises. It is produced by a more complex process whose general nature he sets forth.

Other familiar discussions of the reasoning process, such as one finds in the text-books of recent pragmatists, agree with Professor Pillsbury in assuming that it is of the essence of deduction or of deductive proof to proceed from the general to the particular, and that the significance of deductive proof lies in the fact that one hereby formulates, often somewhat uselessly, the general process by which an adjustment to the environment in cases of initial doubt or difficulty is attained. Apart from these statements more characteristic of pragmatists, a wide-spread tradition, which unfortunately is supported by the older logical text-books themselves, maintains that it is of the essence of deductive reasoning to bring nothing out of the premises except what was already in them, so that the essence of the deduction is "analysis." From this point of view it is supposed that when you engage in deduction you solemnly draw out of the bag the cat which you have already hidden in it. You declare, for instance, that all the A's that are B are indeed B, and solemnly demonstrate that all the white mice must be both white and mice. It is unquestionable that many of the logicians of the past as well as the psychologists of recent times have conspired to give this impression of the deductive process. But whatever the psychology of deduction may be, any fair examination of the work of the exact deductive operations of science, and especially any examination of the work of mathematics, shows that deduction as it exists in real life is simply not this fiction of the older logical text-books. And yet to the psychological analysis of this fiction, with the natural result that such a process is not of very great importance, Professor Pillsbury, as I understand him, devotes himself in his discussion of the place of the syllogism in life.

But anybody who undertakes to deal with the psychology of reason-

ing ought, I think, to take account of the fact that there is nowadays a new logic, that this new logic is in considerable part the work of men whose attention has been attracted to the nature of the deductive process by a wide experience of mathematical procedure, and that this new logic shows us with regard to the syllogism, for instance, two things, first that the essence of the syllogism does not consist in the fact that a particular case is brought under a general principle; and secondly, that the syllogism is by no means the only form of deductive reasoning. From the point of view of the new logic, the student has upon his hands the problem that Poincaré has so well stated at the outset of his book, "Science and Hypothesis." This is the problem presented by the enormous *Fecundity of the Deductive Process.* Our own American logician, Charles Peirce, long since called attention to this fecundity. It is a fact of much philosophical importance. What I mean by the fecundity of deduction as a logical fact can be suggested by what Poincaré mentioned, and also by a summary of the matters to which Peirce has frequently called attention. Poincaré states the case thus: From the point of view of the older interpretation of the nature of the syllogism it would seem impossible that a deductive science such as mathematics could do anything but draw out of premises what it had already more or less overtly or secretly put into them. Nothing novel could result. And in fact if the reasoning of mathematics were all of the kind that Professor Pillsbury supposes to be the typical deductive reasoning, mathematical science would consist in a process as stupid and monotonous as the process of taking the major premise, All men are mortal, and then looking up all the names in a directory and solemnly concluding to write opposite to each name the fact that since this person is a man he is mortal. But now as a fact mathematical science consists of nothing of the kind. The situation is actually this: you can write upon a few sheets of paper an accurate statement of a set of principles from which the whole science of the quantities of ordinary algebra can be deduced. That is to say, the principles of the ordinary mathematical analysis are capable of such a brief statement as this. But the consequences of these principles are such that novel results in vast numbers are annually discovered. These results are not stowed away in the premises in any such way as that in which the mortality of this man is stowed away in the assertion of the mortality of all men.

Poincaré, in the passage to which I have referred, suggests his own theory to account for the fecundity of mathematical analysis. His theory may as a logical theory be questioned. But the fecundity to which he attracts attention ought to be a commonplace to any one who has looked into any branch of mathematics with care. Peirce has called attention to this fact, and speaking as a logician has gone further. Following a lead of De Morgan's, Peirce has shown that any proposition

whatever which has a definite meaning permits you to draw from it an infinite number of deductive inferences, all of which are possible without formulating any other basis for the deductions in question than the assertion of the original proposition and the synthetic power which one indeed has in his hands who is capable of understanding certain simple processes of the construction of relations. Let me mention the instance, famous in modern logic, which De Morgan first formulated; and which as it stands may appear trivial enough. If a horse is an animal, you can deduce from that hypothesis the conclusion that the owner of a horse is the owner of an animal, that the friend of the owner of a horse is the friend of the owner of an animal, and so on. Such deductions in an individual case such as that of the assertion about a horse may seem and are trivial enough. *But they have the character of novelty:* That is, the conclusion does not follow from the premises by the process of first stuffing a vast number of cases into a bag and then pulling them out one by one. But the process of deduction thus illustrated can be used and is used as an instrument of enormous power in those branches of mathematics in which one builds one system of relations upon another system. The number of ways whereby such deductive processes can be accomplished is presumably very great. And it is because such processes are possible that mathematical reasoning possesses its great fecundity.

And now since such fecundity, such proof of novelties, is the essence of the live process of deduction as it exists in the deductive sciences, why should not psychologists study that live process instead of studying the dead body which some text-books have called the syllogism? And if they must study the syllogism as the supposed typical example of deductive reasoning, why should they confine their attention to considering the most traditional and trivial aspect of it?

As modern logic has shown, the really essential feature of the syllogism lies in the fact that what the logicians call the Illative Relation (that is, the relation which is in mind when you consider one proposition as true in case another is true) is a relation which has the property of so-called *transitivity*. That is, the essence of the syllogism may be stated by saying that from the pair of propositions "*A* implies *B*," and "*B* implies *C*" taken together you can deduce the conclusion "*A* implies *C*." In other words, it is of the nature of the illative relation that it permits the use of what James called the principle or axiom of skipped intermediaries. I can not pause to show why this account of the essence of the syllogism is logically correct. But the mere mention of this fact shows that those who analyze the process of deduction, supposing it to be represented by the traditional syllogism, and interpreting the traditional syllogism in the way in which Professor Pillsbury interprets it, simply miss the most interesting feature of syllogistic reasoning.

Nor is the aspect of the syllogistic reasoning which this emphasizes, the only one to which modern logic calls attention. Mrs. Ladd-Franklin long ago pointed out that the entire theory of the syllogism could be stated as a sort of comment upon the fact that a triad of propositions which she called "a triadic inconsistency" has, when considered as a triad, a certain set of logical properties. These logical properties, belonging to such a triad of propositions, can be observed by a process which is of the nature widely illustrated throughout the whole realm of mathematics; but this is certainly *not* the synthesis that Professor Pillsbury has in mind when he analyzes what he supposes to be a typical process of deductive reasoning.

Still more unfortunate for the study of the psychology of the reasoning process is that misunderstanding of the nature of deduction which supposes that the principal use of a deduction is to bring to pass a belief in a certain conclusion by virtue of an appeal to a belief in certain premises. This assumption, common in the recent literature of pragmatism, is false to the most essential use of deduction in the exact sciences. Mr. Russell has well emphasized the fact that, in mathematical science, just in so far as it is pure mathematics, you are not concerned with producing belief in the conclusions themselves. Your interest in pure mathematics, that is to say in that science which deals with deduction proper, lies simply in showing that *certain premises do imply certain conclusions.* That is, you show that "p implies q," where p and q are propositions. The importance of mathematics for the empirical sciences is due to the fact that it gives you a means for testing the hypotheses by first finding out what are their logical consequences. Now it is essential for the fair and unprejudiced testing of an hypothesis, that you should *not* be too much disposed to believe in it before you test it. It is very important, when you do not believe an hypothesis, or when your mind is still perfectly open upon the subject, *to find out with exactness what would be true if the hypothesis were true.* Your purpose in deduction is therefore not to establish belief in certain consequences by virtue of a previous belief in the hypothesis upon which they depend. Your great interest is to produce *no belief whatever either in the hypothesis or in the conclusions from the hypothesis, until the logical issues are precisely defined* for empirical confirmation; and then you are *ready* to appeal to the confirming or refuting experience. It is a strange misunderstanding of the nature of the deductive process to suppose that its principal interest is an interest in producing belief in consequences. The sole logical interest of the deductive inquirer lies in his discovery that certain premises imply certain conclusions.

To sum up, then, this sketch: I assert that in the recent psychology of reasoning, *the nature of the deductive process and its principal purpose have been equally misunderstood.* Deduction in its more developed

forms simply does *not* consist in an analysis of the premises. Nor is it intended to make you believe the conclusion. The true interest of deduction lies in the fact (1) that it is a process possessing an inexhaustible fecundity, and (2) that this fruitfulness results in giving you a knowledge that certain premises imply certain conclusions.

IV

But now let me briefly put before you some genuine deductive processes, and point out what problems with regard to their psychology can be aroused. Let me begin with the instance with which I have often wearied my friends, so that some of you who are here present will have heard me mention it. Raise the question whether a strip of paper can exist which shall have only one side. One would be disposed to settle the question empirically by observing that every strip of paper in one's ordinary experience obviously has two sides. If one passes from a consideration of strips of paper that have two ends, to the consideration of endless strips of paper, that is strips of paper made in ring shape, one sees that in an ordinary ring the two-sidedness of strips of paper still holds good. But if one takes an ordinary strip of paper, say two inches wide and eight or ten inches long, first twists one end of it 180 degrees, and then brings the two ends together, one has the resulting geometrical form of the one-sided ring. The first discovery that such a ring is possible was of course an empirical discovery. But the geometers (I believe it was Möbius who first noticed one-sided surfaces) had their attention at once attracted to the mathematically interesting properties of this form. Now when such a form is viewed as a mathematical object, any one with mathematical interest naturally proceeds to an undertaking of the sort which is characteristic of mathematical science in general. One endeavors to deduce some of these properties from others, or to discover, as the ordinary mind would say, why these properties belong to any one-sided surface. Hereupon let me mention a problem that can be studied as soon as you have once taken note of the one-sided surface and have begun to make a study of the real sense or connection of its structure. Suppose a one-sided surface, a ring strip of the sort that I have described, to be cut down the middle, midway between what appear to be the two edges of the strip, and suppose the cut continued until it returns into itself, what will be the result? There are two ways of answering the question. One is the directly empirical one of making the cut. The other method is to endeavor to see before making the strip what must be the result in view of the one-sidedness of the strip. I once proposed the question to a class, and found a member of the class, who although not a student of mathematics, possessed a relatively clear visual imagination, was ingenious, became interested in the problem, solved it without cutting the strip, then tested

his solution by an actual cut, and then brought me written out the process of reasoning whereby he had solved the problem.

The process of reasoning in question, or any process of reasoning by which the problem could be solved in advance of actually cutting the strip, is in part a genuine instance of deductive reasoning. Just because of the intimate mingling of empirical data and of exactly definable relationships, the case in question forms an admirable instance of the study of the genuine psychology of the deductive process; but I confess that no psychologist would make much of the study who was not fairly well acquainted with deductive processes of a certain complexity, —processes which in their more exact forms you will find anywhere in pure mathematics, where a symbolic language with an exact definition is used, as the only means of presenting data to the imagination.

Let me mention another instance of a deductive process of some complexity, but of great ingenuity and of interesting psychological relationships. We know that about 500 B. C. a member of the Pythagorean school discovered that granting the ordinary principles of metrical geometry as they were then and ever since have been used, the diagonal of a square could not be commensurate with the side of the square. The strictly deductive portion of this proof can be with fair ease distinguished from that portion of the proof in question which is indeed empirical and not deductive. That figures resembling squares exist is a matter of experience. That if you make a square exactly enough and large enough, and measure carefully enough you will discover that by no rule you seem to be certain of stating the ratio of diagonal to side exactly in terms of whole numbers: this again is so far empirical. And the ordinary so-called axioms of metrical geometry, considered as principles about the constitution of the physical world, are of course generalizations from physical experience. On the other hand, the purely mathematical portion of geometry, that is, the purely deductive portion, consists in the discovery; not that the geometrical axioms are self-evident or otherwise certain, and not that the physical world has any properties whatever, but that certain assumed geometrical principles which can be stated wholly in symbols, actually imply certain geometrical conclusions. Now the early Greek geometer who discovered that the diagonal and the side of the square are from the point of view of geometrical theory incommensurable, was no doubt guided by the empirical difficulty of discovering any rule whereby a common measure for the diagonal and the side could be stated. Furthermore, he was not clear in his own mind as to the precise distinction between the deductive and the inductive part of his geometrical science. But he was possessed of the power to draw an exact deductive conclusion. And what he found out was that if certain principles of measurement and certain purely mathematical properties of whole numbers be admitted, the diagonal and

side of the square are incommensurable. That is, he discovered that certain premises imply certain conclusions.

The process by which he discovered this we happen, though somewhat indirectly, to know. The instance is a remarkable one of the fecundity of the deductive process. The conclusion when the Greek reached it was a novelty. It seemed to him so novel and uncanny a conclusion that the Pythagorean school is said by a later tradition to have long regarded the whole matter as a mystery which must not be mentioned to the vulgar. It was reported that the man who revealed this mystery came to a bad end and received special penalties in the underworld, so closely in those days was exact science linked with *tabu* and with superstition. Yet no one who once goes through the little process of reasoning by which the ancient geometer established his result can remain without interest in the psychology of such a process.

V

You see throughout how my account of the whole matter is of course colored by my logical interests, and yet I freely admit that the psychological problems at issue ought to be considered without any undue reference to anybody's philosophical prejudices or concerns. I admit my own bias in the matter, merely because I am thereby enabled to call attention to what the live process of deduction is, and to point out that the recent psychology of reasoning has profoundly neglected to take account of some of the most elementary facts with regard to the nature of this process.

Charles Peirce long ago called attention to the general nature of the psychological processes which are in question in deduction. They are processes of the nature of ideal experiments. The instance of the one-sided strip of paper readily shows how many intermediate steps there may be between such ideal experiments and physical experiments with a strip of paper. On the other hand, as soon as you begin to reason, and to predict what will be true about a given strip of paper if certain hypotheses are met, with regard to its structure, you get an insight into the whole situation which you can not possibly get by cutting the strip of paper without adding the deductive process. And in general, wherever deduction is worth while, the testing of hypotheses after deductions have carefully been made whereby we predict determinate results of such hypotheses, has a wholly different value and interest from that which results from the testing of hypotheses without previous deduction.

The psychology of deduction may then well be characterized as the psychology of the *Gedanken-experiment*. The peculiarity of the experimental processes in question is that whether we use symbols, or diagrams, or mental images, our reasoning depends upon the fact that the

objects in question are wholly under our control, so that in dealing with them we have no "course of experience," to use Charles Peirce's phrase, that is, no series of experiences which we have passively to await to see what they are, but are guided wholly by our own control, and are dealing wholly with objects which are what we make them. A given set of premises we construct in terms of symbols, diagrams, or figures, hereby expressing the meaning of these premises. Our deduction consists of the reading of this meaning from a new point of view. The fecundity of the process depends upon our power to combine at pleasure various constructions in various permitted orders and syntheses. The precise relation between such arbitrary objects, and the objects of ordinary experience, forms a topic of almost inexhaustible interest to the student both of logic and of the mental processes concerned.

I have thus indicated that the problems of the psychology of deduction have thus far hardly been attacked, mainly because psychologists have usually been so little interested in live deduction as it exists in mathematical science. So long as the myth still exists in text-books, that deduction is adequately to be represented by the form of the syllogism and the interpretation of that form which Professor Pillsbury cites and uses; so long as it is imagined that deduction merely lets out of the bag the cat that has already been put in it, our logic will languish and our psychology of reasoning will fail to fulfil the purposes of pragmatism or of any other doctrine of the reasoning process. So long as it is supposed that the main purpose of deduction is to produce belief in the conclusions, the psychology of certain of the most important human thinking processes must be lost. As a fact all tolerance, all considerateness in advance of action, all deliberate working out of ideal consequences of modes of behavior concerning which we deliberate,—all such processes would be impossible. A great deal of toleration depends upon seeing how my opponent's conclusions are related to his premises, although I may have no belief either in his premises or in his conclusions. The process of deduction, in case of a practical deliberation concerning what it is best to do, helps us because we thereby learn in advance what *would* be the case *if* so and so were done, even if we ourselves have no tendency whatever as yet to decide in favor of the hypothetical course of procedure.

It seems to me then that the fecundity of the deductive process, the essence of the ideal experiment, and the genuine use of deduction, where it is not intended to produce belief but to give us insight into a connection of premises and conclusion, should form the topic of psychological studies such as thus far have attracted small attention.

VI

Some of the studies that I thus suggest may be of a nature to be treated by the methods of experimental psychology proper. I do not see why the psychological process of solving deductive problems that really illustrate the fecundity of deduction should not be, in certain cases, proper objects for detailed introspection. Let me mention a few possible cases. The one-sided strip of paper and a considerable number of related geometrical forms, may be made the topic of more or less direct experimental inquiry. Trained observers might undertake to solve such problems, namely, as deductive problems proper, that is, as problems of working out what conclusions follow from what premises. The deductive process proper could be separated in such cases from the special empirical materials used. And if the process is brief enough, or can be sufficiently well divided into stages to be the subject of introspection, there is much that is new to discover.

Let me mention another case of an extremely simple process of deduction of a type of which elementary mathematics is full, the process being one that involves a genuine ideal experiment, and a genuine deduction. Almost anybody knows that if the sum of the digits of a number is divisible by nine, the number is divisible by nine, and conversely. Now let the psychological student be asked, if he does not already know the solution of the problem, *Why,* granting the ordinary principles of number, the numbers expressed in any decadic system must have this property. Let the ideal construction by which the problem is solved in a given case be a topic of introspection. The result could easily throw a light upon the psychology of reasoning which no discussion of the misused syllogism could possibly produce.

But the syllogism itself does indeed involve deductive processes that have a genuine fecundity. Mrs. Ladd-Franklin's theory of the syllogism, briefly restated by her in Baldwin's "Dictionary" and elsewhere, involves a deductive use of a construction which almost any psychologist can grasp with comparatively little trouble. The nature of the proof of the identity of the ordinary syllogism with Mrs. Ladd-Franklin's reconstruction, can be grasped by a process probably too complex to admit of any strictly experimental control. Yet if one once becomes familiar with this process and with repeated operations of it under controlled conditions, he would have material for the psychology of deduction.

There is another very fascinating problem in the psychology of deduction which has been almost wholly neglected. In my address before the Psychological Association years ago I called attention to the psychology of order as a problem still awaiting discussion. So far as I know, the problem has been little considered by psychologists since that time. But here is an aspect which presents curious phenomena. The

relations "such as," "greater than," "to the right of," "to the left of," are transitive. That is, they follow James's axiom of skipped intermediaries. Now all those serial relations that can be expressed in these transitive dyadic relations can also be expressed in terms of the formally triadic relation "between." Thus, let *A, B, C, D* be four objects in a row. I can say, "*B* is to the right of *A, C* is to the right of *B*." I can conclude that *C* is to the right of *A*. And then I can define the relations of order in question. Now it is very easy to see that if *B* is to the right of *A* and *C* is to the right of *B, C* must be to the right of *A* so long as one interprets the relations of right and left as we ordinarily do. But suppose I give you the premises, "*B* is between *A* and *C, C* is between *A* and *D*," and ask you what follows. The conclusion is decidedly hard for most minds to work out. In other words, the triadic relations have a psychological difficulty which we do not feel in the case of the transitive dyadic relations, although we can express equivalent facts in both terms. The difference in question is hardly due to the fact that a set of three objects is more complicated to grasp than a set of two. For a little exercise in attempting to reason in terms of "between," as the geometers often do, will show that the psychological difficulty is out of all proportion to the numerical difference between two and three. The grounds for the difference in difficulty are presumably statable only in psycho-physical terms. But the matter is one for psychological research, and should be undertaken.

Over against these problems of the psychology of deduction which are possibly capable of a more or less direct experimental research, there are vast numbers of problems of deduction which can be attacked more indirectly, some of them by following the records of formation of new habits, some of them by means of more or less exact study of social processes. There exists, for instance, an indefinite range of possibilities for the study of the psychology of the arithmetical processes by a device which, so far as I know, has still been very little used, although I have repeatedly recommended it to students of educational psychology. We hear a good deal of effort to make out the details of the process whereby a child gets control of arithmetical computations. Now it is perfectly easy for any one to put himself near to the beginning of practical arithmetic and into a place where he has to learn very many of his habits as a computer over again, under conditions that will admit of a pretty careful experimental scrutiny of the way in which the new habits get formed, and which will enable us to make precise records of the growth of the new habits. The device in question consists simply in using, instead of our decadic notation and numeration, a dyadic, triadic, or other such system. Dyadic arithmetic is the simplest of all. In this one uses two digits instead of the digits from 0 to 9, inclusive. That is, one uses only 0 and unity; 1 standing alone will mean unity. If one

uses two digits, the digit on the right will stand for units, the digit on the left for twos. The symbol 10 would now stand for two, the symbol 11 would stand for three, and so on. With the triadic system the places would be used exactly as in our decadic system. In the first place would be the units, in the next place to the left the threes, the third place to the left the nines, the fourth place the twenty-sevens, and so on. Now all numbers could be written in any of these systems. In starting with a new system, one could begin to perform additions, subtractions, multiplications and divisions, as with the decadic system. The possibility of an endless range of experiments, with mature persons, who, while retaining all their present arithmetical knowledge, would be instantly reduced to the position of young children, so far as some of the computations were concerned, all this makes an inquiry into the psychology of simple deductions of this type a very attractive one. Whoever wants to study psychology by becoming a little child has here a place for a wide range of study.