ANALOGY IN SCIENCE[1]

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PRESIDENT NEWCOMB, members of the American Psychological Association, ladies and gentlemen:

I listened to what President Newcomb just said to explain why I had been asked to speak here; but it did not clear it up for me entirely. I have thought about this question ever since, in a response to the honor and pleasure of being asked to come, I accepted. I wondered then what a professional physicist should be doing on this platform, a somewhat old and not so lively professional physicist, but still that.

I thought first that your inviting me might have a relation to the Institute for Advanced Study. We have had there a few members who are psychologists; we have an advisory committee consisting of members of this organization, whom we love and trust; and we hope to continue this. I have learned a great deal from them; we have often become friends. But we do not have a program in psychology; we do not have a department of psychology. We have, in fact, only two schools. One is called the School of Mathematics and the other called the School of Historical Studies; and it may help to reveal the limitations of my own scope if I describe very briefly what these are. You may recognize blind spots in me which will be comforting later, as I get on with my talk.

The School of Mathematics and the School of Historical Studies have both, of course, the problem of filtering the immense, fascinating, inchoate, unmanageable complexity of our experience. But they filter in quite different ways. The School of Mathematics is concerned with relations, with forms, with logical structure, and the application of these patterns and their discovery to the empirical sciences. And so it happens that psychologists are members of the School of Mathematics.

The School of Historical Studies uses a different kind of filtration. When I was in England not long ago I talked to Namier, who has undertaken the compilation of the parliamentary biographies of all Members of Parliament, from the origin to now. In the first parliaments almost nothing is available in the record about most of the members, so it is hard to write about this. And at present it is hard to write the biographies because there are such volumes available about everybody; only in the 16th and 17th and 18th centuries is the amount of material fit for human compass. The filtration of history is, of course, a very special one. It not only reduces the volume of available evidence and experience; it does so through the eyes of once living people who, by their actions, their evaluation, their tradition, have selected the things which are to remain meaningful over the years.

It is very often in history that just the unique point, the point that has no satisfactory, exhaustive, formal relation to more general patterns, is what is interesting. In the School of Mathematics it can only be things general enough so that structure can be recognized. I need to add that one mathematician, who has made such great contributions to logic, Gödel, has said of mathematics that it is purely an historical accident that it developed along quantitative lines. This, which is one of the themes which I take as text for today, may moderate somewhat the austerity of the two schools of learning. Yet taking it all-in-all I can only describe the relations of the Institute to psychology by a story.

About twenty years ago for the first time I visited the great laboratory in New York where Professor Rabi and his colleagues were beginning to do the most exciting experiments on molecular beams; and I had a fine time. But, as I left, I noticed that over the door it said in somewhat dusty letters, "Cosmic Ray Laboratory," and I asked Rabi, "What the heck?" "Well," he said, "you see, we don't keep them out."

I have thus given up talking about the Institute; and my second thought is rather simpler. It is to say a few things about physics which are, I think, interesting and which, I hope, may be helpful if not taken too literally and too seriously, also in the various fields of psychology. I know that it is a terrible bear trap to talk of the philosophy of

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science; only in a very, very limited sense am I going to do that.

One would think that the two sciences could hardly be further apart. In all hierarchical schemes they are put far apart. Psychology, to everyone who works in the field, is felt to be a new subject in which real progress and real objectivity are recent. Physics is, perhaps, as old as the sciences come; physics is reputed to have a large, coherent, connected corpus of certitudes. This does not exist in psychology, and only the beginnings of it, the beginnings of things that are later going to be tied together, are now before us.

But I have always had a feeling that there were ways in which the two sciences had a community; in some sense, of course, all sciences do. One very simple one is that each is responsive to a primitive, permanent, pervasive, human curiosity: What material bodies are and how they behave, on the one hand, and how people and the people-like animals behave and feel and think and learn. These are the curiosities of common life and they will never be abated. Both, for this reason, can hardly make important pronouncements of a technical sort which do not appear to have some bearing on our views of reality, on metaphysics. Both manifestly have, and continue to have, a fresh and inspiriting effect on the theory of knowledge, on epistomology.

There are other ways in which we are brothers. In the last ten years the physicists have been extraordinarily noisy about the immense powers which, largely through their efforts, but through other efforts as well, have come into the possession of man, powers notably and strikingly for very large-scale and dreadful destruction. We have spoken of our responsibilities and of our obligations to society in terms that sound to me very provincial, because the psychologist can hardly do anything without realizing that for him the acquisition of knowledge opens up the most terrifying prospects of controlling what people do and how they think and how they behave and how they feel. This is true for all of you who are engaged in practice, and as the corpus of psychology gains in certitude and subtlety and skill, I can see that the physicist's pleas that what he discovers be used with humanity and be used wisely will seem rather trivial compared to those pleas which you will have to make and for which you will have to be responsible.

The point, of course, is that as the relevance of what we find to human welfare and human destiny becomes sharper and more manifest, our responsibilities for explication, for explanation, for communication, for teaching grow. These are rather our responsibilities for being sure that we are understood than responsibilities for making decisions; they are our responsibilities for laying the basis in understanding for those decisions.

There are other ways in which we are alike. The practical usefulness of our professions gives us often the impression that we are right for the wrong reasons, and that our true nature is very different from our public presence. We are both faced with the problem of the need to keep intact the purity of academic and abstract research and, at the same time, to nourish and be nourished by practice. In physics, of course, our debt to technology and engineering is unlimited. I think it would be so in psychology as well.

Both sciences, all sciences, arise as refinements, corrections, and adaptations of common sense. There are no unique, simple, scientific methods that one can prescribe; but there are certainly traits that any science must have before it pretends to be one. One is the quest for objectivity. I mean that not in a metaphysical sense; but in a very practical sense, as the quest to be sure that we understand one another, and that all qualified practitioners mean essentially the same thing. Common-sense language is inherently ambiguous; when the poet uses it, or the rhetorician, he exploits the ambiguity, and even when we talk in ordinary life we almost need ambiguity in order to get by. But in science we try to get rid of that, we try to talk in such simple terms, and match our talk with deeds in such a way that we may differ as to facts, but we can resolve the differences. This is, of course, the first step in the quest for certitude. But certitude is not the whole story. When we move from common sense into scientific things, we also move toward generality using analysis, using observation and, in the end, using experiment. And we also do something which is even more characteristic; we look for novelty, we look for transcendence, we look for features of experience that are not available in ordinary life. Characteristic in physics are the instruments that enable us to transcend elementary, daily experience: the telescope that lets us look deep into the sky, the enormous accelerators which are, today, the logical extension of the microscope, enabling us to look on a finer and finer scale into the structure of matter.

I need to be cautious in citing parallels in psychology; but certainly the use of hypnosis, the use of drugs, are typical extensions into unfamiliar realms of human experience which just bring out characteristics of psychological phenomena that are largely lost in day-to-day experience. There is an example which may be only a physicist's idea of a perfect experiment. It is the work that was done at McGill in the last years on the effects of reducing sensory stimuli, with very simple arrangements to change the level of stimulation; these produce most striking and almost frighteningly great, though essentially temporary, changes in memory, in the intellectual and cognitive life of the subjects. This is again an example of carrying to an extreme something which is indeed encountered in ordinary experience but which only the patience and the abstractness of experimental enquiry is likely to make manifest.

We come from common sense; we work for a long time; then we give back to common sense refined, original, and strange notions, and enrich what men know and how they live. And here, I suppose, the real hero is the teacher.

I chose as my theme, "Analogy in Science." What I am going to talk about is analogy as an instrument in science and, to a much lesser extent, some slight traits of analogies between the sciences; mostly the second theme has led to misunderstanding and limitation; as for the first theme, analogy is indeed an indispensable and inevitable tool for scientific progress. Perhaps I had better say what I mean by that. I do not mean metaphor; I do not mean allegory; I do not even mean similarity; but I mean a special kind of similarity which is the similarity of structure, the similarity of form, a similarity of constellation between two sets of structures, two sets of particulars, that are manifestly very different but have structural parallels. It has to do with relation and interconnection. I would like to quote you a scholastic comment on analogy. It is a translation of Penido, "In a very general sense every analogy presupposes two ontological conditions; one, a plurality of real beings and thus among them an essential diversity. Monism is the born enemy of analogy. And, two, at the very heart of this multiplicity, of this inequality, a certain unity."

It is a matter about which we could argue whether these structural elements are invented by us, or whether they are discovered in the world. I find it very artificial to say that they are invented, in the sense that they are more of an artifact than the particulars which they unite and describe. I may tell one incident in the long history of astronomy and physics, which makes this very vivid for me. For practical purposes, for prophecy and ritual, the Babylonians worked out a method of predicting what days the moon would first be visible, of predicting lunar eclipses and certain rarer astronomical events. They did this by purely mathematical methods. They observed when things happened, and they got the pattern of it. They were very good. They got so good that their methods were in use in the last century in India to predict eclipses within some thirty minutes, using these two thousand year old methods. The Babylonians not only became very good, but they enjoyed it very much and they did it for fun; long after the practical reasons had gone away they published these tables, apparently as we publish articles on the internal constitution of the stars, because it is interesting. They did all of this without any celestial mechanics, without any geometry; nothing moved; there were no objects circulating around in orbits; there were no laws of motion; there was no dynamics; this was just in the field of the numbers.

You know how today we predict eclipses and first risings. It would seem to me very wrong to pretend that the mathematical regularities which were the basis of the Babylonian predictions were something they invented; it would seem to me equally wrong not to recognize in celestial mechanics as we now know it, a far deeper and more comprehensive description of regularities in the physical world. I think that not only because it is a little more useful, I think that not only because it unites more subjects, but because it reveals an aspect of the regularities of the world which was wholly unseen by the Babylonians.

Perhaps I need now to quote from Charles Peirce, and get on: "However, as metaphysics is a subject much more curious than useful, the knowledge of which, like that of a sunken reef, serves chiefly to enable us to keep clear of it, I will not trouble the reader with any more Ontology at this moment."

Whether or not we talk of discovery or of invention, analogy is inevitable in human thought, because we come to new things in science with what equipment we have, which is how we have learned to think, and above all how we have learned to think about the relatedness of things. We cannot, coming into something new, deal with it except on the basis of the familiar and the old-fashioned. The conservatism of scientific enquiry is not an arbitrary thing; it is the freight with which we operate; it is the only equipment we have. We cannot learn to be surprised or astonished at something unless we have a view of how it ought to be; and that view is almost certainly an analogy. We cannot learn that we have made a mistake unless we can make a mistake; and our mistake is almost always in the form of an analogy to some other piece of experience.

This is not to say that analogy is the criterion of truth. One can never establish that a theory is right by saying that it is like some other theory that is right. The criterion of truth must come from analysis, it must come from experience, and from that very special kind of objectivity which characterizes science, namely that we are quite sure we understand one another and that we can check up on one another. But truth is not the whole thing; certitude is not the whole of science. Science is an immensely creative and enriching experience; and it is full of novelty and exploration; and it is in order to get to these that analogy is an indispensable instrument. Even analysis, even the ability to plan experiments, even the ability to sort things out and pick them apart presupposes a good deal of structure, and that structure is characteristically an analogical one.

Let me read you now a few relevant and eloquent words of William James. He wrote them in one of his later accounts of pragmatism, at a time when his own good sense and shrewd observation and wisdom and humanity made him aware of the fact that to say only that an idea was true because it worked was a rather poor description of what went on in science, that something was missing from that account. This is what he wrote:

> The point I now urge you to observe particularly is the part played by the older truths. Failure to take account of it is the source of much of the unjust criticism levelled against pragmatism. Their influence is absolutely controlling. Loyalty to them is the first principle—in most cases it is the only principle; for by far the most usual way of handling phenomena so novel that they would make for a serious rearrangement of our preconception is to ignore them altogether, or to abuse those who bear witness for them.

What I want to do next is to give you five examples of the use of analogy in atomic physics. They will not all be equally familiar; perhaps that is an understatement, for some are very new, even to such a point new that I do not know how good the analogies are and we have not yet found the decisive point at which they are mistaken.

The analogies in physics may very well be misleading for biologists and psychologists, because of the enormous part that rather rigid formal structure plays in physics. This structure is not perhaps necessarily quantitative, though in fact much of it is quantitative. Our ability to write down synoptic relations in symbolic form, our use of formulae, enables us to talk of vast amounts of experience, very varied experience, very detailed experience, in a shorthand way; and to point sharply to mistakes, to correct error on occasion by altering only one letter, that changes everything. These examples are thus not meant as paradigms, but rather as an illustration of the fact that, in what is regarded as one of the most rigorous and certain of the sciences, we use an instrument which has been in great disrepute, because uncritically used it can confuse invention with confirmation and truth.

Let me give a first example which is not from atomic physics, which is almost from pre-physics, because it deals with very familiar things and yet illustrates the nature of the role of form in the use of analogy in physics. This has to do with Jean Buridan and the Paris school of the 14th century and the theory of impetus. What was their classic view? Physics has a special meaning for the word "classic"; classic means wrong, it means a wrong view that was held to be right a little while ago. The classic view was that the natural state of matter was rest, and that where you found bodies in motion you needed to look for a cause. This was the Schoolman's view; it was Aristotle's view. It is, in fact, supported by a lot of observation. It is not well supported by observation on projectiles; the notion that air pushes the bullet becomes less plausible the more you watch. Buridan and his colleagues took a step, making a new analogy, probably the greatest step in the history of Western science. They said, it is true that matter has a natural state, but it is not rest. It is true that when it departs from this natural state this must be ascribed to the intervention of a cause. But the natural state is one of constant impetus, one of constant momentum, one of uniform velocity. And with that the beginnings of rational mechanics and rational physical science were made. This seems a small change, to replace the coordinate by the velocity; it is a small change; and yet it is a change in the whole way of thinking about the physical world.

Let me list the five illustrations from atomic physics: they are what has happened to the idea of waves; what has happened to the ideas of classical physics in the atomic domain, the so-called correspondence principle; the analogy between radioactive decay and emission of light which we owe to Fermi; the analogy between electromagnetic forces and nuclear forces, between electrodynamics and mesodynamics; and a final subject which I will only call strangeness because that is about all I know about it.

Take the wave theory. It originated in the observation of regular, rhythmic changes in matter, waves on water, and was developed by an easily conducted physical exploration of sound waves, where there is a periodic change, a regular change in the density of air or other media. Both of these phenomena exhibit a characteristic. If two waves collide they can cancel each other out, or they can reinforce each other. They show interference. They have another abstract property: If waves pass through an orifice or around an obstacle that is small compared with the wave length, then the obstacle or the orifice does not cast a sharp image or shadow, but there are characteristic blurring effects which are called diffraction. Waves superpose; the sum of the two waves is just what you get by adding algebraically and not arithmetically; you may get zero if you add equal positive and negative waves; this again is interference.

This abstract set of properties is persistent; light is also a wave motion, but there is no matter in motion; there is no substrate. It was a great mark of progress for physics to recognize this disanalogy. There is still motion; and what moves are physically measurable things, rather more abstract things, electric fields and magnetic fields. Again we find *interference, diffraction, and superposition,* the same abstract characteristics, and again in principle, the infinitely regular, infinitely repeated pattern as a special case of a wave.

More extremely abstract examples are the waves of atomic mechanics, of wave mechanics, because these waves in the first place are in multidimensional space, then they are represented by complex numbers so that they are not directly measurable; they are indeed quite unobservable. There is nothing to measure in the physical world that corresponds to these waves. They are indirectly connected with observation; but they have again these same abstract properties—interference, linearity, superposition, diffraction; and when one talks about them, one uses much the same mathematics as for sound and light waves, although it is not the fact that one can use the mathematics but the fact that the structure and the relations are the same that is the decisive discovery. These waves represent, if one wants to say what they are, not matter, not forces, not electric fields, but *essentially the state of* information about an atomic system.

At each point the first scientists have tried to make a theory like the earlier theories, light, like sound, as a material wave; matter waves like light waves, like a real, physical wave; and in each case it has been found one had to widen the framework a little, and find the disanalogy which enabled one to preserve what was right about the analogy.

The second example of analogy is a massive one; it is, I think, the greatest experience in this century for the physicist, even greater than relativity; it is the discovery of atomic mechanics. Here again, in a way very characteristic of scientific theory, *great conservatism presided over and guided the development.* What is all this about? When one gets to the atomic domain, and this is a domain of small actions, of limited distances and limited impulses, of things such as one encounters in atoms and nuclei, then the coarseness of the whole physical world, its granular atomic structure, for the first time begins to manifest itself. This is not yet the granulation of the fundamental particles, but the granulation of atomic physics itself, of the quantum of action. What this turns out to mean is that when one tries to study such a system there are aspects of it which are accessible to experiment but are not compatibly or simultaneously accessible to experiment. A famous example is in the uncertainty relations, that one can determine the location of something in time and space, but if one does that, *he uses an experimental setup which makes it impossible* to know exactly what the impulse or velocity or energy of the system is. One may do the opposite; one can study the impulse and then lose all account of where the object is. And one can, of course, compromise with limited knowledge of both; but one cannot combine; and we call these the complementary aspects of an atomic system, and the complementary character of the fundamental observations. That means that we cannot talk about an atom as we can about a classical mechanical system. We cannot say the objects in it are here and they are moving in certain orbits and so on; in fact, in ordinary atoms there are no orbits. In atoms as they are ordinarily encountered there is something entirely different; there are stationary states which have a stability, a uniqueness, a reproduceability, which has no counterpart in classical physics at all, which could not exist if it were not for a revolutionary new feature.

One can talk about these stationary states in a consistent way; one can describe them accurately, and predict them; but one has a vast change from the familiar experience of bodies in motion, of matter in motion. Sometimes people say that this atomic theory is characterized by the fact that we cannot observe a system without disturbing it. But that is not quite right. It is not the disturbance which makes the trouble; it is the fact that the means of observation would be frustrated as means of observation if we tried to take account of the disturbance which we are making. This is thus a slightly more subtle matter. Sometimes people say that the electron has a position and momentum but we cannot measure them simultaneously. But this is not right either, because only the act of observation, the coupling of the atom with the physical measuring equipment, makes it logically permissible to attribute a position to an electron. We cannot get the right answer by saying that the electron has a position, and since we do not know what it is, let us average. If we do that we get a wrong answer. We have to admit that unless the situation is one which is created by our physical operation on the atomic system to realize, to manifest, to objectify the localization of the electron, then it will not be localized; it will in fact have no properties at all apart from what we do to it.

All of this is extraordinarily radical and extraordinarily unlike Newtonian mechanics. But what does the physicist say? Even before the full answer was found it was said there was something going on here which limits classical ideas; they do not quite apply; but in any situation in which they do apply we know that they are right; and, therefore, whatever laws hold in the atomic domain, they must merge into the laws of classical mechanics. There must be a one-to-one correspondence, an analogy; otherwise, in capturing some insight into this new domain we will throw out all we ever knew, and throw out things that are true. This affirmation is called the correspondence principle. Let me give an example of how extremely compact is this correction of the analogy, which has revolutionized everything, of how one deals with analogy in a highly formalized science.

Each law of classical mechanics may be written so that it is true in atomic mechanics: that the velocity is proportional to the momentum; that the change in time of the momentum is proportional to the force; that the energy is conserved. All of these things hold provided we make one formal change, provided we say that the momentum and the coordinate are not numbers, but are objects such that when we multiply the momentum by the coordinate and when we multiply the coordinate by the momentum we do not get the same answer, and that the difference between these two answers is an imaginary, universal, atomic constant. If we just write that one formula, then everything we had before is formally identical with what we have now. This is not only a powerful illustration of the use of analogy and disanalogy in a formal science; it played a decisive part in the exploration and discovery of the atomic world. We shall have to come back to other aspects of this great development. Let me run rather more briefly over the three other examples.

Radioactive nuclei, almost all of those that are made artificially, and many natural ones, disintegrate by sending out electrons. We puzzled and puzzled over this, since it was quite clear that there were no electrons in nuclei. Then Fermi made the suggestion that one might describe this as one describes the emission of light or light quanta from atoms. Nobody would say there was a light quantum in an atom; but still we observe light coming out; and he made a theory along these lines. It was not exactly right; the analogy was not quite perfect; but with a very little adjustment which took some fifteen years of comparison with the details of experiment, we have a description and a theory that work fine.

The Japanese physicist Yukawa proposed a somewhat braver analogy, whose fortunes are still not entirely clear. He proposed a similarity between electrical and nuclear forces. The way in which one describes the forces between electrically charged bodies is, of course, that one charged body makes electric fields; and these electric fields are propagated to other bodies, and give them some momentum to push them around. Nuclear forces, which are not electromagnetic, but are very strong and spectacular, Yukawa said, would probably be due to a field of a new kind; replacing the electric field there would be this new field; and replacing the light quanta, there would be new kinds of particles. Using general arguments of relativity and complementarity, or quantum theory, he concluded that because the forces between nucleons are of short range these new particles would have a mass some hundreds of times that of the electron; and from other particularities of nuclear forces, he drew conclusions about the nature of these particles. These particles were found in cosmic rays; they are called mesons. The analogy which Yukawa started with has been refined; one has discovered that there are many differences between mesodynamics and electrodynamics. One is at the present time not quite sure what all of the key points of difference, of disanalogy, are. Some of them have been discovered, but they appear to be rather the more trivial ones; yet the theory, as it stands now, has some predictive value; it has brought order and clarity to a part, at least, of nuclear physics; it has kept people at work, busy for twenty years of rather odd and arduous and rarified boondoggling. It has been a very major event in physics and I do not know at the moment how to describe what limits this analogy, why it is not a perfect one. If we were having a seminar on physics I would talk about it for an hour, but I still would not know.

The troubles, though, are probably connected with my fifth example. It is true that these mesons of Yukawa's were discovered; but not very long after that, in the last five years, one has found a whole lot of other objects—about six manifestly different objects and maybe more to come, which are also quite stable and last quite a while, and which are not simple mesons of the kind Yukawa envisaged. Almost certainly their intervention in the picture, which is not something that is provided for in the analogy we started from, will provide a clue to the new point. But, their existence raises a different problem. Whenever in physics one encounters a situation in which something does not happen, or happens very slowly, one finds it interesting; and the great point was why do these new particles not decay quickly. They do decay; but it takes them an inordinately long time, and they come apart into products which one would expect to emerge right away.

We have a great deal of experience with reactions that occur slowly or not at all; and the characteristic reason for that is that something does not tend to change, like the energy of a system, or the total charge: something is conserved. Whenever that turns up, it also turns out that the fact that something is invariant and unchanging is mathematically identical with the statement that something makes no difference to the behavior of the system. Examples of what may make no difference to the behavior of a system may be its position in space, or its orientation in space, or some more abstract circumstance. Thus the first thing that we all did was to try to find the characteristic of these new particles that did not tend to change. That has not been hard to do; and a quite successful theory has been developed which accounts for some of the great peculiarities in this field. We do not have a good name for what does not tend to change, and the inventor of it calls it the "strangeness."

These five examples are not meant to exhaust, but merely to illustrate, the powerful use, the inevitable use, of analogy in a well-developed, in a highly-organized, highly-formalized, highly-coherent science. I need to point out that in every case an immense amount of experience, of measurement, of observation, and of analysis has gone both to the correction of the analogies and to their confirmation.

When I turn to the question of analogies between sciences I talk of something very different. There is first of all the fact that there are often situations that are not analogies at all. There are congruences when, in two different sciences, by different techniques, different language, different concepts, it turns out that the same subject has been explored from two sides. And when it turns out that there is a mapping of one description on the other, usually one description contains more elements than the other, is richer; the other may then be more economical and more convenient. Examples: The chemical theory of valence and atomic physics, which are identical except that atomic physics does give an account of some phenomena, such as resonance, which were hard to cope with within the framework of the classical chemical theory. Another example, newer and perhaps not yet as well explored or understood, lies in classical genetics on the one hand, and the discovery of the genetic substances DNA, RNA, and so on, which are, at the moment, very close to being in a one-to-one correspondence, but in which the biochemical description will turn out richer, more relevant to dynamics, and more subtle.

These are great events of science; when they happen there is rejoicing, and when they do not happen there is hope. These are the great events which bring coherence and order and large structure to the unfolding of scientific life. But probably between sciences of very different character, the direct formal analogies in their structure are not too likely to be helpful. Certainly what the pseudo-Newtonians did with sociology was a laughable affair; and similar things have been done with mechanical notions of how psychological phenomena are to be explained. I know that when physicists enter biology their first ideas of how things work are indescribably naive and mechanical; they are how things would work if the physicists were making them work, but not how they work in life. I know that when I hear the word "field" used in physics and in psychology I have a nervousness that I cannot entirely account for. I think that, especially when we compare subjects in which ideas of coding, of the transfer of information, or ideas of purpose, are inherent and natural, with subjects in which these are not inherent and natural, that formal analogies have to be taken with very great caution.

But for all of that I would like to say something about what physics has to give back to common sense that it seemed to have lost from it, not because I am clear that these ideas are important tools in psychological research, but because it seems to me that the worst of all possible misunderstandings would be that psychology be influenced to model itself after a physics which is not there any more, which has been quite outdated.

We inherited, say at the beginning of this century, a notion of the physical world as a causal one, in which every event could be accounted for if we were ingenious, a world characterized by number, where everything interesting could be measured and quantified, a determinist world, a world in which there was no use or room for individuality, in which the object of study was simply there and how you studied it did not affect the object, it did not affect the kind of description you gave of it, a world in which objectifiability went far beyond merely our own agreement on what we meant by words and what we are talking about, in which objectification was meaningful irrespective of any attempt to study the system under consideration. It was just the given real object; there it was, and there was nothing for you to worry about of an epistemological character. This extremely rigid picture left out a great deal of common sense. I do not know whether these missing elements will prove helpful; but at least their return may widen the resources that one can bring to any science.

What are these ideas? In our natural, unschooled talk, and above all in unschooled talk about psychological problems, we have five or six things which we have got back into physics with complete rigor, with complete objectivity, in the sense that we understand one another, with a complete lack of ambiguity and with a perfectly phenomenal technical success. One of them is just this notion that the physical world is not completely determinate. There are predictions you can make about it but they are statistical; and any event has in it the nature of the surprise, of the miracle, of something that you could not figure out. Physics is predictive, but within limits; its world is ordered, but not completely causal.

Another of these ideas is the discovery of the limits on how much we can objectify without reference to what we are really talking about in an operational, practical sense. We can say the electron has a certain charge and we do not have to argue as to whether we are looking at it to say that; it always does. We cannot say it has a place or a motion. If we say that we imply something about what we ourselves—I do not mean as people but as physicists—are doing about it.

A third point is very closely related to this; it is the inseparability of what we are studying and the means that are used to study it, the organic connection of the object with the observer. Again, the observer is not in this case a human; but in psychology the observer sometimes is a human.

And then, as logical consequences of this, there is the idea of totality, or wholeness. Newtonian physics, classical science, was differential; anything that went on could be broken up into finer and finer elements and analyzed so. If one looks at an atomic phenomenon between the beginning and the end, the end will not be there; it will be a different phenomenon. Every pair of observations taking the form "we know this, we then predict that" is a global thing; it cannot be broken down.

Finally, every atomic event is individual. It is not, in its essentials, reproducible.

This is quite a pack of ideas that we always use: individuality, wholeness, the subtle relations of what is seen with how it is seen, the indeterminacy and the acausality of experience. And I would only say that if physics could take all these away for three centuries and then give them back in ten years, we may well say that all ideas that occur in common sense are fair as starting points, not guaranteed to work but perfectly valid as the material of the analogies with which we start.

The whole business of science does not lie in getting into realms which are unfamiliar in normal experience. There is an enormous work of analyzing, of recognizing similarities and analogies, of getting the feel of the landscape, an enormous qualitative sense of family relations, of taxonomy. It is not always tactful to try to quantify; it is not always clear that by measuring one has found something very much worth measuring. It is true that for the Babylonians it was worth measuring—noting—the first appearances of the moon because it had a practical value. Their predictions, their prophecies, and their magic would not work without it; and I know that many psychologists have the same kind of reason for wanting to measure. It is a real property of the real world that you are measuring, but it is not necessarily the best way to advance true understanding of what is going on; and I would make this very strong plea for pluralism with regard to methods that, in the necessarily early stages of sorting out an immensely vast experience, may be fruitful and may be helpful. They may be helpful not so much for attaining objectivity, nor for a quest for certitude which will never be quite completely attained. But there is a place for the use of naturalistic methods, the use of descriptive methods. I have been immensely impressed by the work of one man who visited us last year at the Institute, Jean Piaget. When you look at his work, his statistics really consist of one or two cases. It is just a start; and yet I think he has added greatly to our understanding. It is not that I am sure he is right, but he has given us something worthy of which to enquire whether it is right; and I make this plea not to treat too harshly those who tell you a story, having observed carefully without having established that they are sure that the story is the whole story and the general story.

It is of course in that light that I look at the immense discipline of practice, that with all its pitfalls, with all the danger that it leads to premature and incorrect solutions, does give an incredible amount of experience. Physics would not be where it is, psychology would not be where it is if there were not a great many people willing to pay us for thinking and working on their problems.

If any of this is true there is another thing that physicists and psychologists have in common: we are going to have quite a complicated life. The plea for a plural approach to exploration, the plea for a minimal definition of objectivity that I have made, means that we are going to learn a terrible lot; there are going to be many different ways of talking about things; the range from almost un-understood practice to recondite and abstract thought is going to be enormous. It means there are going to have to be a lot of psychologists, as there are getting to be a lot of physicists. When we work alone trying to get something straight it is right that we be lonely; and I think in the really decisive thoughts that advance a science loneliness is an essential part. When we are trying to do something practical it is nice to have an excess of talent, to have more sailors than are needed to sail the ship and more cooks than are needed to cook the meal; the reason is that in this way a certain elegance, a certain proper weighing of alternatives, guides the execution of the practical task.

We are, for all kinds of reasons, worrying about how our scientific community is to be nourished and enough people who are good enough are to come and work with us. And then on the other side we are worried about how we are to continue to understand one another, and not get totally frustrated by the complexity and immensity of our enterprises.

I think there are good reasons of an inherent kind, beside the competitive compulsion of the communist world, why we would do well to have more and better scientists. I know that exhortation, money, patronage, will do something about this; but I do not think that is all that will be needed. I think that if we are to have some success it must be because, as a part of our culture, the understanding, the life of the mind, the life of science, in itself, as an end as well as a means, is appreciated, is enjoyed, and is cherished. I think that has to be a very much wider thing in the community as a whole, if we are to enjoy with the community as a whole the healthy relations without which the developing powers of scientific understanding, prediction, and control are really monstrous things.

It may not be so simple, to have in the community at large some genuine experience of the pleasures of understanding and discovery. It may not be simple because what this requires is not merely that this experience be agreeable, but that it have a touch of virtue; that not only the consideration of ends, of products, of accomplishments and status, but the texture of life itself, its momentary beauty and its nobility, be worth some attention; and that among the things that contribute to these be the life of the mind and the life of science. Let us try to make it so.