UNTITLED manuscript by Ernest Nagel

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Steven R. Bayne

(Talk by Dr. Ernest Nagel)

The title of this talk obviously leaves me a great deal of room for scope since it is so very indeterminate as to what the subject is that I am going to talk about. And I think partly in order to place the themes upon which I do want to talk in some detail, and partly in order to outline the range of the subject that one calls the Philosophy of Science, I thought it would be appropriate to say something about how I conceive the structure of science - what is it. And clearly to talk about the structure of science would first of all require one to indicate roughly what one means by science itself. Now, I think we know enough about the controversies that have been going on for a very great number of years as to whether something is or is not a science. These controversies, on the whole, have been exercises in name calling; they have been profitless. Certainly, you may take the position that it is only mathematical physics which is a science and every other sort of investigation is just stamp-collecting. But I had rather not engage in such recriminations, so I would like to think of science in a much more catholic fashion, and I would like to delimit it as being identifiable as an institutionalized human activity which conforms to certain institutional traditions and which is to be distinguished from other institutions in three respects; namely, in respect to its aim. And here I think the aim is, in most general terms, the acquisition of explanations or understanding through a systematic and comprehensive determination of fact, but the emphasis would be upon the attempt to achieve understanding or explanation. This is the general aim. Secondly, not only an aim but certain products, and the products are primarily intellectual, which consist of more or less systematic explanations,the degree of systematization will vary in different disciplines that are counted among the sciences; primarily intellectual, but secondarily consisting of effective control over various kinds of events. And thirdly, in addition to aim and product, there are certain procedures, procedures of inquiry, in which I think one can distinguish, certainly, certain special techniques, each having a fairly limited range of application to specialized inquiries into certain specialized subject matters. But not only special techniques but something which is perhaps less easy to identify, namely - certain logical methods whose scope coincides more or less with the entire domain of science, and these logical methods include, certainly, principles for assessing evidence for the various conclusions that are obtained by inquiring. And allegedly, and certainly more debatably, principles or rules for the actual conduct of research and for the making of scientific discoveries. It is in terms of aim, product and process that I would want to identify something that I was afraid to call a science. And clearly, this account allows for a great deal of variation.

Now, what would an account of 'the structur~~al~~ of science' be, if science is conceived in this way. Well, it seems to me that an account of 'the structur~~al~~ of science' will include the following components.

First, bearing in mind that science, as I conceive it, is an institutionalized activity, directed toward certain aims ~~Of course,~~ because it's an institution ~~First,~~ such an account must include a discussion of the influence of other social institutions and society at large upon the development of science, both in respect to the substantive content of science and also in respect to the canons of standards of scientific procedure. ~~You know,~~ I might say right off that this is an aspect of 'the structure of science' about which I will have nothing to say.

Secondly, an analysis of the ways in which what I have called ~~we uphold~~ the intellectual achievements of science are organized; that is, an analysis of the kinds of explanations that one is able to obtain, and an account of their internal patterns. And also, a discussion of the manner in which explanations become altered through new experimental discoveries or the introduction of new ideas. So, the second aspect that I would want to include in the account that is sufficiently adequate of the 'structure of science' would be a consideration of the organization of the intellectual products. And the intellectual products would include, certainly, a good deal of factual detail, but in terms of the primary aim, it would certainly include an account of the kind of explanation or the sort of understanding one obtains, or seeks to obtain, in various areas of inquiry.

Thirdly, an account of the ways ~~of~~ the explanatory premises are related to matters of observation or experiment and of the requirements which such premises are generally expected to meet if they are to function properly in inquiry. And this, of course, would include, among other things, the consideration of the various ways in which scientific ideas may be defined in terms of each other but more specifically, of the routines or routes which connect ideas which may on the face of it be abstract and rather remote from the materials of observation and experiment to the subject matter that is being investigated.

And fourth, and finally, an account of 'the structure of science' would have to include a reasonably complete codification of, and rationalization or ~~a~~justification for, the logical principles that are employed in evaluating or in warranting the conclusions that are adopted at various stages of scientific inquiry.

Now, as I mentioned, I shall have nothing to say about the impact of society upon science and the way in which scientific activity might be influenced by social considerations. And I will have very little to say, although I will have something to say in passing, about the relation~~s~~ of explanations to matters of observation. And what I will want to concentrate the time I have available upon are certain specific issues that arise in connection with a discussion of the procedure of science or the methods ~~message~~ of science and secondly, specific questions that I would like to discuss dealing with the way in which certain kinds of explanations in science might be related to others. But I will indicate more specifically what I want to talk about in this connection when I come to it.

Let me come, then, first of all to the issue that I wish to discuss in connection with the principles of scientific method. And I thought it would not be entirely inappropriate since, a week ago, Professor Hansen made, I'm sure, a very vigorous plea(perhaps more than that, an argument) for there being such a thing as ~~the~~ a logic of discovery. I thought it would not be inappropriate if I commented on the question whether there is such a thing as a logic of discovery. And I thought the best way of doing this would be to really comment upon the arguments that Professor Hansen uses. I didn't hear his lecture last week but I assume that the things he said in print about these matters would be at least in substantial agreement with what he said here.

Let me first of all say something to place the problem. What is the problem about there being a logic of discovery? Now, some of us are very well familiar with the fact that historically, a great number of thinkers, some of them scientists; others, commentators on science or philosophers, have sought to find certain rules, such that if a man were properly instructed in those rules, then he would be able to advance science by making substantial additions to the content of science. Some of the very well known names in this long tradition are certainly Francis Bacon, of the 17th century, John Stuart Mill of the 19th, and early 19th century, Hershall, who was a distinguished astronomer, who also believed it might be possible to formulate such rules.

In the image that Francis Bacon used, just as a person who is a highly gifted painter or draftsmen would be able to draw freehand a circle which lesser men would be unable to reproduce, but if they are given a pair of compasses, then you don't have to be a ~~jotto~~ Giotto in order to draw freehand something that approximates to the geometrical canon ~~of~~ or standard for a circle , So Bacon argued, just as in the past it had been great men who made important scientific discoveries, if you could find certain rules for making discoveries, and if these could be taught, then much lesser men could make important advances in various branches of science.

Now, as far as this particular quest is concerned, the record, I think, is without qualification uniformly a failure. No one has been able to devise any rules for making discoveries or for making inventions. Many of us have thought that this was a dead issue; that no one who was familiar with the history of thought would try his hand again at resuscitating what seems like a lost cause. However, there has been a revival of interest in this subject and Hansen has been a vigorous exponent of the claim that there is such a thing as ~~the~~ a logic of discovery; not a set of rules for making inventions but a logic in the sense if you examine what are admittedly tentative gropings of the great figures in the history of science, one can find that there is a method in their procedures. And while familiarity with their methods may not enable the rest of us to make scientific discoveries, nevertheless, it isn't just a hit or miss procedure; that the business of making important discoveries is not, as Bertrand Russell once said, just a matter of guesswork, but that there are certain more or less invariant canons or rules by means of which one could formulate why it is that man comes to think of one hypothesis or why he prefers to take seriously one hypothesis rather than another.

Now, the argument as I understand it, and certainly the one that Professor Hansen advances, is that there may be good as well as bad reasons for a scientific inquirer adopting one kind of hypothesis rather than another. And these reasons, so he claims, may but also do differ in type - ~~may be~~ namely, the reasons for his initially adopting, for the purpose of further exploration, one hypothesis or another; that the reasons which lead him to accept one type of hypothesis rather than another, that these reasons are different from the reasons that one advances in order to accept a hypothesis as being the conclusion of a determin~~ing~~ate inquiry. And I think the point could be made ~~more~~ clear in terms of a favorite illustration of Prof. Hansen. Keppler convinced himself that on the basis of the observed positions of the planet Mars, instead of assuming now that the planet has a circular orbit, that the planet moves on an elliptical orbit with the sun at one of the foci of the ellipse. Leaving aside the question of how he came to entertain this hypothesis before concluding that the detailed, factual observational evidence supported beyond possibility of reasonable doubt - - leaving aside that question - - assuming that Keppler had established on the basis of detailed inquiry that this was really a satisfactory hypothesis to account for the observed position of the planet Mars, Keppler turned his attention to other planets, in particular to the planet Jupiter. What kind of an orbit did Jupiter have?

Professor Hansen argues that if Keppler concluded that the likely hypothesis, although you couldn't be sure that this would be, was that Jupiter also had an elliptical orbit, it was because Keppler must have argued something like this: not only is Jupiter like Mars, a planet, but Jupiter, like Mars, is an exterior planet; that it exhibits certain variations in the apparent velocity as it moves through different parts of the heavens. In short, there were certain pronounced analogies between Mars and Jupiter and because of these analogies, he had good reason for supposing that the orbit of Jupiter was also of a noncircular kind; that is the type of hypothesis ~~that~~ which he should entertain was one which would lead him to say that the orbit was not circular and more specifically, that the orbit was an elliptical one.

Thus far, of course, Keppler simply arrived at a suggestion as to what specific hypothesis to explore. He reasoned by analogy to the hypothesis that the orbit of Jupiter was elliptical. This kind of reasoning would not be sufficient, so Professor Hansen argues, and I think probably correctly, this kind of reason is not sufficient to establish the claim that the orbit of Jupiter is elliptical. To establish this, one would have to make detailed observations on the various observed positions of Jupiter to see whether they do or do not fit in within certain limits of the requirement~~s~~ that these positions should fall upon an elliptical orbit. And so, in short, the argument is this: in extending the elliptical hypothesis from Mars to Jupiter, Keppler was reasoning by analogy. In establishing the Jupiter hypothesis that it was elliptical, he was not reasoning by analogy, he was reasoning from a set of examined instances, namely observations on the position of Jupiter - finding that these fit into the hypothesis and in concluding that since the observed positions fall upon the orbit, therefore, all further positions which had not yet been observed, would also fit up to that orbit. And so, he makes what seems like the prima facie reasonable claim ~~.~~ that the type of reasoning which Keppler used, and which investigators use, in making discoveries is different from the type of reasoning that they use in establishing the suggestion that they have arrived at at the preliminary stage of the inquiry. And I think he rests his case upon examples of this sort.

Now, let me just make some very brief critical comments on this. ~~I do not;~~ let me first of all say what my conclusion is and then advance reasons for it. I don't think that the argument that Professor Hanson advances to show that there is something that one might identify as a logic of discovery as distinct from a logic of proof, ~~shall~~ we say, or logic of validation - - I don't think the argument is one that carries very much weight and for the following reasons.

Let's grant that analogies play important roles in the ~~conclusion~~ construction of hypotheses. This, it seems to me, to be indubitable.

Various kinds of analogy, and if there were time it would be really very instructive to examine the variety of analogies that are employed in different domains of scientific inquiry, but my point would be that though there are analogies, which lead a man in one direction rather than another, it is the case that the type of hypothesis to which the analogy leads is controlled by the more or less tacit assumptions which are part of the accepted body of beliefs at the time. So, for example, when Keppler, according to Professor Hansen, arrived at the suggestion that the orbit of Mars was non-circular, he opted for a type of hypothesis which required him to say that the likely hypothesis would be one which would say that the orbit of Mars would be a closed plane curve, smooth in shape, and that Keppler would have rejected the assumption or the suggestion that a likely hypothesis to account for the different velocity - that apparent velocity which Mars exhibits to different points of its orbit is in some way controlled by variations in the apparent color of the planet; so ~~that~~ the color hypothesis is excluded; the type of hypothesis which was required to say that it is a non-circular orbit is regarded as being reasonable. But why is the color hypothesis excluded? Well, I think the answer is very simple - - because of the general beliefs that Keppler had which led him to say that color is not a relevant circumstance to account for the behavior of a planet. Keppler had all sorts of curious ideas about the influence of various sorts of forces and emanations from the sun. Color was not one of the things that played a relevant role in this, and so certain hypotheses are excluded because of the assumptions that a man has at the time.

Just to make the point ~~absolutely~~ perhaps a little more emphatic, suppose an investigator today is confronted with the data that Rhyne at Duke, or some other people working in para-psychology, produce to indicate that some people can correctly call the configuration on a card before they see it and you want to explain this. What hypothesis would seem likely? This will depend very largely upon the kind of a training a man will have had and what he will regard as being antecedently reasonable on the basis of his antecedent scientific experience. I venture to say, although this is an extrapolation from the responses of a few statisticians whom I know, that if a statistician were to be asked to offer a type of hypothesis which would be a reasonable one to adopt, he would say: Well, this unusual success in calling cards, this might be accounted for in terms of the general assumption that the shuffling of the cards, although it is maybe done in accordance with some kind of a randomizing procedure, that no actual randomizing machine is completely random; every randomizing machine, at least over a finite run, exhibits some kind of a bias. Individuals who say that they are making guesses - - whether individuals who have different patterns of guessing would be able to call the cards as successfully as other individuals who have a different pattern of guessing, in that the correlation that is to be made is between patterns of guessing and the pattern of bias which the particular randomizing machine, which shuffles the cards, introduces into the shuffling.

Now, as you know, Mr. Rhyne did not think that this was a reasonable hypothesis. He ~~uses~~ introduces the hypothesis of extrasensory preception. For him, this was the reasonable hypothesis. But what seems reasonable - - what seems credible - - what seems irrelevant is, in a certain sense, controlled or dictated by the set of more or less explicit assumptions a man has as to what factors are to count in giving an explanation.*

And so, instead of calling attention to a logic of discovery, I think that all Professor Hansen has successfully called attention to is the direction in which, given a certain intellectual framework, investigators will prefer to explore one type of hypothesis rather than another because of the tacit or sometimes explicit assumptions with which they operate as being experienced practitioners in a domain. And so I conclude, although to be sure much more needs to be said in order to make my argument conclusive, that the claim that there is a logic of discovery

* ~~Break between tapes~~.

is a claim that is without any substantial support. What there is, according to me, is that there are principles for assessing evidence for conclusions or for hypotheses that are advanced and for which evidence has been produced; that there are no rules for finding conclusions. There are no general rules for finding evidence but, given a certain set of evidential statements, given certain conclusions which are based upon that evidence, there are, so I would submit, certain general principles which control the evaluation or the weighing of the evidence to see whether the evidence supports, strongly supports, powerfully supports, supports beyond ~~without~~ reasonable doubt the alleged conclusion. The conclusion, of course, in this case is not something which follows necessarily from the evidential ~~evidence or~~ premises. Conclusions, to use a well-worn phrase, is only made probable on the basis of the evidence and so the issue is whether there are any rules for weighing the degree of probability. And here, though clearly this is a subject which would require several talks in order to even skate over the surface, I can only say two things which seem to me to be pertinent. First, that though in recent years there have been very ingenuous attempts to develop a calculus for weighing evidence for conclusions that do not logically follow in such a way that it would be possible to assign a numerical value to the degree of evidential ~~evidence~~ ~~to~~ support, thus far, at any rate, there is no reason to suppose that these attempts are more than a bit of scientific fiction; that they have no relevance at all to any thing that plays a role in actual scientific practice. I mean scientific practice in a sense of scientific inquiry even when scientific inquiry is concerned with fundamental theoretical questions rather than with practical ones. So, I think at present one has to say that, at best, we can introduce a kind of a scale, a rough qualitative of a scale of stronger evidence, less strong evidence without being able to assign a numerical degree of probability. And, of course, this would certainly mean that what is known as the mathematical calculus of probability, and which has obviously very important uses, is not applicable to this particular domain when what one is asking for is the degree or the weight of the evidence which supports a particular hypothesis in some area of inquiry.

The second point I would like to make is this; That many attempts that have been made, again in recent years, to codify principles of evidence or principles for weighing evidence have placed a ~~terrific~~ tremendous emphasis upon the sheer number of bits of supporting evidence and there has been a tendency to believe that the more - - the larger is your evidential basis, the stronger is the support that the evidence supports the supposed conclusion. Now, I think that this is not an adequate account. It is partially correct and that it is not an adequate account I think becomes clear if we remember that perhaps one of the elementary points in any sort of a scientific inquiry is to introduce controls in any observation or experiment. I recall a very distinguished physician telling me that until perhaps something like 20 or 30 years ago, patients in hospitals who suffered from typhoid fever were uniformly plunged into a cold bath. And it was believed that this was very salutary sanitary. It had a good effect. When this was done, it apparently had not occurred to those who were in charge of the hospitals to ask the question: Well, look, we are confident that this is so, but do we really have any control; that is, have we tried to do the following very simple things. Have two more or less matched groups of individuals who are suffering from typhoid -- some of them we are going to submit to this cold bath treatment -- others, we are not going to submit to that treatment. See if there is any differential effect that is significant and decide upon the basis of introducing a control~~led~~ group.

Now, what is the control in point of fact indicate? It means that what you are interested in is not simply the sheer number of items of evidence but that you also want to introduce some kind of variety in the evidence; that if you have the same experiment performed over and over again without essential variations in it, that this, in a certain sense, does not increase the degree of support that the evidence gives to a conclusion. Certainly it increases it much less than if you had a fewer number of experiments but they came from different areas; that is, for example, the quantum hypothesis which Planck introduced in order to account for the distribution of energy in the spectrum of black body radiation - ~~which~~ well one could continue making fine experiments about the distribution of energy and find that Planck's formula was in accordance with it. But people really didn't begin to feel confident in the hypothesis until, for example, Einstein was able to show that the photoelectric effect could also be accounted for in terms of the Planck assumption; ~~also,~~ or when Einstein showed that the specific heats of solids could be explained in terms of the quantum hypothesis or when later, Bohr could account for the Balmer Series ~~reason~~ in the spectrum of elements - - again in terms of the quantum; that is, what is important is the variety or, at any rate, it is an essential point in estimating the weight of of evidence and the sheer number without variety seems to me to count very little.

Beyond that, I think there is no time to go, particularly if I do want to say something about one other aspect of this thing that I am calling "the structure of science"; namely, in connection with the analysis of explanations. Now, I suppose from the point of view of a systematic presentation of the subject, one ought to - or at least I ought to, since as far as I know, I am the first one to talk about the subject in the series and there will be others who will be talking about it, I ought to start at the very beginning and indicate first of all, something about the types of explanations that one can identify, raise some of the issues that have ~~to be~~ been considered by students as to whether there is only one type, or whether there are a plurality of different types; that is, raise formal questions about the logical structure of explanations. Similarly, one could ask apart from formal differences, such as for example, some explanations seem to be of a deductive sort where what is being explained follows necessarily from the premises. As, for example, when Newton explained Keppler's law that ~~the~~ a planet moves on an elliptical orbit with the sun as the focus, he explained this by showing that this was a consequence of his assumptions about the laws of motion and universal gravitation. This was ~~the deducted~~ a deductive explanation. Then, there is the kind of ~~an~~ explanation that ~~I want to~~ one calls probabalistic, where the conclusion doesn't follow necessarily from the premises but the premises, in some ways, stand in a relation of probability to the conolusion. For example, if a historian explains the action of some one individual in the past, for example: if a historian seeks to explain Roosevelt's attempts to pack the Supreme Court, why did he do this? No historian, to my knowledge, is able to produce ~~an~~ as explanation – a set of propositions – from which Roosevelt's proposal follows necessarily. All a historian does is to offer statements such that if those statements are true, it makes

Roosevelt's action probable.

And so there are a variety of different kinds of explanations which differ in their formal structure. Then there are explanations, and again I want to hasten over things which I am sure will be covered in greater detail later on - - explanations which differ in the kind of explanation which is offered. What I mean by this is that sometimes we wish to explain a ~~single~~ singular occurrence such as, for example, we wish to explain why it is that there ~~is~~ was a rainfall on a particular day at a particular place. And so what we explain then is a ~~single~~ singular occurrence. Sometimes what we wish to explain is a general feature of something and we do this by subsuming it under something that we might call a regularity -- an empirical law.

Let me offer what you might think is an absurd illustration because it is so elementary and primitive. If I find that a certain object, which is placed into some electrical circuit,conducts electricity very well, as compared certainly with something else that I might introduce into the circuit, then I ask myself ~~why,~~ why does it? And suppose I say, well, of course, you see the reason is that this substance that I have put into the electrical circuit is made of silver and silver is a good conductor. So, I have explained the general characteristic by indicating that it can be subsumed under a simple law like silver is a good electrical conductor.

Now, sometimes, of course, we wish to account for things like, why is silver a good electrical conductor? Now, in this case, I think you will find that after a certain point, the explanations, when they are satisfactory, no longer consist of simple empirical laws like the one that I just mentioned, but that the explanation will be given in terms of what we generally call a theory. For example, the theory which is formulated in terms of the atomic structure of the elements, which will account for differences in the electrical conductivity. And so, my point is that one can distinguish one group between explanations which use ~~as they are used~~ as explanatory premises ~~and~~ empirical laws. If, for example, I want to explain why it is that ice floats; well, you know the answer could be given in terms of Archimedes' law. Archimedes' law is sort of a low level generality. It's empirical - - it could be tested, so to speak, in a laboratory. But now, you say, well -- now, but why should Archimedes' law hold. Well, eventually it led to the principles of mechanics, to Newton's laws of motion, to certain assumptions about forces operating in a liquid; and you no longer have a simple empirical law which explains Archimedes' law, you have something one calls a theory. And so there are differences in explanations of the kind that I have been indicating.

I want to turn, for the remainder of the time that is available to me, to a discussion of theories and to certain problems it seems to me arise in connection with theories. Problems that I think are often perplexing, perplexing, I will venture to say, not only to the layman in science but most frequently to the scientist himself.

First, let me say what I do mean by theory. I have given illustrations but I think I would like to be a little bit more specific as to what I do mean by theory, particularly when I contrast it with an empirical law. One of the things that I think is characteristic of those systems of premises that we call theories is that first of all, they can explain a large variety of different material. If, for example, you think of what ~~the~~ Newtonian mechanics can explain. ~~We~~ You can explain the behavior of a freely falling body; you can explain the behavior of a planet; you can explain the behavior of the tides; you can explain, with some specialized assumptions, ~~--~~ the rise of liquids in thin tubes; that is, these are qualitatively different phenomena and yet, all of them can be subsumed, can be explained, in terms of this theory that we call mechanics. Similarly, with electromagnetics ;

similarly, with the kinetic theory of gases.

There are a tremendously large number of qualitatively distinct phenomena which can be explained. Empirical laws don't do this. I mean, in Archimedes' law, for example, what will it explain? It will account in general for the behavior of solids in liquids, but that is the limit; that is the range of explanation. Newton's theory will account for Archimedes' law but will also account for Keppler's law, for Galileo's law and for a great number of other things. So first of all, one of the things that characterizes a theory is this large range of coverage that it has. Secondly, a theory frequently, if not always, tends to correct an empirical law. For example, Keppler said that all planets - more particularly Mars - moved on an elliptical orbit and now we say, well, yes; his observations certainly confirmed that. On the basis of Newton's theory we say yes, of course, Keppler's law follows from Newton's assumption, provided that there are just two bodies, but if you introduce a third body, then the behavior of Mars - the orbit of Mars - is no longer ~~then~~ that of an ellipse. It must depart from an ellipse and so we know then that on the basis of ~~theory and~~ theoretic considerations, that Keppler's conclusion cannot be quite correct; it might be a first approximation, so that one important function, a characteristic of theory is that they are frequently used in order to correct what are called empirical laws.

On the basis of ~~theory and~~ theoretic considerations, we correct ~~Bohm's~~ Boyle's law, which helds only for so-called ideal gases and show how by introducing more complicated assumptions, we can get Van Der Waals equation~~s~~ and things of that kind. This is all, I ~~think~~ take it, very familiar.

Thirdly, I think it is one of the important functions of ~~the theory~~ theories that they provide directives for experimental research. For example, the kinetic theory of gases, among other things, you see -- you say, well, according to the kinetic theory of gases, each molecule of the gas has a mass. What is the mass of the gas? According to the kinetic theory, a given volume of gas under standard conditions of temperature and pressure will have a certain number of molecules in it. What is this number? Well, the theory raises questions of this kind and then the experimental physicist tries to set up ~~something in~~ some experiments which will enable him to make these determinations.

So, first of all, theories are characterized by the fact that they have this variety of functions that experimental laws in general do not have. Secondly, it is one of the features of theories that they have certain analytical components. And I would like to mention three - - many others could be identified but for my purposes, these three will suffice. First of all, I think it is the case that most terms that occur in what we are prepared to call theories are rather remote from experience; that they cannot, so to speak, be defined in terms of observational terms. This is evident even, I think, in the case of some of the ideas that enter into Newtonian mechanics. Sometimes, Newton ian mechanics is regarded as not being theoretical in this sense which I am indicating because many people claim that the theoretical terms, that the basic terms, really are very close to ~~experiment~~ experience because we are going to talk about position, talk about velocity, talk about mass. But if you look at the way in which Newton proceeds - - of course, ~~the~~ Newtonian ~~mechanists - -~~ mechanics is particle mechanics - - supposes that the things he is dealing with have positions but no dimensions. Well, point masses - - point mass is not an experimental concept; point mass is a theoretical concept. Again, when Newton supposes now that point masses can have instantaneous velocities, instant aneous velocity is not an experimental concept, it is a theoretical concept. And so on down the line.

In the kinetic theory of gases, we don't define what is to be a molecule in what is sometimes called operational terms. We do this in terms of certain postulations upon the character of molecules without defining them in terms of some sort of an overt laboratory procedure. So one of the features of theory is that the terms that play fundamental roles in the theory in general are not defined overtly, the way, for example, the late Professor Bridgeman insisted that all terms should be given operational definitions. This does not seem to me to be the case for theoretical terms.

The second point to observe about theory is that because the terms are not defined operationally, it is an essential step in using a theory that one should introduce what people have called by various names, but I am going to call them rules of correspondence; that you have to have rules which will associate the theoretical term with some kind of a well recognized experimental situation.

Let me illustrate again what I mean. There are no rules of correspondence for the term <u>molecule</u>, for the term <u>velocity of the molecule</u>;

that is, we don't know how to associate the term velocity of the molecule with something that we can identify in a laboratory. What we can do is to define certain theoretical notions in terms of the basic theoretical notions, such as, for example, the mean kinetic energy of molecules and then introduce some kind of a rule of correspondence as well, (mean kinetic energy of molecules) that we are going to associate with temperature, as temperature is defined by some kind of a laboratory procedure. So you have some sort of a range between the theoretical term or a complex of theoretical terms, and something that we might recognize as an experimental notion, which is associated with a fairly definite laboratory or operational procedure. And so, it is required then for a theory, rules of correspondence. Professor Northrop, I think, in the first lecture, called them epistemic correlations; other people have called them coordinating definitions; other people call them semantical rules. I think I use the term rules of correspondence in the way in which Professor ~~Morgan~~ Margenau, about whom I want to say something else, uses it.

The third thing I would like to mention in connection with theories is that by and large, every theory that is used fruitfully will not only have rules of correspondence but will also have something that we will call a model. For example, the kinetic theory of gases, well, it is familiar to say that the kinetic theory of gases has been built on the image of billiard balls, but, of course, that image of billiard balls is not essential to the formulation of the kinetic theory because what does the mathematical work in the kinetic theory are simply the various equations from which further deductions can be made. Instead of formulating the equations explicitly, sometimes it is convenient to say, well, look; let's forget about the finer details; let's think of the theory in such a way that there are certain - in some cases - visualizeable models which satisfy the postulates of the field. Similarly, we have this in quantum mechanics, although there are difficulties in finding a suitable model but certainly some model, either a wave or a particle or some fusion of them, and then you try to get over difficulties by introducing such things as complementarity or what not. All these are devices by means of which you can find something that you will recognize as a model. Now, I think that models are important not simply because they are psychological aids, but because models suggest analogies and in this way, tend to unify different areas and also because models help us to indicate just how I am going to use the formalism of a theory in dealing with some experimental situation.

Now, assuming that this is so, assuming that theories are of this character, the feature of theories to which I would like to address myself for the remainder of my time is this. It is, I think, a well known phenomenom in the history of science that at various stages in the development of science, one theory ~~becomes~~ - is replaced by another, and the things that one theory is used to explain are then explained by another theory; that is, what sometimes happens is that one branch of a science becomes somehow absorbed into another one.~~*~~

As for example, when Maxwell absorbed optics into physimagnetism or when, on the basis of the work of Maxwell, Boltzman and others, what is called thermodynamics became absorbed into mechanics or statistical mechanics, that here you explain certain phenomena or certain laws of phenomena which are explored in one branch of a discipline in terms of theories which have been originally introduced and exploited in connection with a quite different domain of investigation. And now what I would like to say is that whenever this happens, many people feel very uncomfortable - - many people feel very uncomfortable

~~* Break in tape~~.

because it appears to them, and I say many people including scientists, that somehow the material which, so to speak, has been sort of reduced to this more fundamental and pervasive theory; that somehow it has lost its footing in the universe or in the nature of things. And that in particular, that when we somehow give an explanation for the familiar world around us in terms of the refined and abstract concepts of modern physics, that the distinctions we make in connection with the familiar world are really not fundamental distinctions, and that they have been somehow wiped out of existence by the explanation that is given.

Now, just to indicate that I am not inventing this problem, let me offer a few quotations from the most recent book, at least as far as I know, of Professor Margenau, a book which came out, I think, in - maybe - January, February - or March of this year, a book called OPEN VISTAS. In this book, among other things, he tries to show that modern physics is no longer materialistic. He tries, also, to show that modern physics, as contrasted with classical physics, somehow is more favorable to democracy.

I should like to consider only one part of Prof. ~~Mr.~~ Margenau's claim, namely, that modern physics has shown that this bugbear of materialism, which loomed over the imagination of many nineteenth century thinkers, has somehow been eliminated by modern physics. And so Professor Margenau says, among other things, he says:

> "Modern physics has come to recognize the reality of empty space and along with it, the possibility of an influence prevading space, an influence which is not material."

Then, later on, he says:

> "Is everything that exists material? A simple but perfectly proper answer would be - matter

is no longer material in spite of the literal contradiction in terms. But since the ultimate constituents of matter have resolved themselves pretty much into mathematical singularities - haunting space, materialism is no longer the comfortable doctrine it used to be, and one may dismiss it as having lost its major point."

Then, several pages after this, he says:

"An electron cannot be said to have a determinate position at every instance of time.* Widely sponsored by scientists in the early decades of the century has now been sublimated into the current concept of complementarity. This term was introduced by Niels Bohr. Its use animates his writings for it regards complementarity - the need for dual types of descriptions of human experience - as inevitable, as

* Break to change tapes

grounded in the nature of things, and the limitation of man's understanding. The appeal is to the deeper concerns of our being, somewhat reminiscent of a modern trend in theology which suggests that the knowledge of divinity is possible only through myths, allegories and paradoxes."

And now one final quotation, if you will, ~~to~~ permit me, -- in order to indicate how significant current physics is in changing our whole conception of the place of man in nature. Margenau has the following to say:

"Consider a neutron, which is on its way to a uranium bomb. If classical physics were true, a single set of observations on a position and velocity of ~~a~~ the neutron at a suitable time would decide whether an explosion occurs. It would leave room for action only to the very limited extent that if the neutron is found

> headed for the bomb, we can try
> to intercept and deflect it be-
> fore the impact."

But then he says: Well, of course, it moves so fast that before we can move, there is not much we could about it. So, we couldn't in effect prevent the explosion. But he says "according to classical mechanics, the very decision to intercept the neutron must be taken as a physiological determined consequence of all prevading physical reality. ies Our very decision is determined by all sorts of [illegible] conditions. Therefore, decision as such is an impossibility on the basis of classical mechanics. The new physics leaves greater room for action and avoids this difficulty. Even if a set of observations revealed the neutron to be headed for the bomb, we can still hope and act for our survival because what is now dynamically determined is a probability of collision, not a necessity. There is less cause for fatalism but accentuated need for action. According to the new philosophy of nature, man has been transformed from a spectator to an active participant in a drama of becoming. Room has been made for decision and choice which had no place in the older scheme of things."

Now, this is all very nice but the curious thing is that Professor Margenau himself, ~~if I can remember what I did with his card,~~ also says, and rather disarmingly, having said all these things, what ~~these~~ important consequences are -- and how different we can really now conceive our position in nature and the surroundings around us because we have now shown that classical mechanics somehow is not adequate; then, he says:

> "Although statistical reasoning is now recognized as the last resort in quantum mechanics, practically, very little has been changed in the visible world around us, for it happens that the probabilities of quantum mechanics, when calculated for the large and

> heavy objects of our common experience, congeal to certainties, much as they would for a die so biased that it must always fall one way."

So, the point I would like then to discuss in the few minutes I have left is this contrast which is so apparent in Margenau and, of course, it is apparent in a great number of other writers who insist on the one hand on the tremendously great changes in our conceptions of the world which modern physics has contributed, who then say, well, now, what quantum physics has discovered are the fundamental realities; if these are the fundamental realities, then the ordinary way of looking upon various things in our environment - - this is, at least partly, an illusion and the distinctions that it is customary to make cannot really be taken as being fundamental or basic.

Those of us who have lived long enough may recall the great vogue that some of the late

Sir Arthur Edington's popular writings have had, and the very brilliant image with which he starts his book on The Nature of the Physical World and of the setting down of two tables. One is the common, ordinary table and then there is the scientific table. The common, ordinary table is solid and substantial and so on. The scientific table, well, it is chiefly nothing. I mean, it is made of a lot of particles but then chiefly separated by an empty space and so on; and so he contrasts them. And then he asks, which is the real table?

Now, the point I want to make is that this contrast is really the result of a mistaken analysis; and that if it is a mistaken analysis, the contrast should never be made and more specifically, the supposition now that human freedom is possible, granted that quantum mechanics is true but it is not possible if you suppose that classical mechanics is true; that this again is simply a consequence of making a contrast where none should be made. And why should none be made? Well, for the reason for which I have essentially done nothing more than lay the groundwork; namely, the theoretical concepts which operate in a theory, which are present in a theory, these, by themselves - -- if you just have the postulates of the theory which asserts various kinds of connections between the terms that occur in a theory, nothing can be done with that theory until you introduce what I call rules of correspondence. Rules of correspondence between what? Well, certainly between expressions that one calls theoretical and which are unfamiliar and which are not applicable to experimental situations per se; that is, there are no experimental situations to which you can say: well, now this is an atom, or now I am measuring overtly the velocity of an atom. What you have to do is to establish rules which connect expressions like molecule --, velocity of molecule - - field strength - - or what not, with certain terms that refer to gross macroscopic situations, and that the theory itself makes no sense until such rules of correspondence, that is -- makes no sense as a ~~voluble~~ viable scientific theory in terms of which you can explain phenomena. Explain phenomena, ~~- not only~~ namely those on which you make observations but that nothing can be done with the theory apart from the rules of correspondence. Clearly, if such rules of correspondence are essential to operate the theory, one never can, without self-stultification then turn around and say: if I accept the theory, then I really must be skeptical as to whether the terms between which I make the correlations represent something that is a genuine part of nature. In particular, those characteristics - - those bodies - - which we recognize in ordinary, common sense experience; that is, the contrast can't be made because in order to show the relevance of the theory to anything that goes on in the laboratory or the field, rules of correspondence must be introduced which take for granted the very thing which presumably now, after you have gotten the theory you exhibit a certain amount of skeptical doubt. Or, put it in a somewhat different way, those who raise a problem in the way in which I think Professor Margenau raises it, and which a great number of other people raise it, are, in effect, failing to observe that the language of theory is the language of theory. It is not the language of the laboratory. If you want to identify the language of the laboratory as something which deals with macroscopic objects, macroscopic processes, things which, in an obvious sense can be said to be observed, although to be sure, the division between what can be observed and what is not is murky, quite clearly we may make gross distinctions; and the mere fact that you cannot draw a sharp distinction between what is observed and what is not observed no more wipes out the distinction between what is observed and observable and what is not, then is the fact that there is no sharp line which separates what we would call the front of my head from the back of my head. I don't know where - - quite know where to draw the line but it doesn't follow that there is no difference between the front and the back, and similarly in this domain.

Now, what I am suggesting then is that the error consists in -- I am really saying the same thing but putting it in slightly other words - - , in order to try to convey my point: the language of theory is not the language of the laboratory and the mistake - - or the language of ordinary life - - and the mistake consists in supposing that you can use the language of theory in applying it to the things for which the language of the laboratory is the appropriate language.

Now, you have to have both languages. Theories cannot function if you didn't have theoretical terms; that is, you wouldn't have what we call theories if they did not employ terms which were remote from experience, which dealt with objects that are in an obvious sense unobservable, and whose status might be uncertain. The whole point about offering an analysis of the structure of explanations is to clarify difficulties of this kind and not to be caught in the trap of on the one hand, recognize that you have to make distinctions; on the other hand, using those very distinctions in order to undercut the distinctions themselves. Or, if I may make one final application of Professor Margenau's comments upon this problem of freedom, Professor Margenau claims that well, in a universe in which the laws of classical mechanics would be true, there would be no decision, but that in the universe where the quantum mechanical laws are operative, there is decision. I think the only correct thing to say to that is that it is nonsense; and why is it nonsense? Well, for the following reasons: no matter whether quantum mechanics is true or classical mechanics is true, Lord knows what the final answer of physics is going to be about the ultimate constitution of the human body. I don't suppose any of us has any reasonable doubt that in 200 years, the type of explanatory theory is going to be very different from one which is fashionable today. Perhaps there is going to be a return to classical mechanics. Who knows? Certainly the return is not going to be in quite the way in which it was abandoned but something like that. Some kind of a deterministic theory might become the fashion once again, as Einstein believed it would, as Planck does and as De Broglie still continues to believe and so on. But what my point would be - - that we distinguish between acts - human acts that we call free -

from acts that we call, say, that they are not free. I take it everybody would say that a person who is bound hand and foot has no freedom in an obvious sense. A person who is under various kinds of economic or physical pressure, he hasn't got ~~that~~ freedom. A person who is ill; again, say he has some dimension of freedom denied him. But in an ordinary sense, say we do recognize choices that we regard as being our own from choices that are not our own. We didn't really choose this; we were hypnotized; we assented only under duress; that is, no matter what the fundamental explanation is for the mechanism which carries on this activity that we call choice, we distinguish between acts that are the outcome of deliberate choice and acts which are not. I can move my fingers at will; I cannot wiggle my ears. ~~This is~~ -- what the ultimate explanation for this is, I don't know and perhaps nobody knows the whole story. But the real point, however, is that things are as they are. There are differences between acts that we call free, acts that are not. If it should turn out that the fundamental physical theory - - the deterministic theory - - that difference~~s~~ will still be present and the only thing that we will have to do is to adapt the language of free and unfree to whatever might be the fundamental, physical assumption.

And so, I conclude that one of the consequences - - or one of the results of engaging in an analysis of the structure of science, particularly when the analysis is directed to considering the way in which scientific explanations are organized and the way in which scientific explanations are rated to others; and the way in which the history of science - - one finds the elimination or the reduction of one type of explanation to another, is that throughout all of this, there is something that remains permanent; that ~~a~~ is certain logical relations hold~~s~~. And when one talks about the structure of science, obviously one must be talking about something which is structural; which isn't something which is true now and it doesn't appear there. It has a kind of an invariance over time in the history of science and that in particular, in connection with this particular problem to which I addressed myself so very largely this evening; that by recognizing that a theory and a theoretical explanation involves certain indispensable components, certain to be sure perhaps logically elementary but pervasive, and I think from the point of view of our general culture, very serious confusions can perhaps be eliminated.

DISCUSSION

Question by Prof. Maxwell Primmack of Lincoln University.

Professor Nagel, we know in principle that there can be no direct observation of the properties of theoretical entities; e.g. the velocity of a molecule. And if we do not know this in principle, then how does this effect your argument concerning the separateness of the language of theory and the language of the laboratory.

Now, of course, it is quite clear that the answer would have to depend upon how one sort of theoretically conceives of the velocity of a molecule. Suppose, for example, one were to say, well, now, you can take pictures of molecules through an electron microscope and would one call this an observation or a seeing of the velocity of a molecule. Of course, here, I think the question would be what sort of things are you prepared to count as having observed, observed in a sense in which one would say, you observe the height to which a column of mercury rises in a thermometer and so on. Would one be prepared to say this is a case of observation. Would one be prepared to say that you have observed - that you have counted the number of molecules in a liter of a monotomic gas under standard conditions of pressure and temperature. If, on the basis of various kinds of experiments, you then calculate the value of Avogadro's number, is this a case of counting? That is, I think I will be able to answer the question only after having come to some agreement as to how these terms are being employed. But this sounds a bit like a dodge, and so I had rather not proceed in that way. Let me put it more unambiguously, that in some cases, there might be some doubt as to whether something that is represented by a theoretical term is observable. And one might say, perhaps, it becomes observable. In other cases, I think one would say - - in principle one would say - - this is not observable. As, for example, if it should turn out that the closer the way in which certain theoretical notions are defined and given certain - - let's suppose - experimentally or observationally determined limits of human perception, that those things couldn't be seen. That is, if - - take perhaps an absurd illustration. Could one see one wave length? And I think here I would not be risking very much if I were to say, well, this is, in principle, impossible. Similarly for terms like an electron; could one see an electron? And I think again I would be able - - I would feel fairly confident to say this, in principle, is impossible by an analysis of how an electron is defined and the kind of rules of correspondence that are used in translating the theory - - not in translating - - in coordinating the theory with matters of observation. So I think, to be quite precise, that what I would have to say is those terms that occur in theories which, on the basis of procedures such as that I have indicated, would be shown to be in principle incapable of being observed, those terms cannot be used for matters that are subjects of observation. That, I think would be a more careful reformulation of what I actually said.

Question by Dr. Baumrin.

On this same subject, what about Hoyle's hypothesis of the existence of bodies, beyond the bounds of the observable universe, travelling faster than the speed of light? Which hypothesis is deduced from an observed phenomenon, namely the red shift. Here we have something which is in principle unobservable and yet something which requires some to talk about unobservable entities whose existence is to be inferred from an account which is an explanation of an observed phenomenon.

Well, I am not sure you know against what - - maybe this is a wrong assumption and that your question is not directed against something - - maybe this is in connection with this notion of a theoretical - -. Let me say - - first of all, let me see if I can first reformulate the question. What do I make of Hoyle's view that some of the galaxies are receding with speeds that are greater than light and ultimately vanish, at least in the sense that they are no longer capable of being seen through the telescope, and the supposition that they continue. And perhaps; I don't know what the proper formulation would be. Do they continue to exist but no longer are observed, or do they simply disappear from existence? I am not quite sure what would be the correct formulation. But then, the further point is that, well, now - - look: this result is simply the - - I mean - - Hoyle's conclusions are something he arrives at because he is driven to this by data of observation, namely, certain - - I mean - - the red shift in the spectrum of some of the galaxies. Well, of course, one would have to certainly say it isn't just the red shift itself which leads to this conclusion, but the assumption that the red shift itself is to be understood in terms of the Doppler principle; and if the Doppler principle can be extended beyond the areas in which we have made experimental determinations. Now, I talk here certainly only as an interested layman; a question is whether the only explanation for the red shift is in terms of Doppler's effect. Certainly, it is a debatable one and I understand it continues to be debated, so that alternative interpretations can be placed on this and while I take it the tendency is to use that interpretation, there have been people who refused to go that way. What I am not entirely sure about, Dr. Baumrin, is what the implication of this is for my attempt to distinguish between the language of theory or for my emphasizing the importance of the language of theory and the language of sorts of observation.

I mean, clearly the theoretical terms that are introduced in any theory are often suggested by matters of observation. I mean, even take the notion of a point-mass, or the notion of an instantaneous velocity, or the notion of a wave packet; these are, even the language we use - - indicates that these theoretical notions have been suggested by things that we have observed but it seems to me that there is a tremendous difference between the cash value of the theoretical term, if I may be permitted to use Jamesiam language here - - the cash value of theoretical terms and the cash value of the observation terms. That is, what you can do with the observation term is something very different from what you can do with the theoretical term, and though psychologically I might find the theoretical term almost inevitable, this might be nothing more than a testimony to how I happen to think. I mean, other people think differently. To use my previous example of Rhyne; Rhyne's theoretical notion is extra-sensory perception. The notion of Professor Fellar at Princeton, who is a statistician, is that explanation is in terms of biases in series. Now, you are not driven to either of them in one sense, except in terms of some supplementary assumptions. Or, take one other illustration; take the so-called Freudian theory to account for slips of tongue. Well, the Freudian theory, you know, you have the complicated mechanism of the conscious, the unconscious. Freud, for some reason, found this is a congenial way of accounting for behavior of humans - - certain aspects of the behavior of human beings. Other writers were not driven to this sort of hydrostatic mechanism that Freud invented by analogy from water pressure, you know, but have a more physiological mechanism to account for this. But the theoretical terms are certainly not determined by observation and I would have supposed now that this is fairly well established; that two things are, I suppose, fairly well established. If I may mention or quote an authority here in support, I mean - - I think Einstein was perfectly right in saying that theories are free creations of the human mind; that they are not obtained simply by sort of grubbing in the facts and somehow abstracting from the factual details the theoretical notions; that is, I would have supposed now that first of all, that this is so. And secondly, I would argue that the theoretical terms cannot, at least as far as I know, be defined in terms of observation expressions. That is, I do not know how to define, literally define, in any usual sense of definition a term like molecule in terms of expressions which deal with matters of observation.

One of the things that I had supposed that time would always be long enough to talk about is a different interpretation of theory and you see, one of them has a long tradition behind it. 'Well, theoretical terms and the sort of thing that I have called theories, they really are dispensable; they are kind of a luxury that we grant ourselves in order to facilitate certain kinds of jobs - - make certain kinds of jobs easier, but that theoretical notions can be eliminated because all theoretical notions can be defined in terms of observation terms.' Now, this is a point of view that has been advanced and advocated by - - repeatedly - - in the history of thought and by extremely clever and brilliant people and they tried their hand at defining, as far as I know here as in the case of trying to define rules of discovery, the record is that of uniform failure. No - but no one has yet given a definition of any theoretical terms in observational matters. Always a promise - a pie in the sky - a hope and a prayer, but never the goods delivered. Now, I don't know of any theoretical proof that it is impossible to do this, but I think the presumption is that it can't be done. And there are not unreasonable arguments of another kind, not simply that well, nobody has done it; therefore, it is impossible. But other kinds of considerations which would lead one - - -* Now, I am assuming that this is one of the bedrocks, at least it is one of the bedrocks upon which my own discussion rests; if that can be shown to be really just sand, then I am afraid I am lost, but I hope it cannot be shown.

Question by Mr. Gray.

You have talked about rules of correspondence and I wonder if there are rules for defining what are proper rules of correspondence. Would it be fair to say that a rule of correspondence would have to be operationally meaningful in Bridgman's terms?

The question is that I have used the phrase rules of correspondence. What are the requirements for a rule of correspondence? Are

* Break to change tapes.

there, so to speak, rules for indicating what is an appropriate rule of correspondence and more specifically, must rules of correspondence be the kind of rules which Bridgeman called operational definitions?

I think the answer is a little bit complicated, depending upon the state of the field that you are dealing with. Let me put it this way. Sometimes what you do, in order to make a theory relevant to some domain of inquiry, where the terms of the theory now presumably have this characteristic that I have indicated - - what you do is to establish rules of correspondence between terms of that theory and terms of another theory. In this case, the rules of correspondence would not be between theoretical terms and experimental terms, or between things that could be operationally defined, but would be between theoretical terms and other theoretical terms where presumably those theoretical terms eventually are tied down to something else. So, as a general answer to the question, I would say that rules of correspondence are not in general of the sort that Bridgeman called operational definitions. I think in some cases, they are; on the other hand, my one hesitation in this connection is I am not entirely sure what the scope of an operation is in Bridgeman's sense, because you remember that although in his first book, The Logic of Modern Physics, he insisted upon the fact that all concepts in physics, if you are not going to get into trouble, must be operationally defined and the concept is synonymous with the operation. And then later on, when it was pointed out to him, but, look here, Professor Bridgeman, how do you operationally define molecule or what not. He says, well, you have to remember that there are two types of operations; I mean, the kind of operation that one calls the overt laboratory operation and the kind of operation that you call pen and paper operations - or mental operations - and then everything becomes operational and then the term no longer has a distinctive reference. But you see, I want to leave this question somewhat open - - whether rules of correspondence would conform to what Bridgman intended by what he called overt operations, I am noteentirely sure - - even in those cases where you want to have some sort of a connection, because it seems to me, in different areas, the thing is so extremely different. Look, let me indicate why I hesitate. Take terms which you use in geometry like point. Now, in some cases, of course, we say: Well, point, what is that? The term point is something that has position but no dimension. How can you use this theory you call geometry in order to do physics with, or do carpentry with. And in some cases, you know, you say: Well, I have a heap of chalk, a small heap of chalk, and although it isn't quite a geometrical point, it is something you could call a point - - well, in this case - - one could say that this is kind of an operational définition. In other cases, you say: Well, my point is going to be a planet. And now, how about a planet; I mean, is a planet an operationally defined concept? Frankly, I don't quite know because in part it depends upon a theory. In other cases, I take it you will take an entire galaxy as a point. Is a galaxy an operational concept -- an operationally defined concept? Certainly it is not entirely operationally defined. I mean, you have all sorts of really theoretical assumptions built into the notion of a galaxy. It makes me hesitate in saying that all of these definitions, even those which are not of the kind which correlate one theoretical concept to another would have to conform to Bridgman's operational criteria.

Unknown questioner.

Do you subscribe to Born's view that laboratory observational apparatus constitute extension of our senses, and objects so observed are just as much directly sensed objects as those observed with the unaided senses?

Well, certainly, I mean in general, I would certainly entirely subscribed to that - - that to suppose now that the so to speak - - the unaided senses bound the limits of what is real, seems to me an extreme parochialism; that nobody who has ever had a sense of liberation, even from reading science, could fail to recognize that there are certainly much more things for which we have excellent evidence to suppose they are real than we would, suppose we were to fall back upon the kind of naive I'm a man-from-Missouri sort of attitude. I thought that perhaps you were going to ask another question about Born's views which is again a theme I would like very much to talk about. But let me just mention what it is and then say why I find it interesting.

In the preface to a collection of many of his essays, some of them sort of semi-popular, which has the title _My Years in Physics_ or _Fifty Years in Physics_ - - I forget now the precise title - -

he has a preface which was to me, when I read it, a surprising and in a way a dismaying experience. He said: When he started out as a young man, he turned to physics because he thought this was a way of achieving objective knowledge; that he turned away from metaphysics, from art, from poetry. These were wonderful things, to be sure, but they didn't give him objectivity. And now, at the end of his life, he realizes that he was profoundly mistakened; that physics does not yield objectivity and at any rate, it is no more objective than are art and literature and metaphysics, and what not. And it is sort of - - I say it was sort of dismaying, an almost tragic tone in this. A man spent so many energetic years in advancing science in the hope that it will finally put a little salt on the tail of reality and it all turns to nothing, according to him.

And I think that this is a disease now from which many physicists suffer. There is kind of a failure of nerve almost about this; that somehow the fact that in modern quantum physics, we have to introduce some sort of so-called statistical assumptions or indeterminacy assumptions, that this somehow cuts the ground from under the claim that one has objective knowledge. Of course, this is also associated with this claim that you cannot in principle really make a sharp difference between subject and object. This is another area, it seems to me, of confusions of which scientists like laymen are extremely prone. As a matter of fact, if I may end on this bit of a sour note. I have been sort of impressed by the claims made by a great number of very distinguished scientists, certainly whose scientific achievements I have nothing but profound respect for, that things in politics would be very much different if only scientists got into that game. And you recall, Sir Charles Snow, in his book Science and Government, argues for the importance of having scientists active in government, not only there to answer questions when they are asked a technical question, but that they should really be in the position to ask the questions, and not simply to answer them when some professional politicians raise them; that they themselves become, so to speak, sort of professional politicians. And this sounds awfully, awfully good except for the assumption that seems to be so - so prevalent, at least among some of my scientific friends that well, sure, you have to be a great expert in doing physics but you don't have to be an expert in doing politics; that you don't need a know-how in doing that. And that a scientist, if he is a good scientist, can bring a good deal of wisdom to public affairs. I certainly wouldn't deny that some scientists might, but I frankly doubt very much whether in matters of political wisdom, anymore than in matters of philosophical wisdom; that is, in matters where it is a question of having a training and a patience to analyze ideas which don't deal specifically with technical problems in a science, that scientists, without suitable training, are particularly equipped to legislate or pontificate. That's the sour note.