L64-30

Copy No. 1 of 2

THE PROCESS OF SCIENTIFIC CREATIVITY AND INNOVATION

Dr. N. Russell Hanson

Property of the Library
INDUSTRIAL COLLEGE OF THE
ARMED FORCES

NOTICE

This lecture has not been edited by the speaker. It has been reproduced directly from the reporter's notes for the students and faculty for reference and study purposes.

You have been granted access to this unedited transcript under the same restrictions imposed on lecture attendance namely, no notes or extracts will be made and you will not discuss it other than in the conduct of official business.

No direct quotations are to be made either in written reports or in oral presentations based on this unedited copy.

Reviewed by Col R. W. Bergamyer, USAF, 4 November 1963

INDUSTRIAL COLLEGE OF THE ARMED FORCES

WASHINGTON, D. C.

1963 - 1964

The Process
of
Scientific Creativity and Innovation

20 September 1963

CONTENTS

	Page
INTRODUCTION -- Dr. Ralph Sanders, Member of the Faculty, ICAF	1
SPEAKER -- Dr. N. Russell Hanson, Professor of Philosophy, Yale University	1
GENERAL DISCUSSION	22

NOTICE

This lecture has not been edited by the speaker. It has been reproduced directly from the reporter's notes for the students and faculty for reference and study purposes.

You have been granted access to this unedited transcript under the same restrictions imposed on lecture attendance; namely, no notes or extracts will be made and you will not discuss it other than in the conduct of official business.

No direct quotations are to be made either in written reports or in oral presentations based on this unedited copy.

Reviewed by: Col R. W. Bergamyer, USAF Date: 4 November 1963

Reporter: Albert C. Helder

Property of the Library
INDUSTRIAL COLLEGE OF THE
ARMED FORCES

Publication No. L64-30

INDUSTRIAL COLLEGE OF THE ARMED FORCES

Washington 25, D. C.

THE PROCESS
OF
SCIENTIFIC CREATIVITY AND INNOVATION

20 September 1963

DR. SANDERS: Progress in science and technology ultimately depends upon the richness of the human mind. Our speaker today, Dr. N. Russell Hanson, Professor of Philosophy at Yale University, will examine the most essential, but often elusive concept of creativity. Dr. Hanson brings to this subject great competence, both as a philosopher and as a physical scientist.

It is my pleasure to introduce Dr. Hanson as the final lecturer in our section on science and technology. Dr. Hanson.

DR. HANSON: Well, gentlemen, I must say that I find this a little formidable. It's not the usual thing I encounter in the last weeks of September, but I think I had better begin by laying my cards right on the table. The top card is this one; that if I really knew something about making discoveries or being creative, I probably wouldn't be here this morning, I'd be in a garret somewhere with a patent attorney at one elbow and an accountant at the other, and I'd be making of them and reaping it in. So, I'm not really going to be talking as if I were an individual who had established a reputation doing this kind of thing and just telling you what it is all about, because I'm certainly not that sort of person.

What I am is a "logic chopper." That means that I occasionally address concepts and try to see how they are glued together. I'd like to do that with you this morning with these two difficult concepts of discovery and creativity. I'm actually going to say something about the sorts of things that count as discovery, and in the history of science the sorts of things which have looked like the creative process, and then, if I really feel bold at the end of the 45 minutes, I

might make some suggestions about how creativity could be encouraged. Although, that will put me strictly at your mercy; you probably know more about this than I do. Now, usually, nothing whatever is said about discovery and creativity, and for very good reason.

If you pick up a book on the history of science - or the philosophy of science - there might indeed be a chapter about the hunches, the insights, the intuitions and flashes that great discoverers have encountered, but that's just a way of saying that the author doesn't really want to address himself to this complex subject matter at all. Anything that takes place in a flash doesn't seem as if it's going to be very susceptible to detailed analysis. Yet, on the other hand, despite that disarming simplicity in the concept of discovery, there is an additional complexity which goes along with it. One feels that if the individuals who are capable of enjoying these flashes of insight and these hunches are the sort of individuals who are written about, then they must have extraordinarily intricate mental processes. It must be awfully difficult to see a man entertain 77 dimensions at the same time, or play 22 games of chess, and then come up with some great insight. One is inclined to say, "How in the world can an ordinary man making decisions at the management level possibly understand a bloke who can do things with a complicated mental apparatus of that kind?"

And so, usually, as I say, historians, logicians and philosophers say very little about discovery and creativity. There is one good feature of this silence. No one proposes to write a manual or recipe for laboratory researchers on how discoveries might be made. That is the best thing about silence. Many individuals - John Stewart Mill in the 19th Century is a good example - devoted a good deal of attention to the reasons why one couldn't have such a handy manual or recipe book. Unfortunately, there have been some written, but even more fortunately the people

who were actually making the discoveries realized that they're not really very germane to the work they are actually engaged in.

Despite the fact that there is this silence, however, and despite the fact that no one says anything at all about the activity of discovery - the process of creativity - there is a good deal more to be said, and I'm going to try to address our attention to that this morning. There will be some targets floated in front of you and you can pop away at them.

Now, after we grant that very probably a discovery or creative act couldn't be undertaken as a matter of rote, it's very difficult to know what expression a given machine making the discovery - it's very difficult to know what that expression could mean. What would someone be wanting to say if he said that the IBM 7090 just discovered that X. I think one would find some other way of putting the point, and I think it would probably be much more helpful. But this in itself doesn't entail that there isn't a good deal of semantical interest that attend these concepts.

I'll sail into one distinction that I think is relevant; I want to distinguish the context of discovery from the justification of discovery. And I think this is opposite to the two works you've read - the work by Zinofsky (phonetic), and the work by William Neil. The context of discovery is usually the playground of genetic psychologists and individuals who are in charge of making decisions for laboratories. They're individuals who are concerned with making the conditions most favorable for bringing about original work - creative activity. Of course, the major names in this area - Wortheimer is one you will have seen; he wrote a book called "Productive Thinking;" another name is that of Jean Pierrget (phonetic), the Swiss Genetic Psychologist, who has written enormous amounts of quite interesting material on just what it is that encourages an individual to think originally.

Actually, logicians say almost nothing about the context of discovery. Phil-

osophers avoid it, and I think perhaps that's a good thing too because they probably don't know anything about it. This is to be contrasted with another study of discovery, and this is what I call the "justification of discovery." Here you'll find the philosophers and logicians hard at work. This is really where you find ex post facto logical analyses of the finished research report. Now, you can see, really, how these two different kinds of interest come about. On the one hand, because of what I suggested, the alarming simplicity and intricacy which go together vis-a-vis the terms discovery and creativity, this means that a person who really want to come to a conclusion in the course of the discussion of these concepts, will probably leave the area of the context of discovery to the psychologists, the pedagogues, the educational and genetic psychologists.

What the logicians often address themselves to concerns what they might find in a manual like "The Physical Review," or the "Journal of the Astronomical Society." He actually looks at the argument; he looks at the conclusions that come out on the bottom of the page; he notices the premises, the evidence, the data at the top of the page, and tries to see how, as a matter of rational analysis the two hang together. Now, this too is quite a legitimate activity. The question is, "What has it got to do with discovery?"

What I'm going to suggest this morning is that in addition to these two well-worked areas in the field of the analysis of discovery, there is a third about which very little has been said, although the substantial names in this context would be those of Charles Sanders Purse, who taught at Harvard for some years and worked in the Coast and Geodetic Survey for about 30 years, and the other name is that of Aristotle which we can always get back to sooner or later, since he's safe and obscure.

But this third category consists not in an ex post facto analysis of a fin-

ished argument, and not discussion of the sorts of physical conditions like green blackboards or soft lighting - which helps a man to think, originally - but rather, the actual moves an investigator makes in the course of trying to solve a problem. You see, words like hunch, insight and intuition tend to make it appear as if the great undertakings of, let's say, a Newton or a DeRacque (phonetic), are done as a kind of species of water divining, or naval contemplation, or crystal ball gazing, as if there were just no way of decomposing the actual rational validification of what is going on. And this is absurd, because the great men in the history of science and the history of technology do some rather interesting thinking and make some important decisions in the actual business of solving their problems. And this I am submitting along with Aristotle and Charles Sanders Purse, is quite a legitimate area of inquiry and one which ought to interest, certainly, scholars, and, I think, ought to interest individuals who have to make decisions at the management level. Because, proposals will inevitably - I'm sure it happens to you every day - come forward about which you must adopt a posture. You must consider whether the objective, the target or the goal of this particular proposal is well-stated, whether the individual has actually found the intellectual curve from the beginning point - evidence, premises, data, through an intricate argument, to those conclusions. And that, I'm submitting, is exactly in this category.

Now, as I say, logicians don't discuss the context of discovery; psychologists don't discuss the justification of discovery; but I think that everyone should address themselves occasionally to this third category, the actual business of solving problems. I suppose John Dewey, William James, and individuals of this ilk have concerned themselves with this, and I'll come back to it later.

Now, what does it mean by growth distinction; it's a trichotomist distinction between the context, the justification and the rational properties involved in

discovery and creative activity. I'd like to introduce another distinction now, for better or for worse, and this consists simply in distinguishing three varieties of discovery. I'm sure there must be 33 at least, and every one of you will be able to say more about these three categories. But I'd like to distinguish what I call the "~~triple~~ trip over variety" of discovery, the "back into" variety, and the "puzzle out" variety. As you can see, I'm groping for slightly more respectable terminology, but this will have to do for the moment.

First, the trip-over variety. We've all encountered this when we were younger. This is a situation, I suppose, in which we see a young man walking along, he trips over a rock, knocks the rock to one side, and under it there, by golly, is the most marvelously-colored green beetle which may never have been noticed by anyone before. If he has the wit to do so and he realizes that there is something publishable here, it will probably be in the journals within a week. This type of discovery is a kind of happenstance; I think that's a technical term. Now, of course, there are some great names which have been attached to discoveries of this sort.

You all know the discovery attributed to Bequerre (phonetic), the famous French physicist, who apparently was looking for his lunch one day in the upper drawer of his desk. At an earlier date he had taken a bit of radioactive matter and laid it on top of a photographic pate. In the sandwich part of this constellation there was a key, and he found on the photographic plate a beautiful fogged imprint of this key. The interesting thing about this is that he discovered it in a way that was not premeditated or calculated, but he did do an awful lot of rather remarkable reasoning after he had encountered it. And that's what I call a trip-over discovery. Of course, one doesn't trip over the significance of such a discovery, but it is the kind of thing which very often gets into history books and Cecil B. DeMille films, as the most dramatic kind of discovery.

Another example of this is the discovery by Carl Anderson of CalTech in 1932, of the positive electron- the positron. There's no doubt about it, Carl Anderson then a research student running out some decimal points for Milliken, and he was not looking for positively-charged electrons. He was taking quite orthodox standard photographs of some of the cosmic ray tracks which were coming out in his set-up on his apparatus at the Norman Bridge Laboratory, and one of the tracks was most arresting; it was clearly electronic in range and yet it seemed to curve the wrong way. He didn't plan to find it, and he certainly didn't know what to make of it. And he was actually accused of having fiddled around in the dark-room when some of his contemporaries heard what he had found. But I would say that was a trip-over discovery. There are often some very exciting ones of this sort.

It's perfectly clear, I think, that the discovery of a new comet by an amateur astronomer in Japan or anywhere, might be the trip-over variety of discoveries. If, by going to a shelf of books in a library and discovering, "By golly, I didn't know that mother had been an author too," you have a trip-over type. A discovery of a new species, among biologists, is almost certainly of this variety too.

And then, in the history of technology I think of what is reputed to have taken place where an individual was trying to get a very hard metal for use in tools. He apparently got a bit of iron alloy, very very hot, and the thing slipped into a bath of oil. Something happened of course, and there we have an example of oil-tempered metal. This is the story, anyhow; I doubt very much that it actually happened this way; it's about as legitimate, I suppose, as Galileo and the Tower of Pisa, and Newton having apples all over his head; all of these are quite suspect; but they do bring out this trip-over aspect of discovery.

The next kind I'll talk about is the back into variety. These are the discoveries that a man makes, as it were, despite himself. He'd do anything to avoid

it if he possibly could because he finds the conclusion awkward and he'd like to return to the orthodox state of affairs, but unfortunately the data just won't allow this to happen. This requires a different kind of chap. This isn't the fellow who has his eyes wide open and is prepared to take advantage of any opportunity that is made available. This is the man who is prepared to think coolly and rationally, like Sherlock Holmes, as the story goes, despite the fact that the conclusions he is coming up with are quite uncomfortable. There are often good examples in the history of science, of this.

One beautiful example, I think, is the work of P. A. M. Givatt (phonetic) - again on the positive electron - although, this time he was concerned not with the experimental intricacies of this particular particle; he was concerned with working out the general electron theory, which has since that time become a general microphysical theory. What he did was, he tried to refine an early equation - some of you will know it as the "Pliny Gordon Equation" - and he got marvelous results out of his own version of this. In fact, it did everything for electron theory. Unfortunately, it would work just as well if one proposed that the particles that one was concerned with, were particles having what was then called "negative energy."

There are some interesting stories about the period. The great physicist, Gamov (phonetic), didn't know what negative energy meant either, so he began to call these particles "donkey electrons" by acknowledging that what you do with a donkey is that the harder you push it the more it comes at you. This seemed to him like negative energy, but it didn't have a serious physical interpretation. What DeRechep (phonetic) got out of this was the following: For at least two years he tried to cook the equation to get rid of the awkward consequences. In fact, he spoke of it invariably as a blemish in the theory. It worked beautifully except for the fact that there were these strange solutions - 50% of all the solutions, in fact, -

which seemed to describe a particle which was electronic in range and mass, but unfortunately had some peculiar characteristic which was later decided to be a positive charge. And, again, as I say, DeRaque spent a long time avoiding this conclusion, and then there was nothing he could do. In the fall of 1931 he just said, "Well, let's just suppose that there are such bloody things; we'll now just try to explain why they never turn up. So, that was his problem in '31. And, of course, on August 2nd of 1932 it turned up, and this made everything just fine.

Another example is the discovery, or the suggestion by Powley (phonetic) in the mid-'20s, of the existence of the neutrino. Here was another case where the investigator in question wasn't terribly enamored with the hypothesis that he found himself tied to. But, as some of you know, if you take a radioactive substance and you shield it - the sort of thing you find with radium, for example - against the alpha radiation and the gamma radiation, then the beta particles that are coming out will form not a star, not particles coming out of equal rating such as you find in the alpha star, but a spectrum; they go all over the place. And it's terribly difficult - it was terribly difficult under the tests then, to explain how it was that these particles can be identical and yet have such different tendencies.

What Powley did then was to introduce a hypothetical particle which would have just this property; it would explain away the particular problem. It would have just the energy you would have needed in order to give a beta star, analogous to the alpha star - he wasn't terribly convinced of it at the time - the neutrino hypothesis is still not the best-established thing in theoretical elixthemital physics (phonetic) - an effect, which was uncovered at the Savannah River Experimental Station. But, there is some mischief about this particle. My only point about it was that in the first instance it began life as a backed into discovery where the physicist simply had to accept it, otherwise life would look

pretty awkward.

Another example, and possibly the best example with the history of American science is Michaelson's work in the 1880s. He was concerned to discover precisely what was the quantitative aspects of the ether, that medium, undulations within which, constitute what we call "ordinary light." He had no doubt whatever that there was such a medium as ether; he just wanted to find out what its properties were. And he did the famous experiments, as you know - the interferometer pointing in two directions - and didn't get any indication whatever of an ether drift, and was forced there to reach the conclusion, against his better intentions, that there wasn't any such thing as ether. He didn't reach this conclusion, of course, for many years, but again, if asked what he was trying to do at the time he first cut that apparatus loose, he certainly wouldn't have said, as he was sometimes credited lated, he certainly wouldn't have said, "I'm trying to disprove the existence of an ether." Anything but that; that this was something that he was forced to conclude only after everything else failed.

Now, finally, the puzzle out variety. I'm only going through this little anatomy lesson here to show that there are an enormous number of different kinds of activity which count as discoveries, and the Cecil B. DeMille variety of a man saying, "By golly, I've got it," is perhaps overdone a bit. In the puzzle out variety I like to think of the Sherlock Holmes individual in this particular context. I'm going to come back to some of these examples because I think this is the kind of question that we ought to encourage if we're trying to increase in this country the occasion for original and creative thought.

I'll just give one example; the one that I'm always amazed with. The story runs this way: The great Newton in Cambridge on a sunny day - an already unusual circumstance, I suppose, for Cambridge England - was walking through the back,

the grassy area behind Trinity College, and there he was on a bridge. He stopped on the bridge and looked down at the water. I've made up most of the story myself, but it does help to give it a bit of wallop. He looked down off the bridge at the surface of the water, and he saw on the surface of the water the reflection of the sun and clouds overhead. And at the same time, beneath this reflection, he sees the bed of the river - the pebbles and coke bottles and other things that get to the bottom of a river. This, of course, was remarkable to him, because, of course, he wanted to know - this is surely something that everybody on the campus who crosses the bridge has noticed; that the water reflects the sun and he could also see the bottom of the river - Newton had a special problem with this. He thought it was very odd that one and the same medium could at one and the same time transmit this remarkable signal from up above, and also reflect it. It was reflected at that point where he saw it on the surface and it was transmitted from the fact that the bottom was illuminated.

He said, "Isn't this a remarkable kind of phenomenon?" And, of course, it is. I mean, it's only a part of quantum field theory today that it looks as if we're addressing ourselves to what for Newton was in the first instance a remarkable complexity in what was apparently quite simple. Now, I stress this with the best will in the world, because of something you've read. Mr. Bernofsky (phonetic) does say in that article - and I think he's quite right under many circumstances - that the ideal discoverer in this science, has been that individual who sees cosmic simplicities in apparent complexities. He says that in about three or four paragraphs.

Now, Newton didn't seem to me to be doing anything like this. I'd rather characterize it as an individual who addressed himself to something which is apparently quite simple and saw in it a pretty profound complexity. I don't know whether this counts as the occasion for a problem, but it certainly couldn't be

anything like Newton's optical theory unless the problem had presented itself to him in that way. And yet, this is a very commonplace type of observation.

Another, for example, is the fact that it's dark at night - this is one I love - when it shouldn't be dark at night, according to everything we know, or at least it shouldn't have been, according to everything we knew before the Second World War. It should be as bright as can be. The reason is simply this. Keppler showed, in the early part of the 17th Century, that the radiation from a point source of light should fork off as 1 over R squared. That is to say, if I take a given light source as Radius 1 and I move it out to Radius 2, at Radius 2 it will seem only 1/4 as bright as it did at distance 1. At Radius 3, 1/9 as bright; at Radius 4, 1/16 as bright, etc. Well, when you combine this with what Newton himself would have had to call the "cosmological principle" - and this is simply an astrophysical commitment to the effect that the distribution of stars, like our sun, is relatively homogeneous, such that the number of stars, like our sun, will go up in spherical shells as R squared. For each new spherical shell there will be roughly, well, I mean at distance 1 there will approximately about 12 first-magnitude stars. And, by golly, there are - 12 to 15. At Radius 2 - twice the distance - there will be about 48 or 50. And there are just that number of second-magnitude stars; at Radius 3, about 108.

His problem was to explain how it was that given this uniform distribution, one could account for this exact number of first-magnitude, second-magnitude and third-magnitude stars. He did this in virtue of the commitment to the cosmological principle. Now, when you take these two and put them together - let's say Keppler's Law of 1 over R squared, describing the radiation fall-off - the intensity - and the other commitment about the number of stars going up as R squared, then it stands to reason, I think, that if I move out to distance 2, every one of the sour-

ces of light would only have been 1/4 as bright as they would have been at distance 1, but, of course, there will be four times as many of them. And at distance 3 they'll be 1/9 as bright, but there will be nine times as many of them. Consequently, at every spherical shell from our center of observation there should be some finite addition to the amount of light we get at night.

Now, the argument continues - and it's alternately described as a paradox - Ober's (phonetic) Paradox; of course, he wasn't the author, but he's credited with this - he actually determined that within a finite distance, namely 700 million light years, which is a hell of a distance, of course, but it isn't an infinite distance, there should be enough illumination at that distance so that every square inch of the celestial globe is covered with a little sun, and therefore there should be at least a bright glow at night, and probably it should be a blinding and intense sunlight. This follows just from the simplest commitments one could imagine. And, of course, Olbus addressed himself to a simple complexity of just this kind. And, in order to explain this we've had to get into all sorts of mischief today about the red shift and various other astrophysical sophistications which I needn't go into at the moment.

In the beginning - in the beginning of this lecture, I mean - I mentioned that there was a sort of hostility about any talk which concerned itself with a logic of discovery or manual of discovery. And, of course, I'm quite happy about this, since anyone who did imagine that he could encounter interesting phenomena as a result of applying a set of rules, would certainly be in error. Nonetheless, there are things that one wants to address the attention to in the process of discovery. And, by way of illustration, I've set out a few of these already.

In other words, the logician and the philosopher are concerned fundamentally with the techniques of verification of a discovery after it has actually been pub-

lished or announced. Now, this, just as I have suggested, is a perfectly legitimate undertaking. The third variety, the context of discovery and the justification of discovery, would concern itself, however, with the problem of the analysis of good reason, or what goodness of reasons consist, then, the good reason for entertaining a hypothesis before that hypothesis has even registered to be put to a test. This is a legitimate area of inquiry, and this is, I would suggest quite humbly, the area which probably touches the sort of activity which most of you are concerned with every day. Because, when proposals do come forward to you; when suggestions come forward for evaluation either at the management level or involving questions of strategy, or actually involving questions of scientific exploration and research, it falls to you to evaluate the goodness of the reasons that are put forward in support of a given research project.

I submit that this kind of evaluation and analysis can be done in a way which is just as objective, just as dispassionate as anything which the logician undertakes as the final justifictory level. Now, it may be difficult to find what sorts of things would count as a good reason. It's very easy to give examples. I mean, for example when Keppler was concerned about the orbit of Mars - remember he's the chap who ultimately came down with the commitment that Mars moves not in a perfect circle but an elliptical orbit, there were certain hypotheses which he did not consider at all seriously, and concerning which we would say he had a definite reason for not considering seriously. He didn't worry, for example, about Mars' color. He didn't worry about the fact that Jupiter's moons were going into an eclipse - he didn't know very much about them at the time. But he wouldn't have worried if he'd known more about it then. As he didn't worry, for example, about the state of health of his own immediate family.

These did not seem to be hypotheses relevant to the particular problem at

hand. Now, we often say this, and we'll say this of what we're perhaps actually concerned in at a given time, but in each of these cases presumably reasons could be advanced for pointing out the relevance or the irrelevance, the significance or insignificance of certain projections. And here the level at which the establishing of goodness of reasons, it seems to me, is a thoroughly objective and logical undertaking. Now, there aren't any easy handles about this. We certainly aren't going to be getting any rules out of things like Mills' Method. You've all heard about John Stewart Mills' Method of Experimental Inquiry. I suppose there are some sciences which actually expand and develop in terms of Mills' Method, but they seem to me to be awfully dull undertakings, to say the least.

I don't know, and I wouldn't undertake to make any projections along these lines this morning - I don't know what the fundamental criteria of the evaluation of goodness of reasons at this stage happens to be, but I think I can spot it - and I'm sure you can spot it - when you read something like Sherlock Holmes - right? I think he's just great. I think he'd have been great in our Defense Department and other places, and I don't know why, but it seems to me that he is so shrewd. I don't say this just to raise the sales on Conan Doyle's books. It seems to me that here's a chap who is undertaking a series of rational reflections which are not to be identified with any of the sorts of studies we have encountered under the name of discovery or creativity. He's not reflecting on what past solvers of crimes have been able to achieve; in other words, it's not a historical enterprise for him. He's not appraising the structure of an argument ex post facto that he would hang together in a nicely dovetailed logical manner. He's not concerned with the conditions which made it possible for him to solve this particular problem that Mrs. what's-her-name calls up tea at the critical moment and that Watson kept his mouth shut at the right time; none of these things. He's actually

reasoning in a very, I should think, tough-minded way, about the steps toward putting a fairly high priority on one hypothesis as contrasted to others.

It seems to me that this is a context dependency inquiry. You're the individuals who could say what these reasons - the good reasons - would be in favor of a given proposal within a given context. But this could be evaluated in thoroughly straightforward objective terms is something I'm convinced of. And all this talk about hunch, intuition, insight and flashes, although it's very, very interesting - and I'm sure it gets grant at the right time for the right people - is nonetheless, I think, a way of obscuring some of the very exciting affects of discovery and creativity.

I'd like to make another suggestion, and that is this. I think that a study of the history of science - I shouldn't say a study; I should say an exposure - is not unrelated to the point I've just made. Clearly, for individuals like yourselves who have to evaluate the worthwhileness of proposals that come forward - and they must certainly be of interest, and the history of science is full of just such evaluations all the time. In other words, a man either in the Manhattan Project or at Palomar at the moment, a man who is responsible for, as it were, putting the firm's money on one particular horse rather than another, won't do this strictly according to some form book as they do at a racetrack; he will presumably have a good argument in favor of one as contrasted to another. And this is the thing that I'm sure all of you do address yourselves to, and I'm just suggesting that this is a thoroughly objective sort of thing; it's not just a matter of hunch or intuition of the investigator.

I will read, if I may, out of a book which is called, "Scientific Creativity." It was written by a group of psychologists. It goes as follows: "A German patent officer says, 'They ask how I recognize an invention out of this mass of applica-

tions which seek after privileges. It's very simple. While I look through one drawing after another I feel my attention riveted unexpectedly at times by one of them. From the detailed lines before me I see immediately the spark of creative fire. A nervous chill runs along my back. That, then, is an invention.'"

Now, really, I shouldn't have thought this was going to help any one of you - any one of us - in actually saying whether Jones or Smith gets the nod tomorrow morning, as though this is the way it's usually described. One of the things I'm suggesting this morning is that the history of science is, of course, in the overta dicta (phonetic), the cocktail party remarks, full of chat like that. But when you get really down to the cases, when you read a book which concerns itself with just this kind of evaluation - it's not a very interesting book, but it does concern itself with this area of inquiry; and that's Keppler's book on the motion of the planet Mars; they don't write scientific treatises like this anymore today; he tells you everything; all the mistakes he made, when his tummy was hurting; when he seemed to have a good idea and why he rejected it; when he seemed to have a bad idea, why he rejected that; he gives you the whole story. As I say, we don't have that these days, because there are so many journals that we just barely have space for the accepted conclusions. Nonetheless, at that stage, and reading a work of that kind, is what I should have thought would have been relevant to the kinds of decision which you have to make in the interests of all of us.

I had some other things I wanted to say about false discoveries, which I think are rather enjoyable, but I'll save that for later just in case we have to get in that at the moment. I'd just like to say one other thing - and this, I'm sure, is old news to most of you - in universities, and I think in industry, and certainly sometimes in scientific establishments, there is an impression that discoveries which were made, say, in the last century, or five centuries ago, or perhaps even

longer ago than that, are somehow quite simple-minded; they don't have anything like the swish and the complexity of the really exciting stuff we're doing today. This I find quite bothersome - and I think it is relevant to the evaluation of proposals. All one has to do is look at the accounts they're giving of the work of the ancient astronomer, Udacto (phonetic). He was a man who was worried about the fact that the planets didn't move properly and he tried to find a nesting of concentric spheres all of which were moving on different axles and at different velocities - different angular velocities - which would account for the fact that Mars at a particular time comes to a stop and then backs up. It really should be a problem for a natural philosopher who is convinced in advance that everything moves around us in perfect circles.

The complexity of Udacto's theme is really enormous. In fact, one can find everything one needs in Udacto's work, an early textbook on harmonic analysis. And if, in the work of the Second Century astronomer, Aquarius Polemy (phonetic), one sees a good deal of what we now call "Mercuries'Transformations" (phonetic) - he was concerned with the motions that, given an epicyclical wheel for example, might describe as it moves around a larger deferential wheel, either in the same direction or the opposite direction, with the same speed, thrust or lift, and, of course, this traced a very intricate path - well, it's continuous with some of the higher reaches of Mercuriere's transformation today. Why I stress this is because on some occasions when I try to adulate the history of science, and work in the philosophy of science and logical science, it sometimes looks as if this is the old simple stuff and why should we who are concerned with complex problems today address ourselves to past simplicities and simple-mindedness. I think the answer is quite the other way.

Some of these ancient contributions to the history of Western thought are as

intricate and challenging as anything we could possibly find today. In point of fact, just to take one hackneyed example, the great Heisenberg, when asked for the psychological genesis of his great contribution in what he quite recently tried to bring out in microphysical theory, which was going to do everything; it was going to begin with a prophecy-less wave equation for all of matter, which would have been a real mouthful. The inspiration for this, he says, came from the work of Enactomanda (phonetic), the ancient Greek natural philosopher. And I must say it takes a good deal of good will to see the connection. But if he says it, there must be something to it. And he saw in the work of the ancient thinker a great deal of complexity relevant to the sorts of things which are going on today.

Now, I've mentioned a fair amount about the concept of discovery as it actually affects the individual - individuals like yourselves - who must make decisions. This certainly consists, in the evaluation of proposals, in considerations of the types of inferences - I call them "retroductive inferences"- which proceed not from premises at the top of the page to conclusions at the bottom, but just the other way around; inferences, that is, that begin with anomalies, an uncomfortable state of affairs, and one is from that stage trying to find an explanation of these particular anomalies, or anomalous descriptions.

Of course, some of the greatest discoveries in the history of Western thought have been in exactly this form. The individual encounters something which he finds monumentally unsettling and trys to reason his way out of the morass - out of the complexity. This is the way I should suggest the Planet Neptune was originally discovered. The planet Urinus, which was discovered in the 18th Century by Herschel, wasn't keeping time properly, and here is the occasion for a problem. If it isn't keeping time as predicted by classical Newtonian mechanics, how in the world can one explain this in such a way that classical mechanics are still kept unal-

loyed. And, of course, what happened here was not that the investigator began with some new hypothesis which just sprang from the head of Jove and then began to unpack its consequences; rather, he began with the difficulty as it presents itself to him, and then tried to reason the way backward to its ultimate explanation.

Now, the last thing that I want to say this morning, and I'm sure that all of you know a lot more about this than I do - I've just fallen into a series of traps - I occasionally, when the weather is right, speak to the DuPont people about aspects of discovery, and aspects of the philosophy of science, and the history of science, and I've discovered something recently, which is called, euphemistically, "Patent Law." This is a remarkable undertaking, and I should have thought all of you would have gotten a great deal from it, because here, in a legal sense, the questions of priorities and originality are settled once and for all in a fairly crisp manner. Some of the criteria in that field are really arresting.

I'll just take one example. I was appalled to learn that in recent times there was a controversy between the Monsanto Corporation and the DuPont Corporation. I think the ICI and Great Britain were involved too. It consisted of this: An individual in the DuPont Corporation, after a great deal of puzzling and perplexity, had worked out a way of actually getting a process to move in the right direction. And it looked as if something was going to come out at the end of the line. It was nothing as dramatic as nylon, but an object equally saleable. Now, the question of the priorities came up and the patent attorneys all went to work. It turned out that an individual in the other corporation - Monsanto - some 22 years before, had denied that any such process was possible at all. He described the process in a fairly articulate way in the course of denying that it was possible.

The remarkable thing is that the last-named individual, the one who was denying its possibility, got part of the credit for the priority of the discovery on the

ground that he had formulated the concept initially in order to deny that it was possible. Isn't that remarkable? That's straight out of the 14th Century. It really is. When logicians in the 14th Century would argue with each other about whether or not something existed, like a unicorn or a round square, the opposition would invariably say, "Oh yes, I agree that a round square doesn't exist in the sense that you're saying, but you're saying something intelligible about it, and yet it doesn't exist. You're denying it and therefore you must be referring to something in a moderately intelligent and intelligible way, and therefore it must subsist." That was the technical language at the time.

I think that is the remarkable thing about contemporary patent law. The other thing is - and this will be my last or parting shot - the other thing concerns the reference I gave a little while ago to why it's dark at night. By exactly these criteria, by showing just this; that Newton had all the premises that were necessary to generate the great paradox which would have inspired some of the great discoveries which we encountered in the 1929-1935 period, in astrophysics in this century, it's possible to show that Newton was the father of modern astrophysics. Because, he certainly had the concept there of the fall-off of radiation intensity, of the general cosmological principle, and if he had simply drawn the conclusion that therefore it shouldn't be dark at night he would have needed a contemporary reference to the red shift, for example, of Hubble and Humason, in order to explain it. Therefore, what? I'm afraid that in patent law, by the same criteria which obtains right now, he would be given the priority for this particular discovery, by the argument that any conclusion which is down at the bottom of a page must have been there at the top of the page in order to be unpacked deductively from the premises.

So, this is very interesting, and all I'm suggesting as I withdraw into the wings before the sniping begins, is that whatever these criteria are, they certainly

don't have to be bowed down to in terms of hunch and intuition, and other manner of genuflective prose. It seems to me that in every case all of us can ask just why are we opting for this hypothesis rather than that one, and we can expect that the answer should be set forward in terms of criteria which are just as sound and just as valid and tough as we would have in the analysis of a mathematical argument; allowing that it probably does take a genius to come up with bright ideas, whatever they may be, nonetheless, he doesn't do what is irrational in coming up with that idea.

Thank you very much.

QUESTION: Dr. Hanson, I'll put a pull upon the bit of yarn you left hanging out. Would you give us your comments on the false discovery?

DR. HANSON: This is great, for the simple reason - I was once going to write a book, but I was fortunately, for the history of Western thought, dissuaded from doing so, and I was going to have as the theme of it wrong answers for the right reasons. Of course, this is really compatible with what I was driving at this morning, because it's the rightness of the reasons which seems to me to be a neglected area of inquiry. We sometimes pay far too much attention to the fact that what is coming out at the bottom of the page is okay. The best example I know of - you know Galileo's great work where he discovered the proportionality of the instantaneous velocity of a freely-falling body, the proportionality not with the trajectory or the sapce form, but rather with the duration of the fall, he gives an argument in favor of this. He's perfectly right.

But the argument he gives for this is quite erroneous. And that the argument was erroneous was discovered in 1910 by Pierre Dueigne (phonetic). I think this is a remarkable thing. Many physicists were so attentive to the fact that the answer

was right, that they didn't give enough attention to the actual argument which led up to that answer. Because, when one reflects on the fact that a good deal of what happened in analytical mechanics is genetically connected with Galileo's discovery, and when one reflects also that the argument he gave was a lousy argument; in fact, it was absolutely inconsistent. From that argument you can get any conclusion whatever; this ought to give one cause for pause.

Well, I think from the point of view of what one can actually achieve in a training course the business of addressing oneself to the right reasons for any answer whatever, is fairly important. I am quite happy when, on the rare occasions when I am involved in a discussion on theoretical physics, to hear a younger person come up with an answer which is just all wet; I mean, perhaps not only counter-intuitive, but counter-factual, provided that his argument is a good argument. And I should have thought that some of the great contributions in the history of Western thought - of Western scientific thought - have been great not so much because they came up with the right answer, but because they explored new techniques of inference which, in the long-run, have been valuable to us all.

There are plenty of examples of this where the first conclusion, the first approximation that comes out, usually looks pretty bad, but there is reasoning in favor of it.

I'll give you just one more example of this. We all know that X radiation is fundamentally of a wave nature, undulating in character. Of course, this wasn't known in 1910. And then, of course, the accepted technique was to take any transfer of energy and to run it through a diffraction rating of some kind to disperse it and to note whether or not you get an interference pattern. The argument was that if you get an interference pattern - light and dark fringes - the same sort of thing you get on the surface of water when the waves interfere and you get high spots and

low spots; if you got that kind of thing, then, of course, this was a good argument in favor of the phenomenon being undulatory.

Well, now, Max Von Lau (phonetic,) the great German physicist, said, "Whate we've got to do is find the diffraction rating of something on the order of 10 to the minus 4 centimeters - where the spacings occur - so that we can separate X radiation and subject it to the two salitic? experiments, is really what it comes to; you can't do this with man-made diffraction ratings. Michaelson made one of the best man-made diffraction ratings and he got it down to something on the order of 10 to the minus 4. That was much too crude, so Van Lau got the idea of using a crystal, the inter-atomic distances within which were on the order of 10 to minus 8. And this would be enough to disperse the components of X radiation and find out whether or not it actually would cause a pattern.

The argument that he used in the original paper was simply superb. I mean, it's a magnificent thing. In my experience I've never seen a more well-made suggestion at that stage of inquiry. And, of course, there were some aspects of the original determinations by Friedrich and Knipping (phonetic), two graduate students at the time, which seemed to become factual, and some investigators at that time, Schtark (phonetic) of the "Schtark Effect," who is the individual who comes to mind, attacked the whole inquiry because the conclusion was wrong, or, it wasn't exactly sqaure with what was observed. This happens a great deal where you find proponents of two different theories arguing with each other. If one fellow is on the side of the angels for the sake of the argument, but doesn't come out right on the nail with respect to the prediction, the other man will attack that. He'll say, "Your predictions are wrong and therefore you whole argument must be wrong." That's the standard technique.

But here is the case where the discovery in question, I think is attributable

largely to the structure of the reasoning which led up to this conclusion despite the fact that the original observations didn't completely support what he though was going to be supported. So, I think that the structure of the inference is most important. And really, that's another way of putting what I've been trying to say this morning; that in your daily work, in evaluating proposals you can't put yourself in the position of deciding pro or con with respect to something that hasn't even come about yet. You're in a position where you've got to say, "Is it likely, or is it other than likely that it will come about by a line of inquiry of this sort?" So, you're already in the Von Lau situation; you've got to consider the strength of the reasoning, the credentials of the reasoning, and the sort of, I suppose, animation - the kind of man who is doing the reasoning.

QUESTION: One of our speakers suggested that science really came of age when it was separated from philosophy. Would you comment?

DR. HANSON: What he said is absolutely true. How's that for a start? I was separated from science. I was on a microwave research team at Columbia University and everything was going just swell until I encountered something called the "Uncertainty Relations," and I know so little about it even now, that I've written two books about it, which just shows how things like that can get stuck in one's head. But I would say that the uncertainty relations, among other things, concern themselves with the philosophical foundations of theoretical physics. And I think it depends to some extent on the kind of interest that a man has.

I would see a great deal of justification in the ancient terminology - I say ancient terminology which was current until well into the 19th Century, in many countries - where physicists were not called "physicists," but "natural philosophers." These are simply individuals who are speculating and thinking analytically about the structure of matter. I see no difference in principle between this

kind of analytical reflection about the structure of matter, and comparable analytical reflection about the structure of arguments and the structure of context.. They seem to me to have a great deal in common. And one of the ways in which one addresses himself to the physical scientific discoveries of another, is to consider his arguments and consider the way he uses concepts. So, what I have to say about this is that in one sense of the word philosophy - what I call the "naval contemplatory variety" - what one is concerned to do is get much more purple ink and larger capital letters; talk about being, non-being, other than being; all this stuff; he's certainly right.

Fortunately, I have never been exposed to any great extent to this sort of stuff, but it does exist - some of my best friends do it; - but I should say that it never had any business in natural science. And when, in the history of physics, chemistry and biology, the practitioners have been questioned about being programmatic about the future - their discipline - they've made clear that they don't want any more of this blasted philosophy - work in psychology; so, this little symposium is called "Scientific Creativity," by Taylor and Baron" - they're experimental psychologists, and what they want to do is count things, you see; the number of responses given at a different time. And it all seems to them very unlike the sort of things that Kant, Schichter and Dewey were concerned with when they were discussing thinking in general; and I think, in a sense, they're quite right. It's just a question of whether one is prepared to throw out too much of the baby with the bathwater.

There is nothing about the word "philosophy." After all, it's a Greek derivative; it simply means love of wisdom. I'd hate to think that if a scientist were actually nailing up his shingle that he would say, "We have no love of wisdom here." But, I think what I'd better do here is retreat gracefully on this point and say that since my discipline - the center of gravity is within the philosophy de-

partment; you see, I've got to be careful; I must nonetheless specialize that I'm also concerned with the history of science and the philosophy of science and logic, so I want to do two things - which is schizoid - I want to agree with that remark and yet qualify it to a point where one could never recognize it if they came around to it a second time.

QUESTION: Dr. Hanson, would you care to speculate on what fields in either the physical or natural sciences the prospects for the next great discoveries might be set?

DR. HANSON: It will be absolutely uncontrolled. Physics is in a lousy mess at the moment. So, something has to happen. I mean, it's all well and good to say "We're grinding out the right numbers," but again, back to the earlier question, the techniques - the arguments - are just terrible. I'll just give one example for those of you who have been exposed in this area. In quantum field theory there are certain ways of setting out the wave equation descriptions of particles in a given state, which are such that when you begin to expand this equation you end up in a mathematical ampalla (phonetic); that is to say, you're concerned with inter-growth and these tend to diverge, which ultimately will give you an infinite number of solutions to any problem that you start with.

What the physicist does in this context is to select from this infinitude of possible solutions, by mathematically quite extraneous considerations, a finite number that he wants to explore further. Now, this is a terrible argument. Mathematicians usually get sick when they encounter things like this. In fact, I have heard theoretical physics referred to by pure mathematicians as being a species of emetics.

Well, something must happen here. My dear brother-in-law who is a considerable theoretical physicist began life - actually, this was in Cambridge, England - he

began life by considering the numbers of ways in which theoretical physics today was simply broken back from this intellectual point of view. So, something has to happen there. And, I'm pretty confident that with the enormous metabolism that one encounters in the biological disciplines today, especially at the borderline - all this stuff about DNA and the structure of large molecules - this is certainly going to keep going. It just looks like a wide-open field to me. I wouldn't dare to predict; it all looks - let's put it this way. It's very unlike what might have been said at the close of the 19th Century when many practitioners of science - I think it was Reynolds who was the individual responsible for the "Reynolds' Number" somewhat analogous to the "Mach Number" in aerodynamic theory - Reynolds, in 1903, simply said, "The future of physics is fairly dreary; all we're going to do is run out a few more decimal places. All the major principles and laws have been discovered."

Of course, this is identical to what Kant himself said back in the 18th Century. This is a pretty comfortable feeling for anyone to have. The happiest thing we can say about science today is that no one is really in that position. I think everyone realizes that tomorrow will hold many more unexpected things than they ever encountered in the past. And, excitingly enough, in technology it's exactly the same way. I think we live in a remarkably interesting time, and that means that you chaps who have to control some of the avenues of inquiry by pulling the right string at the right time must really have an interesting night's sleep on some occasions. I occasionally am responsible for suggesting to a graduate student that a certain line of inquiry wouldn't be terribly profitable.

My usual example is this. At least once a year I have a young man come in and say, "You know, I think it's possible to construct a perpetual motion machine of the first type, or the second type. The other fellow wants to try it on the second type." They tried to build a heat machine that will actually put out more

work than is fed into it. And, of course, my natural inclination is to have a prejudice and a bias; I say, "Well, I'm sure you're wrong; it's just a question of finding what went wrong with the argument." I must say that occasionally, after teatime I wonder if I said the right thing to this man. Perhaps it would have been better to let him go. Because, tomorrow is so full of the unexpected that it would be difficult to predict just exactly what well-established laws are ready to go down the drain.

Remember the great shock we got in 1957 when Yang and Lee proposed that the principle of the conservation of parity was perhaps not in as solid and sound a shape as everyone in theoretical physics at that time thought. And, of course, there is an imbalance which has had other consequences in theoretical physics. And I think we're in store for lots of that stuff. So, I'm dodging your question.

QUESTION: Going to your third method of discovery, and hopefully speaking in the plainest dimensions, can you give us some idea of the level of effort and prospects for success in attacking the question of the dimensions of the universe, the container if there is one, and if so, what might be beyond that?

DR. HANSON: Well, I'm glad this room is of a finite size. As you know perfectly well, the best estimates as of this moment are geared into the big optical machine at Palomar, and our expectations are that the observable universe will probably, as a matter of principle, never exceed for us a constellation greater than the diameter of four billion light years. This is, of course, related to the power of the big 200-inch machine.

Now, there are some very interesting philosophical questions, if I may use that deadly word, about just this issue. I mean all the exercises of the new cosmology do turn on questions of this sort. The question of what lies beyond is addressed in two quite different ways, depending on the philosophical complexion of

the astrophysicist in question. As you know, there are some astrophysicists - Gamov is the most notable of them - who are believers in the big bang theory. Their argument is to the effect that everything that we encounter at the astronomical level is the result of the initial violent expansion of what is sometimes called the "primeval atoms." It's a "large" collection of matter and energy at infinite density which has since unpacked and is still in this process as is indicated by the red shift. Consequently, what they have to say; they have to make a distinction between what is observable in principle, and what is observed, which does effect our understanding of the principle of the conservation of anything.

But, what they've got to say is this; that given the recession of the remote galaxies, and given the rate of recession, they have to say that galaxies which are very faint from the point of view of the machine at Palomar at the moment, will, within a finite time, pass beyond the limits of observation and consequently they will be passing out into the great unknown. And from the point of view of some astrophysicists - I'm thinking of the new cosmologists like Fred Hoyle, Herman Bondi and Tom Gold - they regard this as a terrible thing to say, for any physicist, because, what the man is saying is that there was a given amount of energy "in the beginning," and a lot of this is receding beyond our powers or capacities of observation at the moment.

What Bondi, Hoyle and Gold are trying to do is, they're trying to take the notion of the conservation of energy and treat it as an operational concept. They want to consider that the observable universe is such that it conserves energy. And, of course, given the big bang theory, it can't do this because they're continually getting extra nebulae which are receding beyond the limits of observation. Consequently, within the big bang theory, in the observable universe the energy level must be falling. Whereas, with the new cosmological theory, they have an ad-

ditional hypothesis which keeps it steady as the hypothesis of the continuous creation of matter which, as it were, keeps the background material within the existing universe at a constant level. Hoyle and Bondi are called "the continuous creators," for this reason. And this is a purely fanatical and philosophical concern. It concerns, really, what these individuals mean by the conservation of energy.

The big bangers get very cross with the new cosmologists because they quite glibly entertain creation as (ex night low?). That is, they say the creation of a hydrogen atom comes out of nothing whatever. This is a kind of awkward premise to have to nail one's flag to, but the consequence of it is that it keeps the energy level constant within the universe. And they say that the big bangers are not respectful enough about the conservation of energy because they let it recede beyond the limits of observation. So, they're both accusing each other of not conserving energy, and they're both being highly interested in what the conservation of energy would consist of.

Am I on the right track with respect to your question now?

QUESTION: I knew my question would bring a laugh. But even if there were nothing there, how far is nothing?

DR. HANSON: Actually, the present prejudices within general relativity and theoretical cosmology and astrophysics, indicate there is no limit. It isn't as if there were any type of Mark Twain wall that we're going to run into.

I remember when I first went into a philosophy class at the University of Chicago, I encountered a situation which bothered me at the time. It concerned the difficulties about considering the universe to be finite. The argument went someo thing like this - I think it originally came from Ancient Greece, somehow. If we imagine that the universe is finite, then presumably we could entertain the possibility of a very strong javelin thrower getting out "toward the edge" and throw-

ing a javelin that way; in other words, toward the nothing. And if it keeps going - and there is something out there, obviously - but if it bumps into something and comes back, then obviously there's something out there too. But really, it does contain the germ of what one can find in some parts of the suggestion of Einstein in 1916. He wanted, certainly, to speak of cosmic space as having the property of being unbounded, but finite. It's a little bit tricky to catch onto, and what one needs is geometry in order to consider how it is that one can think of, for example, a light signal going out in a given direction - say toward the place of "place of recession" at the moment, and yet it being possible as a matter of physical principle, to traverse all the space and yet come back to the point of origin, and still be able to describe the universe as being infinite.

DR. SANDERS: Thank you very much, Dr. Hanson, for an extremely stimulating lecture.