DEVELOPMENT OF SCIENTIFIC CONCEPTS

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NOTICE

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Publication No. L65-28

INDUSTRIAL COLLEGE OF THE ARMED FORCES

Washington, D. C. 20315

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CAPTAIN PHILLIPS: General Steel, Mr. Ward, Gentlemen: You heard yesterday that a knowledge of the past is certainly necessary for an analysis of the present, and that this was just as true of science as it was of governments or civilizations.

To expand our knowledge and to speak on our subject of today, the "Development of Scientific Concepts," we have a man who spent many years studying the men and the methods of science. Dr. L. Pearce Williams is the Associate Professor of History at Cornell University. His specialty is the history of science. Dr. Williams.

DR. WILLIAMS: I am going to start out by changing the title of the lecture. It will cover the same material but I would like to attack it from a different point of view and simply ask the question, "What is science?" and see if the history of science can shed some light on it.

I think we stand today in a period of crisis in science itself. The scientific journals are filled with problems of publication and of information retrieval; there is a feeling that somehow the arteries of science are becoming clogged or hardened by the extraordinary flow of scientific information.

I think the question of what is science is becoming increasingly obtrusive. This itself is a historical repetition. In periods of scientific crisis, this is precisely the question which is raised. It is a clarifying question; it removes a lot of the underbrush, a lot of the brambles, and it gets down to a kind of basic thing.

I would like to call your attention today to two such crises in the past, making the present one a third, for I shall return to that; two crises in the past which have led to an attempt to redefine what the scientific enterprise is. And in turn, should like to try to lay out certain methods and approaches to science which, hopefully,

would get us over the crisis; which would see science through these periods of stress.

The first period I would like to look at is the period at the beginning of the 17th century, the beginning of the Scientific Revolution as it is now called.

The second period is the early days of quantum mechanics; the crisis in the early 20th century when the attempt was made to reduce atomic and subatomic phenomena first to the laws of classical physics and then the rejection of these laws and the attempt to see science as something quite different from what it has been since the Scientific Revolution.

The crisis at the end of the 16th century was a very severe one. The old medieval picture had broken down ; the medieval methods had vanished; reason seemed to be a blind alley; the great medieval philosophers such as William of Ockham, Duns Scotus, had pointed out that reason leads you nowhere; you cannot reason out a theology and theology therefore must be taken on faith from scripture; reason seemed to get you no farther than the quantification of angle density on pinheads. It seemed to be a dead end. And so, the question raised at the end of the 16th and beginning of the 17th century was, "How do we get out of this cul de sac? How do we get out of this dead end and come back once again to some kind of vital relationship with nature and with knowledge?"

Two men, I think, stand out--at least for my purposes they stand out because I want to speak about them this morning; they are Francis Bacon and René Descartes. Neither was really a great scientist, but they were philosophers of science and each felt that he had the answer to this impasse. Each felt that he had the way out, he had the definition, if you will, of what science was, so that, in fact, there could be a new attack, a renovation, a rejuvenation of the intellectual world.

Let me begin with Bacon. His dates, incidentally, if you want them, are 1561 to 1626--to give you a kind of chronological coordinate. Let me first speak about Bacon and his method. Here we will see very definitely a method which is very much alive today. In fact, at the end of this talk, if I have time, I want to come back and point out that Baconianism is probably the official philosophy of science in most of the research laboratories of the world today.

What Bacon revolted against was the scholasticism of the late medieval period, the idea that you can arrive at truth by debate; that two men could get together in an arena and quibble about words. And by quibbling about words somehow arrive at the essence of physical reality. Bacon said, "This is a lot of nonsense. If you want to learn about nature stay out of auditoria; do not go to debates, go to nature itself." This seems healthy, but Bacon had a peculiar way of approaching nature. Bacon was not merely, as he is often pictured, a taxonomist. This is, after all, one way to approach nature.

Gall wasps, as Kinsey pointed out, can be classified--this is the presex Kinsey--into various groups, and one, then, has been able to find a shortcut to some kind of knowledge. But, this is not what Bacon wanted to do. Bacon wanted to arrive at the essences, the fundamental truths--laws, if you will--of physical and natural phenomena. How do you do this? In his great work, the "Novum Organum," the new instrument for research, Bacon laid out his method and illustrated it. The illustration was a happy one because Bacon happened to be right. It was the only thing he was ever right on, but it was a good example.

He said, "Let us find out what the essence of heat is. What is heat? How do you find out what heat is? Well, let us collect every instance of heat that we can find. You can get heat from burning things. You can get heat from rubbing sticks together. There is heat if you pound iron with a hammer. There is heat in putrid material as it ferments and as it works. Let us get all these instances of heat together." There is not, after all, on the surface, much similarity between a pile of dung, and, let us say, a match. But Bacon insisted that the one thing that ties them together is that the pile of manure is, in fact, generating heat and so is the match.

Getting all these facts, these instances, together, Bacon argued that we should abstract to find out what it is that is common in these instances. To him, the one thing that was common was that heat was a form of motion. Here is a kind of primitive kinetic theory, if you will, which is always trotted out when people talk about Bacon and say, "See what a smart man he was; he had the kinetic theory before Maxwell, et cetera, in the 19th century.

As I say, it was the only thing he was right on. His methods may have worked here, but they were singularly unsuccessful in

other areas. What I want to call your attention to is not the fact that he was right, but the method which he advocated for coming to grips with nature. The method is simply this; collect facts. Pile up the facts. Report the facts. The implicit assumption in all this is that when the facts reach a kind of critical mass they will blow up into a theory. It is automatic; enough facts and all of a sudden one day you wake up and the pile of manuscript reduces to a single equation, and you say, "Aha! There it all is."

And so, the principal point in the Baconian method, is fact-gathering. Now let me draw your attention to the consequences of this. In the first place is the primacy of fact; science deals with facts. Facts are objective. That is, they do not depend upon the observer; they are something external to him and are therefore independent of him. This is part of the charm of the Baconian method; it cuts the silver cord between observer and observed, and gives the illusion, at least, that you hold in your hand a nice solid piece of reality. That is point No. 1; the primacy of fact.

Secondly, the role of theory in this is to sum up facts. A theory is merely a concise statement of a large number of facts. Notice the role of mathematics, now, in this. Mathematics serves as shorthand. The most concise expression of facts will be a single equation. What you want the mathematics for here is merely to express the empirical evidence that has been piled up in the laboratory. Also notice that in this system science is democratic. Anybody can do science. That is, you have all the facts in the world around anybody, and all you have to do is train a man how to determine them. You do not have to be terribly smart; you merely have to master techniques. And when you master these techniques you too can collect facts and become a boy scientist.

Finally, and perhaps most importantly, it is a dogmatic science. I want to stress this very strongly. We are constantly bombarded today by statements that science is open-ended; science is not dogmatic; it is not authoritarian. But, in point of fact, Baconian science is. And it must be, because how can you argue with it? A fact is a fact. Now, if I say, "You are wrong," I am saying one of two things; either the fact that you have determined is wrong, or, that you are an incompetent. If I say the fact that you have determined, is wrong, then I must back this up by showing that you are incompetent because you did not get it right, and therefore the two merge together to give you no basis for rational argument. Right

or wrong, there are no interpretations in the Baconian system. I would like to come back to this a little later on.

The great opponent of Baconianism in the 17th century was the French philosopher, René Descartes, 1596 to 1650. Descartes was a rationalist. Descartes' argument was that the universe is a rational creation. In fact, his main point philosophically was that since the Universe had been created by God, and since God is the eminently rational being, and since man is created in the image of God, in this rational sense that is, to Descartes it was man's reason that participated in the Divine, there man's reason and God's reason are congruent. Therefore the universe is rational and man can understand it. Given this, the instrument for discovery, the instrument of research, is not the laboratory, not fiddling around with all these extraordinarily difficult experiments where you never have the same bit of water twice, or the same piece of steel twice; the way to go about scientific research is to think.

Descartes argued that when God created man He gave him certain innate ideas. It was from these innate ideas that were clear and God-given that Descartes drew his earliest thoughts on the Universe, defining matter, for example, as pure extension. This seemed to be obvious. From here you now explain natural phenomena in terms of the rational connections; a reasonable and logical argument is what led you to truth.

This may seem a little outside the mainstream of the modern world. After all, I doubt very much that many Nobel Prize winners would insist that their ideas came from God; that they would insist in any serious way that Cartesian rationalism was their method. Yet, it does exist today. It exists in the sense that the Cartesian system insists upon a rational explanation. In other words, if you have a series of phenomena, what you want, according to the Cartesians, is a reasonable explanation of these phenomena not necessarily drawn from empirical fact but just something that gives you some insight into the phenomena.

I will cite as perhaps the greatest modern example of Cartesian science, Freudian psychoanalysis, where what Freud gives you is a rational explanation of the working of the human psyche, given certain strange entities which seem not to be capable of experimental examination, the id the ego and the superego. These you cannot actually probe and find a little part of the brain that says,

"I am Id." These are mental constructs on Freud's part. But given these, Freud can now give you in good Cartesian terms a rational explanation of the way the human psyche works, and one checks this against human behavior.

Cartesianism and Baconianism, then, are opposed. Baconianism is fact-finding. Cartesianism, is, if you will, theoretical physics in a peculiar sense; a theoretical physics that builds on hypothetical constructs through the use of reason. Note the consequences of this system. First, it places emphasis here on the rational argument; notice, not on the ideas, for the ideas do not belong to the individual; they are God's; you are merely using these to start with. What you want is the chain of ideas; what links them together logically.

Now see what a theory is. A theory in this sense is this chain of argument. And in this system see what the role of mathematics is. Mathematics is a logical tool. It is only in mathematics that Descartes and his followers would insist that you can avoid semantic traps that are set for the unwary in the use of ordinary language; when you use words that mean different things to different people and therefore you get trapped.

You probably all know Jonathan Swift's little parody in Gulliver's travels where he takes this to its ridiculous ultimate extreme. I forget the people who do this, but they never speak; they carry great sacks on their backs and when they want to say something they reach in and pull out the object. So, they do not have ambiguity. They do not use words at all, but use the thing itself to make a point.

For the Cartesians, mathematics is the most refined tool of logical discourse, and this is its primary role. Note too, that Cartesian science is democratic; democratic in a very odd sense; democratic in the sense that all individuals have the same innate ideas, or should have the same innate ideas. Everybody, therefore, is capable of grasping these fundamental truths about the universe. This is the origin of the term "commonsense." It was that intellectual faculty common to all of mankind. So, democratic in this way, but dogmatic as well. For, it is dogmatic in terms of individual reaction to the innate ideas. Let us assume that we are going to argue. I have an innate idea which is clear. I see it very clearly; it seems to me indisputable. I know it is true. But this is

a subjective reaction. I know it is true. You have an innate idea; you know it is true. But I am chairman of the department and you are not; therefore my idea is more innate than yours--or else.

In this sense, you see, there is no referee possible; there is no way in which you can quantify innateness or clarity. So that you have here the ability to create a dogmatic science. And again notice, that this science is really not open to argument on its very basic level. You cannot tell me I am wrong. Or, you can tell me I am wrong but you cannot convince me that I am wrong. I know that I am right. And therefore, why argue? I may argue because it is socially acceptable, but you are never going to show me that, in fact, I have made a mistake.

These are two of the systems. They fought through the 17th century. They each had their proponents through the 18th and the 19th, and, as I have hinted, it seems to me they are present today in modern science: Baconianism in physics only to a certain extent. Physicists have a tendency to be quite sophisticated nowadays, but if you want to see Baconianism in splendid purity, turn to the behavioral sciences. There are the fact collectors par excellence.

The Cartesian method exists, as I suggested, in psychoanalysis and in those areas where it is difficult to isolate what seem to be the fundamental aspects of the science and one must build on a rational argument rather than on any experimental or inductive base.

There is a third and extremely important view of science which emerged from the revolution in physics at the end of the 19th century. This is the view which I would suspect today that 90 percent, if not more, of the theoretical physicists hold when pinned down; they do not like to be pinned down, they like to be in the laboratory and not bothered. But if you meet one at a cocktail party and get him into a corner I think that most of them would accept this view of what science is about.

Its genesis is interesting. It came about as an attempt to deal with the phenomena of atomic physics. The first blow to classical physics, of course, was Planck's discovery of the quantum of energy in "Black-body Radiation" in 1900 and Einstein's great paper in 1905 on the "Photo-Electric Effect," which seemed to imply that energy

not only was emitted in little bundles, but came in little bundles; and finally, with Bohr's attempt to use the quantum theory in solving the problems of spectroscopy.

I am sure you are all familiar with the Bohr atom. It was essentially Rutherford's atom which he had proposed as a result of work with alpha particles shooting through thin gold leaf. He showed that if you have a very massive nucleus which is very small, then some of these alpha particles will turn around and come back at you, indicating there must be something very massive there, and that the electron must be in an orbit around the nucleus.

What Bohr did was simply this. According to classical electrodynamics, of course, such an atom is impossible, because, as the electron goes around the nucleus it is accelerating, since it is moving in a circle. It is well-known that an accelerating electrical charge radiates energy and therefore it would simply spin into the nucleus. So, Bohr said, "That is right; let us forget it. Let us assume that the electron goes around the nucleus and does not radiate. We have spectral lines so there must be specific areas where it does radiate. Let us assume that the electron can jump to another orbit and then when it collapses back, it emits radiation which will have the frequency represented by this jump." And with this in his great paper in 1913 Bohr was able to solve the problem of hydrogen spectra.

You see it is an impossible atom. It cannot exist by classical laws. It has all kinds of ad hoc hypotheses; forbidden orbits; areas where the electron cannot exist but does, for where is it when it "jumps"? How did it get from here to there if it cannot exist in between? It was Werner Heisenberg who said, "This is a lot of rot. We have passed beyond models; we are not little children anymore with erector sets; let us turn to pure science. We have three things we know about atoms."

"We know the frequency of radiation. We know the intensity of the radiation, and we know its plane of polarization. Let us take these three things. These are given facts. Let us see if we cannot work out a mathematical system by which we can predict spectral lines. We do not need atoms; we do not need models."

Heisenberg worked out a system of physics which simply works this way; it says, "We do not, nor can we ever, know the ultimate

units of physical reality. All we can know is what we measure in the laboratory. We can take these measurements and we can operate on them mathematically. We can, in short, give certain quantitative values to these measurements. We can then operate on them according to mathematical tools which will be used now to grind out predictions of other measurements. And then, we go back to the laboratory and we find that the measurement actually works. If it does not, use another mathematical tool. It is just like sitting there with a whole box full of wrenches; if the one wrench does not fit, put it down, use another one until you find the right one. Let us put you in the dark so as to add a little mystery to it."

But this is precisely what you do. You have the entity; you have the tools, and you want to match, now, entity and tool to be able to do something with it. Notice the consequences of this system, if you unite fact and theory in the sense that the facts are what you measure, the theory here is a mathematical equation. The role of mathematics is, of course, central, for reality now is the equation itself. Ask a theoretical physicist, "What is the wave in asserting that wave equation," and he will tell you it is a mathematical fiction, but a physical reality. Because, the wave does not exist in real space, it exists in phase space, and unfortunately we do not. Here the physical reality is not out there but in the equation itself. Therefore the mathematics, now, are the highest point that one can reach.

This is an aristocratic science, for not everyone can learn the mathematics. This is a restricted science, restricted to those people who can, in fact, do the mathematics and do them well enough to use the tool as Heisenberg suggested in this kind of dialogue between experimentally measured facts and mathematical instruments. It too is a dogmatic science, for where, again, can you argue except to say, "You idiot, you are using a monkey wrench when what you really want is a crescent wrench." But once you point out that the guy has the wrong operator, or something similar, where, again, are you going to argue? The facts are there; the frequency is such and such; the mathematics are there. And if you use the tool properly you will get the right answer.

And notice the process, now; tools become the center of your whole endeavor. You want to build an instrument that will really measure frequency, not just to 3 places, or 4, let us have it for 12, and therefore a $30 million grant for building this. You also

want to have proper mathematics. Well, mathematics is too important to be left to mathematicians, so you build computers. They do not make mistakes. And you build bigger and better computers. Pretty soon, hopefully, what you have is the scientist who takes the tape and puts it into the computer. This is the dignity of scientific labor. The scientist is the intermediary between the machine that measures the entity and the computer that figures out what in hell it all means.

I would like to suggest, and I think it should by now be quite obvious, that I consider all three of these views wrong, and that, in fact, it is a kind of stew of all three that has to a certain extent created the crisis in which we exist today. From Baconianism come the papers that are published that mean nothing, or that are lost; that are published and are never sighted again; which literally clog the lines of communication.

I always use, as my favorite, a paper which was published in Nature, which I read for my sins, entitled "The Non-Random Distribution of Bull Sperm in a Test Tube." Well, who can pass by an article such as that? I sat down and read it. The conclusion was that bull sperm do not collide elastically with glass. I gather the practical, important point is, never buy a glass-lined cow. This paper, I am sure, not only was lost, but it should have been lost, and, in fact, should never have been published.

But notice, on the Baconian view, it is after all a fact. And who knows? This is the great justification; you go back--and you can always do this with history--and pull out an example. Look at Semmelweiss in Austria where he showed the germ theory to physicians and nobody paid any attention to it. Supposing they had listened to him? Think of the lives that could have been saved. And so, who knows how important this paper on bull sperm will ultimately be? But, somewhere along the line some kind of critical judgment must be made.

I would like to suggest that there is a fourth way of looking at science, which permits the exercise of this critical judgment and which removes--I would not say all--these difficulties involved, but which removes some of them. This is a view which by no means is original with me; it is the view of Karl Popper--probably many of you know of him--a British philosopher of science, who has written a number of bad books, but who has had, I think, one important idea in his life.

I can perhaps illustrate this theory best by giving you Popper's anecdote on how he was led to it. He was a student in Vienna in 1919, right after the war. There were three very exciting intellectual currents in Vienna at this period. One was Einstein's "General Theory of Relativity"; another was "Freudian Psychoanalysis"; and the third was "Revised Marxism." All three were "scientific." All three claimed to be sciences. And, Popper, obviously, in cafe conversations, investigated all three. He said he felt very uneasy because, although they seemed to be scientific, he did not really feel they were; he did not feel they were on an equal footing. But he could not tell why.

In 1919 an astronomical expedition went out to check the theory of general relativity in which Einstein had predicted that light would be bent from a straight path as it passed through an intense gravitational field. There was an eclipse of the sun; there was a star behind the sun; the star was perfectly well known. The expedition went out, therefore, to see if, as the light passed by the star, the star's image did seem to shift thus indicating that the bending had taken place, and to the exact amount that Einstein had suggested that it would, and it did. The expedition--led by Eddington--triumphantly pronounced the fact.

Popper said, "Aha! There is the difference." Einstein's neck was out this far. Eddington and the boys had the axes; had that starlight not been bent, it would have been back to the Patent Office for Einstein. The theory simply would have been blown up and shown to be false. Popper now adds, "How do you refute a psychoanalyst?" He has all the answers. You love your mother; he can tell you why. You hated your mother; he can tell you why. Any question that you want to raise, psychoanalysis can answer.

So can Marxism. Any question that you want to raise, Marxism has the answer for. You can, in fact, never refute it. You can never say, "All right, here it is; if it goes that way you are wrong." the Marxist may say, "You are right," until it goes that way, and then he will say, "Oh yes, but you are wrong. What I neglected was that this factor is now explained."

Notice that this is the Cartesian tradition. It is a rational explanation, but what it does not contain within it is the capability

of being refuted. You cannot prove it wrong. So, I would like to suggest that one can define science as the keenest tool the human mind has yet developed for the detection of error. Note the consequences of this little epigramatic definition. In the first place it implies--and this should be spelled out--that what a scientist does is first to see some kind of--I will call it a vision because I want to push this, perhaps harder than it should be. He sees a vision of physical reality.

He cannot tell you, "Well, I see this vision because I measured an electrical current yesterday." But there is a certain kind of, let us say, harmony or pattern that he sees perhaps in the whole universe, as Einstein did. You remember his famous remark when he refused to accept quantum mechanics--"I cannot believe that God plays dice with the cosmos." This violated Einstein's esthetic vision of what a proper universe should be.

But, he has a vision which is physical; he sees the reality. In this vision there should be elements that can be tested. If you will, the scientist makes up a universe; it may be even just a universe of semiconductors, but he makes up a pattern. And now he says, "All right, if my pattern is correct, then this should follow. Now we will go to the laboratory and we will test it." If it does not follow, the first thing that the great experimenter does is not to say, "My vision is wrong," but to say "The experiment is wrong. What did I do wrong in the experiment so that I did not get what I should have?" And it is only after the experimentalist has torn that experiment to pieces and isolated every element and seen that the vision is not true, that he will reject the hypothesis, if one wishes to call it this.

The priority here goes to the idea, and the experiment is used not to generate theories, but to test them.

Notice the role of mathematics in this system. It is, as the Cartesians insisted, a logically precise tool. But in this system its major function is to find the logical holes in the theory. That is, you can have mathematics, or it is nice to have mathematics, because this is the best probe you have. If you can put things into mathematical language, then you can perhaps find, by using mathematical logic which is precise, whether you have missed something here or made a wrong turning. But notice, that unlike Heisenberg, it does not insist upon mathematics. It is perfectly possible to have

a qualitative science, except one must be cautious here in one's arguing, since there are semantic traps.

I want to pause here very briefly to indicate the importance of this point. Heisenberg's philosophy is generally the one, in a very watered-down way, to which the social scientist turns. The recent history of the social sciences--I am oversimplifying here a great deal and I would no doubt be cut off at the knees by a social scientist; but I can still run fast on my knees--basically it seems to me what the social scientists have done is to say, "Aha! Look at physics, what it has done by finding quantifiable entities and using mathematics. So, what we, the social scientist, must do is to quantify what we can and use mathematics."

I would suggest that precisely those things which are most uninteresting about mankind are what you can quantify--height, weight, et cetera. And this is where I think the social scientists have gone wrong, for they have followed this definition of science. Whereas, if you take the idea of science as a view of human behavior, if you will, which can be tested, and not necessarily mathematically and quantifiably tested, then I think one opens up a much richer field to the social scientist. To be sure, he will not be able to wear his white coat anymore and use computers, but I think this is a small price to pay for some little knowledge about mankind.

In any case, here one has the escape from the quantifiable which seems to be one of the dogmas of modern science. It is a curious mixture too, of the aristocratic and the democratic. It is aristocratic in this sense; that not everyone has the creative idea; the vision; the Einstein who sees the unity of the universe; the Faraday who perhaps saw a field theory where other people were using particle theory, et cetera. There are only a few of these transcendant geniuses who can, in fact, penetrate farther than their colleagues, see physical reality in a somewhat different way, and report back to stimulate new researches in terms of a totally new interpretation. And if we have time in the question period I would like someone to ask me a question on what do I mean by interpretation. Then I will answer.

Here is the aristocratic element. Yet, you see it is also democratic in this sense; that once the vision is reported, once a Faraday says, "I do not believe an action at a distance, but there must be some medium through which forces act," then the laboratory worker can check this. He can, in fact, devise the experiments which may decide between particle physics or field physics.

It has, then, its elements of Baconianism, but it is a Baconianism under control; it is not the wild proliferation of irrelevant facts, but a constant association between fact and theory. And most importantly of all, I think, is the fact that this is the only science that is, in fact, open-ended. For here, you see, you can argue. Poisson can say, "Look, Michel," to Faraday, "I do very well with particles. What in hell do you want me to give them up for, to take in a field?" But Faraday can say, "Aha, but look what I can do with the field. I can discover things here that you cannot ever think of in particles." Poisson would shrug, as the French do, and say, "Voila, he's right. I still like my particles, but I will now take in the field."

You now have a real dialogue going on. You have, in fact, an argument which is possible in terms of interpretation. These arguments, it seems to me, are the very essence of the scientific discourse that takes place. Very rarely do you find people arguing about facts. There is nothing to argue about; go find out what it is and if your opponent is wrong and he is generous he will say, "I was wrong." But when you start to deal with such things as the Copenhagen interpretation of quantum mechanics versus, let us say, de Broglie or Bohm's interpretation, what one has here, you see, are arguments on a basic vision.

Both must be subjected to experimental attack, but both are different ways of looking at reality. And in this way, just as one can argue about a painting or a piece of music, or a piece of literature, so too in science one can argue about these interpretations. It is from this argument, from this open-endedness, that two things of extraordinary importance, I think, emerge. The first is that it does, in fact, preserve the freedom of the intellect; that if you see science as essentially a critical tool, then the whole rule of the game is not to agree but to find ways of disagreeing. The man who blows up a theory deserves a Nobel Prize as much as, if not more than, the man who creates one.

Here the stimulus is not to shut up because I am chairman, but to try and show that my theory is wrong. You will still get fired, but you will get a Nobel Prize.

Secondly, this is the human part of science. In this view--and it seems to me only in this view--do human beings exist. A fact-finder is not necessarily human--they say they are going to build

machines that can do that pretty soon--the innate ideas, after all, you owe to God; they are not your business, and you have the same ones I have; it does not make any difference whether it is Joe Schmaltz or L. P. Williams has the same idea; we are just sort of tapping the same checking account. And certainly, in Heisenberg's scheme the facts are almost explicitly mechanical.

But here you see, a man may have an idea. Why? He had an argument with his wife and it touched off certain homicidal reactions which led him to an idea which he never thought of before. And it is personal; it comes out of his experience. It must be tested in the laboratory but its origin is in an individual human being's mind. Scientists are, in fact, human beings and science is a human endeavor.

Thank you.

QUESTION: Dr. Williams, could you compare for us, both qualitatively and quantitatively, the education of scientific personnel in our country and the Soviet Union?

DR. WILLIAMS: Let me start with--I will do qualitatively, America, and then we will see how far I get. This is one of my pet hobbies and peeves. Qualitatively, with exceptions, I think in the main, science education in America stinks. And I say this strongly and vulgarly because I feel very strongly about this. It is all built around the Baconian picture of what science is.

I say there are exceptions. There are exceptions, I am sure, in Washington and the big cities, but by and large in your small high school, your small college, and even your small university, what do you learn in science? You are damned intellectual amoebae. You pour into the classroom in the 10th grade and then you are supposed to wrap yourself around increasingly large amounts of facts. This is changing. There is a new physics curriculum and a new biological curriculum in the high schools, and I think probably the high schools are going to be teaching better science than most colleges because it is difficult to teach science, particularly as it should be taught, namely as ideas, as discourse, as argument, where the facts are the ammunition and not the be-all and the end-all.

The standard response is always, "You cannot argue unless you know what you are arguing about." This is perfectly true, but you do not have to pour a person full of the handbook of chemistry and physics so that he can tell you what the boiling point of nitroquinone is, like that, and then say he is a scientist.

Only in the last 2 years of graduate school, I feel, does American science qualitatively being to enter into--again, it is purely personal--what I feel is the very guts of science, namely, the excitement of seeing it as a creative human activity in which ideas are important and in which this constant dialogue between the ideas of the individual and nature itself takes place. This is qualitatively.

Quantitatively we are in the middle of a real revolution which worries me very much, for this reason. You can now make a living as a scientist; this is a very recent phenomenon. I shall never forget when I was in high school, and during the war I was fascinated by atomic physics. My brother was in the Air Corps overseas. He came home and I had time to work up some atomic physics for myself and I showed him my notebook, et cetera. He was my big brother and I said, "I want to be an atomic physicist." My brother told me, "Well, that is very good, but you will never make a living at it." So, I became a chemical engineer and ultimately ended up a historian; I think to the ultimate advantage of atomic physics.

In any case, it is certainly within my lifetime that one could point out that the economics of science were such that you had to be a very dedicated man to go into it. Now, of course, this is all changed. The bright students go on in science because there is money; lots of fellowships; there is always a job. And, we are creating, I think, a whole new class of people; I will call them the scientific technicians. They are men with Ph. D. s; they are men with published papers. But they are men who are basically really not committed to science--and this may sound corny but, again, I feel very strongly about this--they do not see science as a calling; as a way of life.

What do you do? They are bright guys. They are almost always and inevitably, Baconians because you can do science in a Baconian fashion without much trouble. The right guy can, in fact, publish three papers, four papers, five papers a year, on what he is doing in the laboratory. He makes his living this way.

You have scientific administrators who, again, want to quantify. How do you know, when you are running a laboratory with 175 men, who the best ones are? Well, one easy way is to count papers, and so you quantify your personnel. You see that Joe has published 22 papers and Albert has only published 1. So, Joe is 22 times better than Albert; it follows logically.

The result of this, of course, is this famous pressure to publish, which, again, is clogging the lines. Here again, quantitatively we are losing sight of the fact that science is a creative activity. I do not know what to suggest. I have only criticism and no answers.

As for the Soviet Union, if you read the papers you know as much as I do about it. My impression of the Soviet Union, because I am not a Sovietologist, is simply this; there has been a long tradition of science in the Soviet Union; a European tradition; it is not, I think, peculiarly Russian. Children are exposed to science. I do not know how it is stressed; I would suspect it is very much the same way as it is taught here; learn the facts, memorize the textbooks; give facts to the teacher. You do experiments; that is the crucial point.

I am impressed with one small fact that I dug up in a totally different way. I just finished writing a biography of Michael Faraday and in this I tracked down translations of Faraday's work. I was intrigued to find that in the equivalent of the fifth grade in Russia, Russian schoolboys read the English version of Faraday's chemical history of the candle. This is terribly clever of the Russians. It gives them English and it is also a great classic in science writing. And it is an exciting book.

This shows a little imagination, which worries me, because this is an unusual commodity. In any case, I cannot comment more than this except to say that certainly in theoretical physics, which is all I know, the Soviet Union is doing very well. Certainly, I think it is equivalent to what we have. There are areas that, for obvious reasons, are in poorer condition; genetics is a good one. If you are a bright young student and want to go into genetics you should have your head examined. Where there are political implications it seems to suffer.

QUESTION: Earlier you referred to the meaning of interpretation. Would you like to expand on that?

DR. WILLIAMS: I would, very briefly, because I do not have much time. I think this is an important point. Let me give you an historical example. At the end of the 18th Century, the great achievement of 18th Century physics was to reduce the universe to particles in space acting at a distance upon one another. Why? When I say reduce it, what I mean is this. Ponderable matter was composed of atoms acting upon one another by gravitational force. Electricity was supposed to consist of two electrical fluids--positive and negative--in which the same fluids repelled one another; opposite fluids attracted one another. But notice still, particles repelling and attracting one another.

Magnetism was considered to be two magnetic fluids doing the same thing as the electrical fluids, the difference being that whereas electricity could flow from bodies, magnetism seemed to be somehow inside the molecules and could not get out. Light was also considered to be particulate; refraction, for example, could be understood as the drawing in of the light particles, reflection as their repulsion.

Heat was considered to be composed of particles of caloric which repelled one another. There is the universe; particles in empty space; attracting and repelling one another.

There was a school of thought in Germany using the same facts, that argued this way; ultimate reality is not particles--not billiard balls--but the two forces of attraction and repulsion. These forces manifest themselves in certain ways--you see, what I feel from this stage--I do not feel atoms or electrons as they whirl around us but something pushing me up--I feel a repulsive force. And if I jump up I will feel an attractive force. So that, in German philosophy--this ultimately goes back to Kant--it was suggested that reality was the conflicting forces and that matter was one manifestation; light was another; electricity still a third; magnetism a fourth, et cetera.

Here you see you have the same facts, but notice the difference in interpretation. On the one hand, with particles you would never look for electromagnetism; you would never look for the production of magnetism by electricity. You cannot transmute electrical particles into magnetic particles. This is one of the astonishing things in the history of science; that 20 years after the discovery of current electricity the orthodox physicist never looked for electromagnetism. Hans Christian Oersted was a believer in the German

philosophical tradition and he figured if all these forces are merely manifestations of some kind of basic force, then they ought to be transmutable; all you had to do was find the right conditions and then turn electricity into magnetism.

Here was a different interpretation of precisely the same experimental facts; there was no factual difference. But, because one person had a different view of what these facts really meant, he looked for something that the other group never looked for. Michael Faraday spent his entire career by saying he wanted to complete the circle. His whole experimental life was spent trying to turn one force of nature into another. He succeeded. His only failure was that he could not turn gravity into electricity or vice versa. That was the thing that motivated him.

This is what I mean by the personal vision of what reality is. This vision of forces is opposed to the particulate vision of the orthodox physicist. These are the interpretations which end up, I think, as being extremely fruitful in the history of science.

QUESTION: Dr. Williams, we read that many people in the scientific community are concerned with the amount of money the Federal Government is cranking into science--certainly to make a living that way--and this is probably dangerous to science. Would you comment on that aspect of it, and particularly what can the Government do to increase the output of science without killing science?

DR. WILLIAMS: Let me answer the second one first; the Williams plan which I give unsolicited to anybody. Let us leave out the laboratories and talk about the universities. The Williams plan is, whenever the Government gives a grant to the agencies or a specific research project at a university, it gives the university a matching grant with no strings attached. I say this because, increasingly, and people fight against it, but you cannot help it--increasingly, the direction of science is being changed by the grants. There is a new order, and there is going to be a new industry, I am sure, of people who will make out grant applications. Because, an awful lot depends on how you make it out. But this does not necessarily mean that the best scientists are getting the grants.

A man will say, "The AEC lets me do what I want." But he also knows what the AEC wants, and this is going to influence his own

choice of projects. I am speaking in terms of unlimited money--if we are going to be generous, I would think that the universities should get matching funds which can then be made available for the guy who cannot write his grant application well or who does not want to do, perhaps, what the fad and fashion are today. He may be a guy who wants to do something with spectroscopy--and make the money available to him.

There is the feeling that if you do not spend the money you are not going to get it next year. I think this works to the ultimate detriment of science, like supporting some pretty crazy schemes that do not deserve it. I do not want to turn the faucet off entirely. I would like to see a much more rigorous view taken than is now the case.

CAPTAIN PHILLIPS: Dr. Williams, our time has about run out. I did not tell you that yesterday you had quite a buildup from Dr. Hanson. All through his message he kept saying. "I won't tell you about this because my friend, Pearce Williams, will tell you that tomorrow." He said that about five times. The last time he said, "You know, he is really good." And all I can say is, he is very, very low on the field of his adjectives. We think you were superb. Thank you very much.

(July 9, 1965--5, 900) ca:en:ekh

☆ U.S. GOVERNMENT PRINTING OFFICE : 1965 O—785-125