friends. How could we as mathematicians prove to a skeptical outsider that our theorems have meaning in the world outside our own fraternity?

If such a person accepts our discipline, and goes through two or three years of graduate study in mathematics, he absorbs our way of thinking, and is no longer the critical outsider he once was. In the same way, a critic of Scientology who underwent several years of "study" under "recognized authorities" in Scientology might well emerge a believer instead of a critic.

If the student is unable to absorb our way of thinking, we flunk him out, of course. If he gets through our obstacle course and then decides that our arguments are unclear or incorrect, we dismiss him as a crank, crackpot, or misfit.

Of course, none of this proves that we are not correct in our self-perception that we have a reliable method for discovering objective truths. But we must pause to realize that, outside our coterie, much of what we do is incomprehensible. There is no way we could convince a self-confident skeptic that the things we are talking about make sense, let alone "exist."

A Physicist Looks at Mathematics

HOW DO PHYSICISTS view mathematics? Instead of answering this question by summarizing the writings of many physicists, we interviewed one physicist whose scientific feelings were judged to be representative. Since the summary which follows cannot represent his full and precise views, his name has been changed.

Professor William F. Taylor is an international authority in Engineering Science. He is actively engaged in teaching and research, and maintains extensive scientific connec-

tions. In August, 1977, the writer interviewed Professor Taylor in Wilmington, Vermont where he and his wife were on vacation enjoying tennis and the Marlboro Concerts. In the interview, an attempt was made not to confront the interviewee with opposing views and not to engage in argumentation.

Professor Taylor says that his professional field lies at the intersection of physics, chemistry, and materials science. He does not care to describe this combination by a single word. Although he uses mathematics extensively, he says he is definitely not an applied mathematician. He thinks, though, that many of his views would be held by applied mathematicians.

Taylor makes frequent computations. When asked whether he thought of himself as a creator or a consumer of mathematics, he answered that he was a consumer. He added that most of the mathematics he uses is of a nineteenth century variety. With respect to contemporary mathematical research he says that he feels drawn to it intellectually. It appears to unify a wide variety of complex structures. He is not, however, sufficiently motivated to learn any of it because he feels it has little applicability to his work. He thinks that much of the recently developed mathematics has gone beyond what is useful.

He seemed to be aware of the broad outline of the newly developed "nonstandard" analysis. He said,

> That subject looks very interesting to me, and I wish I could take out the time to master it. There are numerous places in my field where one is confronted with things that are going on simultaneously at totally different size scales. They are very difficult to deal with by conventional methods. Perhaps nonstandard analysis with its infinitesimals might provide a handle for this sort of thing.

Taylor asserts that only seldom in his professional work does he think along philosophic lines. He has done a small amount of reading in the philosophy of science and the philosophy of physics, principally in the area of quantum physics. He finds questions as to how and to what extent processes are affected by the mode of observation particularly interesting. He says that such questions have affected his professional work and outlook somewhat although he has not written anything of a formal nature about it.

Although his personal familiarity with the philosophy of science may be said to be slight, he believes it to be an important line of inquiry, and he welcomed the present interview and framed his answers thoughtfully and with gusto.

Taylor is unaware of the main classical issues of mathematical philosophy. In response to the question of whether there were or had been any crises in mathematics, he answered that he had heard of Russell's Paradox, but it seemed to be quite remote from anything he was interested in. "It was nothing I should worry about," he said.

Taylor's approach to science, to mathematics, and to a variety of related philosophic issues can be summed up by saying that he is a strong and eloquent spokesman for the model theory or approach. This holds that physical theories are provisional models of reality. He uses the word "model" frequently and brings around his arguments to this approach. Mathematics itself is a model, he says. Questions as to the truth or the indubitability of mathematics are not important to him because all scientific work of every kind is of a provisional nature. The question should be not how true it is but how good it is. In the interview, he elaborated at length on what he meant by "good" and this was done from the vantage point of models.

As part of his elaboration, he answered along the following lines. There are many situations in physics that are very messy. They may contain too many mutually interacting phenomena of equal degrees of importance. In such a situation there is no hope whatever of setting up something which can be asserted to be the "real thing." The best one can hope for is a model which is a partial truth. It is a tentative thing and one hopes the best for it. All physical theories are models. A model should be able at the very least to describe certain phenomena fairly accurately. Even at this level one runs into trouble in constructing models. The models that one constructs are of course dependent upon one's state of knowledge. Ideally, a model should have predictive value. Therefore it is no good to construct

a model which is too complex to support reason. Whether it is or it is not too complex may depend upon the current state of the mathematical or computational art. But one has to be in a position to derive mathematical and hence physical consequences from the model, and if this is found to be impossible—and it may be so for a variety of reasons—then the model has little significance.

Professor Taylor was asked to comment on the contemporary view that the scientific method can be summed up by the sequence: induction, deduction, verification, iterated as often as necessary. He replied that he went along with it in its broad outlines. But he wanted to elaborate.

> Induction is related to my awareness of the observations of others and of existing theories. Deduction is related to the construction of a model and of physical conclusions drawn from it by means of mathematical derivations. Verification is related to predictions of phenomena not yet observed and to the hope that the experimentalist will look for new phenomena.
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> The experimentalist and the theoretician need one another. The experimentalist needs a model to help him lay out his experiments. Otherwise he doesn't know where to look. He would be working in the dark. The theoretician needs the experimentalist to tell him what is going on in the real world. Otherwise his theorizing would be empty. There must be adequate communication between the two and, in fact, I think there is.

When asked why the profession splits into two types—experimentalists and theoreticians—he said that apart from a general tendency to specialize, it was probably a matter of temperament. "But the gap is always bridged—usually by the theoretician."

Professor Taylor was asked how he felt about the often quoted remark of a certain theoretician that he would rather his theories be beautiful than be right.

> This cuts close to the bone. It really does. But as I see it, mere aesthetics doesn't pay dividends. In my experience, I should be inclined to replace the word "beautiful" by the word "analyzable." I should like my models to be beautiful,

> effective, and predictive. But the real goal is the understanding of a situation. Therefore the models must be analyzable because understanding can come only through analyzability. If one has all of these things, then this is a great and rare achievement, but I should say that my immediate goal is analyzability.

What were his views on mathematical proof? Professor Taylor said that his papers rarely contain formal proofs of a sort that would satisfy a mathematician. To him, proofs were relatively uninteresting and they were largely unnecessary in his personal work. Yet, he felt that his work contained elements that could be described as mathematical reasoning or deduction. Truth in mathematics, he said, is reasoning that leads to correct physical relationships. Empirical demonstrations are possible. True reasoning should be capable of being put into the format of a mathematical proof. It is nice to have this done ultimately. Proof is for cosmetic purposes and also to reduce somewhat the edge of insecurity on which one always lives. However, for him to engage in mathematical proof would seriously take him away from his main interests and methodology.

In view of Professor Taylor's familiarity with computational procedures, he was asked to comment on the current opinion that the object of numerical science or numerical physics is to replace experimentation. He thought a while and then replied,

> I think one has to distinguish here between the requirements of technology and those of pure science. To the former, I would reply a limited "yes"; to the latter "no." Consider a problem in technology. One has a pressure vessel which is subject to many many cycles of heating and cooling. How many cycles can it stand? Now, if one really knew the process that leads to failure (which is not yet the case) one could say that in a specific instance it might be much more effective to make a computer experiment than an actual experiment. Here one is dealing with something like a "production" situation.
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> On the other hand, in pure science, the elimination of experimentation is a contradiction in terms. The way one finds out what is going on in the universe is through experimentation. This is where new experiences, new facts come from.
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> There is no point to run experiments on bodies falling in a vacuum. Newtonian mechanics is known to be an adequate model. But if one goes, say, to cosmology, where it isn't known whether existing models are adequate or are not adequate, then numerical computation is insufficient.

Asked whether it would be possible to imagine a kind of theoretical physics without mathematics, Professor Taylor answered that it would not be possible.

Asked the same question for technology, he answered again that it would not be possible.

He added that the mathematics of technology was perhaps more elementary and more completely studied than that of modern physics, but it was mathematics, nonetheless. The role of mathematics in physics or in technology is that of a powerful reasoning tool in complex situations.

He was then asked why mathematics was so effective in physics and technology. The interviewer underlined that the word "effective" was one used by Professor Eugene Wigner in a famous article, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences." "This has to do," he answered,

> with our current convention or system of beliefs as to what constitutes understanding. In these fields we mean by 'understanding' precisely those things which are explainable or predictable by mathematics. You may think this is going around in circles, and so it may be. The question of course is fundamentally unanswerable, and this is the way I care to frame my answer. Understanding means understanding through mathematics.

"Do you rule out other types of understanding?"

> There is what might be called humanistic or cross-cultural understanding. I have been reading Jacques Barzun and Theodore Roszak recently. What is the great concern with numbers and decimal points, they seem to be asking. One sees it in the old poem of Walt Whitman called "The Astronomer." Whitman had heard a lecture in astronomy in Cooper Union Hall. After the lecture he went outside,

looked up at the heavens, and felt a certain release at being freed from theories and symbols. He felt the exhilaration of being confronted by naked experience, if you will.

Now this may be a valid point of view, but it leads to a different end result. Quantitative science—that is, science with mathematics—has proved effective in altering and controlling nature. The majority of society backs it up for this reason. At the present moment, they want nature altered and controlled—to the extent, of course that we can do it and the results are felicitous. The humanist point of view is a minority point of view. But it is influential—one sees this among young people. It seems to have a defensive nature to it, a chip on its shoulder, but because it is a minority point of view, it poses only a minor threat to quantitative science.

"With regard to the conflict of the 'Two Worlds,' which of the two, the scientist and the humanist, knows more about the other man's business?"

The scientist very definitely knows more about the humanities than the other way around. The scientist—well, many that I know anyway—are forever reading novels, essays, criticism, etc., go to concerts, theatres, to art shows. The humanists very seldom read anything about science other than what they find in the newspaper. Part of the reason for this lies in the fact that the locus of the humanities is to be found in sound, vision, and common language. The language of science with its substantial sublanguage of mathematics poses a formidable barrier to the humanist.

The goals of society may change, of course. If they do, then the goals of quantitative science may be weakened. Science and mathematics might be pursued only by a small but interested minority. It might not be possible to make a living at it. We saw a very slight indication of this in the late sixties and early seventies.

"Can there be knowledge without words, without symbols?"

Knowledge, as I understand it in the technical sense, implies that it can be expressed in symbols. Moving towards humanistic questions, one might say that a skillful writer evokes a mood by his use of words. Or when a Mozart score

is played, it evokes a kind of conscious state. The symbolic words and the music are a model for the state.

"Does a cat have knowledge?"

"A cat knows certain things. But this is knowledge of a different kind. We are not dealing here with theoretical knowledge."

"When a flower brings forth a blossom with six-fold symmetry, is it doing mathematics?"

"It is not."

"Would you care to comment on the old Greek saying that God is a Mathematician?"

"This conveys nothing to me. It is not a useful concept."

"What is scientific or mathematical intuition?"

"Intuition is an expression of experience. Stored experience. There is an inequality in people with respect to it. Some people gain intuition more rapidly than others."

"To what extent can one be deceived by intuition?"

"This occurs not infrequently. It is a large part of my own work. I say to myself, this model seems to be sufficient, but it just doesn't sit right. Or, I ask myself, is my model a better one than their model? And I probably have to answer on the basis of intuition."

The final question put to Taylor was whether he is a mathematical Platonist in the sense that he believes that mathematical concepts exist in the world apart from the people that do mathematics. He replied that he was, but in a limited sense. Certainly not in a "theological" sense. He believed that certain concepts turn out to be so far superior to others that it is only a matter of time before these concepts prevail and are universally adopted. This is something like a Darwinian process, a survival of the fittest ideas, models, constructs. The evolution of mathematics and theoretical physics is something like the evolution of biosystems.