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APPLIED MATHEMATICS: WHAT IS NEEDED IN RESEARCH AND EDUCATION*

A Symposium

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H. J. GREENBERG[5] (Chairman)

Dr. H. J. Greenberg. Today we will discuss the subject of Applied Mathematics and what is needed in Research and Education. It is evident that a revolution is taking place in education and teaching. However, even if applied mathematics as taught and practiced today were adequate for present needs, a major overhaul would very likely be required to meet the needs of the next decade. But even today it appears to many that applied mathematics is something of a stepchild; I might even say an out-of-step child, whose creations are looked upon with equal disinterest by mathematicians, physicists and engineers. The state of applied mathematics, good or bad, in the United States is surely the result of historical accident, more than deliberate design. Hopefully, by taking cognizance of the situation as it is and the needs as they exist, something of a design for the future of applied mathematics may take shape. This morning we have brought together an eminent panel of four distinguished scientists and educators whose combined interests and experience cover much of the spectrum of applied mathematics. We have asked them to state their convictions on these matters in the frankest possible terms, since candor is called for certainly in the critical times in which we live. What they have to say, I'm sure, will be considered carefully by a wider audience than the one presently before us. The program this morning will begin with statements of approximately 20 minutes each by each of the four speakers. After that we will have a round table discussion. Although members of the audience will not be able to join in these discussions on the platform, I wish to invite questions from the floor for possible inclusion at the close of the discussion period. I can't guarantee that we will take up all questions, but certainly we will try to include those questions, as time permits, which raise points that would otherwise have been missed in the discussion.

I would now like to introduce our first speaker who will be Professor Richard Courant of New York University. Professor Courant brings to this platform the accumulated wisdom of several generations of the world's great mathemati-

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cal thinkers, among whom he is numbered. His experience spans the traditions of two great mathematical institutes, the one at Göttingen, and the one which is to permanently bear his name at New York University. It is a great pleasure to introduce Professor Courant.

PROFESSOR R. COURANT. Many of you have seen in the last issue of the American Mathematical Monthly [October, 1961] an article which has particular relevance for our present panel discussion. The article [by Professor Marshall Stone] discusses the "Revolution in Mathematics"; it asserts that we live in an era of great mathematical successes, which outdistance everything achieved from antiquity until now. The triumph of "modern mathematics" is credited to one fundamental principle: abstraction and conscious detachment of mathematics from physical and other substance. Thus, the mathematical mind, freed from ballast, may soar to heights from which reality on the ground can be perfectly observed and mastered.

I do not want to distort or belittle the statements and the pedagogical conclusions of the distinguished author. But as a sweeping claim, as an attempt to lay down a line for research and before all for education, the article seems a danger signal, and certainly in need of supplementation. The danger of enthusiastic abstractionism is compounded by the fact that this fashion does not at all advocate nonsense, but merely promotes a half truth. One-sided half truths must not be allowed to sweep aside the vital aspects of the balanced whole truth.

Certainly mathematical thought operates by abstraction; mathematical ideas are in need of abstract progressive refinement, axiomatization, crystallization. It is true indeed that important simplification becomes possible when a higher plateau of structural insight is reached. Certainly it is true—and has been clearly emphasized for a long time—that basic difficulties in mathematics disappear if one gives up the metaphysical prejudice of mathematical concepts as descriptions of a somehow substantive reality.

Yet, the life blood of our science rises through its roots; these roots reach down in endless ramification deep into what might be called reality, whether this "reality" is mechanics, physics, biological form, economic behavior, geodesy, or, for that matter, other mathematical substance already in the realm of the familiar. Abstraction and generalization is not more vital for mathematics than individuality of phenomena and, before all, not more than inductive intuition. Only the interplay between these forces and their synthesis can keep mathematics alive and prevent its drying out into a dead skeleton. We must fight against attempts to push the development one-sidedly towards the one pole of the life-spending antinomy.

We must not accept the old blasphemous nonsense that the ultimate justification of mathematical science is "the glory of the human mind". Mathematics must not be allowed to split and to diverge towards a "pure" and an "applied" variety. It must remain, and be strengthened as, a unified vital strand in the broad stream of science and must be prevented from becoming a little side brook that might disappear in the sand.

The divergent tendencies are immanent in mathematics and yet prove an ever-present danger. Fanatics of isolationist abstractionism are dangerous indeed. But so are conservative reactionaries who do not discriminate between hollow pretense and dedicated aspirations.

Perhaps the most serious threat of one-sidedness is to education. Inspired teaching by broadly informed competent teachers is more than ever an overwhelming need for our society. True, curricula are important; but the cry for reform must not be allowed to cover the erosion of substance, the propaganda for uninspiring abstraction, the isolation of mathematics, the abandonment of the ideals of the Socratic method for the methods of catechetic dogmatism.

Fortunately, it seems that some of the extreme suggestions related to mathematics in schools are being toned down. At any rate, it would be without doubt a radical and vitally needed remedy for many ills in our schools and colleges if a close interconnection between mathematics, mechanics, physics and other sciences would be recognized as a mandatory principle which must be vigorously embraced by the coming generation of teachers. To help such a reform is a solemn obligation of every scientist.

DR. GREENBERG. Our second speaker will be George Carrier, Professor of Mechanical Engineering at Harvard University. Professor Carrier is one of an extremely rare breed. A mathematician who knows engineering and physical science and who, with exceptional skill, formulates and solves problems of interest to engineers, scientists, and mathematicians. With pleasure I introduce Professor Carrier.

PROFESSOR GEORGE F. CARRIER. I have been told that Joe Keller once initiated some remarks concerning Applied Mathematics with the definition, "Applied mathematics is that science of which pure mathematics is a branch." As a statement, I can agree heartily with this, but as a definition I feel it is far too conservative. It seems to me that applied mathematics is that science of which not only is pure mathematics a branch, but of which all other sciences become branches as soon as they become sufficiently quantitative. The discussion of this broad a field, of course, would contain nothing but puerile generalities, so I shall confine my remarks to some *educational* questions associated with what I regard as the *core* of applied mathematics. I confine myself to educational questions because I feel that when highly qualified men are well educated and prepared for a challenging profession, they will perceive clearly the exciting and worthwhile research needs and will take care of them. I confine myself to the *core* of applied mathematics because I feel that, in that area, virtually no undergraduate curricula are to be found in the United States.

Perhaps I should first define what I mean by the core of applied mathematics; I shall do so by describing the objectives, abilities and educational needs of the men who populate this core. Their objective is to understand scientific phenomena quantitatively. To do so, such men must be so thoroughly informed in the fundamentals of some broad segment of the sciences (be they physical, biological, economic, or whatnot) that they can pose the question or family of questions they pursue as a mathematical query using, as the occasion demands, either time-honored and well established scientific laws (as in mechanics) or carefully conjectured models (as in younger sciences). Such an applied mathematician must also have an understanding of mathematics, a knowledge of technique, and such skill that he can use either rigorously founded techniques or heuristically motivated methods to resolve the mathematical problem, and he must do so with a full realization as to the implications of each with regard to reliability and interpretation of results. In particular, the applied mathematician must be very skillful at finding that question (or family of questions) such that the answer will fill the scientific need while the extraction of the answer and its interpretation are not prohibitively expensive. I must emphasize that such an individual is not a mathematician nor, ordinarily, is he a specialist in a particular branch of science; he is distinguished from these primarily by his attitude and his objectives, but also by the scope of the scientific and mathematical disciplines on which he must draw.

As I said earlier, there are very few undergraduate programs which expose the student early and consistently to these objectives and attitudes. What should such a program be? I firmly believe that this exposure, this program, should begin at the high school level! I think, in particular, that the mathematics curriculum would be enriched and that the student would be better motivated and his perspective improved if, when trigonometry and analytic geometry are taught in the high schools as they should be, the science of statics were presented—so coordinated with the mathematical instruction that the student perceives the need for and utility of mathematical study. Such an exposure is really necessary to balance the new secondary school emphasis on the beauties of precise mathematical reasoning. I think, in fact, that much of the bitter controversy which has arisen over secondary school mathematics might well be resolved if the student encountered not only precise mathematical thought but also well motivated scientific utilization of that mathematical thought and the techniques it breeds. Furthermore, at this level, such an exposure should include all students who study such mathematics, whatever career they intend to pursue. I find it incredible that a supposedly well educated man should find it unnecessary in our age to understand at least this much of his environment.

This coordinated view of the sciences and mathematics should continue with the college curriculum, necessarily for novices in applied mathematics and, I would hope, for many others as well. When calculus is introduced the student should study dynamics and those rudimentary aspects of other sciences for whose quantitative understanding the calculus suffices. The coordination should be such that in some instances the mathematical topic is suggested by the scientific question and in others the mathematical development precedes its exploitation. In either case, the topic once introduced must be taught as mathematics, remembering that the ideas are far more important than the epsilons!

A more broadly informed person than myself could pursue a description of courses which gives in chronological order the mathematical material and the coordinated scientific material from which an applied mathematics student could profit, but I think that the idea should be clear now without such an outline. In a nutshell, the student must see a consistent pursuit of the understanding which the interplay of mathematics and science can give, and he should find it distinguishable from, for example, the mathematician's pursuit of the full implications of fundamental postulates in which he needs and expects no interplay with the physical universe. Or again, he should distinguish applied mathematics from the physicist's pursuit of hitherto unknown fundamental laws and their verification and immediate implications. I draw these lines much too sharply, of course, but I do think such a description clearly distinguishes the main objectives of these various aspects of quantitative science.

Perhaps I can emphasize the foregoing further by airing a particular complaint which I have entertained for several years. It is concerned with the education of what I shall call digitally oriented scientists. I must preface the complaint with certain remarks. No responsible person doubts the enormous present and future values of the computational sciences and their attendant machinery. When wisely used, we can extract information which was not remotely within reach before the rapid evolution of this discipline. Nevertheless, this discipline, like any other when unwisely used, can be a mixed blessing indeed. I claim that, when treating a quantitative problem, the man who uses a rigorously justified technique does not need to know nearly as much as one who uses a heuristically motivated method. The more heuristic and the less precise the foundation of the method, the more critical and the less routinely formal must be the procedure. The extreme of this in my experience is the use of approximations involving computational science and machinery. The very youth of the science, the difficulty of identifying the consequences of badly drawn approximations, and the inability of the device to see and describe unexpected singularities of almost any kind imply that the responsible digitally oriented applied mathematician must understand extremely thoroughly the mathematics and the scientific fundamentals whose intricacies his digital science is intended to obviate. Computation is not and cannot be a substitute for thought or a replacement for understanding.* Despite this, I have seen several educational channels which eject digitally oriented degree holders whose curriculum has not led them to that understanding of intermediate level analysis which enables them, for example, to solve rather simple Laplace equation problems without computational science, or to detect that certain badly and obviously overspecified problems have no solution. I have even been offered digital solutions of what were stated to be appropriate approximations to the latter. I don't claim that such events are typical! I hope and I think they are not. However, they do exist and my plea to those of you who hold responsibility for digitally oriented education is that you insist on a broad and deep program in those sciences without which digital science could become a sterile and isolated discipline appropriate only to routine operations instead of the powerful adjunct to all of science that it can be.

To summarize then, I think a major deficiency in our educational structure is

* In view of our Moderator's professional affiliation, I am tempted to suggest that the much ridiculed sign often seen in IBM establishments may not have been, in the right context, as inappropriate as we frequently thought.

the very small number of undergraduate educational opportunities wherein the student consistently finds instruction which emphasizes and is motivated by the close interplay of science and mathematics on a broad scale. I hope that such programs will find their way into many of our schools.

DR. GREENBERG. Thank you very much. When I suggested to George that he was going to talk for 20 minutes, knowing the tradition that when he gives an hours talk it lasts about half an hour, I was afraid he would be done in three minutes. But I guess he feels very deeply about this subject because he practically filled up the full twenty minutes.

Our next speaker will be Professor Paul Rosenbloom of the University of Minnesota. Professor Rosenbloom is recognized for his penetrating and scholarly researches in mathematical analysis. Yet, quite simply the outstanding thing about Professor Rosenbloom, to many of us, is that he is a great teacher. His devotion to the task of teaching mathematics—and I mean really teaching and real mathematics—has carried him from the university classroom to the public school system of the State of Minnesota, where he is, in addition to his other posts, Director of the Mathematics Section of the Minnesota National Laboratory in the State Department of Education. Professor Rosenbloom has also contributed to the writing of new curricula for secondary schools under the School Mathematics Study Group Program. We are very anxious to hear Professor Rosenbloom's remarks.

PROFESSOR PAUL ROSENBLOOM. I can best illustrate what I think is needed, what the basic problems are in education for applied mathematics by looking at some aspects of theoretical physics. First, what mathematics does one need to know to understand Schrödinger's equation and related physics? Now I think that among other things one is going to need to know about Hilbert space. One certainly needs to know about probability theory. One needs to know a good deal about partial differential equations. One has to know some matrix theory and also a good deal more about group representation for the theory of elementary particles. Now, in our present educational system students don't usually get this far until one or two years of graduate school. The physicist by this time has a very heavy load in physics, and doesn't usually have the time for that much mathematics. So what one finds usually are courses and textbooks which are either cookbooks or compromises. Things like Schiff's book give the student lots of techniques for solving specific problems but no understanding of why these methods work. Then we have compromises by Kemble and March, for example, which introduce some of the underlying mathematics into the education of the physicist. The education of the physicist is in very great danger right now of going in the direction of medical education. We now have a medical education situation where a person isn't ready for serious professional work until the age of 35 and in some cases until the age of 40, which is actually far beyond the optimum age for creativity. I refer you, for example, to the footnote in Kemble's book in which he lists the ages at which most important contributors to quantum mechanics did their first important research, or G. H. Hardy's "A Mathematician's Apology", where he refers to mathematics as being a young man's game. We are approaching a situation where the education of a professional physicist isn't finished until the age of 35 or 40. The same thing is happening in many branches of engineering. See, for example, what's going on in aerodynamics. Look at what's going on in economics, in psychology and even in mathematics. The same thing is going to be happening in all these fields. We're in very great danger that if we leave the educational system as it is that we will dry up the production of new knowledge just because of the time that it takes for a person to get to the frontiers of research.

Now, with respect to applied mathematics there's also the problem of whether one can define any fixed body of basic knowledge which people in applied mathematics should know and which would be adequate for them. If you look at the finished product of mathematics, you find that any attempt to do this is both too much and too little. I can illustrate this best with a book like Morse and Feshbach which is too fat for any human being to read, and at the same time I don't think it has anywhere near enough for the professional training of a physicist or anyone who applies mathematics to physics because there is nothing in there about how you would actually set up a problem. How would you apply any of those techniques? In another direction the answer is shown by the recent work of Wightman in quantum electrodynamics where there are heavy applications of modern theory of analytic functions of several complex variables. If we try to define the prerequisites for working applied mathematics too specifically in terms of what we now know is applicable, what it may mean very well is that we cut off people from being able to jump into new fields and off-beat ideas like Wightman's. The second question is, even if one could define certain courses for the curriculum as Stone, in this article to which you [Professor Courant] have referred, tries to do, (he tries to say here is the knowledge that you have to have in order to achieve this or that in mathematics or to be a cultured mathematician or whatever it may be) would that do the job for the applied mathematician? I refer you to one of Dirac's papers on quantum electrodynamics where he says that he finds that usual business of taking a physical situation and trying to create a mathematical model to fit it is too much of a straitjacket on his imagination. He finds it much more fruitful to create a mathematical model and then look around for a physical interpretation. And one striking example of this way of doing things is, of course, exactly what Yukawa does in his first paper on the meson. Let's imagine a particle whose behavior is described by such and such a differential equation. Suppose we say—"This is the charge—this is the mass, etc., and this is the current. Now let's see how this particle would behave." Then somebody else goes on and discovers it in the laboratory.

To give another illustration of the same sort of thing, I recall the reaction of Fran Freedman, a theoretical physicist connected with PSSC, to the 7th grade SMSG course where I had taken great pains to put in, for example, applications of mathematics to physical science in the form of an elementary unit on the lever, and to put in some illustrations of mathematics as applied to social sciences in a chapter on statistics. He said the most practical thing in this book is the chapter on the finite mathematical systems because this shows students how to create new models.

I think there is a danger if we try to find what is needed in terms of specific courses or in terms of specific knowledge of what mathematics is as it has been created already. The basic problem is that applied mathematics is an art in which mathematics is only a part. We have a situation in the real world from which you have to create a mathematical model by idealization and simplification. The real world is too complicated for our poor feeble human minds to grasp as it really is and so we are forced to simplify and idealize. We then study the mathematical model using all the power and technique of mathematics on that, often using our intuition from the interpretation that we had in mind. Then the test of our model is whether, when you interpret it back in reality, it works. And the middle part, the study of this mathematical model, which is the game of the mathematician, is only a part of the whole process of applied mathematics. As long as we teach applied mathematics or mathematics just as a finished product, then we are not giving the students any idea of how you face a real situation, what you do to it mathematically, and how you test whether your mathematics really does the job. There is another point here in the treatment of the mathematical model. Often one makes bold approximations and drastic hypotheses. Let me give you an illustration, in the theory of the skin effect. The problem that you actually solve is the problem of a plane wave hitting a conductor which fills up half of space, and then you apply it to a real conductor consisting of a little wire and any kind of wave but a plane wave. What we are doing is picking out a special case which we can solve, for which we can get an answer, and which is typical enough so that you can apply it to a real situation which is much too hard to be handled. Another point which has to be made is, very often, several quite different models will fit the data within any reasonable experimental error. For example, from what we know about the solutions of equations with small parameters we could always goose up Maxwell's equations either linearly or non-linearly, so that any number of other theories will fit the real world as well as Maxwell's equations. This is what Maxwell did in the first place. The term involving D in Maxwell's equations, he got by symmetry. This was a term which, as far as the experimental data went at that time, was so small that it would not have made any difference whether he put it in or left it out. But he said that it made the equations prettier to have a term like this in. They're more symmetrical.

Take another example. In the general theory of relativity there are only one or two particular problems that are simple enough that we have actually carried through the solution far enough to make predictions and to compare with observations.

These are very simplified situations which are terribly symmetrical; one point particle with no other masses at all, and so forth.

Practically all physicists are pretty sure that this is not exactly the right theory, but that this must be essentially the right way of looking at things. The reason that they believe this is, not because of the agreement of the theory with observation, because, as I have pointed out, there is very inadequate evidence, situations where we have solutions that one can compare with observations. The real criterion there is that it is aesthetic, elegant; it is simple; it is economical; it explains many different things and unifies many different things. Even if this is not exactly the right picture, this is the most satisfying way of putting it at the moment. If we leave out aesthetic elements, if we teach applied mathematics strictly from the utilitarian point of view, we're going to miss one main way in which we judge things in applied mathematics.

Now, let's go back for a moment to this business of studying applications of the mathematical model to the real world. The difficulty can be illustrated in two concrete ways. For example, many of us know the difficulty in teaching differential equations. The students have no trouble learning how to solve the differential equations of the types that we discuss. The real difficulty is always in taking a physical problem, setting it up with differential equations for it, and then interpreting the solution after they have gotten it.

Another example is the difficulties we have around the country in the universities in the place of statistics in relation to the mathematics department. In most mathematics departments statistics is thought of as a branch of the mathematical theory of probability, and so a person that does the kind of thing that Doob does is considered a statistician. Doob is a great mathematician; I'm not denying that. But what I'm saying is that in many mathematics departments this kind of thing or the kind of thing that one finds in Feller's book is thought of as statistics. It's very hard in many mathematics departments to get the people who run them to understand that there are very serious problems in devising models. There are very serious problems in testing the model to find out whether it really works, whether it really does fit the situation. You jump into many problems of both clean and dirty analysis in trying to do this. You may have theoretically an assignment of treatments in this and that, but where you have the practical situation you find that you can't do it. You are experimenting on human beings who won't volunteer for this kind of randomization. You do the best you can in a practical situation. Then you have to estimate, perhaps in a crude way, to what extent your deviation from the hypotheses of the model is likely to affect the result. On doing the dirty problems in data gathering in the real world, I can give many illustrations. But the point is that as long as the person can't get a job teaching statistics in the mathematics department unless he is doing mathematical probability, and as long as there is nobody in the mathematics department that understands anything about the real world in this sense, then what it means is that you are going to have somebody in the college of agriculture doing agricultural statistics and somebody in economics doing economic statistics, etc., doing the things that have to be done and very often with no control on their scientific competence in problems on the mathematical side.

What I would like to take as a model for the kind of things that have to be done in teaching applied mathematics is the little book of von Karman and Biot. This example, I think, has to be generalized to many different parts of mathematics and many different kinds of applications. You simply can't hope to teach by formal courses everything that an applied mathematician needs to know and you couldn't predict in any way what everyone needs to know, since you can't predict the creation of a new theory like information theory and game theory.

What you have to do in courses is to select a wide variety of topics in mathematics. You have really to interweave the applications into the course. A general strategy would be to introduce, after you decide what mathematics you want to teach, a real problem to be drawn from any of the sciences. You pick a problem for which this mathematical topic is important. You have to set up the mathematical model for this, then show how this real problem leads to a mathematical difficulty which you have to solve. Then, you can concentrate on mathematics, you can develop the basic mathematical theory as the Committee on the Undergraduate Program recommends in its national project. Then you apply the basic theory to the original problem, and to other problems as well, to illustrate what the real power of abstraction in mathematics is, that the same mathematical model may have many different concrete interpretations.

So, we may start out with a problem of electromagnetic theory leading to Laplace's equation. Then besides applying it to our original electromagnetic problem, we also go into Brownian motion or fluid mechanics which also uses Laplace's equation.

With reference to the kind of thing that Professor Courant was worried about, I think you have to show the challenge of the concrete problem to the general theory. Here Louis Nirenberg's work on parabolic equations illustrates the point. He proves very general existence theorems for equations with continuous coefficients, or even with Lebesgue integrable coefficients. But here is a problem that comes from Sewall Wright's mathematical theory of evolution. In the attempt to get a quantitative theory of evolution he runs into parabolic equations to which none of this general theory applies because the coefficients are too singular. You have a nice big general theory and you can't even handle a simple case of diffusion of a single gene in populations of variable size.

Inasmuch as you can't cover everything in formal courses you must make arrangements for independent work. We have to have some baby research. There is enough money for anybody who wants to have an undergraduate research program in applied mathematics. All we have to do is take an interest in organizing it.

Professor Courant talked about how you must teach in close relation to reality. Well, the educational system as it is from ground up is part of the reality that you have to have your feet in. In other words, if you want to make a change you have to know the initial conditions. On the college and university level the initial conditions are that most of the staffs are dominated by pure mathematicians who were brought up in a tradition of disdain for application. I don't think I have to go into the historical reasons for this, but it is part of the general struggle since the creation of the first graduate school in the United States, Johns Hopkins, in 1876, to get research recognized as a legitimate activity of the faculty, research without having to be justified for service or utilitarian reasons. This struggle to justify research within the university was won so thoroughly by 1940–1950 that now one of our biggest problems is to obtain recognition for teaching as a legitimate activity of the faculty.

One sees this, surprising enough, even in some of our major institutes of technology where the mathematics departments have had a long struggle to free themselves from being service departments. They often pride themselves on not having anybody interested in applications, or on not bothering their best people with applications. "We have those second-raters to teach the engineers, and we're doing the real mathematics around here." You must recognize that most of the people upon whom you must rely to teach any applied mathematics that you want to get into the college or university curriculum have come into mathematics for aesthetic reasons, rather than for utilitarian reasons. That means that if you want to get applied mathematics into the curriculum of the college and the graduate school, you are going to have to present it in such a way as to emphasize that applied mathematics can also be beautiful, can also be deep. The problems are exciting and challenging, just as challenging as the four color problem or the homotopy groups of spheres.

You are going to have to show how exciting it is to take a real situation, say, how electrons behave. Here are the experiments. Electrons behave in crazy ways. Here is Dirac's mathematical model to describe how one electron behaves. Look at all the underlying mathematics this suggests which nobody has touched yet, a gold mine of problems, new frontiers to explore! Look at where such abstract mathematics can lead. See these solutions of the equations corresponding to particles with "negative energy". What can they mean? We go from a weird theory of places where an electron isn't, to holes in semiconductors, to transistors and a Nobel prize. What sheer poetry! What drama! Was there anything like this in Xanadu?

With this object lesson in mind will you help us grope for a model to describe two interacting electrons? Who knows what we will stumble on? Shall we explore this virgin territory together?

You've got 2,000 colleges and universities in the country. If you want to get an adequate number of them to teach applied mathematics the way you want, to get a lot of people who were brought up as aesthetes to teach calculus in a different way, you have to write it out for them.

Why do so many colleges offer a course in Fourier series and boundary value problems? Churchill wrote a book, you see. Before Churchill wrote a book they couldn't give a course because nobody there could give the course out of his own head. Before 1950 only the colleges and universities who had somebody doing research in mathematical logic could give a graduate course in mathematical logic. After the books by myself and Suppes and Kleene came out, lots of other colleges introduced courses in mathematical logic. If you want to have the ideas such as Professor Courant and Professor Carrier presented, or ideas that Morris Kline talks about, in the curriculum, then it has to be incorporated in a practical textbook that can be used in classes.

If you want to get at the real bottleneck, it will have to be before people have made their decisions about careers. That means, it will have to be in the early part of the curriculum. These courses should not be just useful mathematics, but they must include real applications to modern science and engineering. Now look, for example, at a typical calculus text. You have, just for practice on integration, a lot of calculations of moments, very often without any mention of the physical meaning in mechanics. If you want practice on definite integrals, you may as well calculate expected values of observables in quantum mechanics. This is just as good practice. Or you can show how an engineer uses moments in dealing with real problems.

There is also a problem of communication. I don't have time to go far enough with this. There is a growing number of physicists, economists, and psychologists who are pretty sophisticated in mathematics.

There are very few books like the two little books of Khinchin which explain the problems of applied mathematics to the pure mathematician. You look at a typical book on theoretical physics. It is written for people who have a very good background on the empirical side. It is for those who know what happens in the real world very well, but whose mathematical preparation is weak. What often happens is the elementary physics is passed over as though the reader already knows and understands the experiment that has been performed and the technical terms in physics that are used. The author assumes that you are not going to understand anything very highbrow on the mathematical side, and this is the way the book is written. A very different type of book is needed for the college mathematician and his students.

I'll make one final remark. There was some discussion about the importance of looking at the real world. If you want to have these things done at one university it will be enough to have Professor Courant there, and he will see to it that an activity of this sort goes on and will build it up. This will influence one small area. If you want to have something done in 60 graduate schools and 2,000 colleges, you have to have the teachers with you. It will have to be a crusade. You look at activities such as SMSG. They are effective because they are a crusade. You have your privilege of either boring from within as Burt Colvin and I tried to do, or organizing your own crusade. But just standing on the side lines and bellyaching about what's wrong is not going to get anything done.

DR. GREENBERG. Our last speaker is Professor C. N. Yang of the Institute for Advanced Study at Princeton. Professor Yang is known to us as one of the world's leading theoretical physicists. His work with T. D. Lee on non-conservation of parity brought them the Nobel Prize in Physics in 1957. Historically, much of what we think of as applied mathematics had its origin in the problems of physics. Accordingly, it seems most appropriate for us in considering what is needed for the future of applied mathematics to hear from Professor Yang representing modern mathematical physics. We are most grateful to him for taking time off to become involved in what is after all, "The Mathematician's Dilemma." Professor Yang.

PROFESSOR C. N. YANG. While it is usually good to have the last word it is distinctly disadvantageous to be the last speaker on a panel discussion. I therefore ask your indulgence if I have to repeat, perhaps in different words, some of the ideas already expounded here by the previous speakers.

To discuss the needs of applied mathematics it is necessary to understand first what applied mathematics is. There is considerable difficulty in meeting this requirement. As a zero-th approximation applied mathematics is something between theoretical physics and mathematics. Let us therefore take a look at what the physicists and the mathematicians think about the middle ground that stands between their respective subjects.

First take the physicists. There is a story circulating among us describing the feelings of a physicist when he consults a mathematician. A man carried a large bundle of dirty clothes and searched for a laundry without success for a long time. He was greatly relieved when he finally found a shop displaying a sign "Laundry done here" in the window. He went in and dumped the bundle on the counter. The man behind the counter said,

"What's this?"

"I want to have these laundered."

"We don't do laundry here."

"But you have a sign in the window advertising that you do laundry."

"Oh! *That!* We only make signs."

In a little book [1] called "A Mathematician's Apology" G. H. Hardy gave his views about the difference between pure and applied mathematics, about their respective usefulness, and about their intrinsic appeal to the intellect. He said:

> "I hope that I need not say that I am not trying to decry mathematical physics, a splendid subject with tremendous problems where the finest imaginations have run riot. But is not the position of an ordinary applied mathematician in some ways a little pathetic? If he wants to be useful, he must work in a humdrum way, and he cannot give full play to his fancy even when he wishes to rise to the heights. 'Imaginary' universes are so much more beautiful than this stupidly constructed 'real' one; and most of the finest products of an applied mathematician's fancy must be rejected, as soon as they have been created, for the brutal but sufficient reason that they do not fit the facts."

He implied that applied mathematics is dull. In other passages he claimed that applied mathematics is in fact less useful than pure mathematics, or real mathematics as he sometimes called it. To avoid answering embarrassing questions he included Maxwell and Einstein, Eddington and Dirac among the real mathematicians.

The book was delightful. I derived great pleasure out of reading his eloquent passages marshalling all kinds of arguments—some deeply philosophical, some just plainly witty—to justify mathematics as a pure art.

My point in telling you the story and in telling you Hardy's views on applied mathematics is that they seem to me to define precisely what applied mathematics should not be: Applied mathematics should not be the dull subject that Hardy believes it to be. Nor should it be the art of making signs.

Applied mathematics is a creative subject lying mostly between mathematics and theoretical physics, but also between mathematics and other subjects, in which phenomena of the physical world are synthesized in mathematical language. The difference between an applied mathematician and a theoretical physicist should only be that of a slight difference in the emphasis put on the inductive process leading from physical reality to mathematical formulation and the deductive process of going from mathematical formulation to physical reality. The theoretical physicist emphasizes more the inductive process and the applied mathematician emphasizes more the deductive process. A truly good theoretical physicist should in fact also be a good applied mathematician and vice versa. I believe this view is in agreement with that held by the applied mathematicians themselves. It is certainly in agreement with that expressed by Professor Greenspan in his passionate defense [2] of the field of applied mathematics at a meeting of the Mathematical Association of America in January 1961.

If we agree to this characterization of applied mathematics, it seems to me that education and research in applied mathematics must lay primary emphasis on physical reality. Let us analyze a little more the reasons behind this statement.

We know that for a computing machine the wiring system and built-in subroutines determine its basic mode of operation. While different machines with different wirings and different subroutines can in principle perform the same work by subsequent programming, they do so with different degrees of ease. In the same way the education that a student receives determines his later style of thinking, his taste in selecting problems, and his psychological and emotional response to the challenge of new difficulties. As Hardy's passage quoted before vividly demonstrates, a pure mathematical training is not conducive to producing an appreciation of the beauty of mathematical interpretation of physical phenomena. For an applied mathematician, the lack of such an appreciation is disastrous.

On the other hand, a training leaning toward physics is not a handicap to later absorption of mathematical concepts. In fact it is oftentimes a help in creative mathematical thinking. Why this is so is apparently a profound topic, perhaps related to the question why the physical world can be subject to mathematical formulation at all, and perhaps also related to the question how many mathematical concepts have in fact originated in physical reality. But in any case it is a truism admitted even by the mathematicians themselves. Hadamard [3], for example, declared in his, "The Psychology of Invention in the Mathematical Field," that physical interpretation is in general a very sure guide in mathematical creation and had very often played that role for him.

Of course, I do not imply that every physicist can learn to become a mathematician and not vice versa. But given a talented student and given the task of training him to be an applied mathematician it seems to me advantageous to first expose him to the taste and the discipline of a physicist before he is spoiled by the more radical, less tolerant, and more imaginative style of mathematics.

Traditionally, especially before this century, physics had been an important source of new mathematical ideas. Fourier said in 1822, and I quote from a translation [4] of his work,

> "Profound study of nature is the most fertile source of mathematical discoveries. Not only has this study, in offering a determinate object to investigation, the advantage of excluding vague questions and calculations without issue; it is besides a sure method of forming analysis itself, and of discovering the elements which it concerns us to know, and which natural science ought always to preserve: these are the fundamental elements which are reproduced in all natural effects."

But perhaps Fourier is regarded as too ancient. And maybe he is regarded anyway as not enough of a pure mathematician to be worth listening to. Let us therefore hear from Hermann Weyl, who was as pure a mathematician as any. He wrote in an article [5] entitled, "Relativity Theory as a Stimulus in Mathematical Research," that

> "The relativity problem is one of central significance throughout geometry and algebra and has been recognized as such by the mathematicians at an early time.
>
> ". . . for myself I can say that the wish to understand what really is the mathematical substance behind the formal apparatus of relativity theory led me to the study of representations and invariants of groups; and my experience in this regard is probably not unique."

Let me add that the group representations studied by Weyl had in time exerted profound influence on the developments of modern atomic physics.

This example provides a beautiful illustration of the fruitfulness of a creative mathematical thinking that originates from physical concepts, soars to the height of abstraction and returns to enrich the development of physics. It seems to me that if applied mathematical research is to remain a dynamic field it must formulate its new problems and search for its new ideas in the infinite and magnificent manifestations of physical reality.

REFERENCES

1. G. H. Hardy, *A Mathematician's Apology*, pp. 74–75, Cambridge Univ. Press, 1940.
2. H. P. Greenspan, *Applied Mathematics as a Science*, Am. Math. Monthly, 68, 1961, pp. 872–880.
3. J. Hadamard, *The Psychology of Invention in the Mathematical Field*, Princeton Univ. Press, 1945, p. 52.
4. J. B. Fourier, *The Analytic Theory of Heat*, Cambridge Univ. Press, 1878. (English translation.)
5. H. Weyl, Proc. Am. Phil. Soc. 93, 1949, p. 535.

Panel Discussion

Dr. Greenberg. I'm sure that the speakers may want to start on other topics, but there are certain topics which we would be very remiss if we didn't discuss this morning. I'd like to start the discussion by throwing out this general question to the group. One hears a lot about modern applied mathematics, and it's a great fashion to talk of finite mathematics. One wonders whether this is something different which has great implications for the future of applied mathematics or whether it's something familiar which we are incorporating sufficiently into our curriculum and activities so that it doesn't need to be singled out for special attention. Would someone like to comment on the role of finite mathematics as a new part of applied mathematics?

Professor Courant. I know what mathematics is, and I do not quite know what applied mathematics is. But what "finite mathematics" is, in distinction from mathematics as it has developed and been pursued since antiquity, this I have not understood. It may be a selling point as a title of books, but, if we want to eliminate the concept of limit or of infinity, this seems to me like advertising music without sound, or painting without color.

Dr. Greenberg. No argument anyone? Have we disposed of finite mathematics? Let me ask the question this way. We are frequently told—people who are doing work in numerical mathematics and who have something to do with computers —"You people are doing new kinds of things with mathematics. What sort of training should students get to be able to participate in this?" As though there were some body of mathematical knowledge that was distinct and which is required for this sort of effort above, let's say, simultaneous linear equations. I think they are referring to topics like Boolean Algebra. The question is: Is this something new; is this an important thing for an applied mathematician who is addressing himself, let us say, to problems in theoretical physics? Or to combinatorial problems, perhaps?

Professor Rosenbloom. If I may answer you, these combinatorial problems come up mainly in modern applications to the social sciences and, somewhat, in the biological sciences. There is certainly a legitimate place—there are certainly some very deep and difficult combinatorial problems. If you look in the book by Mann, "Analysis and Design of Experiments", and the more recent book by Riordan, you'll get some idea of that. However, in the attempt to redress a certain balance in our elementary college curricula—our traditional freshman or sophomore courses, if they have mentioned any applications at all, have made mention only of elementary physics—a number of people like Kemeny and Tucker and so on attempted to write freshman and sophomore courses which would give adequate attention to the applications to the biological and social sciences. The only thing is, being enthusiasts, they went overboard in that direction. Somebody has to write something which restores the balance without neglecting what they did. The hard core of applied mathematics is still analysis and, therefore, based on the concept of limit, calculus, the real number system, and so on. And, as a matter of fact, in the application to the biological and social sciences, as soon as you want to do anything real with probability, you are back to analysis again. Somebody is going to have to write freshman and sophomore courses which would knock Kemeny, Snell and Thompson out of the market by doing an adequate job on the calculus and giving enough illustrations of many kinds of applications to give some idea of how mathematics is connected with the real world.

Dr. Greenberg. If the applied mathematician is going to continue and have influence in new areas, it would seem, if one looks at the courses of applied mathematics which have traditionally been offered and which continue to be offered and which have to do principally with branches of mechanics—solid and fluid mechanics, that substantial changes are needed in the curriculum. The question is: If applied mathematics is going to reach into realms of modern physics and even newer fields like biology and medicine, how is it going to be possible to weave these subjects into the training of the mathematicians so that they can have some influence in the development of these fields, and vice versa?

Professor Carrier. My suspicion is that it will be woven in in the same way that most of the topics which now appear as classical have been woven in. The professional applied mathematician, whose interests are broad and who ventures into these new fields, will see the kinds of mathematics that he has to understand in order to make any progress, and at some stage, probably at the graduate levels of work, he'll introduce this material to students; then, as the need seems to grow, the material will be taught at the earliest level at which the student can assimilate it. I don't think you deliberately put things into the curriculum until you've encountered a need for them, if you're working from the applied mathematician's side. Curricula are already crowded enough with things we know we need. In mathematics departments, there may well be offered very strong courses and very much broader courses than the applied mathematics student can afford the time to look at, but it behooves the applied mathematician to be aware of these fields of mathematics and to be educated as to their general character to the point that he can recognize when one of them is something that he is likely to be able to exploit.

Dr. Greenberg. This touches on a point in a question which was submitted by one of the people in the audience, Dr. Gray from the Johns Hopkins Applied Physics Laboratory. He asks this question: "In the education of the scientist, how is it possible to give him the best possible chance, so that later when he is faced with a problem, he may be aware of the existence of some powerful mathematical technique fitted to his problem which may not previously have been felt to have any connection?". The point there is that it's rather hard to foresee what the mathematical education of the applied mathematician should consist of in terms of pure mathematics.

Professor Courant. The term "applied mathematics" does not refer to a definite store of knowledge. What distinguishes the "applied mathematician" from the "pure mathematician" is essentially an element of motivation, an element which ought also to be present in the soul of a purist: a profound interest in the connection between mathematics and what may be called reality. To be strongly motivated by this attitude is all that characterizes the "applied mathematician". As to the ability of solving problems imposed from outside, one cannot be prepared for all possible contingencies and problems that may be presented. Of course, it is a tragicomedy when an engineer comes with a question to a mathematician and then the mathematician takes a long time meditating, modifying and simplifying and then returns happily with an answer, only to learn that in the meantime the questioner has completely lost interest.

Dr. Greenberg. Let me raise the 2^6 dollar question. There seems to be general agreement that we need to interweave the subject matters of mathematics and science, and that books have to be written to serve as a basis for the curriculum. Now, we know that books are being written and that there is a crusade. Professor Rosenbloom has taken a part in this activity. I think many people are curious as to the success of these efforts. There is a recent article by Morris Kline which I am sure many people are aware of which rakes the new curricula over the coals, perhaps in an extreme fashion. I would like to ask Professor Rosenbloom as a participating member of SMSG [School Mathematics Study Group] to say to what extent he feels that progress is being made and also to what extent people in applied mathematics and science can contribute to furthering this project; to do more of the things which I'm sure people in SMSG are aware need to be done to fill the needs more broadly.

Professor Rosenbloom. First of all, with reference to my good friend Morris Kline's article and his criticisms, he repeated without much change the things that he said in a debate that I had with him in January, 1960, in Chicago. Apparently, he didn't learn very much from the debate. At that time, I already challenged him to put up or shut up. Namely, I agree with him on the substance of many of the things he says about the mathematics curriculum, but I don't think that it's going to do the least bit of good to stand off on the side-lines and say what is wrong. I think that if you want to bring about the change that has to be made, you have to have a positive program of action. Now, in this business of trying to revise teaching of mathematics in the schools, we had many problems. Among them was the problem of social engineering, which was that you had to make a big change quickly under conditions where no one had the power to impose anything on anybody. You had to get a lot of people to decide voluntarily to do what they ought to do. And so it was not just a problem of figuring out what mathematics should be taught in grades 7 or whatever other grade. But there was also a problem of actually getting this into a large number of schools quickly without being able to force them. So, the procedure had to be one where a large number of people were assembled from different parts of the country and representing different points of view, both from mathematics and from education, from the schools and so on, to write books in a very permissive atmosphere. Essentially, the problem was to be able to say that this ninth grade course, for example, represents the consensus of mathematicians and teachers. The Seattle school system has introduced SMSG into the Seattle school system because their mathematics supervisor was on the writing team. The National Council of Mathematics Teachers has had a voice in this. The chairman of their secondary school curriculum committee was on the 11th grade writing team, and so on. You have these problems, and to a certain extent, what came out depended upon the persuasiveness and the maturity of judgment of the people that got on the writing teams that represented various points of view. In the

7th and 8th grade and in the 12th grade writing teams, there were a number of people who felt about the same way as Kline and I do, and so we got, to a large extent, the kind of thing written that we felt was needed. In the writing teams for grades 9 to 11 some of us were disappointed that people who we had thought would be advocating applications, came up with writing stuff about open sentences, and the like. I would say that machinery now exists for getting things tried out on a large scale and that we've got a crusade rolling. I would say that it's much more practical to make use of this machinery if one wishes to propose alternatives, rather than to try to set up competing machinery. Actually, the way SMSG is set up, if there is any group that feels that an alternative 9th grade course is necessary and is willing to put in the work of writing it, then this can be done through the mechanism of SMSG and the machinery exists for tryout and for getting the stuff used in many places in the country right away. For example, last summer they had an alternative 10th grade course written with more emphasis on analytic geometry than the earlier one. I'm writing a text on applied mathematics for the 12th grade for SMSG and, when this is written, I'm sure that [Professor E. G.] Begle will see to it that it will be tried out in competition with the 12th grade text on matrix algebra which they already have. We hope ultimately to have about a half dozen alternative 12th grade texts. Naturally, I would prefer to see a lot of the things that I have put in, such as a discussion of mathematical theory of the struggle for life, Volterra's theory in terms of difference equations, rather than differential equations, and a finite mathematical introduction to quantum mechanics, interwoven in the proper places in the regular courses, but I'm only one person. I can't write all the textbooks myself. I only had time to write a certain number of the chapters in the 7th and 8th grade. If we want to have the kind of thing done that has to be done, then somebody like Kline or Polya or you will have to gather together with a group of half a dozen good school teachers and write for kids. You've got right in New York City—I could name for you in five minutes a list of good high school teachers in New York City—teachers that would have the talent and the mathematical ability to work with someone like you or Morris Kline or any other mathematician to produce a new program for grades 9 through 11, where I think the need is greatest. I think that Begle would be glad to cooperate in putting his machinery at the disposal for getting these in schools. But I would say to Morris Kline or you, "put up or shut up". It's no excuse to say that "really good applied mathematicians like me don't have the time to do this work; somebody else ought to do it". And this is what he implied in that article, isn't it? He said, essentially, that good applied mathematicians were really doing important scientific work and—he didn't actually put in "like me"—didn't have time to do this work themselves.

Dr. Greenberg. I wonder if we have to give equal time at a SIAM meeting to Morris Kline.

Professor Courant. I don't take issue with all that you said, but I feel that things should not be done exclusively by committees. It is not certain that really stimulating textbooks for high schools or kindergarten can be written by committees. I think that those agencies which support large committees with millions of dollars should support writing of textbooks by inspired individuals who are willing to do the work.

Professor Rosenbloom. May I mention that this is already being done. First of all, within SMSG I can write an individual text or we can set up a large committee. Now that we have got the mechanism, this business of setting up a large writing team and getting a lot of agreement is not necessary. I think that conditions are quite satisfactory now for, say, writing teams to be set up for grades 9 through 11, one mathematician and two high school teachers or some similar format. (Added March 2: A few individual outstanding mathematicians have the flair for writing for youngsters, but most would need help from a good school teacher.)

Professor Courant. I still think one should write as an individual, not as a member of a team under supervision of a superior committee. That has been done in France, for example, many decades ago. Outstanding mathematicians, as individuals, have written important textbooks for high schools with great success. It's being done in Russia now.

Professor Carrier. I must say that my worries go a little further than the writing of books. One thing I'm concerned with is the training of the high school teacher. As you probably all know, the NSF (National Science Foundation) is supporting summer institutes for upgrading the background of high school teachers and I sat in on some of the decisions as to which ones should be supported and which ones shouldn't. In each of the proposed institutes which I encountered, essentially all of the material to be offered to the secondary school teacher was designed to provide depth of background and perspective in mathematics; the very discouraging observation I made was that none of the material would provide any background or perspective regarding the *use* of mathematics.

Now, while I fully concur that these teachers must acquire the appropriate precision of mathematical thought and a thorough understanding of certain mathematical disciplines, it is equally essential that he acquire some perspective and understanding of the way in which mathematics is *used.* In fact, he really should acquire some *skill* at using mathematics to deal with rudimentary quantitative questions in science. After all, the vast majority of students who will be taught by our secondary school mathematics teachers will not become professional mathematicians but will have occasion to use mathematics in connection with science and other professional activities. Thus it seems to me that the secondary school mathematics teacher *must* study carefully prepared and presented material which prepares him both to lead the student into the habits of precise mathematical thought and to introduce him to the attitudes and skills which underlie and exemplify the challenging variety of uses of mathematics. In particular, these teachers must be led to recognize that the currently popular notion that the beauty and purity of mathematics are somehow contaminated when they are related to questions involving the real world is not a very intelligent view. The use of mathematics in science and elsewhere can be as challenging, as esthetically pleasing, and as valuable to society, as the "pure" selfcontained discipline. Our teachers and our students had better find this out—soon!

Professor Yang. I have very little experience with high schools or high school students, but I did talk to them on one occasion a few years ago and the result of that was very alarming to me. Perhaps it's not completely out of place to report to you this experience I had. I live in Princeton and there is a very good high school in Princeton. Two years ago, a group of students from this high school in their senior year, obviously very talented and very interested in science, organized a physics club and they called up various physicists in Princeton asking whether they could contribute some time to guide them in studying physics. So a number of us said we would and a meeting was arranged to discuss how the classes should be conducted and what should be the subject matter. We went there, (among others I remember Wheeler, White, and myself) and one of the boys of the club got up and said 'We are going to devote the next half year to the following topics'. Thereupon he wrote on the blackboard a list of topics starting from mechanics, going on to electricity, electromagnetic theory, quantum mechanics, thermodynamics, nuclear physics, quantum electrodynamics, elementary particles, etc. We were quite taken aback by this and tried very hard to persuade the boys that there is no shortcut to learning and it is not useful—in fact it is harmful—to learn of many terminologies without having understood what they are all about. That turned out to be not so easy a task. I do not know what should be done about this, nor do I know how widespread this kind of phenomenon is. I suspect that with the great public interest and emphasis of late on science, especially in physics, talented high school children are likely to pick up lots of terminology without asking about their content. This attitude of trying to swallow superficially in a great hurry, to learn *of* many things, but not the things themselves, is a disease that spreads very easily.

Dr. Greenberg. Let me ask a question that we have not considered at all; that is, the graduate schools in applied mathematics in the United States—are they doing well or are they doing poorly? One question which someone raised, and it's a nice one, asks us to discuss the role of the Ph.D. thesis problems in regard to the present state of pure and applied mathematics. Are we doing good graduate work in applied mathematics in our institutions as far as the Ph.D. theses which are being assigned? Are they meaningful in furthering the role and status of the applied mathematician in the sense that he is led to do good scientific work? I think Professor Carrier and Professor Courant may have the answer to this.

Professor Carrier. The obvious answer is that you can't answer that question uniformly. There are some graduate schools that I think are doing exceedingly fine work. They have people on the staff who know what worthwhile research is (by some reasonable criterion); they assign meaningful work to students, and, while guiding these students through their thesis work, they see to it that the student uses his own imagination, that he assimilates the attitude of the applied mathematician, that he learns a great deal, and that he really does contribute something of research value rather than just manipulation. In such schools the research is worthwhile and, more important, the student emerges with an appropriate background on which to build a productive research career. There probably are other schools that fail to provide a good Ph.D. education, frequently because the staff does not do research itself; whether the Ph.D. research is worthwhile usually rests with the research advisor or the research committee of the student; obviously there are able ones and there are less able ones. I like to think that we do a good job (at Harvard), and I'm sure that everybody here feels exactly the same way about the work at his own institution. In any event, I'm sure that good research in applied mathematics *is* being done in many schools in the United States.

Dr. Greenberg. If the source of good applied mathematics of the future is going to come from science and, to some extent, also from technology—where engineering attempts to exploit new discoveries—what about the interaction of the graduate school with the industrial community as a source of problems, and so on?

In other words, does the applied mathematician in his department have access to new problems, unless he is a consultant on the outside and has a lot of contacts? Of course, a lot of people do, and that is a source of fresh, new problems, but is it a necessary source of fresh, new problems?

Professor Carrier. I don't know. I know some people who do very good research who have essentially no industrial contact; they're very fine applied mathematicians and some of them are fine engineers too. On the other hand, many of the problems that I personally find exciting and stimulating are ones that in one way or another I encounter from industrial contact—it may be casual or it may be actually consulting. But I know other people who from their knowledge of the state of the art and their knowledge of the literature and so on, detect and work on very worthwhile scientific questions without such external stimulus; so I repeat that there is no single answer. It depends on the individual who is doing the research and who is providing problems for his students (when that needs to be done) whether an outside stimulus is needed. Different people are stimulated in different ways.

Professor Courant. I would like to make a remark only loosely connected with this discussion. I feel, with others of the speakers, that the great difficulty is to establish and maintain fruitful contact between the mathematician and other fields. The "applied mathematician" should work together with physicists, engineers and so on—and I don't mean just as a casual consultant. The system of teaching in the universities and in the graduate schools, in particular, will probably have to be modified under the pressure of masses of students of different intellectual makeup. In applied mathematics, the most important problem is the transition from the problem of reality to the formulation of a mathematical model, followed by the mathematical analysis of the model, and by the next step, that is translating of the results again into the language of reality. At present, this question of double transition is largely neglected. In universities, I think the only way of remedying this deficiency is to establish close cooperations in seminars or work groups between a rather small number of outstanding mathematicians and physicists or engineers in an informal way, as was done during the war in OSRD. For example, the whole field of programming and operational analysis which came up so vigorously through our British friends during the war can be made meaningful only by close cooperation between the mathematically and statistically trained investigator and people who present reality, and an interplay between them. I feel—but I have no recipe that I can offer—that universities should try to develop activities in such directions, crossing the boundary line between disciplines.

Professor Carrier. I have something to add to avoid misinterpretation as to what I mean by the word "suggested." I said that many of the problems that stimulated me are suggested by contact with the industrial world. I don't mean that the question that they ask one to answer is literally the question that the applied mathematician or any other scientist will necessarily find stimulating (although in many instances it may be). The stimulating line of research may be an abstraction or a generalization of such questions, so when I say "suggested," I mean it in a very broad sense; I certainly don't mean that a productive research career is likely to be one in which one attempts to answer the questions that literally are put to him by some external concern. On the other hand, one can very profitably concern himself with the fundamental questions which underlie the phenomena which give rise to such "industrial questions."

Dr. Greenberg. There are a couple of more questions from the floor that I would like to try to cover. One is directed to George Carrier. In singling out numerical calculations for special remarks, this person feels that you implied it may not be the function of the applied mathematician to actually touch the machine, that they should have other bodies such as programmers and so on. He wants to know whether the applied mathematician should do the whole problem right through the numerical analysis, even if it means doing some programming himself.

Professor Carrier. Ordinarily I think the answer to this is "yes." The most consistently successful use of machines that I have seen has been at Los Alamos, where just about every man follows his research right through any necessary calculations involving machines. But this answer is not uniformly appropriate, of course. There are certain routine problems for which all the programming is really already available except for certain formal substitutions; the inversion of not too large matrices I believe is in this category, although my own ignorance may now be showing. There is no doubt, I think, that routine operations can be done by someone who has the job of doing routine operations, and is not an applied mathematician in the sense that I would like to define it. But when dealing with subtle research problems where, in the first place, the procedure is not necessarily routine, where the structure of the result cannot be accurately anticipated, and where a great deal of understanding is required in order to decide what one wants the machine to do, the applied mathematician should carry out the non-routine aspects of the investigation himself.

Dr. Greenberg. I wonder if in this connection Professor Yang would comment because I know he has recently had some large calculations done for him and was very personally involved in it and that he feels it was successful.

Professor Yang. Well, I don't know how I can generalize on this subject but, as Professor Carrier said, it depends very much on the problem, and also on the investigator, and also on the programmer. My personal experience has been that if you give the problem in the undigested state to a programmer, after a while you are very afraid, at least I was very afraid, of the connection between the numbers produced and the problem that was given to him.

Dr. Greenberg. There is a question also here for Professor Yang which I would like to pass on. The question is: if early training in pure mathematics creates a disdain in the student for applications, don't physicists teach their pupils to scorn engineering applications? I would like to generalize this a bit and ask, don't the physicists frequently have a built-in disdain for mathematicians, as well as engineers, and why is this? Don't they know the stories about Fourier and the others?

Professor Yang. I am afraid I cannot answer for all physicists, but I seriously doubt that there are many physicists who are so narrow-minded as to look down upon either pure mathematicians or engineers. I do not find that this is the case with physicists that I have been in contact with. I think that by the nature of his discipline, a physicist learns to respect both academic questions which are of purely intellectual appeal and very practical questions. If, unfortunately, some physicists give the impression Doctor Greenberg described, let me quickly disassociate myself from it.

Dr. Greenberg. Are there any last remarks that any of you gentlemen would like to get on the record to correct false impressions?

Professor Yang. Let me just tell you a story that I heard about the development of quantum mechanics, that is not unrelated to the question of how much mathematics should be taught in order to prepare a person to become a good applied mathematician. Around the years 1924–1925, it became quite clear that some new mechanics would develop very soon. In fact it was already repeatedly pointed out that perhaps the energy levels of, say, the hydrogen atom should be interpreted as eigenvalues of some operator. Göttingen, as you know, was a great center of such problems so there was a large amount of effort spent in trying to find the operator which would give the correct eigenvalues. Needless to say, all such efforts were in vain. What finally happened was that from physical arguments, an operator equation was written down which was solved by a physicist giving precisely the eigenvalue wanted. It seems very difficult for the human mind to anticipate the correct combination that nature favors. I think it is probably futile to try too much to anticipate nature in providing a person with all kinds of mathematical tools. What is important, is not any particular knowledge about this or that mathematics, but as Professor Courant has emphasized, an attitude.

Dr. Greenberg. Well, I think that we'll conclude and I wish for all of us to thank the participants of the panel very much for being here this morning.