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Principles of Advanced Mathematical Physics

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Preface: On the Nature of Mathematical Physics

Reasoning in mathematics and reasoning in physics have very different textures. Mathematics is held together by short-range forces that bind each step in a deduction directly to the preceding steps, whereas physics is held together by the much longer-range forces of analogy and intuition and all sorts of indirect supporting evidence. The comparison of physical science with cryptanalysis ("deciphering the secrets of nature," etc.), though overworked, is apt. When one has solved a cipher and the message rings out loud and clear, one does not think of calling in a mathematician to provide a uniqueness proof, even though conceivably there might be a different solution, i.e., a different message. In physics, existence and uniqueness proofs are many decades behind current research (because of the inherent complexity of nature) and are often somewhat irrelevant, because they can be no more convincing than the hypotheses on which they are based, which in turn are matters of physics, while a large body of indirect evidence is often fully convincing. In mathematics, on the other hand, intuition and analogy are notoriously untrustworthy; although they often lead to useful conjectures, the conjectures never become part of the structure until proved. When one is proving a theorem in mathematics, one is not permitted to use any hypotheses except those present in the statement of the theorem.

A first consequence of this difference in texture concerns the attitude we must take toward some (or perhaps most) investigations in "applied mathematics," at least when the mathematics is applied to physics. Namely, those investigations have to be regarded as pure mathematics and evaluated as such. For example, some of my mathematical colleagues have worked in recent years on the Hartree–Fock approximate method for determining the structures of many-electron atoms and ions. When the method was introduced, nearly fifty years ago, physicists did the best they could to justify it, using variational principles, intuition, and other techniques within the texture of physical reasoning. By now the method has long since become part of the established structure of physics. The mathematical theorems that can be proved now (mostly for two- and three-electron systems, hence of limited interest for physics), have to be regarded as mathematics. If they are good mathematics (and I believe they are), that is justification enough. If they are not, there is no basis for saying that the work is being done to help the physicists. In that sense, applied mathematics plays no role in today's physics. In today's division of labor, the task of the mathematician is to create mathematics, in whatever area, without being much concerned about how the mathematics is used; that should be decided in the future and by physics.

Specialization has, of course, gone too far, but even with less of it, it would be out of the question for the methods of contemporary mathematics to be transplanted all the way over into the area of contemporary physics and produce significant results. The differences are just too great. Today's physicists know how to use mathematics; they know how to formulate problems, devise methods of solution, and perform long derivations and calculations, but they cannot create the mathematics. Experience has shown that the discovery and purification of abstract concepts and principles is peculiarly in the realm of mathematics. The division of labor is important and ought to be taken seriously.

There is no objection to a mathematician's working in the areas that have come to be designated as applied mathematics, and if he can derive inspiration for his mathematics from the physical world, that is very much to the good, but the value to physics of the fruits of his labor will be determined by their quality as mathematics.

There is also no objection to a mathematician's doing physics, provided he is qualified. The prime example was von Neumann—when he did physics, he talked, thought, and calculated like a physicist (but faster). He understood all branches of physics (including elementary particles as they were known then), and chemistry and astronomy, and he had a talent for introducing those and only those mathematical ideas that were relevant to the physics at hand. Anyone, regardless of professional affiliation, who can do physics one tenth that well should be encouraged to do it, but the objectives and methods are quite different from those of applied mathematics, whose purpose is to create mathematics.

Here are some quotations from Hardy's "A Mathematician's Apology":

1. "I said that a mathematician was a maker of patterns of ideas, and that beauty and seriousness were the criteria by which his patterns should be judged." (page 98)
2. "It is not possible to justify the life of any genuine professional mathematician on the ground of the 'utility' of his work." (page 119)
3. "One rather curious conclusion emerges, that pure mathematics is on the whole distinctly more useful than applied." (page 134)
4. "I hope I need not say that I am not trying to decry mathematical physics, a splended subject with tremendous problems where the finest imaginations have run riot." (page 135)

Another consequence of the difference in texture concerns the word "rigor," which is badly misused by both mathematicians and physicists and possibly ought to be banished from our language. Physicists think mathematicians spend an inordinate amount of time making sure that all i's are dotted and all t's crossed, and the mathematicians shake their heads and wonder how those sloppy physicists ever get anything right. Both attitudes result from failure to recognize the methodological difference between the two disciplines. The situation becomes a little clearer when one teaches mathematics to physicists, for it turns out that although the physicists are not to be deterred from their accepted and successful ways of investigating the physical world, they demand rigor in mathematics. They want to know exactly what is true and what is false and exactly why (although they are eager to be told lots of additional things without proof), and they want to see lots of examples and counterexamples, in order to delineate the areas of relevance of the theorems.

In one branch of physics, quantum field theory, the difference in texture has almost disappeared, owing to the failure of the traditional methods. In 1900 Max Planck said "let's quantize the electromagnetic field," and he showed what wonderful things would happen if we could do it. Einstein showed more. In a certain measure, all modern physics is based on that suggestion, but the task has proved to be enormously more difficult than was supposed. In many attempts during the first half of this century, based on the intuitive methods that had been so successful in other parts of quantum mechanics, emission and absorption rates and line breadths were successfully calculated, but only by arbitrary suppression of infinities and inconsistencies, and for the most part in cases where the required result was already known from experiment and cruder theories. In the 1950s, various physicists began to take seriously the suppression of the infinities by the introduction of precise new axioms ("renormalization"), and a flood of exciting new results came out (Lamb shift, precise magnetic moments, etc.). Still, we do not yet have a water-tight theory, and each new attempt to overcome the difficulties of previous attempts has involved the introduction of more precise and more powerful mathematical tools. It now seems that intuitive methods are just as untrustworthy in quantum field theory as in pure mathematics, and contemporary work in field theory has very much the same texture as pure mathematics; there is the feeling of "definition, lemma, proof, theorem, proof, etc." if not the actual words. Presumably, when success finally comes, it will be through interplay between physical intuition and the newly found mathematical rigor.

The consequence for mathematical physics is an increased relevance of the careful study of operators, distributions, Banach algebras, functions of several complex variables, representations of noncompact groups, and so on.

People in other areas are usually unaware of the wide range of mathematics now used in physics. They assume that physicists are interested only in analysis and specifically the part of analysis appropriate for nineteenth-century physics, as set forth in Courant and Hilbert. Most books, including recent ones, on "mathematical methods for physicists," and the like, contain no group theory, which has played an important role in physics since about 1925, and the authors give no indication they have ever heard of the mathematical principles and concepts basic to modern quantum mechanics, relativity, cosmology, scattering theory, quantum field theory, statistical mechanics, topological dynamical systems, and so on, to say nothing of the concepts and principles that have not yet found their way into physics, but are likely to do so in the near future and are likely to come from areas such such as algebra, logic, set theory, and topology. No part of mathematics is devoid of potential interest for physics.

For our purpose, mathematical concepts and principles are more important than methods, and the main goal of courses in mathematical physics, in my opinion, is to explain the concepts and principles in such a way that one can see their relevance for physics. Here is an example:

Manifolds in relativity: In 1916 Karl Schwarzschild derived the static spherical solution of the Einstein field equations in the form now known by his name. His formula appeared to indicate some sort of singularity at a radius now called the Schwarzschild radius. There followed forty-four years of confusion about the "Schwarzschild singularity." As time went by, it became gradually clear that Schwarzschild's formula described only a part of the relevant physical space-time, and in 1960 Martin Kruskal gave a description of the geodesically complete manifold of which Schwarzschild's formula determined a part. It was then seen that although certain interesting phenomena are associated with the Schwarzschild radius, there is no singularity there. Relativists now take the attitude that by a solution of the Einstein equations one has to understand not just a formula for a line element $ds^2 = \cdots$, but rather a complete manifold, and that the global topology of the manifold may be of cosmological significance. The introduction of the geometric notion of *manifold* into relativity is a prime example of mathematical physics. The theory of manifolds is set forth in Volume II.

An earlier example was the introduction of abstract Hilbert space theory into quantum mechanics, mainly by von Neumann, which made it possible to construct a solid theory on the basis of the powerful intuitive ideas of

Dirac and other physicists. No less important was the introduction of groups and group representations, mainly by Wigner and Weyl.

A recent example is the introduction by Ruelle and Takens of ideas from the topological theory of differentiable dynamical systems into the study of the onset of turbulence. These ideas are likely to play a role in other parts of physics, where nonlinear differential equations appear.

The basic mathematics of physics belongs in physics courses. The proper formulation of boundary-value problems, asymptotic expansions, consequences of symmetries, and so on are all matters of physics. Although the ideas are further clarified and analyzed in mathematical physics courses, their first introduction should appear as part of the physics. A physics instructor ought never to say to his students, "just how those things work will be explained in your mathematics courses." Physics and mathematics cannot be separated in that way, and it is not the purpose of courses in mathematical physics to relieve physics teachers of the responsibility of explaining their subject. In practice, however, the best physics courses cannot be adequate on all topics. For example, most books on quantum mechanics are hopelessly unclear about Hilbert spaces and operators, and students need to learn about those things after they have first studied quantum mechanics as an intuitive subject. It is not just a question of "rigor." Whether a given symmetric operator has self-adjoint extensions and, if so, how many different ones, is a matter of physics, because the self-adjoint operators are the observables. The probabilistic interpretation of the spectral family of a self-adjoint operator gives the physical interpretation of the observable even for states that are not in the domain of the operator, and so on. I interpret mathematical physics so as to include the explanation of these things.

Most good ideas turn out to be simple ones, and I believe it is important that they be so presented, without unnecessary ramification of other ideas. In my view, for example, distribution theory should be based (rigorously, of course) on the Riemann integral and advanced calculus, and L^2 spaces and the theory of differential operators should be based on distribution theory. The students can learn about measure theory and topological vector spaces later. It has been my intention in these two volumes to present the fundamental ideas in the most down-to-earth fashion possible. At the same time, I have not hesitated to introduce further ideas that have independent interest, for example transfinite cardinals in the chapter on Hilbert spaces.

Boulder, December 1978 Robert D. Richtmyer