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Physics and mathematics at the frontier

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ABSTRACT **The relation between elementary particle physics and mathematics is explored. The beauty and effectiveness of the mathematical structures that appear in fundamental physics are discussed.**

We celebrate today 100 years of the American Mathematical Society, and thereby American mathematics, by exploring the relations between mathematics and the natural sciences, from elementary particle physics to computer science. I shall be discussing the interaction of physics and mathematics in the field of elementary particle physics. Here, in the exploration of the fundamental laws of nature, mathematics and physics have had the longest union and the most fruitful exchanges.

I should, however, qualify my use of "fundamental," a buzz word that might raise the hackles of my colleagues who object to statements that imply that one field is more fundamental than another. By fundamental I do not mean supreme, preeminent, or dominant but rather basic, elementary, and underlying. In that sense, the fundamental laws of physics are those that we would start with if we were to teach physics in a logical fashion, as opposed to the traditional historical method. In this approach the laws of fluid dynamics would be seen as a consequence of the microscopic laws of classical dynamics, themselves an excellent approximation to the nonrelativistic laws of the quantum mechanics of atoms. The atoms would be understood, in an excellent approximation, by nonrelativistic quantum mechanics that describes electrons interacting with nuclei; the nuclei would be understood as bound states of quarks and gluons—all these ingredients being part of the standard theory of elementary particle physics, which itself (together with the law of gravity) is part of who knows what. It is the business of elementary particle physics to search for the next rung in this ladder, to discover the "who knows what" from which we could deduce our current, somewhat incomplete, description of matter and its interactions. It is this realm of fundamental physics that is intimately intertwined with mathematical research at the frontiers of mathematical study, where new patterns are being discovered and new edifices are being constructed.

This has been true from the beginning of modern physics, when Galileo first enunciated the proposition that the natural language of physics was mathematics. Newton, one of the greatest mathematicians of his day, invented the calculus of infinitesimals to calculate planetary orbits as well as to solve pure mathematical problems. In the following centuries there was little distinction between theoretical physics and mathematics, with many of the greatest contributors—Laplace, Legendre, Hamilton, Gauss, Fourier—being regarded as physicists by physicists and as mathematicians by mathematicians.

The 20th century has witnessed two revolutions in physics and the completion of a theory of ordinary matter and its interactions. Once again we have called on mathematics to supply the tools and framework for this task. When Einstein created general relativity, the dynamical theory of space and time, in 1915, the necessary tools of differential geometry were available. They had been created by Gauss and by Riemann in the previous century. The effect of general relativity on mathematics was electrifying; Riemannian geometry became a central topic of geometry. The development of quantum mechanics built on the understanding of Hilbert spaces and influenced the development of functional analysis. Early particle physics drew heavily on the theory of continuous groups, which itself was partly motivated by the desire to understand the spatial symmetry of crystalline structure.

Nonetheless, during the middle part of this century mathematics and fundamental physics have developed in very different directions with little significant interaction between them. This was due, in part, to an atmosphere of increased abstraction in the mathematics community, as well as an insistence on rigid formal rigor as exemplified by the famous Bourbaki school. (This school, by the way, has had a disastrous effect on the style of mathematics writing, whereby authors are encouraged to remove from the description of their work all traces of intuitive reasoning or any hint at how they arrived at their ideas. This style, which lately is beginning to change, has made it difficult for nonspecialists to follow the progress of modern mathematics.) However, much of the reason for this separation was due to developments in physics. First, the early development of quantum mechanics and the early applications of quantum mechanics to elucidating the structure of matter required little mathematical sophistication. It has been said that "In the 1930s, under the demoralizing influence of quantum theoretic perturbation theory, the mathematics required of a theoretical physicist was reduced to a rudimentary knowledge of the Latin and Greek alphabets" (R. Jost, quoted in ref. 1). These tools largely sufficed for the first applications of quantum mechanics to the study of matter. During the first decades after World War II the vistas of particle physics rapidly expanded. These times were dominated by experimental surprises and theoretical model building required little more than traditional mathematical tools.

This situation changed dramatically 10 years ago when, prompted by decades of experimental exploration, we arrived at the nonabelian gauge theories of the strong, weak, and electromagnetic interactions. These theories are now universally accepted as yielding a complete description of all the interactions of matter at energies and distances that are experimentally accessible at present. This development is surely one of the most remarkable accomplishments of 20th century science. Attention has more recently turned to the exploration of the structure of these theories and to even more ambitious attempts to construct unified theories of all the interactions of matter together with gravity. In the development of these gauge theories—the so-called "standard model"—it has happened that many significant physical problems have led to significant concepts in modern mathematics. Many of these concepts in fact were invented independently by physicists and by mathematicians. Thus, for example, in 1931 Paul Dirac discussed, in one of the most beautiful papers in theoretical physics, the possible existence of elementary magnetic charges—magnetic monopoles. He showed that in quantum mechanics such magnetic monopoles made sense if and only if the product of their charge, g, with the electric charge of the electron, e, was an integer multiple of Planck's constant $\hbar$: $ge = n\hbar$. This was very exciting since it meant that as long as there existed one magnetic monopole in the universe all charges had to be quantized in units of $\hbar/g$. In mathematical terms Dirac had discovered an integer that characterized the topological classification of vector bundles, mathematical constructs that were being invented at about the same time by mathematicians. These concepts have come to play a role of increasing importance in modern gauge theories.

We have borrowed much from modern mathematics but now the debt is being paid back. Methods developed in quantum gauge theories, using so called "instantons," were borrowed a few years ago by Donaldson, Taubes, and Floer to deduce some deep and astounding properties of the geometry of three- and four-dimensional spaces (9, 10). In what is unlikely to be the final chapter in this saga, Witten has recently reinterpreted Donaldson's theory in physical terms, using it to speculate on a new phase of quantum gravity (11). Finally, recent developments in superstring theory, an ambitious theory that attempts to construct a unified quantum theory of matter and gravity, have begun to meet real mathematical frontiers. These theories have attracted much attention from mathematicians since they give strong hints of connections between hitherto separate parts of mathematics. Many physicists believe that the final understanding of the structure of string theory will involve fundamental generalizations of geometry. Perhaps we are entering a golden era in the long history of cooperation between fundamental mathematics and physics. More on this later.

On the Unreasonable Effectiveness of Mathematics in Physics

Eugene Wigner wondered, almost 30 years ago, about the "unreasonable effectiveness of mathematics in the natural sciences." The effectiveness of mathematics in physics is indeed impressive and, all too often, taken for granted. Wigner argued that it is not at all obvious that mathematical concepts are appropriate for the description of natural phenomenon. These concepts are certainly not conceptually simple, conceptual simplicity is not one of the primary goals of mathematics, nor are they necessarily inevitable. However, they are certainly useful. The mathematical formulation of physics often leads to a remarkably accurate description of many phenomena. This record of agreement provides convincing evidence that mathematics is the correct language for physics. Wigner pointed out that "The enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and there is no rational explanation for it. It is not at all natural that 'laws of nature' exist, much less that man is able to discover them. The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve" (2). I suppose he meant by the last remark that if we do not understand it we do not deserve it.

Indeed it is something of a miracle that we are able to devise theories that allow us to make incredibly precise predictions regarding physical phenomena and that we can carry out controlled experiments that allow us to measure these quantities to incredible precision. To give one of the most astounding examples, consider the magnetic moment of the electron, $\mu = g(eh/2mc)\mathbf{S}$. Naively, the gyromagnetic ratio of the electron, g, should equal 2, and the deviation of g from 2 was one of the anomalies that stimulated the development of the relativistic quantum theory of the electromagnetic field. After long, long calculations quantum electrodynamicists can predict and after careful, careful measurements atomic scientists can measure this parameter to one part in hundred billion! The result is [using $\alpha^{-1} = 137.035963(15)$]

$$g_{\text{theory}} = 2\cdot\left[1 + \frac{\alpha}{2\pi} - 0.328478445\left(\frac{\alpha}{\pi}\right)^2 + 1.183(11)\left(\frac{\alpha}{\pi}\right)^3 \ldots\right]$$
$$= 2\cdot[1.000{,}159{,}652{,}459 \pm 0.000{,}000{,}000{,}123]$$
$$g_{\text{experiment}} = 2\cdot[1.000{,}159{,}652{,}193 \pm 0.000{,}000{,}000{,}004].$$

I am not sure which is more impressive, the theoretical accuracy or the experimental accuracy. Experimentally, this accuracy is achieved by the tour de force of trapping a single electron for a very long time in a magnetic-electric bottle (a Penning trap) (3). Theoretically, one must calculate up to fourth order in the perturbative expansion of quantum electrodynamics, with the fourth-order term requiring the evaluation of 891 Feynman diagrams, an equivalent theoretical tour de force (4). The error is totally dominated by the uncertainties in the determination of the fine-structure constant α.

The ability to achieve this kind of precision is dependent on many fortunate circumstances—on our ability to isolate the studied physical phenomena from the environment and on the invariance of basic physics under time and spatial translations so that one can repeat experiments elsewhere at some other time—features lacking in the discussion, say, of social phenomena. However, they surely also depend on this miraculous overlap (or isomorphism, to use the mathematical term) between the pure mathematical structures that underlay quantum field theory and the real, material physical world. Perhaps we can gain some insight into this mystery if we deepen it by examining yet another miracle in the connection between mathematics and physics.

On the Unreasonable Beauty of Mathematics in Physics

The mystery of the effectiveness of mathematics in fundamental physics is much deeper than just the miracle of its astonishing utility. After all, it is no surprise that we need mathematics to deal with complicated situations involving systems composed of many parts, all of which are in themselves simple. We have also learned that even simple systems whose microscopic laws of evolution are easy to describe can exhibit extremely complex behavior. However, we might expect to be able to describe the microscopic laws in terms of simple mathematics. The strangest thing is that for the fundamental laws of physics we still need deep mathematics and that as we probe deeper to reveal the ultimate microscopic simplicity, we require deeper and deeper mathematical structures. Even more, these mathematical structures are not just deep but they are also interesting, beautiful, and powerful. As Dirac put it "It seems to be one of the fundamental features of nature that fundamental physical laws are described in terms of great beauty and power" and "As time goes on it becomes increasingly evident that the rules that the mathematician finds interesting are the same as those that Nature has chosen" (5).

This hyperbole is full of ill-defined terminology: interesting, beautiful, powerful. What do we mean when we say that an equation is beautiful or that a physical concept is powerful? Consider the mathematical formulation of the "standard model," the aforementioned theory of the strong, weak, and electromagnetic interactions, that we believe describes all the constituents of matter and their interactions down to distances of 10^{-15} cm. The action that describes this theory, from which we believe that, in a frenzy of reductionism, we could describe all of low energy physics, is given by the following mathematical expression:

$$
\begin{aligned}
S = \int d^4x\sqrt{g}\Bigg[& \frac{1}{4} g_{\alpha\beta} g_{\gamma\delta}\{A^{\alpha\gamma}A^{\beta\delta} + \mathrm{Tr}B^{\alpha\gamma}B^{\beta\delta} + \mathrm{Tr}C^{\alpha\gamma}C^{\beta\delta}\} \\
& + \frac{\theta_2}{32\pi^2}\mathrm{Tr}B^{\mu\upsilon}\widetilde{B_{\mu\upsilon}} + \frac{\theta_3}{32\pi^2}\mathrm{Tr}C^{\mu\upsilon}\widetilde{C_{\mu\upsilon}} \\
& + \sum_{i=1}^{3} g_{\mu\upsilon}(\overline{Q}_i\gamma^{\mu}D_Q^{\upsilon}Q_i + \overline{L}_i\gamma^{\mu}D_L^{\upsilon}L_i) \\
& + g_{\mu\upsilon}\mathrm{Tr}(D_{\phi}^{\mu}\Phi)^{\dagger}(D_{\phi}^{\mu}\Phi) - V(\Phi) \\
& + \sum_{ija} (\overline{Q}_i\Gamma_{Qa}^{ij}Q_j\Phi^a + \overline{L}_i\Gamma_{La}^{ij}L_j\Phi^a) + R\Bigg].
\end{aligned}
$$

Is this beautiful? Perhaps, but only in the eye of an informed beholder. Clearly the notion of beauty in mathematics, as in art, is an acquired taste. To appreciate mathematical beauty requires long education and training and is always a subjective judgment. Nonetheless, there tends to be a large degree of consensus among mathematicians and physicists as to what is beautiful and what is not. In the above theory there is much that most of us feel is beautiful and there is much that is not. The beautiful parts are those that explain the forces of nature as arising from the powerful symmetry principles that are the essence of these "gauge theories." These are beautiful to physicists since from a simple principle of symmetry we deduce in an almost unique fashion the nature of the forces of nature and the existence of the carriers of these forces—the graviton that underlies gravitational forces, the photon of light, the gluons that hold nuclei together, and the W and Z mesons that are responsible for their radioactive decay. This part of the theory is also beautiful to mathematicians since these gauge theories provide interesting, very interesting as it turns out, mathematical structures—the fiber bundles I referred to above.

The ugly parts are those that describe the strange spectrum of matter. These do not follow from any symmetry principle and must be put in by hand, with many, much too many, parameters to yield agreement with observation. In fact it is largely due to this lack of beauty, as well as the large number of unexplained parameters, 19 all in all, that we believe that this theory is not the end of the story—it is simply not pretty enough. Of course these two defects are correlated. In both mathematics and physics the beauty and power of a concept are strongly correlated. We find concepts and structures beautiful if they enable us to derive new results, understand new phenomena—if they are powerful.

The most beautiful part of the standard model is the idea of local, or gauge, symmetries. These are unlike the more familiar global symmetries of the world, according to which the laws of physics are unchanged if we perform a symmetry transformation on the whole world at once, say by rotating it about some axis. If the world possesses a gauge symmetry one can make local rotations by an amount that might differ from place to place. This symmetry first appeared in Maxwell's formulation of the laws of electromagnetism, although its full significance was not realized until the development of quantum mechanics. A theory that possesses a local gauge symmetry necessarily requires a special field (the gauge field, or mathematically the connection) with which one can connect objects that are separated in space. Associated with the gauge field is a particle and a force that is mediated by this particle. In the case of electromagnetism, the gauge field is simply the electromagnetic field and the associated gauge particle is the photon of light that mediates the electromagnetic forces between charged particles. In the case of the nonabelian generalizations of gauge theory that appear in the standard model, the gauge particles are the gluons and the W and Z particles that mediate the strong and the weak interactions. Mathematically, it is convenient to think of attaching to each point of ordinary space an internal space upon which the local symmetry acts. This combined object is called a fiber bundle and is of central concern to modern mathematics.

C. N. Yang, one of the inventors of nonabelian gauge theories, tells the story of his meeting with the mathematician Chern, who had done pioneering work on the differential geometry of fiber bundles. He relates that when he learned that mathematicians had been talking for years about the identical structure that physicists had discovered he was very surprised. He remarked to Chern that "it is both thrilling and puzzling, since you mathematicians dreamed up these concepts out of nowhere." Chern replied "No, no, these concepts were not dreamed up. They were natural and real" (6). This is a fascinating reply. Chern was expressing a point of view that, from my experience, is not uncommon among creative mathematicians—namely, that the mathematical structures that they arrive at are not artificial creations of the human mind but rather have a naturalness to them as if they were as real as the structures created by physicists to describe the so-called real world. Mathematicians, in other words, are not inventing new mathematics, they are discovering it.

If this is the case then perhaps some of the mysteries that we have been exploring are rendered slightly less mysterious. If mathematics is about structures that are a real part of the natural world, as real as the concepts of theoretical physics, then it is not so surprising that it is an effective tool in analyzing the real world. Similarly, we might expect that physical and mathematical structures would share the characteristics that we call beauty. Our minds have surely evolved to find natural patterns pleasing.

There is an obvious objection to this point of view. Theoretical physicists are constrained by experiment. Their constructions must not only be beautiful and powerful they must also be correct. They must agree with experiment and go beyond mere explanation to successful prediction. Mathematicians seem not to be constrained by these shackles. If physicists are searching for the one logical structure that describes the real world, mathematicians are exploring the space of all conceivable logical structures, only a portion of which overlaps the real, unique world. This is quite correct, nonetheless it does not contradict the idea of a common underlying structure that is a real feature of nature.

If it is the case that the concepts and structures that underlie fundamental mathematics and physics are common, then it might be advantageous for workers in both fields to search for new ideas and structures in each other's back yard. This strategy was promoted and followed by Dirac who said "The research worker in his efforts to express the fundamental laws of nature in mathematical form should strive mainly for mathematical beauty" and "It may well be that the next advance in physics will come along these lines: people first discovering the equations and needing a few years of development in order to find the physical ideas behind the equation" (7). Conversely, mathematicians should study the structures that physicists discover for possible hints of new mathematics. The absorption of structures from physics was of enormous importance in the early development of mathematics and this cross fertilization has recently been revitalized, not just in particle physics but also in the study of chaos in simple dynamical systems, in the discovery of fractal geometry, and in many other examples.

The revitalization of the connections between mathematics and physics is especially true in the realm of elementary particle physics. Recent attempts to construct unified theories of matter and gravity have led to a radically new kind of theory—string theory, which gives hints of essential connections to many frontier areas of modern mathematics. String theory, which was originally discovered accidentally in an attempt to understand nuclear forces, has emerged in recent years as a promising realistic theory of all the interactions and, for the first time, a consistent theory of quantum gravity. To some extent string theory is a simple generalization of the ordinary framework of quantum field theory, in which the basic constituents of nature are not pointlike but are extended one-dimensional objects—strings. Remarkably, this seemingly minor extension from pointlike particles to extended strings, without modifying in any other way the fundamental principles of physics, leads to an incredible structure. This structure implies that the only forces that can exist are just of the kind we see—gravitational and gauge interactions. It can also produce the matter content of the world as we know it as well as the specific pattern of forces that we observe. It also has bizarre implications, requiring that space–time be 10-dimensional. To agree with the crudest of observations, it must be the case that 6 of the spatial dimensions are curled up into a little closed space so that we do not notice them. This can be achieved since, as a generalization of Einstein's theory of general relativity, the theory incorporates the dynamics of space–time and possesses solutions with 6 compact, curled up, directions of space.

String theory has already provided many interesting mathematical connections. The theory makes use of deep structures in differential geometry and in algebraic geometry, connects to the theory of modular functions and finite groups. It even appears to have a place for branches of mathematics that I thought would never play a role in physics—such as number theory and knot theory. I once described this development to a famous mathematician who was intrigued by the theory and the mathematical ideas it drew upon. However, his first question to me was "But is it physics?"

The original, highly optimistic expectation that this theory, which in principle has the power to allow us to calculate all the parameters of the standard model as well as understand the reason behind many of its features, would lead rapidly to new predictions and tests, has undergone sober reevaluation. It is not that there are any experimental contradictions, nor are there any indications of internal inconsistency, rather it is clear that we do not yet know enough about the structure of the theory to control its dynamics sufficiently to make contact with experiment. Part of the problem is that we have stumbled on this theory by accident, without knowing what the basic logical setting for the theory is or will be (it has been said that string theory is of the 21st century, discovered by accident in the 20th century).

A more immediate problem is that in trying to discover the principles of this theory and applying it to the real world to test its validity, we are faced with the fact that the basic distance scale of the theory is very, very small. The fundamental length scale of string theory, or indeed of any unified theory of gravity and matter, is the Planck scale, the length that can be formed from the three-dimensional fundamental constants of nature: Newton's constant of gravitation G, Planck's quantum constant h, and the speed of light c:

$$l_{\text{planck}} = \sqrt{\frac{G}{hc^3}} \approx 10^{-33} \text{ cm}.$$

(Alternatively, we can express this scale in units of time: $t_{\text{planck}} \approx 10^{-44}$ s, or in units of mass: $M_{\text{planck}} \approx 10^{19} M_{\text{nucleon}}$). Unfortunately, this length scale is smaller by 17 orders of magnitude than the smallest distances that we can see with our most powerful microscopes, our most energetic particle accelerators. The fact that this number is so small is responsible for some of the most striking features of our universe. For example, the reason stars are so big is that at the scale of the radius of ordinary atoms and nuclei, gravity is very weak (because this scale is 17 orders of magnitude below the Planck scale). Thus, gravitationally bound aggregates of nuclei can contain approximately $(M_{\text{planck}}/M_{\text{nucleon}})^3 \approx 10^{57}$ nuclei before collapsing.

The value of this number presents us with one of the major problems of theoretical physics, since it is very difficult to give a natural explanation for a number that is so small. In any case it implies that string theory is an attempt to extrapolate far, far beyond present day experiment. Even if we have an idea of the physics at these incredibly small planckian distances, it is very hard to make our way up to the distances at which measurements are done at present. There is a lot of physics that occurs along the way that we must understand if we are to make contact with experiment.

An extrapolation of this enormity is unprecedented in the history of physics. One has every right to express skepticism as to the chances of success of such a risky venture, as some of my colleagues have recently done in a vocal and public fashion. It does little good to remind these critics that at high energy we have learned that the correct scale of distances is logarithmic (i.e., physics changes as the logarithm of the distance scale for very short distances), so that an extrapolation by a factor of 10^{17} is really only a factor of $\log(10^{17}) \approx 40$. It is equally useless to point out that we have no choice but to attempt such an extrapolation if we wish to address fundamental questions.

What is clear is that new strategies are required in today's climate, different from those that we used in previous decades when our field was led by the nose by experimental discovery. Not that we do not expect new experimental discoveries. All unified theories, string theory included, predict much new physics that could be seen at the superconducting supercollider (SSC), which we hope and trust will be built. Experiments with the SSC, although they will not take us to the energy scale at which gravity becomes as strong as the nuclear force, will be of crucial importance in providing clues to the connection between Planck-scale physics and our low-energy world. Without the SSC particle physics will die.

At this moment, however, when we are faced with no experimental surprises or paradoxes and when string theory hints at deep mathematical structures, Dirac's strategy has become more and more appealing. Many string theorists are exploring the mathematical structures that have been thrown up by string theory in the hope that they will provide the underlying framework for the theory and give clues as to its dynamics.

Our critical colleagues denounce these efforts, indeed all of string theory, and call it by the dirtiest name they can come up with—recreational mathematics. Although I resent being called a recreational mathematician, I admit that there is a valid (albeit small) point to these criticisms. They remind us of the danger, in following the diracian dictum, of turning into mathematicians. This for some theorists is an ever-present temptation. This would not be a good outcome for physics nor I suspect for mathematics. Let us remember some of the differences between mathematics and physics.

The bottom line for mathematicians is the proof of their theorems, the logical consistency of their results. The final judge of theoretical physicists is experiment. Personally I feel that experiment is a harsher mistress than consistency. Dirac, motivated as he was by mathematical ideas, nevertheless stated "I am not interested in proofs but only in what nature does." Indeed, when faced with the astounding prediction of his relativistic electron equation that there should exist a positively charged particle with precisely the same mass as the electron and no evidence for such a particle (the positron was discovered by Anderson 5 years later), he contemplated abandoning some of the beautiful symmetry of his theory to identify the positron with the proton, which is 1836 times heavier than the electron. Weyl, who more than any other mathematician in this century saw mathematics and physics as an organic whole said, in contrast "My work always tried to unite the true with the beautiful, but when I had to choose one over the other, I usually chose the beautiful." On the other hand, in the case of the positron, it was Weyl who recognized the charge conjugation symmetry of the Dirac equation and who objected strenuously to the identification of the positron with the proton. In the end the positron was discovered with the same mass as the electron in accord with the symmetry of the Dirac equation. Weyl was proved right and beauty prevailed. In this case truth and beauty were the same, consistent with my message that most of the time there need not be conflict between these two principles. Nevertheless, if a choice between beauty and truth arises each of us must retreat to our individual corners of security.

Mathematicians also think differently and have different habits of work than physicists, even when they are exploring similar structures. Mathematicians love to generalize, to extend their concepts to the most general possible case, to construct the most inclusive possible theory. Physicists are of course interested not in the most general case but in the special case of the real world. They also work by simplification, idealization, and by the construction of specific examples. We might say that mathematicians labor to construct interesting and useful definitions from which good theorems flow, physicists to construct interesting and useful models from which good predictions flow.

Mathematicians and physicists also have different strengths. I find the most remarkable attribute of great mathematicians is their power of abstraction. They are capable of feats of abstraction that leave me breathless. I suspect that mathematicians similarly admire physicists for their intuition, by which they are able to use mathematical formalism much as a poet uses language. Unlike mathematicians they are allowed to neglect the constraints of rigor, to guess what is true without proving it, proceeding as rapidly as possible to confront one's ideas with experiment. The Soviet mathematician Manin agrees: "The choice of a Lagrangian on the unified theory of weak and electromagnetic interactions . . . the introduction of Higgs fields, the subtraction of vacuum expectation values and other sorcery, which leads, say, to the prediction of neutral currents—all this leaves the mathematician dumbfounded" (8).

Finally, physicists and mathematicians are taught to think differently. Even the chronology of the standard curriculum is different for the two fields. Physics is always taught historically, from the bottom up. We start with classical mechanics, then proceed to teach nonrelativistic quantum mechanics, and only at the last stages teach modern, relativistic physics. This allows us to teach our students physical intuition by allowing them to practice on concrete everyday phenomena. Modern mathematics is often taught from the top down. This teaches the power of abstraction. Manin writes that "it would be wonderful to master both types of thinking, just as we master the use of a right and a left hand" (8). This is probably impossible, it must violate some kind of uncertainty principle:

$$\Delta \mathit{Mathematics} \times \Delta \mathit{Physics} \geq C.$$

In any case both approaches are necessary. We need each other's special talents and insights. Let us continue the collaboration and extend it.

But vive la difference!

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