Speaking with the Natives: Reflections on Mathematical Communication

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As an amateur student of both theoretical physics and languages, I have had many occasions to contemplate the problems of transmission of mathematical information between people in different disciplines. What follows is a loosely connected series of observations based on my experience—and a few other things.

It is a truth universally acknowledged that almost all mathematicians are Platonists, at least when they are actually doing mathematics rather than philosophizing about it. As Hardy [8, §22] said, "I believe that mathematical reality lies outside us, that our function is to discover or *observe* it, and that the theorems which we prove, and which we describe grandiloquently as our 'creations', are simply the notes of our observations." One might maintain that mathematicians can create new structures within mathematical reality just as engineers can create new structures within the physical world, but most of us have no trouble with the idea that there *is* such a reality and that our job consists of studying it. Moreover, we are all trained to believe that the universe that encompasses this reality consists of *sets*, and that every respectable mathematical object should possess a precise definition as a set.[1]

It can therefore take the working mathematician by surprise to discover that most nonmathematicians who use mathematics are not Platonists. Nor are they intuitionists or constructivists, eager though people of the latter persuasions might be to claim their allegiance. They are formalists. For them mathematics is the discipline of manipulating symbols according to certain sophisticated rules, and the external reality to which those symbols refer lies not in an abstract universe of sets but in the real-world phenomena that they are studying. As Dirac put it in the first edition of his classic book on the principles of quantum mechanics [5, §7], discussing the symbols that represent the states of a quantum system: "One does not anywhere specify the exact nature of the symbols employed, nor is such specification at all necessary. They are used all the time in an abstract way, the algebraic axioms that they satisfy and the connexion between equations involving them and physical conditions being all that is required."[2]

This point of view has consequences that can cause some perplexity when mathematicians and scientists try to talk to one another. The algebraic axioms to which Dirac refers, for example, amount to the condition that the symbols representing states are the names of vectors in a Hilbert space $\mathcal{H}$ and that other symbols representing observables are the names of self-adjoint operators on $\mathcal{H}$. Mathematicians would usually prefer to have the state space for a specific quantum system identified as a specific concrete Hilbert space, with the important observables described by explicit formulas, and they are disconcerted when these ingredients are missing from the recipe. Physicists, on the other hand, would maintain that committing oneself to such a specific choice at the

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[1] *Some recent developments such as topos theory take a broader view, but they are not part of the standard conceptual toolkit.*

[2] *In later editions of* [5] *Dirac did not make the point quite so baldly. I imagine he had been softened up by conversations with mathematicians, but his basic attitude remained unchanged.*

outset is a tactical error, like committing oneself to a specific coordinate system for describing phenomena on a manifold. If one is studying specific phenomena, one may wish to *represent* $\mathcal{H}$ (that is, to set up an isomorphism between $\mathcal{H}$ and a concrete Hilbert space) in a way that makes the analysis more transparent, but $\mathcal{H}$ itself is just what it is: the state space. (Even introducing the notation $\mathcal{H}$ for the set of state vectors reflects my bias toward the language of set theory; most physics books don't give it a name.)

In a similar vein, when asked to describe a Lie algebra, physicists will usually say something like "a set of generators $X_1, \ldots, X_n$ that satisfy the Lie algebra $[X_i, X_j] = \sum_k c_{ij}^k X_k$". This seems decidedly off-key: the "generators" are what we would call basis elements, and the "Lie algebra" is what we would call the structure equations for the Lie algebra. But it makes sense when one realizes that it is meant as a description not of a set with some additional structure, as mathematicians would expect, but of an algebraic structure unattached to any particular set. Again, one can *represent* it by particular sets (of matrices, for example) if one wishes, but the essence of the structure is independent of such a representation.

There is also a sociological aspect to such dialectal differences. The phrase quoted in the preceding paragraph may prejudice a mathematical reader against the writer in the same way that the statement "I ain't workin' there no more" may lower the speaker in the estimation of one whose normal dialect is standard American English. I believe it works the other way, too: a definition such as "a set equipped with the structure of a vector space together with a bilinear product that is skew-symmetric and satisfies the Jacobi identity" might induce a sentiment in some readers that is the scientific equivalent of "Upper-class twit!" Needless to say, such instinctive responses are an impediment to effective communication.

It must be admitted that there are many situations in which the mathematicians' insistence that mathematical objects should be precisely defined as sets becomes a mere dogma rather than an aid to understanding what is going on. When we are working with the real numbers—solving calculus problems, say—it is rarely helpful to think of a number such as π as a subset of the rationals (the appropriate Dedekind cut) or an equivalence class of Cauchy sequences. Everything that we really need to know in order to use the real numbers is contained in the axioms for a complete ordered field, and the intuitive picture of numbers as points on a line suffices as a concrete model. In a similar vein, if we are thinking about points in the Cartesian plane, most of us would dismiss the assertion that $(1,3) \cap (3,1) = \{1,3\}$ as nonsense, although it is quite correct according to the standard definition of an ordered pair: $(a,b) = \{\{a\},\{a,b\}\}$. People from other disciplines are right to dismiss the invocation of formal set-theoretic definitions in such situations as an exotic tribal ritual with which they need not concern themselves.

On the other hand, scientists' willingness to mathematize on the formal level can sometimes lead them into very murky waters. Perhaps the most egregious examples at present come from quantum field theory. Sixty years after the development of quantum electrodynamics as a successful physical theory, a satisfactory mathematical model for the field operators on which it is based is still lacking, and the non-Abelian gauge field theories that complete the current "standard model" of the elementary particles are no better off. One can treat the much simpler theory of free (noninteracting) fields in a mathematically respectable way, although that is harder than one might expect—the fields are linear maps from the Schwartz space of rapidly decreasing smooth functions on $\mathbb{R}^4$ to the set of unbounded operators on a Hilbert space—but interacting fields in four-dimensional space-time remain in the realm of mathematical fiction. This does not stop the physicists from writing down symbols for them and performing formal calculations at great length to investigate their properties. There is also another approach to quantum field theory through certain "functional integrals" (integrals over infinite-dimensional spaces of classical fields), which also lack a mathematically respectable definition (so far), although they bear some kinship to genuine integrals that are familiar to probabilists. One of the Millennium Prize Problems is, in essence, to rectify this situation; see [10]. But, meanwhile, the mathematician who wants to learn more about elementary particle physics has a problem. My attempts in this direction were frustrated for many years by my getting stuck in the morass of ill-defined concepts, although eventually I found a way to pick out a path around the edges of the swamp. An account of the route can be found in [7].

Other forms of nonrigorousness may cause annoyance and frustration, but they are usually not so unnerving. The most common sort involves the use of informal reasoning that can be made airtight by any competent mathematician who wishes to take the trouble. Arguments involving infinitesimals in calculus are generally of this type, and the opprobrium with which they are regarded in some circles may fairly be regarded as a sort of mathematical prudery. More serious is the use of approximation procedures without proper mathematical justification—that is, without the derivation of error estimates that would guarantee that the claimed approximation is really good enough. But here the appliers of mathematics have a different criterion for success: they ask whether their approximated quantities are close not to some "true" mathematical quantity but to the real-world quantities that they are studying. If so, they are happy, and the approximation must be regarded simply as part of the model. In this connection one must keep reminding oneself that, in applied analysis, even on the fundamental level, approximations and simplifications are *always* part of the picture. Newton's laws of motion, for example, are valid only in inertial frames of reference, and the latter are mythical beasts if one insists on arbitrarily high precision. (A laboratory on the surface of the earth does not qualify because of the nonuniform motion of the planet on which it sits.)

Even pure mathematicians' standards of rigor are not as strict as popular belief would have it; logicians find our claims about insisting on formal proofs quite amusing. (The word "formal," by the way, is slippery. A "formal proof" is supposed to be logically sound, but a "formal calculation" may not be.) In particular, the language in which we couch our arguments has its share of idioms and peculiarities like any other language. Learning these idioms is part of becoming a mathematician, and an unfamiliarity with them is part of the reason why people in other disciplines have trouble. To introduce an example, let me give a quote from V. I. Arnold [1, p. 14]: "An author, claiming that A implies B, must say whether the inverse holds, otherwise the reader who is not spoiled by mathematical slang would understand the claim as 'A is equivalent to B.' If mathematicians do not follow this rule, they are wrong." Arnold may be overstating the case; perhaps the Russian word for "implies" has a slightly different flavor. (He is speaking, in English, about having a paper rejected by a Russian physics journal.) But the fact is that there is a situation in which mathematicians routinely say "if" when they mean "if and only if": in definitions. "A number is called *perfect* if it is the sum of its proper divisors," we say, and we expect the reader to understand that a number is *not* called perfect otherwise.

Another situation in which we commonly suppress part of an assertion is in implications involving variables. When we say

$$\text{(*)} \qquad \text{If } x > 2 \text{ then } x^2 > 4,$$

we are not talking about a particular number x; the statement implicitly contains a universal quantifier "for all real numbers x". Such unspoken quantifiers generally remain in the background without giving any trouble, but they can cause confusion in interpreting statements like (*), and their ubiquity has even led some people who should know better [4, p. 29] to suggest that the words "if … then" are a way of expressing universal quantification. The way to force the universal quantifiers out of the shadows is to consider negations, for the resulting existential quantifiers cannot be elided: the negation of (*) is "There is an x such that $x > 2$ and $x^2 \leq 4$." (I could go on for pages about the perils of expressing quantifications, implications, and negations in ordinary language, but that is another essay.)

Every technical discipline has its own specialized vocabulary, but mathematical jargon is distinguished by its propensity for adopting common words as technical terms. Many people have observed that the discrepancies between common and technical usages of words such as "limit", "group", and "series" present an obstacle (not the only one) to students' mastery of mathematical terminology; see, for example, Boas [2], Edwards and Ward [6], and Hersh [9]. But they can also have unintended consequences for the perception of mathematics by the general public. On April 9, 1975, Congressman Robert Michel addressed the House of Representatives on the possible misuse of taxpayers' money by the National Science Foundation, and he cited several recently approved grants whose significance he found dubious. One of them was a grant for US$27,400 entitled "Studies in complex analysis".[3] To the mathematical reader this title seems innocuous enough, but Michel's reaction [3] was, "Well, for that amount of money I certainly hope it is complex. 'Simple analysis' would, hopefully, be cheaper."

Michel also cited a grant entitled "Measurement of the stratospheric distribution of the fluorocarbons and other constituents of interest in the effect of chlorine pollutants in the ozone layer". He said, "At least in that title there was one word I understood: constituents. And so the thought occurs to me, if one of my constituents should ask me my feelings about [this] project—which I implicitly approve when I vote the NSF appropriation—what in the world could I possibly say?"

Well, what can *I* possibly say? One might have hoped that Michel would have read about holes in the ozone layer in the newspaper. I suppose we should just be grateful that none of the NSF grants that year had to do with perverse sheaves. As far as I know, that unfortunate bit of whimsical nomenclature has not caused us any serious embarrassment yet, but if it does, I suggest that its perpetrators be sentenced to a year of hard labor teaching remedial algebra.

Goethe once quipped that "mathematicians are a sort of Frenchmen; they translate whatever you say into their own language, and forthwith it is something entirely different." At times we may have the same feeling about what others do with the things *we* say. The problems of communication can produce annoyance and frustration, but to an inquisitive mind they offer entertainment and illumination, too.

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[3] *I was one of the junior investigators on it.*

References

[1] V. I. Arnold, From Hilbert's superposition problem to dynamical systems, in E. Bierstone et al. (eds.), *The Arnoldfest: Proceedings of a Conference in Honour of V. I. Arnold for his Sixtieth Birthday*, American Mathematical Society, Providence, RI, 1999, pp. 1–18.

[2] R. P. Boas, Can we make mathematics intelligible?, *Amer. Math. Monthly* **88** (1981), 727–731.

[3] *Congressional Record* **121** (1975), 9510.

[4] J. P. D'Angelo and D. B. West, *Mathematical Thinking: Problem Solving and Proofs* (2nd ed.), Prentice-Hall, Upper Saddle River, NJ, 2000.

[5] P. A. M. Dirac, *The Principles of Quantum Mechanics* (1st ed.), Oxford University Press, Oxford, U.K., 1930.

[6] B. S. Edwards and M. B. Ward, Surprises from mathematics education research: Student (mis)use of mathematical definitions, *Amer. Math. Monthly* **111** (2004), 411–424.

[7] G. B. Folland, *Quantum Field Theory: A Tourist Guide for Mathematicians*, American Mathematical Society, Providence, RI, 2008.

[8] G. H. Hardy, *A Mathematician's Apology*, Cambridge University Press, Cambridge, U.K., 1940.

[9] R. Hersh, Math lingo vs. plain English: Double entendre, *Amer. Math. Monthly* **104** (1997), 48–51.

[10] A. Jaffe and E. Witten, Quantum Yang-Mills theory, in *The Millennium Prize Problems*, J. Carlson, A. Jaffe, and A. Wiles (eds.), American Mathematical Society, Providence, RI, 2006, pp. 129–152.