

VOIR-DIRE IN THE CASE OF MATHEMATICAL PROGRESS

Poincaré wrote very ironically about logicism, so that it can be hard to sort out his views on it. But he declared his faith in logic itself so many times that I think we have to believe him. Some commentators neglect this side of Poincaré, largely viewing him as Russell's opponent and Brouwer's predecessor. To a pedagogical audience in 1899 he said:

If we read a book written fifty years ago, the greater part of the reasoning we find will strike us as devoid of rigor.... One admitted many claims which were sometimes false. So we see that we have advanced towards rigor; and I would add that we have attained it and our reasonings will not appear ridiculous to our descendents....But how have we attained rigor? It is by restraining the part of intuition in science, and increasing the part of formal logic.... Today only one [intuition] remains, that of whole number; all the others are only combinations, and at this price we have attained perfect rigor (Poincaré 1899, 157).

And in 1900 he called the 2nd International Congress of Mathematicians to order. We will look at Hilbert's remarks to the Congress later. But Poincaré asked a plenary session:

Have we finally attained absolute rigor? At each stage of the evolution our fathers also thought they had reached it. If they fooled themselves, do we not likewise fool ourselves? [and he answered] Now in the analysis of today, when one cares to take the trouble to be rigorous, there can be nothing but syllogisms or appeals to this intuition of pure number, the only intuition which can not deceive us. It may be said today that absolute rigor is attained (Poincaré 1900, 121-2).

Of course Poincaré was fooling himself about absolute rigor by just a few years. It came with ZF set theory, or formal meta-mathematics, or anyway it came just a bit later. But however that may be, his pronouncement does not say much about the detailed rigorization of mathematics. I'm more interested in the way Dieudonné agrees with Poincaré on the specific issue of analysis:

what history shows us is a *sectorial* evolution of "rigor". Having come long before "abstract" algebra, the proofs in algebra and number theory have never been challenged; around 1880 the canon of "Weierstrassian rigor" in classical analysis gained wide acceptance among analysts and has *never* been modified.... It was only after 1910 that uniform standards of what constitutes a correct proof became universally accepted in topology ... [credit to Brouwer and Weyl] .. this standard has remained *unchanged ever since* (Dieudonné 1989, 15-6).

Nor has this process stopped. I believe Feynman integrals have not yet been made rigorous. No one thinks it will take a huge shake-up of methods but this active branch of mathematics with important applications has not yet found rigorous form.

I want to focus on a point Poincaré raised at the 4th International Congress of Mathematicians, 1908. He was to talk on “The Future of Mathematics” but needed emergency surgery. So Darboux actually read the essay, including:

In mathematics rigor is not everything, but without it there is nothing. A demonstration which is not rigorous is nothingness. I think no one will contest this truth. But if it were taken too literally, we should be led to conclude that before 1820, for example, there was no mathematics; this would be manifestly excessive; the geometers of that time freely understood [»sous-entendaient volontiers«] what we explain by prolix discourse. This does not mean they did not see it at all; but that they passed over it too rapidly, and to see it well would have necessitated taking the pains to say it (Poincaré 1908, 171).

My abstract says I will explore the relation between content “seen” and the means of “saying”, but the more I worked on this the less I could tell them apart. I’ve run into this before, and it always makes me sympathetic to the formalist and logicist schools that claim modern pure mathematics has no content at all — only expression. I do not think there is a useful distinction here between content and means. Yet there is clearly a difference, and the distinction was very clear at the time, between what a mathematician like Riemann could see and what he could say to anyone outside his circle of students.

THE QUESTION OF AUDIENCE

I think the best approach to this is in terms of audience, and “voir-dire”. “Voir-dire” used to mean testimony under oath and now in U.S. law it means the process of jury selection: the same shift of focus between what is said and to whom it is said. And here too content, the “seen,” is effaced in favor of “saying.” The “voir” in “voir-dire” comes not from the French “to see” but from the Old French “voir” meaning “the truth”, from Latin “verus.”

Progress towards rigor includes finding and filling in gaps in definitions and proofs. That’s what we most often look at. But the other aspect is replacing idiosyncrasy with widely shared means of expression. These are not separate parts — as if you could do one without the other — but inseparable aspects of one effort. This is just the “anti private-rigor” argument. I do not know if there can be a private language of some kind, but at least not of a kind to distinguish rigorous from sloppy reasoning. That’s the force of Wittgenstein’s argument. Rigor has to be shared, and to create shared means of expression is at the same time to create an audience sharing them. Voir-dire as “truly saying” is finally inseparable from voir-dire as finding who will hear the case.

Of course you do not have to pick one audience and stick with it. Poincaré sharply distinguishes expert and popular audiences. His mathematical writing is uncompromisingly difficult, and notoriously ignores the very standards of rigor he praises in his popularization, and even in chatty talks to the International Congress of Mathematicians. It was common by Poincaré’s time for research to conform to

something like Weierstrassian standards, and Poincaré would have none of that. He was not even writing for the average mathematical researcher.

Hilbert has a more unitary view. His popularization does read differently from his research — but even when we say that, we have to deal with his *Foundations of Geometry* (1899). Here is a book any Gymnasium math teacher could read, and yet it is at the same time a famous research work. And who is *Geometry and the Imagination* written for? Apparently for the people Hilbert says need geometric intuition: “not only the research worker, but also anyone who wishes to study and appreciate the results of research in geometry” (Hilbert and Cohn-Vossen 1932, iii).

And Hilbert set the style of 20th century mathematics. Not with his proof theoretic formalism, of course, though I would not claim to say how much that helped. Nor will I try to gauge the success of his school’s broad, orchestrated attack on the nature of mathematics. Most of what I know of that I learned from Peckhaus (1990) and since the author tells me he feels Hilbert’s philosophy was less successful than I do, I will not argue the point. But I do say Hilbert’s working formalism, his axiomatizing practice, has become the normal working style in mathematics — the normal means of saying and thus of seeing. And I claim this has a tremendous unifying effect on mathematics, both within the discipline and in unifying it with other disciplines.

This runs against a lot of current interpretation. Some critics within mathematics see modern style very differently. V.I. Arnol’d is a currently active Russian analyst and geometer very much in Poincaré’s tradition. I first found his views on this in a remark about “the widely spread custom of thinking that replacing $y=f(x)$ by $f:N \rightarrow P$ one obtains a new theorem” (Arnol’d 1976). This is not a claim about category theory, but lets the usual notation of (in this case differential) topologists stand for the modern attitude. He claims that the modern style, which he himself masters when he needs it, too often blocks comprehension of actual mathematics.

And there are historians such as Herbert Mehrtens. Mehrtens’s identification of “modern mathematics” seems fine to me, and he follows Minkowski as I would in calling Hilbert its “General Director.” But his analysis of the audience seems wrong. He says,

The “modern” form of communication in mathematics ... is an expression of the modern social system of mathematics. The form of communication determines a sharp boundary between the system and the outside, and it also tends to sharpen internal boundaries between specialties.... No layman, e.g., in a ministry of education or research, can evaluate what mathematicians do or should do (Mehrtens 1987, 209).

This is also the lead theme of Mehrtens (1990). Are we to believe that Ministers of Education coped better with Riemann and Dedekind in their time than with Deligne and Faltings today? That they were, or felt, or were taken to be more competent at judging mathematics than they are today? I think not. I notice no becoming modesty in congressional discussion of the National Science Foundation. And if the comparison seems unfair because lay Ministers have been replaced by experts running and consulting government funding agencies, I think this counts as well against Mehrtens’s claim. I will come to internal boundaries in the course of the paper.

Indeed, Foucault's "death of the author" theme is more pertinent to modern mathematics than that of institutionalization. Foucault is not just casting about when he says "we could also examine the function and meaning of such statements as 'Bourbaki is this or that person'" (Foucault 1969, 122). And Mehrrens (1990) makes some nice points bearing on this. But what does it really mean for mathematics that, by submerging individual authorship into corporate, Bourbaki became a last bastion of authorship as authorizing role in mathematics? When Dieudonné wrote or spoke as Bourbaki (without getting the collective approval required for mathematical publications under that name) he claimed a different authority than in his own name. And what of Grothendieck's *Séminaire de Géométrie Algébrique*, where numerous contributors wrote under their own name, but these pieces include shared work and in effect the whole thing is often called Grothendieck's?

BEFORE 1820 THERE WAS NO MATHEMATICS

We need to notice how seriously people have taken the idea that 'before 1820 there was no mathematics' because Arnol'd is going to turn this claim around — from a claim of superiority over the past to a reproach against modernism. Of course Poincaré does not make the claim either; he attributes it to a hypothetical audience that takes the demand for rigor too literally. But Dedekind said in 1872 and reaffirmed in 1887 (Dedekind 1963, 22 and 40) that "theorems such as $2^{(1/2)} \cdot 3^{(1/2)} = 6^{(1/2)}$ to the best of my knowledge have never been established before." And Russell in 1901 claimed,

Pure mathematics was discovered by Boole, in a work which he called the *Laws of Thought*. This work abounds in asseverations that it is not mathematical, the fact being that Boole was too modest to suppose his book was the first ever written on mathematics (Russell 1917, 59).

Russell did remark when he reprinted this essay that "the editor [of the American magazine *The International Monthly*] begged me to make the article 'as romantic as possible'" (Russell 1917, 7).

The boast of Dedekind and Russell is a complaint of Arnol'd. Following him we could say that in the most practical sense, for most mathematicians today, there was hardly any mathematics before 1920. They have trouble with things earlier mathematicians did easily, like finding the limit as x goes to 0 of

$$\frac{\sin(\tan(x)) - \tan(\sin(x))}{\arcsin(\arctan(x)) - \arctan(\arcsin(x))}$$

Arnol'd mentions that Gerd Faltings did it quickly, but he claims that this exception just confirms the rule (Arnold 1989, 28). Moreover, contemporary mathematicians cannot recognize old ideas they meet in new sources. I will get to examples later. Arnol'd offers an explanation:

For modern mathematicians it is generally difficult to read their predecessors, who wrote "Bob washed his hands" where they should simply have said: "There is a $t_1 < 0$ such that the image $\text{Bob}(t_1)$ of t_1 under the natural map $t \rightarrow \text{Bob}(t)$ belongs to the set of people having dirty hands..." (Arnol'd 1990, 109).

The passage goes on at length, to make sure hardened modern mathematicians get the idea. Arnol'd uses this level of explicitness when he must. He does not follow Poincaré in disdaining to meet current standards.

PROOFS WITHOUT RIGOR

When Poincaré spoke of old proofs which "strike us as devoid of all rigor" he very likely had Lagrange in mind since their styles are very close. Poincaré's approach is much more like Lagrange's than like Cauchy's, let alone that of Weierstrass.

Lagrange would study a function f by its Taylor series around a fixed point x . Using a variable i he writes:

$$f(x+i) = f(x) + i \cdot p(x) + i^2 \cdot q(x) + i^3 \cdot r(x) + \dots$$

He calls the functions p , q , r and so on "derivative functions" of f . He shows they are proportional to the usual derivatives, so the series exists if and only if f has derivatives at x of all orders in the usual sense. And he shows that the equation holds for small (but explicitly not infinitesimal) values of i . As to assuming f has these derivatives, he says:

This supposition is verified for the various known functions by [actually giving the series]; but no one to my knowledge has tried to prove it *a priori*, which seems to me all the more necessary since there are particular cases in which it is not possible [(Lagrange 1797, 7) and (*Oeuvres* 22)].

He goes on to prove every function has derivatives at every x , and says:

This proof is general and rigorous as long as x and i remain indeterminate; but it may cease to be so when one gives x determinate values... [(Lagrange 1797, 8) and (*Oeuvres* 23)].

Every function is differentiable at all 'indeterminate' points; only 'determinate' ones can cause trouble!

Lagrange found some of his own work "not founded on clear and rigorous principles, but nonetheless correct, as you can assure yourself *a posteriori* [i.e., by examples]" (Lagrange 1772, 451). And the work I've described makes serious mistakes by any standard. To get derivative functions he claims that if $f(0)=0$ then $f(x)$ is divisible by x as a real valued function around 0. But this fails when $f(x) = x^{(1/3)}$. His claim about convergence fails at 0 for $f(x) = \exp(-x^{-2})$, with $f(0) = 0$. These are both functions Lagrange recognized.

But to say the work lacks rigor merely sweeps aside what is going on. In fact the work can be, and was, cleaned up in several directions and most of it is rigorous one way or another. Some may be cleaned up in terms of calculus with the modern epsilon-delta definitions. Some, especially that involving "determinate" versus

“indeterminate” values, may be cleaned up in terms of algebraic geometry using “formal derivatives.” Freudenthal speaks aptly of early algebraic geometry and:

the congenital defects with which it would be plagued for many years — the policy of stating and proving that something holds “in general” without explaining what “in general” means and whether the “general” case ever occurs (Freudenthal 1970, 450).

But such methods eventually became rigorous, taking somewhat longer than the analytic ones.

The cost of this pluralistic strategy is that we must distinguish different sorts of spaces and functions and derivatives and give their relations. The apparatus piles up quickly, as it does all over mathematics today. It requires the extensively explicit notation whose effects Arnol’d deplors. Rather than “unrigorous,” Lagrange’s work is idiosyncratic. With no general standard for analysis in place, each author had to use his own tacit assumptions, which graded imperceptibly into blind spots. It took more people than Lagrange to sort the assumptions out.

This points up the need for Dieudonné’s idea of sectorial rigor even if there is also absolute rigor in some universal foundation. Lagrange could hardly be expected to argue all the way down to an absolute foundation, even if he could have found one. And neither can anyone today, outside of areas very close to foundations themselves. We need practical standards for what may be assumed without comment for a given audience in a given field. And such standards are necessarily communal, not personal.

THE RIEMANN-ROCH THEOREM

Another mathematical example serves several purposes here so we take a moment to state it in mildly anachronistic terms.

A compact Riemann surface is a closed surface with some number of handles. That number is called its “genus.” A sphere has genus 0, a torus or doughnut surface has genus 1, a twist pretzel surface has genus 3. A Riemann surface also has analytic structure, so we can define derivatives of complex functions on it. No non-constant complex function on a compact Riemann surface has a derivative at every point. There have to be at least some points where it goes to infinity. A point where it goes to infinity, like some multiple of $1/z$ when z goes to 0 (but not as fast as $1/z^2$), is called a “simple pole” of the function.

Riemann asked, given a surface S of genus g , how many different (i.e., linearly independent) functions f are there on S with derivatives everywhere except for simple poles at n given points $p_1 \dots p_n$? His answer: There are at least $n-g+1$. His proof: each of the n simple poles gives one degree of freedom in defining f — choosing what multiple of $1/z$ to use. Each of the g handles may cost one degree of freedom — i.e., the differential df gains at least one degree of freedom varying across the handle, but it loses two since df must have integral 0 over each closed loop (and there are two different ways around a doughnut). The $+1$ counts the one dimensional family of constant functions — with no poles and 0 differential.

Riemann used his infamous “Dirichlet principle” to quickly complete the claims about adding degrees of freedom. He knew he had no real proof of this principle about solutions to a certain differential equation. But he told Weierstrass this did not bother him (See Monna 1975, 34). He knew lots of examples and applications and he could see it was right—he freely understood it. Farkas and Kra (1992) do a beautiful modern job following Riemann’s lines. They assume results from measure theory, differential geometry, and algebraic topology. Then to correctly complete Riemann’s strategy (for poles of all orders, which adds no difficulty) takes about 60 pages. This is what Poincaré meant by prolix.

In 1864 Riemann’s student Roch completed the Riemann inequality to an equation now called the Riemann-Roch theorem. Now we come back to those functions with derivatives at all but finitely many points (where they act like some power of $1/z$). On the Riemann sphere, i.e., the surface consisting of the complex plane plus a point at infinity, these have a simple algebraic form. They are all fractions $P(z)/Q(z)$ with $P(z)$ and $Q(z)$ polynomials in the one variable z . We can even define their derivatives purely formally by the product and quotient rules. Similar algebra works for these functions on any compact Riemann surface.

Some people attempted more algebraic proofs of the theorem. Clebsch gave one, saying that after great effort he was unable to understand the Riemann-Roch proof (Tappenden 1995, 15). The important one for current mathematics was given in 1882 by Dedekind and Weber, using ideas they in fact shared with Kronecker, though they did not share his sweeping condemnation of transcendental methods. They defined and proved the Riemann-Roch equation entirely algebraically, without using continuity or limits. They avoided the Dirichlet principle, and all use of analysis. It was clear that Dedekind and Weber’s proof would work for other fields besides the complex numbers. It was more general than the analytic proofs, but this generality was fairly formal at the time. It only applied to a few fields known then, since it did use some special algebraic properties. However, there was no known motive for applying it to them.

Tappenden (1995) looks at the various proofs of Riemann-Roch (and other 19th century mathematics) to elucidate the various meanings of arithmetic and geometry in Frege’s time, and to show that Frege’s search for new proofs of established facts paralleled important work in mainstream mathematics. He finds the main motive for the more general proofs was that,

Different proofs, using different methods, may provide different diagnoses of the nature of the proposition proven. Such concerns will be especially salient if one believes, as Frege does, that a proof may fall short of being fully adequate, even if all the steps are logically cogent, if the proof does not respect the proper logical order of things (Tappenden 1995, 27).

Recall Mehrten’s claim that modern mathematics “tends to sharpen boundaries between specialties” (Mehrtens 1987, 209). In the 19th century it seemed normal that leading mathematicians who preferred an analytic approach to Riemann-Roch should not even understand an algebraic approach to the same theorem, and vice versa. There remain stylistic schisms in mathematics today, and people sometimes genuinely

question recent major proofs. But the kind of “boundaries” set up in the 19th century even around this one theorem do not exist today.

By the mid-twentieth century the Riemann-Roch theorem had been generalized to all fields, and since then it has been extended to other structures (in effect families of fields varying over some space). I’ll come back to it at the end.

HILBERT’S STYLE

The work on Riemann-Roch was one of several ways that Dedekind began modern abstract algebra. Poincaré was very friendly to axiomatics in geometry. He loved to defend non-Euclidean geometry by saying “A mathematical entity exists, provided its definition implies no contradiction” (Poincaré 1921, 61). But he never took to abstract, axiomatic algebra. Since Poincaré’s fondness for groups has come up several times here I will mention that these are always transformation or permutation groups. And even then, if he knows a given one to be commutative he prefers not to call it a group but a “faisceau”. What we now call an abstract Abelian group he just called a “system with addition like arithmetic”.

This brings us to the second most famous speaker at the 1900 International Congress of Mathematicians, David Hilbert. Here is an excerpt from his talk to the Congress, the “Mathematical Problems:”

It is an error to believe that rigor in the proof is the enemy of simplicity.... The very effort for rigor forces us to find simpler methods of proof I should like on the other hand to oppose the opinion that only the concepts of analysis, or even those of arithmetic alone, are susceptible of a fully rigorous treatment.... wherever mathematical ideas come up, whether from the side of the theory of knowledge or in geometry, or from theories of natural or physical science, the problem arises for mathematics to establish them upon a simple and complete system of axioms...in no respect inferior to those of the old arithmetical concepts (Reid 1970, 78-9).

Here is axiomatization as a uniform format for sectorial rigor, a means of simplifying, and explicitly a means of relating mathematics to other fields. In mathematics it was propagated especially by Emmy Noether, and popularized largely through her student van der Waerden. We can also see this attitude in Noether’s mathematical physics — the attempt to find a simple universal description of what lies behind many different conservation laws.

This is the approach canonized by Bourbaki. Dieudonné tells us,

the Bourbaki treatise was modeled in the beginning on the excellent algebra treatise of van der Waerden. I have no wish to detract from his merit, but as you know, he himself says in his preface that really his treatise had several authors, including E. Artin and E. Noether, so that it was a bit of an early Bourbaki (Dieudonné 1970, 136).

I hope by now it is known that Emmy Noether has far the largest share in creating this algebra [see (Kimberling 1981)].

COUNTERATTACK

Did Hilbert simplify mathematics? I claim this is a non-question. As Carl Linderholm puts it in *Mathematics Made Difficult*:

Simplicity is relative. To the great majority of mankind — mathematical ignoramuses — it is a simple fact for instance that $17 \times 17 = 289$, and a complicated one that in a principle ideal ring a finite subset of E suffices to generate the ideal generated by E . For the reader and for others among a select few, the reverse is the case (Linderholm 1971, 9).

That is, once you are comfortable with the terminology, it is simpler to say one is finite than to say $17 \times 17 = 289$. In fact ideal theory was one of the first modern achievements, and Linderholm updates Dedekind's joke, writing to Fröbenius, when he describes his proof with Weber of the Riemann-Roch theorem as "this long work, but easy to read for 'idealists'" [letter to Fröbenius 8 June 1882, in (Dugac 1976, 278)].

Spivak deals with the same question of simplicity. He gets a series of classical theorems on integrals as trivial applications of a modern form of Stokes's theorem, itself proved by trivial calculations. However, it "cannot be understood without a horde of difficult definitions.... There are good reasons why the theorems should all be easy and the definitions hard" (Spivak 1965, ix). For better or worse, and I think for better, this is the style that has descended from Hilbert through Noether and Bourbaki: develop enough terminology that it will suggest the results.

Arnol'd stands out as warning against getting too comfortable with the terminology, and against shoving difficulties to the fore in definitions. Incidentally, Arnol'd does not believe that mathematics has isolated itself by these debilities. He thinks it has infected physicists with them. Arnol'd (1990) is full of examples of 17th century mathematics unrecognized by mathematicians today because it is not written out as explicitly as we expect. Newton showed that, in a gravitational field, any given initial state of motion of a body (with less than escape velocity) fits into an elliptic orbit. So he claimed Kepler's first law followed from the inverse square law of gravity.

But who said, ask the physicists experienced in the mathematical niceties, that there does not exist any other trajectory satisfying the same initial conditions along which the body can move, observing the law of universal gravitation, but in a completely different way? ...

That is, the physicists think Newton proved existence of elliptic orbits without proving uniqueness.

"In fact, all this argument is based on a profound delusion," Arnol'd writes (1990, 31). The delusion is not (what we might have expected) anachronistically thinking that since we are concerned with badly behaved vector fields with discontinuous first derivatives and non-unique trajectories, Newton should have been too. Arnol'd points out that Newton's elliptic solutions are explicitly given, and explicitly depend smoothly on initial conditions. And Newton knew that if the solutions are smooth they themselves yield a coordinate system in which the force field is constant. Then uniqueness is obvious. Newton's proof is rigorous in the modern setting. The delusion is thinking he ought to labor the point as we do.

But I'm afraid Arnol'd's advice on this is the counsel of perfection. I'm even afraid Arnol'd means it that way. His most striking historical discovery is that "In the *Principia* there are two purely mathematical pages containing an astonishingly modern topological proof of a remarkable theorem on the transcendence of Abelian integrals." Unfortunately it was

incomprehensible both from the viewpoint of his contemporaries and also for those twentieth century mathematicians brought up on set theory and the theory of functions of a real variable who are afraid of multi-valued functions (Arnold 1990, 83).

Since it escaped the 19th century as well, the proof was evidently unrecognizable to any reader of the *Principia* until Arnol'd. That's quite possible. But then it seems pointless to blame it on modern set theory. Arnol'd's solution to the weakness of modern mathematics, as he sees it, is just that mathematicians should work very much harder. Fine advice, but not really an alternative to Hilbert's style.

HILBERT'S SUCCESS

The first major success of Hilbert's method outside mathematics was von Neumann's axiomatization of quantum mechanics, which fed back into pure mathematics as an impetus to functional analysis. And while Weyl's famous work on group theory and quantum mechanics is not an axiomatization of physics it relies centrally on axiomatic theories of groups, vector spaces, and topology; Weyl's work is if anything more visible than von Neumann's in today's particle physics. With economists applying game theory and fixed point theory, and engineering using wavelets, and people all over campus now turning from chaos theory to complexity theory, I can not believe modern mathematics has sharpened its boundaries against the outside. Nor can I agree with those who claim this is not 'modern' mathematics.

Mehrtens proposes that since the sixties, modern mathematics has ceded to a postmodern focus on "heterogeneous specific problems" (1990, 20). I think he is right about the passing of one phase in modern mathematics, and his rough timing for it is plausible, but I do not see a shift from grand theory to special problems. His claim strikes me as an excessively direct transcription of common views of postmodernism into the history of mathematics.

Others have spoken of a turn from theory to applications. But I see rather a unification of theory with applications. If Grothendieck's scheme theory is applied to the security of computer codings, if cohomology is basic to Penrose's twistor program, and to handling semi-simple Lie groups for supersymmetry in string theories of particles: do we see here a turn from theory to application? Wiles spent years on Fermat's last theorem, and finally proved it. This is a particular problem. But he attacked it by making a major step in Langland's sweeping program for unifying number theory and function theory that starts with the whole Grothendieck apparatus.

I will close with the recent evolution of the Riemann-Roch theorem. This is paradigmatically modern mathematics, building a vast machinery of high level

abstraction. It is a key element in the work prompting Siegel to write to Mordell in 1964:

I am afraid that mathematics will perish before the end of this century if the present trend for senseless abstraction — as I call it: theory of the empty set — cannot be blocked up. Let us hope your review [of Lang's book *Fundamentals of Diophantine geometry*] may be helpful (Lang 1995, 340).

Lang naturally takes the opposite view. He says “drawing closer together various manifestations of what goes under the trade name of Riemann-Roch has been a very fruitful viewpoint over decades” (Lang 1995, 344). At least four Fields Medals are directly tied to it — Grothendieck in 1966, Atiyah in 1966, Quillen in 1978, and Faltings in 1986 (for proving Mordell's conjecture: if an algebraic equation with rational coefficients defines a complex surface with genus 2 or more, then it has at most finitely many rational solutions). Finding mathematicians who say abstraction has gone too far is like finding people (as Resnik has mentioned) who say society is going downhill. Such people can have very good points. But the process does not stop, and I think the overall objection is misplaced.

Lang stresses number theory and algebraic geometry but also mentions partial differential equations and “Thus comes a grand unification of several fields of mathematics, under the heading of the code word Riemann-Roch” (Lang 1995, 347). It also lies behind the Atiyah-Singer index theorem, used for conformal fields and gauge field theory in physics. Marquis argues that K-theory, a Riemann-Roch descendent, is a “tool” for mathematicians rather than an object of “mathematical reality” (Marquis, 1997, 262). The chief basis for this is that K-theory plays a unifying role for theories which are not then subsumed into it. Marquis emphasizes applications in topology.

None of this work on new versions of Riemann-Roch or K-theory is easy. But it is hardly meant to isolate the experts from the rest of us. The greatest achievement is to solve a problem no one else could, in a way that is easy for everyone to understand once you have done it. Nor is this work meant to separate mathematics from the outside. For evidence, look at the expert expository efforts made for these sort of results in the volume, *From number theory to physics*, (Waldschmidt 1992).

ACKNOWLEDGMENTS

This work is supported in part by a grant from the National Endowment for the Humanities.

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