lativitätstheorie erwiesen, und auch von der Entwicklung der Spektraltheorie durch Hilbert bis zu ihrer Anwendung in Schroedingers Quantenmechanik vergingen zwanzig Jahre, aber umso bemerkenswerter waren die Früchte.

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Mordell's review, Siegel's letter to Mordell, diophantine geometry, and 20th century mathematics

by Serge Lang

In 1962, I published *Diophantine Geometry.* Mordell reviewed this book [Mor 1964](note 1), and the review became famous. Immediately after the review appeared, in that same year, Siegel wrote a letter to Mordell to express his agreement with Mordell's review concerning the overall nature of the book, and to express more generally his negative reaction to trends in mathematics of the 1950's and 1960's. I learned of Siegel's letter to Mordell only in the seventies by hearsay, without knowing its precise content. At that time, in a letter dated 11 December 1975, I wrote to Siegel to tell him I got the message, and I sent a copy of my letter to many people. There was considerable gossip about Siegel's letter to Mordell, but I saw the letter for the first time only in March 1991, when I received from Michel Waldschmidt a copy which he made from the original in the Cambridge library of St John College.

Siegel's letter is a historical document of interest from many points of view. I would like to deal here with one of these points of view having to do with the relation between number theory and algebraic geometry, or what has come to be known as the number field case and the function field case. I shall document part of the 20th century history of the way these two cases have benefited from each other, and the extent to which both Mordell and Siegel failed to understand the accomplishments of the fifties and sixties in connection with them (note 2). Among other things, Siegel wrote to Mordell:

- that "the whole style of the author [of *Diophantine Geometry*] contradicts the sense for simplicity and honesty which we admire in the works of the masters in number theory..";
- that "just now, Lang has published another book on algebraic numbers which, in my opinion, is still worse than the former one. I see a pig broken into a beautiful garden and rooting up all flowers and trees";
- that "unfortunately, there are many 'fellow travelers' who have already disgraced a large part of algebra and function theory";
- that "these people remind [Siegel] of the impudent behaviour of the national socialists who sang: 'Wir werden weiter marschieren, bis alles in Scherben zerfällt!'";
- and that "mathematics will perish before the end of this century if the present trend for senseless abstraction - I call it: theory of the empty set - cannot be blocked up".

I shall also deal with some concrete instances of the more general problem mathematicians face in dealing with advances in mathematics which may pass them by.

§1. From Dedekind-Weber to the Riemann Hypothesis in function fields over finite fields

The analogy between number fields and function fields has been realized since the latter part of the 19th century. Kronecker was already in some sense aware of some of its aspects. Dedekind originated a terminology in his study of number fields which he and Weber applied to function fields in one variable [Ded-W 1882]. Hensel-Landsberg then provided

Göttingen, March 3, 1964

Dear Professor Mordell:

Thank you for the copy of your review of Lang's book. When I first saw this book, about a year ago, I was disgusted with the way in which my own contributions to the subject had been disfigured and made unintelligible. My feeling is very well expressed when you mention Rip van Winkle!

The whole style of the author contradicts the sense for simplicity and honesty which we admire in the works of the masters in number theory — Lagrange, Gauss or, on a smaller scale, Hardy, Landau. Just now Lang has published another book on algebraic numbers which, in my opinion, is still worse than the former one. I see a pig broken into a beautiful garden and rooting up all flowers and trees.

Unfortunately there are many „fellow-travelers" who have already disgraced a large part of algebra and function theory; however, until now, number theory had not been touched. These people remind me of the impudent behaviour of the national socialists who sang: „Wir werden weiter marschieren, bis alles in Scherben zerfällt!"

I am afraid that mathematics will perish before the end of this century if the present trend for senseless abstraction — I call it: Theory of the empty set — cannot be blocked up. Let us hope that your review may be helpful.

I still remember the nice time we had together during your visit in Göttingen. —

With best wishes, also to Mrs. Mordell,

Carl Siegel

a first systematic book treatment of basic facts concerning these function fields [Hen-L 1902], using the Dedekind-Weber approach. Artin in his thesis [Art 1921] translated the Riemann hypothesis to the function field analogue (actually for quadratic fields). Several years later F. K. Schmidt treated general analytic number theory including the functional equation of the zeta function for function fields of arbitrary genus [Schm 1931]. However, Artin thought that the Riemann hypothesis in the function field case would be as difficult as in the classical case of the ordinary Riemann or Dedekind zeta function (he told me so around 1950). It was Hasse in 1934 and 1936 who pointed out the "key for the problem" in the function field case through the theory of correspondences, as Weil writes in [Wei 1940]. (Hasse also indicated another way through reduction mod p using complex multiplication in characteristic zero.) Hasse himself proved the Riemann hypothesis (Artin's conjecture) for curves of genus 1 [Has 1934], [Has 1936] (note 3). Then Deuring pursued the higher dimensional generalization of Hasse's theory of endomorphisms on elliptic curves and correspondences [Deu 1937], [Deu 1940], by showing that some results of Severi [Sev 1926] could be proved so that they applied in characteristic p, especially to curves over finite fields. Weil went much further than Hasse and Deuring in this direction. "Directly inspired" [Wei 1948a, p. 28] by works of Severi [Sev 1926] and Castelnuovo [Cas 1905], [Cas 1906], [Cas 1921], Weil developed a purely algebraic theory of correspondences and abelian varieties; and he formulated the positive definiteness of his trace (which he related to Castelnuovo's equivalence defect), thus yielding the Riemann- Hypothesis in the function field case for curves of higher genus [Wei 1940], [Wei 1948a], [Wei 1948b].

Hasse also defined a zeta function for arbitrary varieties over number fields and conjectured its analytic continuation and functional equation. This point of view was promoted by Weil in the fifties. There was a serious problem of algebraic geometry even dealing with varieties of higher dimension over finite fields, let alone number fields, because as Weil conjectured, the analogue of the functional equation and Riemann hypothesis in this case would depend on finding algebraic analogues of homology groups (homology functors) satisfying the Lefschetz fixed point formula [Wei 1949].

In the forties and fifties several subjects in mathematics, including algebraic topology and algebraic geometry, systematically developed new foundations and internal results. Indeed, homological algebra developed first from algebraic topology, but soon saw its domain of applications extend to several other fields including algebraic geometry. This algebra was affectionately called "abstract nonsense" by Steenrod (with a quite different intent and meaning from Siegel's "senseless abstraction"). A large body of material, recognized to be fairly dry by some of its creators, had to be systematically worked out to provide appropriate background for more extensive applications. The dryness was unavoidable.

One also saw the simultaneous development of commutative algebra. One of its motivations was that in the context of algebraic geometry, a curve over a number field can be defined by equations whose coefficients lie in the ring of algebraic integers, and so can be viewed as a family of curves obtained by reducing mod p for all primes p, or even mod p^n for higher n so as to include infinitesimal properties. This case can be unified with the case of algebraic families of curves over an arbitrary field, including curves over the complex numbers. Furthermore, one wants to treat higher dimensional varieties in the same fashion, in an algebraic and analytic context. For the analytic context one is led to work over power series rings, and more generally over complete local rings because of the presence of singularities and the infinitesimal aspects. One was also led to globalize from modules to sheaves, in a context involving both homological algebra and commutative algebra, thus leading to further abstractions.

These developments were a prelude to the subsequent conceptual unification of topology, complex differential geometry and algebraic geometry during the sixties, the seventies, and beyond. For such a unification to take place, it was necessary to develop not only a language, but an extensive theory containing very substantial results as well, starting with commutative algebra and merging into algebraic geometry. In the fifties and sixties, these developments appeared as "senseless abstractions" to some people, including Siegel, who writes as if these developments deal only with the "theory of the empty set". But it is precisely the insights of Grothendieck which led to an extension - including an abstraction - of algebraic geometry whereby he defined the cohomology functors algebraically; whereby he proved the Lefschetz formula [Gro 1964]; and whereby finally a decade later, Deligne finally proved the analogue of the Riemann hypothesis for varieties in the higher dimensional case [Del 1974]. Deligne also proved related applications, because in a Bourbaki seminar talk [Del 1969], he had previously shown how to reduce the Ramanujan-Petersson conjecture for eigenvalues of modular forms under Hecke operators to this Riemann hypothesis, in a direction first foreseen by Sato and also using some insights of Kuga-Shimura, to whom he refers at the begining of his Bourbaki seminar talk. A very short and clear account of the ideas, leading from a classical problem involving the partition function to the most advanced uses of Grothendieckian algebraic geometry, is given in the first two pages of [Del 1969].

§2. Some implications

We now pause a moment to consider some implications of these great developments. As I wrote in my 1961 review of Grothendieck's *Éléments de géometrie algébrique* [Lan 1961] a decade before Deligne's applications of Grothendieckian geometry occurred: "The present work...is one of the major landmarks in the development of algebraic geometry...Before we go into a closer description of the contents of Chapter 0 and I [*which were just appearing and prompted the review*] it is necessary to say a few words explaining why the present treatise differs radically in its point of view from previous ones." I then mentioned four specific points like those already listed above: the need to deal with algebraic families of varieties, applications to number theory and reduction modulo a prime power, defining algebraically the functors from topology such as homology and homotopy, and the study of non-abelian coverings. I also emphasized throughout the importance and far reaching implications of Grothendieck's functorial point of view.

I ended my review as follows: "To conclude this review, I must make a remark intended to emphasize a point which might otherwise lead to misunderstanding. Some may ask: If Algebraic Geometry really consists of (at least) 13 Chapters, 2,000 pages [*it turned out to be more like 10,000*], all of commutative algebra, then why not just give up? The answer is obvious. On the one hand, to deal with special topics which may be of particular interest only portions of the whole work are necessary, and shortcuts can be taken to arrive faster to specific goals...But even more important, theorems and conjectures still get discovered and tested on special examples, for instance elliptic curves or cubic forms over the rational numbers. And to handle these, the mathematician needs no great machinery, just elbow grease and imagination to uncover their secrets. Thus as in the past, there is enough stuff lying around to fit everyone's taste. Those whose taste allow them to swallow the *Elements,* however, will be richly rewarded." Thus I did not see the developments of Grothendieck's algebraic geometry as incompatible with doing beautiful or deep mathematics with only a minimum of knowledge.

Five years later, when I wrote to Mordell the letter reproduced in [Lan 1970] and [Lan 1983], I continued to take such a balanced view. Since Mordell had written in his review: "When proof of an extension makes it exceedingly difficult to understand the simpler cases, it might sometimes be better if the generalizations were left in the journals" (see below for the context of this judgment), I replied:

"I see no reason why it should be prohibited to write very advanced monographs, presupposing substantial knowledge in some fields, and thus allowing certain expositions at a level which may be appreciated only by a few, but achieves a certain coherence which would not otherwise be possible.

"This of course does not preclude the writing of elementary monographs. For instance, I could rewrite Diophantine Geometry by working entirely on elliptic curves, and thus make the book understandable to any first year graduate student (not mentioning you)...) Both books would then coexist amicably, and neither would be better than the other. Each would achieve different ends. [*In fact, I eventually wrote Elliptic Curves: Diophantine Analysis, Springer Verlag, 1978.*]

"...When I write a standard text in Algebra, I attempt something very different from writing a book which for the first time gives a systematic point of view on the relations of diophantine equations and the advanced contexts of algebraic geometry. The purpose of the latter is to jazz things up as much as possible. The purpose of the former is to educate someone in the first steps which might eventually culminate in his knowing the jazz too, if his tastes allow him that path. And if his tastes don't, then my blessings to him also. This is known as aesthetic tolerance. But just as a composer of music (be it Bach or the Beatles), I have to take my responsibility as to what I consider to be beautiful and write my books accordingly, not just with the intent of pleasing one segment of the population. Let pleasure then fall where it may."

Thus I advocated "aesthetic tolerance" - which is certainly absent from Siegel's letter, to say the least.

It is of course not only a matter of "taste" or "aesthetic tolerance". It may also have to do with one's natural limitations. For instance, I had my own limitations vis a vis Grothendieck's work (and other works). Having gone through Weil's *Foundations* right after my PhD, I myself was unable later to absorb completely Grothendieck's work, and I was unable to read much of that work, as well as some of its applications, such as those by Deligne [Del 1969], [Del 1974]. However, I did not put down Grothendieck's work. I admired it (as quoted above), and merely regretted my own limitations. I also could not read the Italian geometers myself and I needed van der Waerden and Weil as intermediaries in algebraicizing and modernizing italian geometry.

§3. Diophantine results over number fields and function fields

Next we consider diophantine questions over the rationals or over number fields. At the turn of the century, Poincaré defined the "rank" of the group of rational points on an elliptic curve over the rational numbers [Poi 1901]. By "rank" he actually meant something different from what we mean today. Roughly speaking, he meant the smallest number of generators of the set of rational points using the secant and tangent method to generate points. Poincaré wrote as if this rank is always finite. The finite generation was proved by Mordell [Mor 1921], and again Weil extended this result to abelian varieties over number fields using more algebraic geometry in his thesis at the end of that decade [Wei 1928]. The analytic parametrization of abelian varieties, and especially Jacobians of curves, was a convenient tool at the time, and for this particular application a complete algebraization of curves and their Jacobians was not yet needed.

At the purely algebraic level, the fifties saw a clarification of the Mordell-Weil theorem and its relations to the algebraic-geometric situation in the function field case. The Artin-Whaples product formula of the forties [Ar-W 1945] was the number theoretic analogue of the geometric theorem that a rational function on a curve has the same number of zeros and poles (counting multiplicities), or in higher dimension that the degree in projective space of the divisor of a rational function is zero. I used this product formula as the basic axiom for the theory of heights in *Diophantine Geometry,* applicable simultaneously to the number field and function field case, in any dimension. Mordell complained that here "we have definitions which many other authors do not find necessary". However, varieties over number fields have their analogues in algebraic families of varieties over any field, especially over the complex numbers. Rational points have their analogues in sections of such families, and in fact *are* sections when the proper language and setting has been defined. The analogy has been interesting and fruitful not only because it has allowed techniques to go back and forth enriching the two cases, but because for instance in the study of algebraic surfaces, cases occur systematically when these varieties are generically fibered by curves of genus 1. One then wants to know which fibers have rational points, and how many. In case the generic fiber of an algebraic family is an abelian variety, the sections form a group, and Lang-Néron proved that this group is finitely generated modulo the subgroup of "constant" sections [La-Ne 1959], this being the function field analogue of the Mordell-Weil theorem. Furthermore, Severi long ago conjectured that the algebraic part of the first cohomology group, i.e. the group of divisors modulo algebraic equivalence, was finitely generated (Theorem of the Base), and he had the intuition that such a result was also analogous in some way to the Mordell-Weil theorem. Néron proved Severi's conjecture [Ner 1952], and Lang-Néron established an actual isomorphism between the Néron-Severi group of a variety, and a subgroup of the group of sections (modulo constant sections) of the Jacobian of the generic curve in some projective imbedding. These results of the fifties formed the backbone of my book *Diophantine Geometry,* but were viewed as "senseless abstraction...the theory of the empty set" by Siegel.

Using Weil's results, and his own results on diophantine approximations (Thue-Siegel theorem), Siegel proved that an affine curve of genus at least 1 over a number field has only a finite number of integral points [Sie 1929]. In [Lan 60] and in another part of *Diophantine Geometry,* I also showed how the Thue-Siegel-Schneider-Roth theorem and Siegel's theorem on integral points had analogues in the function field case. The interdependence between the number field case and the function field case lies not only in the analogy of results and methods applicable to both cases, but also in the fact that when, say, a curve depending on parameters defined by a family of equation $f_t(x, y) = 0$ has solutions in polynomials $x = x(t)$ and $y = y(t)$, such polynomials may have complex coefficients, or in a more arithmetic setting they may have ordinary integer coefficients. In the latter case, by specialization of the parameter t in integers, one obtains integral solutions of the specialized equation. It is a problem to classify all surfaces which admit such a generic fibration by rational curves, over the complex numbers and over the ordinary integers. More generally, one can consider the case when x and y are integral affine algebraic functions rather than polynomials. In order to treat both the number field and function field case simultaneously, there developed a language and results which are now natural throughout the world. At the time, this language and results appeared unnatural or worse to some people. As Siegel wrote to Mordell: "The whole style of the author contradicts the sense for simplicity and honesty which we admire in the works of the masters of number theory".

In part of the proof of Roth's theorem, it is necessary to solve certain linear equations with upper bounds on the size of the solution. A lower bound on the number of solutions is required in the number field case, and a lower bound on the dimension of the space of solutions is required in the function field case. Classically, the Riemann-Roch theorem on curves provides the desired estimates in the function field case, and I drew the analogy explicitly with the number field case by an appropriate axiomatization, whereby I treated both cases simultaneously. But Mordell states in his review: "The author claims to follow Roth's proof. The reader might prefer to read this which requires only a knowledge of elementary algebra and then he need not be troubled with axioms which are very weak forms of the Riemann-Roch theorem." But drawing closer together various manifestations of what goes under the trade name of Riemann-Roch has been a very fruitful viewpoint over decades. Already in [Schm 1931] we see the Riemann-Roch theorem closely related to the functional equation of the zeta function in the function field case. In the thirties, Artin recognized the functional equation of the theta function as an analogue of Riemann-Roch in the number field case. Following the ideas in a course of Artin, Weissinger gave the connection between Riemann-Roch and the functional equation of L-functions in the function field case [Weis 1938]. Weil went further by giving an analogy of Riemann-Roch not only to the problem of counting lattice points in parallelotopes, but also by formulating an analogue for Cauchy's residue formula in the number field case [Wei 1939]. In my book on algebraic number theory, I emphasized the Riemann-Roch viewpoint in these ways. First I gave a formula for the number of lattice points in adelic parallelotopes, asymptotic with respect to the normalized volume; and second, I reproduced the formulation and proof of the functional equation for the zeta function and L-functions via the adelic method in Tate's thesis, especially the adelic Poisson summation formula having as corollary what was properly called by Tate a number theoretic Riemann-Roch theorem (note 4). But Siegel found my book on algebraic numbers "-still worse than the former one". Nevertheless, I shall continue below to describe the ever expanding extent to which the Riemann-Roch umbrella covers aspects of number theory and algebraic geometry.

Naturally, to deal simultaneously with the number field and function field case in diophantine geometry, I had to assume the basic language and results of algebraic geometry and abelian varieties. Mordell in his review of the book complained: "Let us note some of the concepts required in the chapter. There are a 'K/k-trace of A', a 'Theorem of Chow', 'Chow's Regularity Theorem', 'Chow Coordinates', 'compatibility of projections and specializations', 'blowing up a point', 'Albanese Variety', 'Picard variety', 'Jacobian of a curve,' 'Chow's theory of the $k(u)/k$-trace'. When proof of an extension makes it exceedingly difficult to understand the simpler cases, it might sometimes be better if the generalizations were left in the Journals." I ask: exceedingly difficult to whom? Current readers and subsequent generations can evaluate for themselves Mordell's admonition to leave what he calls "generalizations" to the journals. But Mordell went on: "The reviewer was reminded of Rip Van Winkle, who went to sleep for a hundred years and woke up to a state of affairs and a civilization (and perhaps a language) completely different from that to which he had been accustomed." Siegel accepted the comparison with Rip Van Winkle when he wrote to Mordell: "My feeling is very well expressed when you mention Rip Van Winkle." In particular both Siegel and Mordell had difficulty understanding some basic notions of algebraic geometry as recalled above. But these notions were of course accepted without further ado by younger mathematicians and by other schools of mathematics and algebraic geometry, notably by the Russian school, whose contributions to diophantine geometry were to dominate the sixties and seventies, as we shall now indicate.

Mordell himself in [Mor 1922] had conjectured that a curve of genus at least 2 over the rational numbers has only a finite number of rational points. In [Lan 1960] and in *Diophantine Geometry* I translated this conjecture into the function field analogue, to the effect that for an algebraic family of such curves, there is only a finite number of sections unless the family is constant, in a suitable sense. Independently, Manin had already started his investigations of the Picard-Fuchs differential equations and their connections with algebraic families of curves, their Jacobians and their periods, via horizontal differentiation and the Gauss-Manin connection [Man 1958]. Manin put these two mathematical threads together by proving the function field analogue of the Mordell conjecture via his differential methods [Man 1963]. We note in passing that the function field analogue of Siegel's theorem on integral points is needed to complete that proof. (See [Col 1990].) Manin's work kindled various people's interests in various directions lying between algebraic geometry and the theory of algebraic differential equations. Furthermore in 1970-1971 Deligne proved the semisimplicity of the action of the monodromy group on the cohomology of a family of projective smooth varieties [Del 1972]. After Coleman pointed out that Manin's "theorem of the kernel" had not been completely proved [Col 1990], Deligne's theorem was applied by Chai to complete the proof, independently of the application to the Mordell conjecture in the function field case [Cha 1990]. Even more recently, Buium has pursued the application of differential algebra in this direction and he has obtained a substantial extension of results showing that the intersection of a curve with certain subsets of its Jacobian defined by algebraic differential conditions is finite [Bui 1991].

I learned of Manin's proof on a trip to Moscow in 1963, and I lectured on it at the Arbeitstagung in Bonn upon returning. Grauert was in the audience, and was then led to find another proof of the function field case of Mordell's conjecture [Gra 1965] (see among others the final remarks of the introduction to his paper). Grauert's method also involved horizontal differentiation, taking the derivative of a section into the projectivized tangent bundle. For the latest development of this method in a quantitative direction, see Vojta [Vo 1991]. Grauert's proof also worked in characteristic p, as pointed out by Samuel [Sa 1966]. For further insight in the problem in characteristic p, see Voloch [Vo 1990].

To this day, no one has seen how to translate Manin's or Grauert's proofs of Mordell's conjecture from the function field case to the number field case. However, in the early sixties, Shafarevich conjectured that over a number field, given a finite set of places, there exists only a finite number of isomorphism classes of curves of given genus at least 1 and having good reduction outside this finite set [Sha 1963]. In 1968, Parshin showed how Shafarevich's conjecture implied Mordell's conjecture [Par 1968], and he proved the analogue of Shafarevich's conjecture in the function field case (under an additional technical condition, later removed by Arakelov [Ara 1971]). Parshin's proof was based entirely on the intersection theory of surfaces, without making use of horizontal differentiation. This provided hope for an eventual translation to the number field case. As we have already mentioned, a curve over the ring of integers of a number field can be viewed as a family of curves obtained by reduction mod p for all primes p. In a fundamental paper, Arakelov showed how to complete such a family of curves over a number field by introducing the components at infinity, and by defining a new type of divisor class group taking the components at infinity into account [Ara 1974]. With this point of view, a curve over the ring of integers of a number field is called an arithmetic surface. Whereas the Artin-Whaples product formula had been the starting point for unifying the case of number fields and function fields in one variable, Arakelov theory laid the foundations for unifying intersection theory on arithmetic surfaces and the classical intersection theory, thus making Parshin's method more accessible to the number field case (note 5).

Arakelov defined intersection numbers at infinity as the values of Green's functions, and made extensive use of hermitian metrics on line bundles [Ara 1974]. His foundations could lead in several directions. In one direction, inspired by the basic idea of carrying out algebraic geometry with complete objects, including the components at infinity and the metrized line bundles, Faltings gave his proof of Mordell's conjecture a decade later [Fal 1983] (note 6). Be it noted that Faltings also depended on the full-fledged abstractions of contemporary algebraic geometry, for instance by using techniques of Raynaud [Ray 1974], reducing modulo a prime power (actually mod p^2), to bound the degrees of certain isogenies of abelian varieties.

We now come back to the Riemann-Roch theme. In the direction of algebraic geometry, the Italian algebraic geometers dealt classically with the Riemann-Roch theorem on algebraic surfaces. Hirzebruch in the early fifties gave an entirely new slant to the theorem by his formula expressing the (holomorphic-algebraic) Euler characteristic as a polynomial in the Chern classes, for non-singular projective varieties of arbitrary dimension [Hir 1956]. Thus Hirzebruch drew together algebraic geometry, topology, and complex differential geometry. Siegel did not appreciate Hirzebruch's mathematics any more than some other mathematics of the period. Indeed, Siegel was the principal factor causing the collapse of negotiations between Göttingen and Hirzebruch in the fifties, when Hirzebruch was in the process of returning to Germany after his stay in America. Furthermore, in 1960, there was an early attempt to create a Max Planck Institute to be headed by Hirzebruch. Siegel wrote negatively about Hirzebruch and his mathematics in this connection (note 7).

Later in the fifties, Grothendieck vastly extended Hirzebruch's Riemann-Roch theorem partly by formulating it in such a way that it applies to families and partly by making the theorem more functorial [Bor-S 1957]. Still later, he further expanded the formulation of the theorem so that in particular, it applied over arbitrary Noetherian rings, and therefore could be used in the number theoretic context over the ring of algebraic integers of a number field [Gro 1971]. These extensions required the full fledged abstractions of algebraic geometry and algebraic topology which he had developed, including both the cohomology functors and the K-theory functors [Gro EGA], [Gro SGA]. An especially interesting application of Grothendieck Riemann-Roch was made by Mumford in his contributions to the theory of moduli spaces for curves and abelian varieties [Mum 1977].

Just before Faltings proved Mordell's conjecture, he developed Arakelov theory so far as to give an arithmetic version of the Riemann-Roch theorem on arithmetic surfaces [Fal 1984]. This version was vastly extended recently by Gillet-Soulé, for varieties of arbitrary dimension, putting together the Hirzebruch-Grothendieck Riemann-Roch theorems, the complex differential geometry inherent in the components at infinity, and also the theories of real partial differential equations most recently developed by Bismut, necessary to handle the analogues of Green's functions in the higher dimensional case [Gi-S 1990], [Gi-S 1991]. Thus comes a grand unification of several fields of mathematics, under the heading of the code-word Riemann-Roch. At the moment, a complete translation of Parshin's proof of Mordell's conjecture from the function field case has not yet taken place. It still requires a proof of an inequality conjectured by Parshin in the number field case, whose known analogue in the case of algebraic surfaces evolved from work of van de Ven, Bogomolov, Parshin, Miyaoka and Yau [Par 1989] (see also [Voj 1988]). Such an inequality is related to the so-called Noether formula in the theory of algebraic surfaces. It is known that such an inequality implies Fermat's theorem for all but a finite number of cases, which cases depending on how effectively the Parshin inequality can be proved.

The Riemann-Roch story in its arithmetic context does not end there. Vojta, in a major development, showed how to globalize and sheafify on curves of higher genus the basic ideas of the proof of Roth's theorem, in such a way that he found an entirely new proof of Mordell's conjecture (Faltings' theorem) [Voj 1990]. Be it noted that Vojta first gave his proof in the function field case, using intersection theory on surfaces [Voj 1989]. He then translated his proof to the number field case using the Arakelov type intersection theory and the newly found (asymptotic) arithmetic Riemann-Roch theorem of Gillet-Soulé. Although Bombieri subsequently simplified Vojta's proof by eliminating the Arakelov part [Bom 1990], he still used the classical Riemann-Roch theorem on surfaces. The use of Riemann-Roch in one form or another occurs at the same point in the pattern of proof as in Roth's theorem, but of course in the more sophisticated context of curves of higher genus and their products, rather than the projective or affine line. Vojta's idea and a heavy dose of algebraic geometry were then used by Faltings to prove a conjecture of mine dating back to [Lan 1960], concerning higher dimensional diophantine analogues for subvarieties of abelian varieties [Fal 1990]. Neither Vojta, Bombieri nor Faltings has shown that he is "troubled" about using Riemann-Roch theorems, and major breakthroughs have thus been made by expanding the perspectives on old problems, rather than by narrowing the viewpoint to "simpler cases".

Thus we see that since the translation of the Riemann hypothesis in the twenties and the very first translations of the Mordell-Weil theorem from the number field case into the function field analogue in the fifties, there has been constant interaction between the number field case and the function field case. A number of subsequent results have been proved first in the function field case, using geometric intuition and methods from algebraic geometry as well as differential geometry. In some, but not yet all cases, these proofs could then be translated back to the number field case, thus giving new results in number theory.

§4. Further implications

Mordell and Siegel were great mathematicians, a fact which is made obvious once more by their great theorems cited repeatedly in this article. But their lack of vision and understanding at certain periods of their life obstructed the development of certain areas of mathematics in their own countries. Of course they did not have absolute power. In England, Atiyah could develop the Riemann-Roch theme in the topological and analytic direction for elliptic operators on vector bundles, for instance, but the direction of number theory in England was seriously affected by Mordell's obstructions. In Germany, Hirzebruch could create an independent center in Bonn, but Siegel did have an effect in Göttingen and some other places, although his influence has waned to the point where I don't see it explicitly any more.

In the Soviet Union and France, the obstructing influence of Mordell or Siegel in algebra and algebraic geometry was nil. The development of Grothendieck's school in France needs no further comment. In the Soviet Union, one sees the absence of obstructing influence in the existence of the school of algebraic geometry created by Shafarevich. One also sees the absence of obstructing influence in concrete instances, such as the introductions to Manin's and Parshin's papers [Man 1963] and [Par 1968] (as mentioned in note 5). Furthermore, for the Russian translation of *Fundamentals of Diophantine Geometry,* I was asked if it was OK with me to omit the appendices consisting of Mordell's review and my review of his book, and to replace them with an appendix by Parshin and Zarhin describing previous work of theirs on a net of conjectures (Mordell-Shafarevich-Tate), as well as the latest developments concerning Faltings' proof of these conjectures. I agreed without reservations.

In the United States, the influence is more complex to evaluate. Be it noted here only that as recently as December 1989, in the context of my continued activities concerning the non-election of Samuel P. Huntington to the National Academy of Sciences, MacLane wrote me a letter commenting in part on my own 1986 election to the NAS: "I welcomed your election to the NAS. But please observe that if some social scientist had then known and used Mordell's famous comments on your Diophantine book plus the silly mistakes in the last chapter of your Differential Manifolds plus...you too would have been soundly defeated on the floor of the Academy."

Mordell used to pull out Siegel's letter from his wallet to show people, to my knowledge without receiving comments that both his and Siegel's attitudes were parochial and blind (if not worse) (note 8). Thus some members of the mathematical community behaved with "collegiality" and bowed to authority, in the face of claims such as those quoted at the beginning of this article. Those members of the mathematical community who did not stand up to Mordell and Siegel are not entirely blameless for the obstructing influence of Mordell's review and Siegel's letter, such as it was.

Notes:

1. I reproduced Mordell's review *in toto* as an appendix to the greatly expanded version *Fundamentals of Diophantine Geometry* [Lan 1983] because I wanted future generations to evaluate his position for themselves. I also reproduced my review of his book [Lan 1970], including a letter which I wrote to Mordell in 1966.

2. For an account of results and conjectures in current diophantine geometry, much more systematic and complete than I can give here, as well as looking to the future rather than the past, see my book *Number Theory III: Diophantine Geometry,* Encyclopedia of Mathematics Vol. 60, Springer Verlag, 1991.

3. In 1932-1933 Davenport and Hasse started collaborating on a classical paper concerning Gauss sums [Dav-H 1934]. Davenport had previously been concerned with Gauss sums, and he learned from Hasse the connection with the Riemann hypothesis in function fields as formulated by Artin. I find it appropriate to quote here a historical comment made by Halberstam, who edited Vol IV of Davenport's collected works, and states p. 1553: "In fact, Davenport spent part of the academic session 1932-33 with Hasse in Marburg; he obviously learnt a great deal from Hasse (cf. [8], [18], [27]) - in later years he would say that he had not learnt nearly as much as he would have done if he had been 'less pig-headed' - and it seems that he in turn sharpened Hasse's interest in the arithmetical questions discussed above. ...According to Mordell, Hasse was led to his proof of (5) [*RH in elliptic function fields*] in response to a challenge from Davenport to produce a concrete application of abstract algebra."

4. Be it said in passing that the adelic method of Tate's thesis was to become standard in the treatment of analogous situations on linear algebraic groups.

5. Parshin himself was quite aware of the historical context in which he was writing, and gives a very different perspective from Mordell and Siegel, as we find in the introduction of [Par 1968]: "Finally when $g > 1$, numerous examples provide a basis for Mordell's conjecture that in this case $X(\mathbb{Q})$ is always finite. The one general result in line with this conjecture is the proof by Siegel that the number of integral points (i.e., points whose affine coordinates belong to the ring $\mathbb{Z}$ of integers) is finite. These results are also true for arbitrary fields of finite type over $\mathbb{Q}$. Fundamentally this is because the fields are global, i.e., there is a theory of divisors with a product formula, which makes it possible to construct a theory of the height of quasi-projective schemes of finite type over K. Lang's book [*Diophantine Geometry*] contains a description of that theory and its application to the proof of the Mordell and Siegel theorems. It appears that further progress in diophantine geometry involves a deeper use of the specific nature of the ground field. This is confirmed by Ju. I. Manin's proof of the functional analogue of Mordell's conjecture."

6. In his thesis [Wei 1928] Weil refers explicitly to "Mordell's conjecture", and states that "it seems confirmed to some extent" by Siegel's theorem on the finiteness of integral points on curves of genus at least 1. In [Wei 1936] he makes a similar evaluation without reference to Mordell: "On the other hand, Siegel's theorem, for curves of genus > 1, is only the first step in the direction of the following statement: *On every curve of genus* > 1, *there are only finitely many rational points.*" However, some forty years later, he inveighed against "conjectures", when he wrote [Wei 1974]: "For instance, the so-called 'Mordell conjecture' on Diophantine equations says that a curve of genus at least two with rational coefficients has at most finitely many rational points. It would be nice if this were so, and I would rather bet for it than against it. But it is no more than wishful thinking because there is not a shred of evidence for it, and also none against it." Finally in comments in his collected works made in 1979 (Vol. III, p. 454), he goes one better: "Nous sommes moins avancés à l'égard de la 'conjecture de Mordell'. Il s'agit là d'une question qu'un arithméticien ne peut guère manquer de se poser; on n'apperçoit d'ailleurs aucun motif sérieux de parier pour ou contre." First, concerning a "question which an arithmetician can hardly fail to raise", I would ask when? It's quite a different matter to raise the question in 1921, as did Mordell, or decades later. As for the statements in 1974 and 1979 that there is no "shred of evidence" or "motif sérieux" for Mordell's conjecture, they not only went against Weil's own evaluations in earlier decades, but they were made after Manin proved the function field analogue in 1963; after Grauert gave his other proof in 1965; after Parshin gave his other proof in 1968, while indicating that Mordell's conjecture follows from Shafarevich's conjecture (which Shafarevich himself had proved for curves of genus 1); at the same time that Arakelov theory was being developed and that Zarhin was working actively on the net of conjectures in those directions; and within four years of Faltings' proof.

7. In June 1991 I wrote to the President of the Max Planck Society to ask for a copy of Siegel's letter so that one has primary sources on which to base factual historical reporting. I received a friendly answer and the letter was sent to me. Siegel wrote four and a half pages, discussing institutes in general, and giving his evaluation of Hirzebruch in particular, as follows: "Was den zum Schluß vorgeschlagenen Leiter des zu gründenden

Instituts betrifft, so habe ich auch darüber eine abweichende Meinung,.. Seine [Hirzebruchs] mathematischen Leistungen wurden allerdings damals auch hier ziemlich hoch bewertet, insbesondere wegen seiner Jugend. Jetzt erscheint es mir aber zweifelhaft, ob sich das von ihm bisher bearbeitete sehr abstrakte Gebiet weiter erschliessen und fruchtbar machen lässt, und ich halte es für möglich, ja sogar für wahrscheinlich, daß diese ganze Richtung sich schon in wenigen Jahren totlaufen wird. Nach den vorhergehenden Ausführungen möchte ich Ihre Fragen 1), 2), 3) und 5) mit **Nein** beantworten."

As for others, according to a letter from Behnke to Hirzebruch dated 7 September 1960: "Im übrigen liegen von Ihnen außer von Siegel nur die glänzendsten Gutachten vor. Es gibt jetzt zwei Hauptbedenken: 1) Man darf Sie nicht aus dem Universitätsleben nehmen, weil die Lücke nicht zu ersetzen ist... 2) Es würde nur die abstrakte Mathematik gepflegt..." Courant was among those who wrote along these lines: "Hirzebruch ist sicherlich einer der allerbesten unter den Mathematikern der jüngeren Generation. Ich bin stets für ihn eingetreten und hege sehr freundschaftliche Gesinnungen für ihn. Er ist einer der besten Dozenten, die ich kenne. Nach meiner Meinung würde es ein schweres Unrecht an der Mathematik sein, ihn aus seiner produktiven Lehrtätigkeit herauszureißen. Außerdem würde er als Hauptleiter des Max Planck Institutes die Präponderanz der abstrakten Richtung weithin sichtbar symbolisieren. Leistungen und Renommée würden dies im Moment wohl rechtfertigen. Aber, auch in Hirzebruchs eigenem Interesse, und sicherlich in dem der Wissenschaft rate ich dringend davon ab. Es ist nicht nötig, das Institut in einer solchen persönlichen Art zu organisieren, um den höchsten Grad der Wirksamkeit zu erreichen..." But Courant also added: "Meine Bemerkungen sind nicht sorgfältig ausgearbeitet. Sie brauchen nicht vertraulich behandelt zu werden..." Thus Courant also expressed himself with caution. Courant made his letter public at the time.

Some letters to the Max Planck Society were unreservedly for the creation of the Institute, for instance van der Waerden's. After listing Hirzebruch's qualities in all directions (mathematical, personal, and administrative), he asks: "Was will man mehr?" For more on the history of the Max Planck Institute, see Schappacher [Scha 1985].

8. For instance, Mostow remembers distinctly Mordell showing Siegel's letter about me to those members of the math department at Yale in the sixties, when they were at dinner at the local restaurant Mori's. An instructor at Yale today, Jay Jorgenson, heard gossip about this letter when he was a freshman at the University of Minnesota several years ago. An official of the National Science Foundation was shown the letter by Mordell some 25 years ago in Washington. And so it goes on and on.

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Interview mit dem Vorsitzenden des Wissenschaftsrates

Zum ersten Mal wurde ein Mathematiker als Vorsitzender des Wissenschaftrates gewählt, Herr Prof. **Karl-Heinz Hoffmann** *von der TU München. Im Anschluß an die Euler-Vorlesung im Mai in Potsdam hatte ich Gelegenheit mit ihm über seine neue Tätigkeit zu sprechen.*
Gerd Fischer

Was ist der Wissenschaftsrat, was sind seine Aufgaben, wie ist er zusammengesetzt?

Er wurde im Jahre 1957 durch ein Abkommen des Bundes und der damals 11 Länder gegründet. Als wissenschaftspolitisches Beratungsgremium des Bundes und der Länder erarbeitet der Wissenschaftsrat Empfehlungen zur inhaltlichen und strukturellen Entwicklung der Hochschulen, der Wissenschaft und der Forschung. 1969 wurde ihm die Aufgabe übertragen, Empfehlungen zum Rahmenplan für den von