A 1940 Letter of André Weil on Analogy in Mathematics

Translated by Martin H. Krieger

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For André Weil, "having a disagreement with the French authorities on the subject of [his] military 'obligations' was the reason [he] spent February through May [of 1940] in a military prison." When he was released, he went into the service. Weil wrote this fourteen-page letter to Simone Weil, his sister, from Bonne-Nouvelle Prison in Rouen in March 1940, sixty-five years ago this month. (Keep in mind that the letter was not written for a mathematician, even though Simone could not understand most of it.)

I first heard of the letter from a small passage translated in a book by D. Reed (*Figures of Thought*; London: Routledge, 1995). At the time I was trying to understand the range of solutions to the Ising model in mathematical physics, and in going to Weil's letter I found poignant his exposition of a threefold analogy out of Riemann and Dedekind, one that proves to organize a great deal of disparate material. Moreover, I had just begun to appreciate the significance of the Langlands Program for my problem. [See the "Notes Added in Proof" to Martin H. Krieger, *Constitutions of Matter: Mathematically Modeling the Most Everyday of Physical Phenomena* (Chicago: University of Chicago Press, 1996), pp. 311–312.] Eventually, in chapter 5 of *Doing Mathematics*, I worked out the analogy and provided an exposition of the Weil letter. A recent *Notices* article ("Some of what mathematicians do", November 2004, pp. 1226–1230) summarizes the argument of that book, including what I called the Dedekind-Weil analogy.

The Weil letter is a gem, of wider interest to the mathematical and philosophical community, concerned both with the actual mathematics and with how mathematicians describe their work. I provided a translation from the French in the book's appendix. I am grateful to the editor of the *Notices;* publication herein will allow for an even wider audience.

The letter is from André Weil, *Oeuvres Scientifiques, Collected Papers*, volume 1 (New York: Springer, 1979), pp. 244–255. The translation aims to be reasonably faithful, not only to the meaning but also to sentence structure. Brackets are in the *Oeuvres Scientifiques* text. Braces indicate footnotes therein. My editorial insertions are indicated by braces-and-brackets, {[ ]}. It is slightly revised, as taken from Martin H. Krieger, *Doing Mathematics: Convention, Subject, Calculation, Analogy* (Singapore: World Scientific, 2003), pp. 293–305. In the notes to the *Oeuvres Scientifiques*, Weil indicates that he was wrong then about the influence of the theory of quadratic forms in more than two variables and that Hilbert is explicit about the analogy in his account of the Twelfth Problem (for which see David Hilbert, "Mathematical problems", *Bulletin of the American Mathematical Society* **37**, 2000, 407-436).

While this article was in proof, Philip Horowitz sent me his unpublished translation of the letter, which I had not known of before. I am grateful to Horowitz for allowing me to use his translation to improve mine in a number of places.

—Martin H. Krieger

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March 26, 1940

Some thoughts I have had of late, concerning my arithmetic-algebraic work, might pass for a response to one of your letters, where you asked me what is of interest to me in my work. So, I decided to write them down, even if for the most part they are incomprehensible to you.

The thoughts that follow are of two sorts. The first concerns the history of the theory of numbers; you may be able to understand the beginning; you will understand nothing of what follows that. The other concerns the role of analogy in mathematical discovery, examining a particular example, and perhaps you will be able to profit from it. I advise you that all that concerns the history of mathematics in what follows is based on insufficient scholarship, and is derived from an a priori reconstruction, and even if things ought to have happened this way (which is not proven here), I cannot say that they did happen this way. In mathematics, moreover, as much as in any other field, the line of history has many turning points.

With these precautions out of the way, let us start with the history of the theory of numbers. It is dominated by the law of reciprocity. This is Gauss's *theorema aureum* (?I need to refresh my memory of this point: Gauss very much liked names of this sort, he had as well a *theorema egregium*, and I no longer know which is which), published by him in his *Disquisitiones* in 1801, which was only beginning to be read and understood toward 1820 by Abel, Jacobi, and Dirichlet, and which remained as the bible of the number theorist for almost a century. But in order to say what this law is, whose statement was already known to Euler and Legendre [Euler had found it empirically, as did Legendre; Legendre claimed more in giving a proof in his *Arithmetic*, which apparently supposed the truth of something which was approximately as difficult as the theorem; but he complained bitterly of the "theft" committed by Gauss, who, without knowing Legendre, found, empirically as well, the statement of the theorem, and gave two very beautiful proofs in his *Disquisitiones*, and later up to 4 or 5 others, all based on different principles.]: it is necessary to backtrack a bit in order to explain the law of reciprocity.

Algebra began with the task of finding, for given equations, solutions within a given domain, which might be the positive numbers, or the reals, or later the complex numbers. One had not yet conceived of the ingenious idea, characteristic of modern algebra, of starting with an equation and *then* constructing ad hoc a domain in which it has a solution (I have a fair amount to say about this idea, which has shown itself to be extremely productive; moreover, Poincaré has somewhere or other some beautiful thoughts, a propos of the solution by radicals, on the general processes whereby, after having searched for a long time and in vain to solve this problem by a foreordained procedure, mathematicians inverted the question and began to develop adequate methods). The problem had been solved subsequently for all second-degree equations which had solutions in negative numbers; when the equation had no solution, the usual formula having led to the imaginaries, about which there remained many doubts (and it was thus until Gauss and his contemporaries); just because of the suspicion of these imaginaries, the so-called Cardan and Tartaglia formula for the solution of the equation of the 3rd degree in radicals produced some discomfort. Be that as it may, when Gauss began the *Disquisitiones* with the notion of congruences for building up his systematic exposition, it was also natural to solve congruences of the second degree, after having solved those of the first degree (a congruence is a relationship among integers a, b, m, which is written $a \equiv b$ modulo m, abbreviated $a \equiv b \pmod{m}$ or $a \equiv b(m)$, meaning that a and b have the same remainder in division by m, or $a - b$ is a multiple of m; a congruence of the first degree is $ax + b \equiv 0(m)$, of the second degree is $ax^2 + bx + c \equiv 0(m)$, etc.); the latter lead (by the same procedure through which one reduces an ordinary second degree equation to an extraction of roots) to $x^2 \equiv a \pmod{m}$; if the latter has a solution, one says that a is a quadratic residue of m, if otherwise, a is a non-residue (1 and -1 are residues of 5, 2 and -2 are non-residues). If these notions were around for some time before Gauss, it was not necessary that they be associated with a notion of congruence; for the notions presented themselves in diophantine problems (solutions of equations in integers or rationals) which were the object of Fermat's most important work; the first degree diophantine equations, $ax + by = c$, are equivalent to first degree congruences $ax \equiv c \pmod{b}$; the second degree equations of the type studied by Fermat (decompositions in terms of squares, $x^2 + y^2 = a$, and equations $x^2 + ay^2 = b$, etc.) are not equivalent to congruences, but congruences and the distinction between residues and non-residues play a large role in his work, in truth they did not appear explicitly in Fermat's work (it is true that we do not possess his proofs, but he seems to have employed other principles about which we can make some approximate inferences), but which, as far as I know (based on second-hand evidence) were already well in evidence in Euler.

The law of reciprocity permits us to know, given two prime numbers p, q, whether q is or is not a (quadratic) residue of p, if one knows already whether, (a) p is or is not a residue of q; (b) if p and q are respectively congruent to 1 or -1 modulo 4 (or for $q = 2$, if p is congruent to 1, 2, 5, or 7, modulo 8). For example, $53 \equiv 5 \equiv 1 \pmod 4$, and 53 is not a residue of 5, *therefore* 5 is not a residue of 53. Since the problem for non-primes leads naturally to the problem for primes, this law gives an easy means of determining if a is or is not a residue of b as soon as one knows their prime factorization. But this "practical" application is insignificant. What is crucial is there be *laws*. It is obvious that the residues of m form an arithmetic progression of increment m, for if a is a residue, it is the same for all $mx + a$; however it is beautiful and surprising that the prime numbers p *for which* m is a residue are precisely those which belong to certain arithmetic progressions of increment $4m$; for the others m is a non-residue; and what is even more amazing, if one recalls on the other hand that the distribution of prime numbers in any given arithmetic progression $Ax + B$ (which one knows from Dirichlet will have an infinity of primes as long as A and B are relatively prime) does not follow any other known law other than a statistical one (the approximate number of primes which are $\leq T$, which, for a given A, is the same for any B prime to A) and appears, for each concrete case that one examines numerically, to be as "random" as a list of numbers generated by a roulette wheel.

The rest of the *Disquisitiones* contains above all:

1. the definitive theory of quadratic forms in 2 variables, $ax^2 + bxy + cy^2$, having *among other* consequences the complete resolution of the problem which gave birth to the theory: to know if $ax^2 + bxy + cy^2 = m$ has solutions in integers.

2. the study of the n-th roots of unity, and, as we would say, the Galois theory of *the fields given by these roots* and their subfields (all without using imaginaries, nor other functions other than the trigonometric ones, and ending up with the necessary and sufficient conditions for the regular n-gon being constructible by ruler and compass), which appeared as an application of earlier work in the book, as preliminary to the solution of congruences, on the multiplicative group of numbers modulo m. I will not speak of the theory of quadratic forms of more than two variables since it has had little influence until now on the general progress of the theory of numbers.

Gauss's subsequent research was to study cubic and biquadratic residues (defined by $x^3 \equiv a$ and $x^4 \equiv a \pmod{m}$); the latter are a bit simpler; Gauss recognized that there were no simple results to be hoped for by staying within the domain of ordinary integers and it was necessary to employ "complex" integers $a + b\sqrt{-1}$ (a propos of which he invented, at about the same time as Argand, the geometric representation of these numbers by points on a plane, through which all doubts were dissipated about the "imaginaries"). For the cubic residues, it was necessary to have recourse to the "integers" $a + bj$, a and b integers, $j =$ the cube root of 1. Gauss recognized as well, and even thought (there is a trace of this in his notes) of studying the domain of the n-th roots of unity, at the same time thinking to try to provide a proof of "Fermat's theorem" ($x^n + y^n = z^n$ is impossible), which he suspected would be a simple application (that is what he said) of such a theory. But then he encountered the fact that there was no longer a unique prime decomposition (except for i and j, as 4th and 3rd roots of unity, and I believe also for the 5th roots).

There are many separate threads; it would take 125 years to unravel them and assemble them anew into a new skein. The great names here are Dirichlet (who introduced the zeta functions or L-functions into the theory of quadratic forms, through which he proved among other things that every arithmetic progression contains an infinity of primes; but above all, since that time we have only needed to follow his model in order to apply these functions to the theory of numbers), Kummer (who elucidated the fields generated by roots of unity by inventing "ideal" factors, and went far enough in the theory of these fields in order to obtain some results on Fermat's theorem), Dedekind, Kronecker, Hilbert, Artin. Here is a sketch of the picture that results from their efforts.

I cannot say anything without using the notion of a field, which according to its definition, if one limits oneself to its definition, is simple (it is a set where one has in effect the usual "four elementary {[arithmetic]} operations," these having the usual properties of commutativity, associativity, distributivity); the algebraic extension of a field k (it is a field k', containing k, of which all elements are roots of an algebraic equation $\alpha^n + c_1\alpha^{n-1} + \cdots + c_{n-1}\alpha + c_n = 0$ with coefficients $c_1, \ldots, c_n$ *in* field k); and finally the *abelian* extension of a field k; that means an algebraic extension of k whose *Galois group* is abelian, that is to say commutative. It would be illusory to give a fuller explanation of abelian extensions; it is more useful to say that they are almost the same thing, but not the same thing, as an extension of k obtained by adjoining n-th roots (roots of equations $x^n = a$, a in k); if k contains for whatever integer n, n n-th distinct roots of unity then it is exactly the same thing (but most often one is interested in fields which do not have this property). If k contains n n-th roots of unity (for a given n), then all abelian extensions of degree n (that is to say, having been generated by the adjunction to k of *one* root of an equation of degree n) can be generated by m-th roots (where m is a divisor of n). Abel discovered this idea in his research on equations solvable by radicals (Abel did not know of the notion of the Galois group, which clarifies all these questions). It is impossible to say here how Abel's research was influenced by Gauss's results (see above) on the division of the circle and the n-th roots of unity (which lead to an *abelian* extension of the field of rationals), nor what connections they had with the work of Lagrange, with Abel's own work on elliptic functions (where the division takes place, from Abel's point of view, in the *abelian* equation [the roots generating abelian extensions], results which were already known to Gauss, but not published, at the very least for the particular case of the so-called lemniscate) and abelian functions, or with Jacobi's work on the same subject (the same Jacobi who invented "abelian functions" in the modern sense and gave them that name, see his memoir "*De transcendentibus quibusdam abelianis*"), nor with Galois's work (which was only understood little by little, and much later; there is *no* trace in Riemann that he had learned from it, *although* (this is most remarkable) Dedekind, Privatdozent in Göttingen and close friend of Riemann, had since 1855 or 6, when Riemann was at the height of his powers, given a course on abstract groups and Galois theory).

To know if a (not a multiple of p) is a residue of p (prime), is to know whether $x^2 - a = py$ has solutions; in passing to the field extension of $\sqrt{a}$, one gets $(x - \sqrt{a})(x + \sqrt{a}) = py$, so in this field p is not prime to $x - \sqrt{a}$, which, nevertheless, it does not divide. In the language of ideals, that is as much to say that in this field p is not prime, but may be decomposed into two prime ideal factors. Thus one is presented with a problem: k being a field (here the field of rationals), k' (here, k' is k adjoined by $\sqrt{a}$) an algebraic extension of k, to know if a prime ideal (here, a number) in k remains prime in k' or if it decomposes into prime ideals, and how: a being given, the law of reciprocity points to those p for which a is the residue, and so resolves the problem for this particular case. Here and in all of what follows, k, k', etc. are fields of algebraic numbers (roots of algebraic equations with rational coefficients).

When it is a question of biquadratic residues, one works with a field generated by $\sqrt[4]{a}$; but such a field is not *in general* an abelian extension of the "base field" k unless the adjunction of a 4th root of a brings along at the same time three others (namely, if α is one of them, the others are $-\alpha, i\alpha$, and $-i\alpha$), this requires that k contains $i = \sqrt{-1}$; one would have nothing so simple if one takes as the base field the rationals, but all goes well if one takes (as did Gauss) the field of "complex rationals" $r + si$ (r, s rational). The same is the case for cubic residues. In these cases, one studies the decomposition, in the field k' obtained by the adjunction of a 4th (or, respectively, 3rd) root, starting with a base field k containing i (respectively, j), of an ideal (here, a number) prime in k.

So, this problem of the decomposition in k' of ideals of k is completely resolved when k' is an abelian extension of k, and the solution is very simple and it generalizes the law of reciprocity in a straightforward and direct manner. For the arithmetic progression in which the prime numbers are found, with residue a, one substitutes ideal classes {[des classes d'idéaux]}, the definition of which is simple enough. The classes of quadratic forms in two variables, studied by Gauss, correspond to a particular case of these classes of ideals, as was recognized by Dedekind; Dirichlet's analytic methods (using zeta or L-functions) for studying quadratic forms, is translated readily to the more general classes of ideals that had been considered in this theory; for example, for the theorem on arithmetic progressions there corresponds the following result: in each of these ideal classes in k, there is an infinity of prime ideals, therefore an infinity of ideals of k which may be factored in a given fashion in k'. Finally, the decomposition of ideals of k into classes determines k' in a unique way: and, by the theorem called *the law of Artin reciprocity* (because it implicitly contains Gauss's law and all known generalizations), there is a correspondence (an "isomorphism") of the Galois group of k' with respect to k, and the "group" of ideal classes in k. Thus, once one knows what happens in k, one has complete knowledge of *abelian* extensions of k. This does not mean there is nothing more to do about abelian extensions (for example, one can generate these by the numbers $\exp(-2\pi i/n)$ if k is the field of rationals, thus by means of the exponential function; if k is the field generated by $\sqrt{-a}$, a a positive integer, one knows how to generate these extensions by means of elliptic functions or their close relatives; but one knows nothing for all other k). But these questions are well understood and one can say that *everything* that has been done in arithmetic since Gauss up to recent years consists in variations on the law of reciprocity: beginning with Gauss's law; and ending with and crowning the work of Kummer, Dedekind, Hilbert, is Artin's law, *it is all the same law*. This is beautiful, but a bit vexing. We know a little more than Gauss, without doubt; but what we know more (or a bit more) is just that we do not know more.

This explains why, for some time, mathematicians have focused on the problem of the nonabelian decomposition laws (problems concerning k, k', when k' is any nonabelian extension of k; we remain still within the realm of a field of algebraic numbers). What we know amounts to very little; and that little bit was found by Artin. To each field is attached a zeta function, discovered by Dedekind; if k' is an extension of k, the zeta function attached to k' decomposes into factors; Artin discovered this decomposition; when k' is an abelian extension of k, these factors are identical to Dirichlet's L-functions, or rather to their generalization for fields k and classes of ideals in k, and the identity between these factors and these functions *is* (in other words) Artin's reciprocity law; and this is the way Artin first arrived at this law as a bold conjecture (it seems that Landau made fun of him), some time before being able to prove it (a curious fact, his proof is a simple translation of another result by Tchebotareff that had just been published, which he cited; however it is Artin, justly having it bear his name, who had the glory of discovering it). In other words, the law of reciprocity is nothing other than the rule for forming the coefficients of the series that represents the Artin factors (which are called "Artin L-functions"). As the decomposition into factors remains valid if k' is a non-abelian extension, it is these factors, for these "non-abelian L-functions", that it is natural to tackle in order to discover the law of formation of their coefficients. It is worth noting that, in the abelian case, it is known that the Dirichlet L-functions, and consequently the Artin L-functions, which scarcely differ from them, are entire functions. One knows nothing of this sort for the general case: it is there, as already indicated by Artin, that one might find an opening for an attack (please excuse the metaphor): *since* the methods known from arithmetic do not appear to permit us to show that the Artin functions are entire functions, one could hope that in proving it one could open a breach which would permit one to enter this fort (please excuse the straining of the metaphor).

Since the opening is well defended (it had defied Artin), it is necessary to inspect the available artillery and the means of tunneling under the fort (please excuse, etc.). {The reader who has the patience to get to the end will see that as artillery, I make use of a trilingual inscription, dictionaries, adultery, and a bridge which is a turntable {[or a turnbridge]}, not to speak of God and the devil, who also play a role in this comedy.} And here is where the *analogy* that has been referred to since the beginning finally makes its entrance, like Tartuffe appearing only in the third act.

It is widely believed that there is nothing more to do about algebraic functions of one variable, because Riemann, who had discovered just about all that we know about them (excepting the work on uniformization by Poincaré and Klein, and that of Hurwitz and Severi on correspondences), left us no indication that there might be major problems that concern them. I am surely one of the most knowledgeable persons about this subject; mainly because I had the good fortune (in 1923) to learn it directly from Riemann's memoir, which is one of the greatest pieces of mathematics that has ever been written; there is not a single word in it that is not of consequence. The story is not closed, however; for example, see my memoir in the Liouville Journal (see the introduction to this paper). {["Généralisation des fonctions abéliennes," *Journal de Mathématiques Pures et Appliquées* IX 17 (1938): 47–87, pp. 47–49.]} Of course, I am not foolish enough to compare myself to Riemann; but to add a little bit, whatever it is, to Riemann, that would already be, as they say in Greek, to do something {[*faire quelque chose*]}, even if in order to do it you have the silent help of Galois, Poincaré and Artin.

Be that as it may, in the time (1875 to 1890) when Dedekind created his theory of ideals in the field of algebraic numbers (in his famous "XI Supplements": Dedekind published four editions of Dirichlet's Lectures on the theory of numbers, given at Göttingen during the last years of Dirichlet's life, and admirably edited by Dedekind; among the appendices or "Supplements" of these lectures, which contain nothing indicating they are Dedekind's original work, and which indeed they are only in part, beginning with the 2nd edition there are three entirely different expositions of the theory of ideals, one for each edition), he discovered that an analogous principle permitted one to establish, by purely algebraic means, the principal results, called "elementary", of the theory of algebraic functions of one variable, which were obtained by Riemann by transcendental {[analytic]} means; he published with Weber an account of the consequences of this principle. Until then, when the topic of algebraic functions arose, it concerned a function y of a variable x, defined by an equation $P(x, y) = 0$ where P is a polynomial *with complex coefficients*. This latter point was essential in order to apply Riemann's methods; with those of Dedekind, in contrast, those coefficients could come from an arbitrary field (called "the field of constants"), since the arguments were *purely algebraic*. This point will be important shortly.

The analogies that Dedekind demonstrated were easy to understand. For integers one substituted polynomials in x, to the divisibility of integers corresponded the divisibility of polynomials (it is well known, and it is taught even in high schools, that there are other such analogies, such as for the derivation of the greatest common divisor), to the rationals correspond the rational fractions {[?of polynomials, or the rational functions]}, and to algebraic numbers correspond the algebraic functions. At first glance, the analogy seems superficial; to the most profound problems of the theory of numbers (such as the decomposition into prime ideals) there would seem to be nothing corresponding in algebraic functions, and inversely. Hilbert went further in figuring out these matters; he saw that, for example, the Riemann-Roch theorem corresponds to Dedekind's work in arithmetic on the ideal called "the different"; Hilbert's insight was only published by him in an obscure review (Ostrowski pointed me to it), but it was already transmitted orally, much as other of his ideas on this subject. The unwritten laws of modern mathematics forbid writing down such views if they cannot be stated precisely nor, all the more, proven. To tell the truth, if this were not the case, one would be overwhelmed by work that is even more stupid and if not more useless compared to work that is now published in the journals. But one would love it if Hilbert had written down all that he had in mind.

Let us examine this analogy more closely. Once it is possible to translate any particular proof from one theory to another, then the analogy has ceased to be productive for this purpose; it would cease to be at all productive if at one point we had a meaningful and natural way of deriving both theories from a single one. In this sense, around 1820, mathematicians (Gauss, Abel, Galois, Jacobi) permitted themselves, with anguish and delight, to be guided by the analogy between the division of the circle (Gauss's problem) and the division of elliptic functions. Today, we can easily show that both problems have a place in the theory of abelian equations; we have the theory (I am speaking of a purely algebraic theory, so it is not a matter of number theory in this case) of abelian extensions. Gone is the analogy: gone are the two theories, their conflicts and their delicious reciprocal reflections, their furtive caresses, their inexplicable quarrels; alas, all is just one theory, whose majestic beauty can no longer excite us. Nothing is more fecund than these slightly adulterous relationships; nothing gives greater pleasure to the connoisseur, whether he participates in it, or even if he is an historian contemplating it retrospectively, accompanied, nevertheless, by a touch of melancholy. The pleasure comes from the illusion and the far from clear meaning; once the illusion is dissipated, and knowledge obtained, one becomes indifferent at the same time; at least in the Gitâ there is a slew of prayers (slokas) on the subject, each one more final than the previous ones. But let us return to our algebraic functions.

Whether it is due to the Hilbert tradition or to the attraction of this subject, the analogies between algebraic functions and numbers have been on the minds of all the great number theorists of our time; abelian extensions and abelian functions, classes of ideals and classes of divisors, there is material enough for many seductive mind-games, some of which are likely to be deceptive (thus the appearance of theta functions in one or another theory). But to make something of this, two more recent technical contrivances were necessary. On the one hand, the theory of algebraic functions, that of Riemann, depends *essentially* on the idea of birational invariance; for example, if we are concerned with the field of *rational* functions of one variable x, one introduces (initially, I take the field of constants to be the complex numbers) as the *points* corresponding to the various complex values of x, including the point at infinity, denoted symbolically by $x = \infty$, and defined by $1/x = 0$; the fact that this point plays exactly the same role as all the others is essential. Let $R(x) = a(x-\alpha_1)\dots(x-\alpha_m)/(x-\beta_1)\dots(x-\beta_n)$ be a rational fraction, with its decomposition into factors as indicated; it will have zeros $\alpha_1, \dots, \alpha_m$, the poles $\beta_1, \dots, \beta_n$, and the point at infinity, which is zero if $n > m$, and is infinite if $n < m$. In the domain of rational *numbers*, one always has a decomposition into prime factors, $r = p_1 \dots p_m / q_1 \dots q_n$, each prime factor corresponding to a binomial factor $(x - \alpha)$; but nothing apparently corresponds to the point at infinity. If one models the theory of functions on the theory of algebraic numbers, one is forced to give a special role, *in the proofs*, to the point at infinity, sweeping the problem into a corner, if we are to have a definitive statement of the result: this is just what Dedekind-Weber did, this is just what was done by all who have written in algebraic terms about algebraic functions of one variable, until now, I was the first, two years ago, to give (in Crelle's Journal {["Zur algebraischen Theorie der algebraischen Funktionen", 179 (1938), pp. 129–133]}) a purely algebraic proof of the main theorems of this theory, which is as birationally invariant (that is to say, not attributing a special role to any point) as were Riemann's proofs; and that is of more than methodological importance. {Actually, I was not quite the first. The proofs, to be sure very roundabout, of the Italian school (Severi above all) are, in principle, of the same sort, although drafted in classical language.} However fine it is to have these results for the function field, it seems that one has lost sight of the analogy. In order to reestablish the analogy, it is necessary to introduce, into the theory of algebraic *numbers*, something that corresponds to the point at infinity in the theory of functions. That is what one achieves, and in a very satisfactory manner, too, in the theory of "valuations". This theory, which is not difficult but I cannot explain here, depends on Hensel's theory of p-adic fields: to define a prime ideal in a field (a field given *abstractly*) is to represent the field "isomorphically" in a p-adic field: to represent it in the same way in the field of real or complex numbers, *is* (in this theory) to define a "prime ideal at infinity". This latter notion is due to Hasse (who was a student of Hensel), or perhaps Artin, or to both of them. *If one follows it in all of its consequences*, the theory alone permits us to reestablish the analogy at many points where it once seemed defective: it even permits us to discover in the number field simple and elementary facts which however were not yet seen (see my 1939 article in *la Revue Rose* which contains some of the details {["Sur l'analogie entre les corps de nombres algébriques et les corps de fonctions algébriques," *Revue Scientifique* 77 (1939) 104–106, and the comments in the *Oeuvres Scientifiques*, volume 1, pp. 542–543]}). It is not so much this point of view that has been used up to now for giving satisfactory statements of the principal results of the theory of abelian extensions (I forgot to say that this theory is most often called "class field theory"). An important point is that the p-adic field, or respectively the real or complex field, corresponding to a prime ideal, plays exactly the role, in arithmetic, that the field of power series *in the neighborhood of a point* plays in the theory of functions: that is why one calls it a *local field*.

With all of this, we have made great progress; but it is not enough. The purely algebraic theory of algebraic functions in any *arbitrary* field of constants is not rich enough so that one might draw useful lessons from it. The "classical" theory (that is, Riemannian) of algebraic functions over the field of constants of the complex numbers is infinitely richer; but on the one hand it is too much so, and in the mass of facts some real analogies become lost; and above all, it is too far from the theory of numbers. One would be totally obstructed if there were not a bridge between the two.

And just as God defeats the devil: this bridge exists; it is the theory of the field of algebraic functions over a finite field of constants (that is to say, a finite number of elements: also said to be a Galois field, or earlier "Galois imaginaries" because Galois first defined them and studied them; they are the algebraic extensions of a field with p elements formed by the numbers $0, 1, 2, \ldots, p-1$ where one calculates with them modulo $p, p =$ prime number). They appear already in Dedekind. A young student in Göttingen, killed in 1914 or 1915, studied, in his dissertation that appeared in 1919 (work done entirely on his own, says his teacher Landau), zeta functions for certain of these fields, and showed that the ordinary methods of the theory of algebraic numbers applied to them. Artin, in 1921 or 1922, took up the question again, again from the point of view of the zeta function; F. K. Schmidt made the bridge between these results and those of Dedekind-Weber, in the process of providing a definition of the zeta function that was birationally invariant. In the last few years, these fields were a favorite subject of Hasse and his school; Hasse made a number of beautiful contributions.

I spoke of a bridge; it would be more correct to speak of a turntable {[?turnbridge]}. On one hand the analogy with number fields is so strict and obvious that there is neither an argument nor a result in arithmetic that cannot be translated almost word for word to the function fields. In particular, it is so for all that concerns zeta functions and Artin functions; and there is more: Artin functions *in the abelian case* are *polynomials*, which one can express by saying that these fields furnish a *simplified* model of what happens in number fields; here, there is thus room to conjecture that the non-abelian Artin functions are still polynomials: *that is just what occupies me at the moment*, all of this permits me to believe that all results for these fields could inversely, if one could formulate them appropriately, be translated to the number fields.

On the other hand, between the function fields and the "Riemannian" fields, the distance is not so large that a patient study would not teach us the art of passing from one to the other, and to profit in the study of the first from knowledge acquired about the second, and of the extremely powerful means offered to us, in the study of the latter, from the integral calculus and the theory of analytic functions. That is not to say that at best all will be easy; but one ends up by learning to see something there, although it is still somewhat confused. Intuition makes much of it; I mean by this the faculty of seeing a connection between things that in appearance are completely different; it does not fail to lead us astray quite often. Be that as it may, my work consists in deciphering a trilingual text {[cf. the Rosetta Stone]}; of each of the three columns I have only disparate fragments; I have some ideas about each of the three languages: but I know as well there are great differences in meaning from one column to another, for which nothing has prepared me in advance. In the several years I have worked at it, I have found little pieces of the dictionary. Sometimes I worked on one column, sometimes under another. My large study that appeared in the Liouville journal made nice advances in the "Riemannian" column; unhappily, a large part of the deciphered text surely does not have a translation in the other two languages: but one part remains that is very useful to me. At this moment, I am working on the middle column. All of this is amusing enough. However, do not imagine that this work on several columns is a frequent occasion in mathematics; in such a pure form, this is almost a unique case. This sort of work suits me particularly; it is unbelievable at this point that distinguished people such as Hasse and his students, who have made this subject the matter of their most serious thoughts over the years, have, not only neglected, but disdained to take the Riemannian point of view: at this point they no longer know how to read work written in Riemannian (one day, Siegel made fun of Hasse, who had declared himself incapable of reading my Liouville paper), and that they have rediscovered sometimes with a great deal of effort, in their dialect, important results that were already known, much as the ideas of Severi on the ring of correspondences were rediscovered by Deuring. But the role of what I call analogies, even if they are not always so clear, is nonetheless important. It would be of great interest to study these things for a period for which we are well provided with texts; the choice would be delicate.

P.S. I send this to you without rereading …I fear …having made more of my research than I intended; that is, in order to explain (following your request) how one develops one's research, I have been focusing on the locks I wish to open. In speaking of analogies between numbers and functions, I do not want to give the impression of being the only one who understands them: Artin has thought profoundly about them as well, and that is to say a great deal. It is curious to note that one work (signed by a student of Artin who is not otherwise known, which without proof to the contrary, allows one to presume that Artin is the real source) appeared 2 or 3 years ago which gives perhaps the only example of a result from the classical theory, obtained by a *double* translation, starting with an arithmetic result (on abelian zeta functions), and which is novel and interesting. And Hasse, whose combination of patience and talent make him a kind of genius, has had very interesting ideas on this subject. Moreover (a characteristic trait, and which would be sympathetic to you, of the school of modern algebra) all of this is spread by an oral and epistolary tradition more than by orthodox publications, so it is difficult to make a history of all of it in detail.

You doubt and with good reason that modern axiomatics will work on difficult material. When I invented (I say invented, and not discovered) uniform spaces, I did not have the impression of working with resistant material, but rather the impression that a professional sculptor must have when he plays by making a snowman. It is hard for you to appreciate that modern mathematics has become so extensive and so complex that it is essential, *if* mathematics is to stay as a whole and not become a pile of little bits of research, to provide a unification, which absorbs in some simple and general theories all the common substrata of the diverse branches of the science, suppressing what is not so useful and necessary, and leaving intact what is truly the specific detail of each big problem. This is the good one can achieve with axiomatics (and this is no small achievement). This is what Bourbaki is up to. It will not have escaped you (to take up the military metaphor again) that there is within all of this great problems of strategy. And it is as common to know tactics as it is rare (and beautiful, as Gandhi would say) to plan strategy. I will compare (despite the incoherence of the metaphor) the great axiomatic edifices to communication at the rear of the front: there is not much glory in the Commissariat and logistics and transport, but what would happen if these brave folks did not consecrate themselves to secondary work (where, moreover, they readily earn their subsistence)? The danger is only too great that various fronts end up, not by starving (the Council for Research is there for that), but by paying insufficient attention to each other and so waste their time, some like the Hebrews in the desert, others like Hannibal at Capua {[where the troops were said to have been entranced by the place]}. The current organization of science does not take into account (unhappily, for the experimental sciences; in mathematics the damage is much less great) the fact that very few persons are capable of grasping the entire forefront of science, of seizing not only the weak points of resistance, but also the part that is most important to take on, the art of massing the troops, of making each sector work toward the success of the others, etc. Of course, when I speak of troops the term (for the mathematician, at least) is essentially metaphoric, each mathematician being himself his own troops. If, under the leadership given by certain teachers, certain "schools" have notable success, the role of the individual in mathematics remains preponderant. Moreover, it is becoming impossible to apply a view of this sort to science as a whole; it is not possible to have someone who can master enough of both mathematics and physics at the same time to control their development alternatively or simultaneously; all attempts at "planning" become grotesque and it is necessary to leave it to chance and to the specialists.