Functoriality in the theory of automorphic forms: its discovery and aims

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Abstract

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All footnotes were added by the translator.
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Einführung (Introduction)

Initially, this essay was intended to be historical. However, in the writing, it has become autobiographical and also didactic. It is ostensibly about a letter that marked a turning point, almost a break, in my life, not because of the addressee, André Weil, whom I chose by chance, but because of its content. In retrospect, and even before, André Weil may have seemed to be the obvious recipient of the letter, because its content is mostly number theoretic, and Weil was one of the leading number theorists of his time. The letter, however, as well as the addressee, came about spontaneously, without intent, and the choice of the recipient was ultimately hardly important. I thought Harish-Chandra understood better the scope of the questions posed, but it was only chance that let the letter fall into his hands.

This was not only because the letter was difficult to understand without a knowledge of the theory of Lie groups and of their infinite-dimensional representations. Indeed, in the sixties, this theory had begun to strongly influence the theory of automorphic forms. To this day, representation theory remains strange to many number theorists, so that I would like to describe how I came to it as well as to the theory of automorphic forms.

1 Jugenderrinerungen (Adolescent memories)

I shall take the opportunity to write a few words about my mathematical history before the letter, that is, before January 1967. I go very far and ask a lot of patience from the reader. I started my mathematical education in Canada, when I was almost eighteen and in my second year of university, with a very basic introduction to infinitesimal calculus. My serious training only started the following year. On the recommendation of a teacher I had procured the English translation of Courant's two-volume work *Lectures on Differential and Integral Calculus*. We also had lectures on algebra for which the proposed textbooks were, in many respects, on the right level for me, an introduction to linear algebra by Murnaghan entitled *Analytic Geometry*, as well as a book of Dickson on elementary algebra, *New First Course in the Theory of Equations*, which, it seems to me, when I look at it now, contains many beautiful things, the importance of which I recognized only in retrospect and very late, in particular, the Vandermonde determinant and the solution by radicals of a third degree equation. As very often in my intellectual [geistigen] life — as in my life in general — I wanted to learn too quickly, and did not take the time to think about some important things. On the other hand, I found two books on linear algebra, the book *Finite-dimensional Vector Spaces* by Halmos, which was my introduction to modern mathematics, and whose abstract exposition of the topic I greatly admired, as well as the English translation of a book, actually two books, *Introduction to Algebra and Analytical Geometry* and *Lectures on Matrices* by Schreier and Sperner that I discovered myself. They were my introduction to the theory of elementary divisors and gave me an important head start in my later study of Hecke operators.

In my fourth year at university, I attended various lectures. Although some, like those on the basic concepts of the theory of partial differential equations and the special functions of mathematical physics, left a strong mark on me, this had to mature before it was useful to me. We also had lectures that offered an introduction to function theory, based on translations of a series of books by Konrad Knopp. The last of these little books, on Weierstrass's theory of elliptic functions, was not part of the curriculum. I read it anyway. The algebraic theory of these functions came later when, during my first years at Princeton, I had the opportunity to teach an elementary series of lectures, based on the book of Walker, *Algebraic Curves*. Obviously, I was lagging behind in algebraic geometry. Unfortunately, I have remained so.

These books of Knopp and Walker, as well as many other excellent books, often in their original language, were available at very low prices in paperback editions by Dover Press. These, together with a number of reprinted editions available at Chelsea Press, provided an opportunity, especially for young people, to get out-of-print or otherwise unavailable books for their libraries. However, this was partly a consequence of the seizure of German publishing rights during the war.

In my hurry to learn as much as possible in the shortest possible time, I also took part in my fourth year in a seminar on noetherian rings, which was not intended for undergraduates. The little book, Northcott's *Ideal Theory*, provided the basic theme. Although I thought

I understood the essentials of what was presented, I would have found an introduction to algebraic geometry, especially the theory of algebraic curves, with a subsequent general ascent into the algebraic geometry of the time, more engaging. I nonetheless found the little book beautiful, and I was delighted with its contents. The following year I even tried to write my master's thesis in this field. I can not say that I am particularly proud of the outcome. This year, the fifth and last of my studies in Vancouver, was in no way brilliant. I was in too much of a hurry. I had to teach and wanted to acquire the dignity of a master's degree as soon as possible, so that I could get into graduate school and start my PhD. The result was that I had to attend a lot of lectures and write a thesis, all within one year. I did it. But I remember very little of that year. However, my reading was different. First, as part of my fulfilment for the master's degree, I made up my mind, to read Dixmier's *Les algèbres d'opérateurs dans l'espace hilbertien*. The subject attracted me very strongly at the time because of its abstractness. I managed to read the book and understand a bit of it. However, in the end I did not find the topic particularly inspiring and never came back to it.

I had not read a foreign language mathematical textbook until then. I had ordered van der Waerden's Modern Algebra and tried to read it without success. It is not clear whether the failure was due to the material or to my ignorance of the German language, which I had tried to study on my own between my first and second years, without having understood what it means to learn a foreign language. It is hardly surprising that an English-speaking North American boy did not understand this. It would not occur to him to try something like that these days. As a student, I had very little time in the summer for education or training, since not only did I have to earn money for the winter, but I also didn't want to completely neglect the normal inclinations of youth. In the fourth academic year, we also had a seminar on geometry, based on the Russian book by A.D. Alexandrov, ..., which made me very happy because I had studied Russian in my second year. There were only three students and the professor. We did not get very far, perhaps because my fellow students were neither enthusiastic nor diligent.

Crossing between Canada and the United States in the fifties was different than it is today. As far as people were concerned, it was not that strict. You could cross the border without an ID. For goods, it was more difficult because everything was cheaper in the United States, and the duties in the direction from south to the north were expensive. Goods to a very limited extent could be imported duty-free once or twice a year after a two-day stay south of the border, so that people back then, probably still if not as much, went south on the weekends. This urge was also felt by me and my relatives, so I once drove across the border to Seattle in my fifth year. Books were probably duty free. But there were no mathematical books for sale in Vancouver, so that I wanted to visit especially the bookstore at the University of Seattle, where I could even buy some second-hand math books. They were, it seemed, on a scientific level, I had not reached. It was also clear that they were all from the same small private library, mainly books published by the Princeton University Press. I bought several, among them *Introduction to Topology* by Lefschetz*, Classical Groups* and *The Theory of Algebraic Numbers*, both by Weyl.

My serious intention was to read all three at once. In fact, I didn't start reading the first until several years later, in the early 1960s, when I was already in Princeton. I found some of its exercises impossible, which discouraged me because it was meant to be a beginner's book. I stopped reading the book. I mentioned my failure to a friend, a topologist, who helped by assuring me that Lefschetz, without drawing the reader's attention, had scattered some important unsolved problems in topology among the ordinary exercises. Still, I didn't open the book again and, unfortunately, never came very close to topology. I read the book on the theory of algebraic numbers, if not immediately a short time after the trip, and discovered with enthusiasm that the quadratic reciprocity law was an immediate, almost self-evident, consequence of a very general and very beautiful theory and not as I had thought up until then, an elementary, random fact. Later I also learned how beautiful, if not the theorems themselves, some of the elementary proofs are. But it was initially my enthusiasm for the beautiful general theory that has become firmly embedded in my consciousness. On the other hand, Weyl's book on classical groups never inspired me. I would like to come back to it in the future.

The two books were written back-to-back, *Classical Groups* was published in 1939, and *The Theory of Algebraic Numbers* in 1940. From my own experience, I would guess that both represent an attempt by Weyl, as an aging mathematician, to finally understand topics from his younger years. In his second memoir on Hilbert after his death, Weyl describes his admiration when he, then a young student, read Hilbert's Zahlbericht, and swore to read everything Hilbert had written. Then later, after the early death of his wife, who had also studied mathematics in Göttingen, Weyl describes, in an unpublished book about their life together, how the two of them studied the report together a few years later, without mentioning that he himself already had read it before. His wife had also listened to the Princeton lectures, from the records of which his own book grew, and occasionally writes to her son, Joachim, also a mathematician, about how diligent his father was in preparation and how he wanted to understand everything for himself. Weyl's book is certainly not a repetition of Hilbert's book. I have the feeling, without having thoroughly compared the Zahlbericht with Weyl's book, that Weyl wanted to understand and explain various different viewpoints, such as those of Dedekind and Kronecker, which Hilbert, who had his own ideas, had left aside.

Although Weyl himself contributed little, if anything, to algebraic number theory, he was one of the outstanding figures in the development of the representation theory of semisimple groups and in their application in physics, especially in spectroscopy. In the beginning, the representation theory of finite groups as well as of the three-dimensional rotation group provided the most important examples for physics. Weyl's book *Group Theory and Quantum Mechanics*, published in 1928, was influential and is still worth reading today. His three works *Theory of the Representations of Continuous Semi-Simple Groups by Linear Transformations*, I , II, III, published in 1925 and 1926, have also become classics. Strangely enough, I have so far found nothing in Weyl's work about the mutual influence of the two areas, especially about the influence of developments in physics on the representation theory of the mathematicians, not even on his own mathematical work. I can't find a plausible description of this influence anywhere else. I may not have made sufficient effort. Weyl's book *Classical Groups, their Invariants and Representations* is dedicated to the exiled Berlin mathematician Issai Schur, who played a leading role in the transition from finite groups to algebraic groups. I never completed it. Perhaps I should take it up again, hoping to bring Weyl's views on this transition to light.

When I wrote these lines, I wanted to find the book in my library. I have searched for it for half an hour without finding it, and slowly became convinced that my memory was wrong, and that I had never even owned the book at all. In the meantime, I came across two other books, also from Princeton University Press, which I had bought second-hand at the same visit, Pontrjagin's *Topological Groups*, a translation, and Widder's *The Laplace Transform*. The second I really enjoyed. It was my introduction to classical real analysis. The first I have read, but the general theory of topological groups has always remained abstruse for me. After a long search, I finally found *Classical Groups* in the series of books with labels that made it clear that I had bought it in Seattle too. When I read the introduction, I immediately understood why I had never studied the book more closely.

As always with Weyl, the introduction is beautiful from a literary point of view. On the other hand, I find it difficult to agree with the opinions expressed in it. Weyl thinks, "Important though the general concepts and propositions may be with which the modern industrious passion for axiomatizing and generalizing has presented us, in algebra more than anywhere else, nevertheless I am convinced that the special problems in all their complexity constitute the stock and core of mathematics; and to master their difficulties requires on the whole the harder labor." I am of the same opinion. General concepts are nonetheless important, and the higher mind learns how to separate the concrete examples that lead to significant general concepts and which are fundamental for them from other examples that are only beautiful or difficult to understand, as well as to separate the general concepts that bring to light significant concrete examples from those that have nothing to offer but their generality. I find it disappointing that Weyl, with Élie Cartan one of the founders of the representation theory of semisimple algebras and groups, failed to predict or, better, not recognize the unity of this theory. In his early work there is already a kind of unity, and I find overwhelming the conciseness of the theorem in his book on quantum mechanics that all quantum numbers are characters of group representations. For me it is just as strange, as in the context of functoriality, which we shall come to later when we describe the developments that were slowly undertaken as a result of the possibilities that were hinted at in the letter to Weil, that the representations of all semi-simple groups or of all the reductive groups are linked to one another in an unexpected manner.

I remain, however, a devoted admirer of Weyl and admire his strength as an analyst, his broad knowledge of mathematics and related fields, and his literary talent. I hope, in the years that remain to me, to not only reread some books that I have already read, such as the *The Idea of a Riemann Surface*, but also to read for the first time some books and works that I have never read, such as *Space, Time, Matter*. Even during my time at Yale University, although I already had a family and very little money, I took the liberty of ordering *Selecta*, which had been given to him on the occasion of his seventieth birthday by his friends and colleagues, a beautiful published book as no longer exists today. In it I read, not long after arriving in Princeton, his beautiful work on the representations of compact semisimple groups.

2 Yale University.

After five years at the University in Vancouver, I went to Yale University in Connecticut for further training as a mathematician. Although I only stayed there for two years, I left not only with my doctorate but also, although I did not know it, with the beginning of many years of work. The main interest of the mathematicians at Yale was functional analysis. I knew that beforehand, and I was really excited about the lectures in the first year. We had lectures on the material in the books *Linear Operators* by Dunford and Schwartz, in fact only from the first volume, and *Analytic Semi-groups* by Hille and Phillips. Dunford himself was responsible for the first; Hille was old enough to entrust the second to a younger colleague, Cassius Ionescu-Tulcea. I have studied both books thoroughly. Traces of my work can be found in the second volume by Dunford-Schwartz, which was published later. Although the main subject of his book is in itself of limited interest, Hille was above all an analyst, and the book was in some respects an extensive source of classical real analysis. It gave me a lot.

I had, perhaps only in the second semester, attended lectures of Felix Browder on partial differential equations, especially on a priori bounds that corresponded to his subject. He was not a conscientious lecturer, spoke off the cuff, and had to start over three times every attempt to prove a theorem. Nevertheless, I found the lectures good. I went home every night with a set of messy notes and made a neat exposition out of it. I learnt a lot, and I wanted to write my dissertation in the field. That did not happen.

In the lectures on semigroups, there was posed a problem about Lie semigroups, a term introduced by Hille himself, which I solved. A little later I developed some of my own ideas about analytical semigroups, partly with the help of some results from the theory of parabolic differential equations, so that after about a year I had the material for a doctoral thesis. Although I never really published them, the results for analytical semigroups were included by Derek Robinson in his book, *Elliptic Operators and Lie Groups*.

Students at the end of the first year had to pass a kind of qualifying exam. I hadn't prepared for it at all. However, I had had time to skim through various books or even to study them properly. I read the first edition of Zygmund's book on Fourier series with some care, as well as the book by Burnside on finite groups, but this with less attention. I dreamed of proving his famous conjecture about simple groups of odd order, not yet proven at the time, but without having the slightest idea of how to achieve this. I had also, I think, read at the time M.H. Stone's book *Linear Transformations in Hilbert Spaces*. I later used the results presented there in the theory of Eisenstein series. Although I did not perform well in the exam, mainly because I had forgotten, or never understood, everything about commutative algebra from the seminar on noetherian rings, thanks to Zygmund's book, I had an extraordinary knowledge of the convexity theorems in the theory of Fourier series and integrals, which came to my aid. Fortunately for me, one of the examiners, Shizuo Kakutani, knew a lot about the subject and asked many questions about it.

With the examination and the doctoral thesis already behind me, my second year at Yale was entirely free, so that I could completely surrender to my mathematical curiosity. How it went in sequence I cannot say, but providence favored me twice that year. The most important was that Steven Gaal announced a series of lectures on analytic number theory. As I had hoped, because of Weyl's book on algebraic number theory, that I would also study number theory at Yale, and if I remember correctly, even class field theory, I attended these lectures. It turned out that Gaal had spent two years in Princeton at the Institute for Advanced Study with Selberg. Selberg had invited him there after the Hungarian uprising of 1956. In Princeton he came into contact with Selberg's ideas on the analytic theory of automorphic forms, which, although brief, appeared in Selberg's short publication with the long title *Harmonic analysis and discontinuous groups in weakly symmetric Riemannian spaces with applications to Dirichlet series*. This was the substance of Gaal's lectures at Yale. I studied this work in connection with his lectures. What seemed incidental in itself, was an attempt by Browder and Kakutani to hold a seminar on analytic functions of several complex variables. The two did not get along, and so the seminar ended after one or two lectures. In the meantime, I had had the opportunity to learn a little about the domains of definition of analytic functions of several variables, in particular of the convexity properties of these domains. This allowed me to prove the analytic continuation of some series introduced by Maass and, more generally, by Selberg. They were later named Eisenstein series by Godement. I did not take this result very seriously, although I liked it.

Later, after I came to Princeton, I had to give a lecture in the weekly analysis seminar. Since I had nothing else at hand, I talked about this result. Bochner was very excited. Now that I have had more experience with the curriculum vitae of young mathematicians, I would guess that this was for two reasons. Firstly, he was very fond of Dirichlet series, as I certainly realized at the time; second, this research had absolutely nothing to do with my dissertation. It was suggested by myself and carried out on my own. My dissertation was also essentially the result of independent thinking. But that could not have been known to Bochner. Up until the time of my lecture, I had had nothing to do with Bochner. I came to Princeton on the recommendation of Edward Nelson, himself a very young mathematician at the time, who had come to Princeton only a year or two before from Chicago, where he had studied. He recommended me to his colleagues because of my work on holomorphic semigroups, a topic that also interested him.

Bochner encouraged me a lot afterwards. It was not so much that my position in the department was constantly improving, although it was. It was rather that he constantly encouraged me to continue the research I had already started on automorphic forms, which I had not taken very seriously. In addition, at his suggestion I assume, Selberg first invited me to a meeting in his office at the Institute for Advanced Study, and then later to a one-year stay at the Institute. Bochner had also proposed other potential research problems, this time in differential geometry, of which nothing came, mainly because I did not have the time to pursue them. But I am being too hasty, because I have not yet reached Princeton.

3 Princeton.

I came to Princeton in the fall of 1960, where I initially stayed for seven years, with interruptions. It helps my memory when I separate these years: 60/61 and 61/62 at the University; 62/63 at the Institute; 63/64 at the University; 64/65 in California; 65/66 and 66/67 at the University.

Although Bochner was primarily an analyst and geometer, he contributed to many areas of mathematics, and was interested in even more. He had been a student of Erhard Schmidt in Berlin and was later for a few years, until 1933, in Munich. He had good relationships with many mathematicians, if I am not mistaken, among others, with Helmut Hasse and Emmy Noether. He was obviously interested in their work. As part of his encouragement of my investigations into Eisenstein series, he initially wanted that I extend my first results, which were for $\mathrm{GL}(n)$ over $\mathbb{Q}$, or rather, as adèles were still unknown to me, for $\mathrm{GL}(n,\mathbb{Z})$, to an algebraic number field, which led me to read Hecke's work on the Dedekind zeta function, as well as to Landau's little book *Introduction to Elementary and Analytic Theory of Algebraic Numbers and its Ideals*. It also led me to the Siegel's work, which enabled me to use special methods to justify the analytic continuation of various Eisenstein series for different groups.

In these first two years, I also continued trying to understand Selberg's ideas, first for the trace formula, where the immediate task was to determine the dimension of the space of holomorphic automorphic forms of a given type on a compact quotient of a bounded symmetric region. In that period, European mathematics, particularly French, was more important than it is today, and in order to familiarize oneself with the theory of automorphic forms of the time, it was absolutely necessary to read the Paris seminars — Bourbaki, Cartan, Chevalley, Lie, and others. I don't remember when I discovered this, probably after Siegel, but I don't know whether before or after Harish-Chandra. However, the sequence of events was such that I realize that when I started I was not yet thinking within the framework of representation theory.

I initially took the integrals that occur when trying to compute the dimensions in Selberg directly from Selberg's work and was not able to calculate them. I discussed this difficulty with a young mathematician who died early, David Lowdenslager, then a colleague at the University. He suggested that it was widely believed that the work of Harish-Chandra would be useful for such questions. I immediately started reading Harish-Chandra's papers. In his research he had not yet reached general discrete series. But he knew a lot about holomorphic discrete series. When I read his work, I recognized, although not immediately, that the integrals appearing in Selberg's work were nothing more than orbital integrals for the corresponding group. That was a great discovery for me, which led to my first work on the trace formula.

I only met Harish-Chandra myself a little later, although he was in Princeton, perhaps not yet as a professor at the Institute. When I started reading his work, I had asked him by post for some reprints, as was still the custom. I did not get them for a long time. Only, I suppose, after my name was mentioned to him, perhaps by Bochner, did he introduce himself to me before a seminar and hand me the requested reprints. Afterwards I had many opportunities, until his untimely death twenty years later, to talk with him and get to know him better. Not long after our meeting, he declared, somewhat condescendingly, not so much to me as to the absent Selberg, that if you look at the matter correctly, namely in terms of group theory and representation theory, not just my imagined discovery but even the trace formula itself, at least for compact quotients, was not particularly impressive. He was right about my discovery. As for the trace formula, it is useful, even important, to understand the trace formula's relationship to Frobenius duality, which I had not understood before. The scope of Frobenius duality in itself, as Harish-Chandra understood, was not to be compared with that of the trace formula.

During these two years I got to know Selberg. Probably thanks to the mediation of Bochner, Selberg invited me to meet him in his office at the Institute. He explained to me the proof of the analytic continuation of Eisenstein series for a general discrete subgroup of $\mathrm{SL}(2,\mathbb{R})$, but under the usual assumptions about its fundamental domain. That was an experience for me, my first conversation about mathematics with a mathematician of the first rank [literally, purest water]. With Bochner I had only talked about research possibilities, never about the mathematics itself. The proof actually belongs to the spectral theory of an ordinary differential equation of second order on the half-line. I was, in principle, familiar with the theory from the book *Theory of Ordinary Differential Equations* of Coddington and Levinson, which I had read. Still, I had never before seen anyone who handled material of this kind and at this mathematical level so masterfully.

Later when I was able to prove, mostly on the basis of various results of Siegel, that this or that Eisenstein series could be continued, I would report it to Selberg over tea at the Institute, where I could usually find him. He would say, correctly I now think, only, "We need a general proof." When I reported to him later, probably as early as 1963, that I could deal with the problem, and later in the spring of 1964 sent him the fully written and typed proof, he did not react at all. I was a little disappointed, but not particularly worried about it. Only years later a possible reason for this occurred to me, which I will come back to later. We were colleagues for many years, and for more than twenty years, until his death, we sat in adjacent rooms. But Selberg was not a talkative person. In personal conversation, as well as in meetings, his preferred method was the monologue. He fixed his gaze on a corner of the room and spoke, for some time, to someone whom he alone could see. We rarely talked, always about everyday things, but he was always friendly.

Bochner had encouraged me very early, in 1963/64, even compelled me morally, to give lectures on class field theory. That was very brave of him, but not rash. Firstly, it was not customary for a young, inexperienced mathematician to be allowed to give lectures at this level, that is, for graduate students and colleagues at the University itself or guests of the Institute. Second, not only the subject, but even algebraic number theory itself, was foreign to me. The previous year I had attended a student seminar that Armand Brumer, who was also a student at the time, had given and in which he gave most of the lectures. I made a fool of myself with my stupid questions. Third, the lectures should begin in two or three weeks. I wanted to reject Bochner's proposal. He would not allow me.

The audience consisted of three or four students, Roy Fuller, Daniel Reich, and, at least at the beginning, Dennis Sullivan, and three or four guests of the Institute. The majority seemed to be satisfied. I probably used Landau's book *Einführung in die elementare und analytische Theorie der algebraischen Zahlen und ihre Ideale*, as well as Hecke's *Vorlesungen über die Theorie der algebraischen Zahlen* and Chevalley's *La théorie des corps de classes* from 1939. This was another case where my knowledge, unfortunately, remained superficial. My knowledge of German gradually improved, but was not satisfactory, so that I was unable to leaf through the works of the great German number theorists of the nineteenth century, and I was not inclined to simply take an untargeted, contemplative joy in their way of thinking and to the reading material itself. I did not warm to them, perhaps because I was more attracted to unsolved problems than solved ones. I am now trying to make up for this omission. The problem of nonabelian class field theory was, however, thanks to the lectures, strongly imprinted on my consciousness.

I had spent the previous year, 1962–63, at the Institute for Advanced Study, again thanks to a suggestion of Bochner, but indirectly through Selberg. Harish-Chandra did not join the Institute as a professor until 1963, so I did not meet him during my year at the Institute. During this year I spoke mainly to Weil and especially Borel. It was after the Stockholm Congress of the IMU, at which Gelfand had lectured, and during the semester, that I managed to read his lecture. Strangely, from this lecture, I understood for the first time the concept of a general cusp form. It is the key to the general continuation of Eisenstein series. About the same time I started to understand the general principles of reduction theory, as in the work of Borel and Harish-Chandra, whose results were announced in 1961, and the modern principles of automorphic forms, as explained in the works of Godement and Harish-Chandra. Thanks to this understanding, I managed during the year at the Institute, to truly attack the problem of continuation. In solving it, however, there were serious difficulties to be overcome, and I only fully succeeded over the course of the next academic year. One sees in the bibliography of the work, *On the functional equations satisfied by Eisenstein series*, in which I later published this research, that the above-mentioned reading, namely, the books by Dixmier and Stone, benefited me.

The theory of Eisenstein series is a spectral theory in the sense of functional analysis. It is about an exact description of the common spectrum of a commutative family of differential operators on a manifold. Eisenstein series are invariant differential operators on a manifold $\Gamma\backslash G$, where G is a semisimple or reductive Lie group. The eigenfunctions are defined by infinite series, and it is a matter of continuing these series analytically, and then showing how the spectrum is built up from the continued functions.

The structure of the spectrum consists of two steps, that is, the spectrum is first divided into parts that are parameterized by means of certain parabolic subgroups P, and which are then divided into finer parts. Each of the first parts has a dimension m, the rank of P, and also corresponds to a family of cusp forms of a Levi factor M of P. The first stage is then described by means of this division into families.

The first step in proving general continuability is to establish the possibility of an analytical continuation of the Eisenstein series attached to a family of this kind. The continuation is then a meromorphic function in several variables, and it is proved that the most general Eisenstein series is then formed as a multiple residue of these functions. During the transition to the residue, the number n of parameters in the function becomes smaller, $n \to n-1$, and the number n may be considered as a parameter in the second stage. The second step in the proof of general continuability is to show that, among the functions that arise in this method, there are present all that are necessary for the spectral decomposition. The first step was not so hard, especially for someone who, on the one hand, had mastered Selberg's method for SL(2) and, on the other hand, understood the theoretical framework Harish-Chandra had developed in his representation theory. The second step, however, was much more difficult and several attempts were necessary, so that the theory ended up taking almost a year of my life.

I have already mentioned that Selberg did not respond at all to my achievements. Over the years I have wondered why. Selberg was definitely a proud, even vain, if strong, mathematician, a loner. I can imagine him thinking, "If this greenhorn can find a proof, so can I." I found this view reasonable. Recently, however, in the last few years, it has occurred to me that he probably had not understood some some basic ideas, not even afterwards. I had always assumed that Selberg already knew what was in Gelfand's 1962 lecture. It seems to me possible now that he did not have the notion of a general cusp form, and that he had never seen Gelfand's lecture. I can not know now. Certainly he did not have the skill with differential operators on semisimple groups that Harish-Chandra's theory offered.

In the summer of 1964, I went with my family to Berkeley, California, where we stayed for a year. Now that I had a general theory of Eisenstein series in hand, I wanted to try, and perhaps had already tried in Princeton, to formulate and prove the trace formula in general. I did not succeed, either because I was too tired or because I was not smart enough. I am inclined to the second explanation. I would certainly never have achieved as much as Arthur did a few years later.

4 Kalifornien (California)

I had in California a year off without obligations and I arrived full of hope. I had made up my mind, in particular, to familiarize myself with algebraic geometry, primarily with the help of Weil's book, *The Foundations of Algebraic Geometry*, which corresponded to the level in Princeton at the time and seemed to me to be a natural textbook. The name Grothendieck did not appear at all in Princeton in the early sixties.[1] I also wanted to familiarize myself with the theory of abelian varieties with the help of Conforto's book *Abelian Functions and Algebraic Geometry*. Phillip Griffiths was at the time a faculty member at Berkeley, and we held a seminar together, of which I have no memory. Griffiths has since gone very far in the field, but I did not. At the request of some students, we also conducted a seminar from Joseph Lehner's book *Discontinuous Groups and Automorphic Functions*. I suppose I thought it useful to better understand the classical geometric, not the algebraic geometric, theory of modular forms. I do not understand why anymore.

I also remember trying to build a theory of the matrix coefficients of representations of semisimple groups by representing them as integrals in the spirit of the theory of hypergeometric functions. I found, and still think, the theory of hypergeometric functions beautiful.

[1] In contrast, by 1964/65 in Cambridge, Mass, it was all Grothendieck.

Unfortunately, the attempt did not lead to anything. I had at the end of the year the feeling that it had been wasted.

In retrospect, the year does not look that bad. During this time, I showed how the volume of a quotient $G(\mathbb{Z})\backslash G(\mathbb{R})$ could be calculated using the theory of Eisenstein series when G is a split group over $\mathbb{Q}$ or more generally over a number field. The same method also applies more generally to quasi-split groups, so that one can easily prove Weil's conjecture about the Tamagawa number in these cases. A few years later, in the lecture notes written with Jacquet, we showed in a special case, namely for forms of SL(2), that the result can be transferred from a quasi-split group to other forms of the same group using the trace formula. Much later, after he and Arthur had begun to develop the stable trace formula further, Kottwitz succeeded in proving the conjecture in general. I had also discovered a scalar product formula that I still today consider to be beautiful and useful.

Influenced by an article of Griffiths, I had an idea for getting a form of the Borel–Weil–Bott theorem for infinite-dimensional representations. This conjecture was soon proved by Wilfried Schmid, then a student of Griffiths. It is fundamental for the general theory of Shimura varieties and for $\{\mathfrak{g}, K\}$-cohomology. In addition, in a letter to Harish-Chandra, I had presented an idea that he would later use, or at least mention, in a paper whose title escapes me. It is an idea for how one could, based on published work of Harish-Chandra, perhaps prove the Plancherel formula. In the end, he proved the formula differently. I was nonetheless flattered by the mention. I can not find a copy of the letter, and the letter itself is probably in a cardboard box in the attic of his house, where his widow can not find it. I do not remember what I had suggested exactly.

Despite these small successes, I felt at the end of the year in Berkeley that I had not come very far, and rightly so. I had no real goal.

The journey from Berkeley to Princeton passed through Boulder, where I participated in a meeting on algebraic groups and discontinuous subgroups that Armand Borel and G.D. Mostow had organized. This meeting was my introduction to the algebraic and arithmetic theory of algebraic groups, and I have never learned, before or afterwards, so much at a conference.

5 Wieder Princeton (Princeton again)

The year 1965/66 was not particularly encouraging in terms of my mathematical progress. This year and the next I gave some lectures to undergraduates, that I enjoyed, for example, on algebraic curves from Walker's book, elementary but instructive, and also on applied mathematics for students in the engineering school of the University, especially for students of electrical engineering, where I had the opportunity of studying Maxwell's book *Electricity and Magnetism*. Having fun like that did not make a good impression on the chairman of the department, who thought the elementary material below the level of a professor at Princeton University. Nevertheless, the electrical engineers were happy with me and my teaching, but some mathematics students, including Wilfried Schmid, were dissatisfied with the level of Walker's textbook, in his case, probably rightly.

I devoted much time and energy to two lofty attempts during this year to no avail. I was looking for, on the one hand, a generalization of Hecke's theory and, on the other hand, a nonabelian class field theory. The second was hopeless; the first was not. The second was more of a dream, but I spent a lot of time trying to attach L-functions to general automorphic representations. I did not find anything satisfactory and I was discouraged.

I had an invitation to give a lecture at the meeting of the IMU in Moscow, probably because of my contributions to the theory of the Eisenstein series. I declined it, partly because I thought I had not done anything beyond this theory, but also because I did not want to visit Russia or the former Soviet Union until I had a satisfactory knowledge of the language. Until then, I had only come to the United States from Canada. I had never visited a real foreign country and had no idea what was expected of a North American overseas. Not only did I overestimate the expectations in terms of language skills, but also the level of the lectures at a conference of this kind. I do not regret that I neither rushed to an international conference nor rushed to visit Russia. I now hope to get there soon and better prepared.

It is quite possible that my general fatigue and discouragement played a role in the decision. I had already thought about abandoning mathematics during the year at Berkeley because I did not seem to be succeeding. I had expressed these thoughts to a Turkish friend and he suggested that I spend some time in Turkey. He was not a mathematician, but an enthusiastic patriot. At first I did not respond to the proposal. But sometime in the year 1965/66 I started to consider the proposal seriously. Since my wife was a brave being — and still is — I could, in my rather selfish way, in spite of our four children, seriously consider the opportunity. We decided to go. Afterwards, I never regretted the decision, and my wife rarely. For me in particular, it has brought a lot of unexpected things over the years.

It was not a well-considered decision. I knew nothing about a foreign country, had no serious knowledge of a foreign language, and had no idea how to learn a foreign language. I greatly underestimated the difficulty of trying to really settle in a foreign country. Careless as the decision was, it was also a relief to me. As a mathematician, I was no longer under pressure to achieve anything. I started learning Turkish and resumed studying Russian. I dreamed of a trip from Russia to Turkey over the Caucasus, which never came about.

Since I was a bit bored after the decision, I started, without any particular goal, to calculate the coefficients of the functional equations of the Eisenstein series. I then saw not only that these functions were quotients of Euler products, but also that the numerator, whose form was also the form of the denominator, could be described in terms of representation theory. The representations in question were algebraic representations of a complex algebraic group, the L-group, a name that was proposed only much later by Tate. Not only had I recognized the shape of these functions. Thanks to the general theory of Eisenstein series, I was also able to prove that the functions could be continued as meromorphic functions.

I had to calculate the representation ρ in question every time, specifically for the various parabolic subgroups ${}^LP = {}^LM\,{}^LN$ of the L-group ${}^LG$. Only later, during a lecture, Tits recognized that the desired representation is always the natural representation of ${}^LM$ on the Lie algebra of ${}^LN \subset {}^LG$. In this way, I had attached to each M, and hence to almost every G, initially only for split groups, a small set of L-functions. The general definition of the function $L(s, \pi, \rho)$ then was obvious, and my goal of defining the Hecke L-functions in general, was, as soon as I realized it, achieved. The special ones, those that occur in the theory of Eisenstein series, were later thoroughly investigated by Shahidi.

At the time, we lived, my wife and I with four children, on Bank Street, an alley in Princeton just a few steps away from the University, so that I could work in my university office in the evenings and during holidays. The office was not in what is now Fine Hall, which did not yet exist, but in the then current Fine Hall, which housed the Mathematics Institute and which was solely dedicated to mathematics and mathematicians. The building was built in 1929 based on a concept by Oswald Veblen. I consider it to be one of the small blessings of my life that I was able to spend a few years of my professional life under its roof, and also that I had left the university before the move to the new large building. The old brick building is still there, but inside so devastated that you can no longer recognize it. During my final years at the University, I had a modest room to the right of the main entrance, with oak paneling and leaded window panes, as in all rooms in the building. To the right of the entrance was a seminar room with a large table and a slate, probably ten meters long, and a view of the garden of the residence of the rector of the university. I could think there in the evenings and during holidays quietly alone.

In particular, not long after I came up with the general definition of automorphic L-functions, standing at the window of this room, I pondered how to prove their analytical continuation. I also thought — my memories are not very precise in this regard — generally about their definition and properties. It was certainly during the Christmas holidays in 1966/67. Suddenly I realized a possible solution to all mysteries. What I had in mind, was a satisfactory definition of the functions

$$L(s,\pi,\rho) = \prod_v L_v(s,\pi,\rho) = \prod\nolimits'_v \frac{1}{\det\left(1-\frac{\rho(\gamma(\pi_v))}{q_v^s}\right)},$$

although the functions L_v were unknown at a finite number of places. These places are here omitted. I still had no idea how to define them. The element $\gamma(\pi_v)$ belongs to a conjugacy class in a complex Lie group, now denoted ${}^LG$. Although I was able to prove the existence of a meromorphic continuation for a significant number of these functions, I had no idea how I could prove the existence of the meromorphic continuation in general, or whether its holomorphic continuation could be proven at all.

I thought about this in front of the window. Suddenly — the whole thing was, at least according to my memory, present in my head — I recognized or remembered the following:

1) Tamagawa had already considered the function $L(s,\pi,\rho)$ when the group G is an inner form of $\mathrm{GL}(n)$ and ρ a representation of ${}^LG = \mathrm{GL}(n,\mathbb{C})$, and had proved analytic continuation. I saw no reason why his proof would not apply to $\mathrm{GL}(n)$.

2) For the group $G = \{1\}$, my general definition just gives the Artin L-functions attached to a complex representation of a finite Galois group. It would be possible to carry out in general what Artin had done for one-dimensional representations. He had proved that every finite-dimensional abelian Artin L-function is equal to the L-function attached to an idèle class character [Idelklassencharakter]. In other words, an n-dimensional representation of the group $\mathrm{Gal}(K/F)$, where $[K\colon F] < \infty$ and $[F\colon\mathbb{Q}] < \infty$, can also be interpreted as a homomorphism

$$\phi\colon \mathrm{Gal}(K/F) = {}^LH \to {}^LG = \mathrm{GL}(n)\times \mathrm{Gal}(K/F), \qquad H=\{1\}, \quad G=\mathrm{GL}(n).$$

The generalization of Artin's theorem would be that this homomorphism is attached to an automorphic representation $\pi(\varphi) = \bigotimes \pi_v$ of the group $\mathrm{GL}(n,\mathbb{A}_F)$ such that for almost every place v the image, $\varphi(\mathrm{Frob}_v)$, of a Frobenius element Frob_v is equal to the Frobenius-Hecke class $\{\gamma(\pi_v)\}$.

3) If that were the case, then I could just as well hope that if two reductive groups H and G are given over a finite algebraic number field F, together with a map $\phi\colon {}^LH \to {}^LG$ that is compatible with the maps ${}^LH \to \mathrm{Gal}(K/F)$ and ${}^LG \to \mathrm{Gal}(K/F)$, and an automorphic representation π_H, then there exists an automorphic representation π_G such that, for almost every place v, the image of the class $\{\phi(\gamma(\pi_{H,v})\}$ is the class $\{\gamma(\pi_{G,v})\}$.

I would add that the class $\{\gamma(\pi_v)\}$ is often called the Satake class, a name I never liked. Within the framework of the representation theory of real semisimple groups, Harish-Chandra discovered and proved the structure of the ring of K-bi-invariant differential operators. The knowledge of this structure was fundamental to the spectral theory of spherical functions, which was developed before the general Plancherel formula. When the structure theory of semisimple p-adic groups was developed and their representations began to be investigated, Satake realized that the Iwasawa decomposition for p-adic groups allows one to prove a theorem for spherical functions on p-adic groups similar to Harish-Chandra's theorem. That was useful, though not particularly difficult, and the isomorphism underlying functoriality is a direct consequence of this theorem.

The L-group of a quasi-split semisimple or reductive group over a p-adic field F that splits over an unramified extension K, $[K\colon F] < \infty$, can be defined to be $\widehat{G}(\mathbb{C}) \rtimes \mathrm{Gal}(K/F)$. Let Frob be a Frobenius map. The fundamental isomorphism is that between the algebra of spherical functions on $G(F)$ and the algebra of restrictions to $\widehat{G}(\mathbb{C}) \rtimes \mathrm{Frob}$ of invariant algebraic functions on ${}^{L}G(\mathbb{C})$. It can be interpreted as a reformulation of the Satake theorem. The assertion that every homomorphism of the second algebra to $\mathbb{C}$ is given by an element of the set $\widehat{G}(\mathbb{C}) \rtimes \mathrm{Frob}$, is therefore also a consequence of the Satake theorem. Formulated as this assertion, but only in this way, the theorem is fundamental, because it is then the core and the origin of the problem of functoriality. However, this functoriality only makes sense within a theory built up from invariant harmonic analysis, endoscopy, Hecke L-functions, and the theory of Galois extensions as inherited from Dedekind, Frobenius, and others. In this theory, spherical functions play no special role in determining element in groups except for those that are quasi-split and split over an unramified extension. I would highly recommend highlighting this structure and its architectural elements by adding the conjugacy class $\{\gamma(\pi)\}$ in ${}^{L}G$, which is attached to an irreducible unramified representation π^{st}, called the Frobenius-Hecke class. This class was introduced in the letter, but not named. Unfortunately, someone who did not understand the matter, later presumed to name it.

I have described in the letter my thoughts in front of the window. I read the letter now probably for the first time since I wrote it. I find, I admit impudently and also astonished, that if I do not miss anything, firstly, there is nothing wrong in the letter, and secondly, the essentials are there, although the assertion that everything came to me in Fine Hall at once, is a little exaggerated.

6 Der Brief (the letter)

As I write this article, I reflect on the circumstances under which the letter was written. I have already mentioned, at the beginning of this essay and elsewhere, that its creation was the result of chance. On January 6, 1967, I attended a lecture by Chern at the Institute for Defense Analysis, which was at the time in Princeton, where it can still be found. Since I am unfortunately a rather punctual man, I arrived a little early. Weil, who once remarked at a meeting of mathematicians at the Institute as we and others many years later waited for an often late colleague, "Punctuality is the politeness of kings", came almost at the same time. We stood alone in the hallway outside the door, which was locked. Since we already knew each other, although not particularly well, we had to talk. He stayed silent; I searched a little awkwardly for a topic. My thoughts in front of the window occurred to me and I started telling them to him.

I probably did not express myself very clearly, and in recent years I thought I remembered that Weil then did what I have done in my life, when a young person who was intrusive and probably not quite right mentally, tried to explain something confusing to me. He suggested that I set out my thoughts in a letter and send the letter to him. Under these circumstances, one would expect there would be no letter, and that one would have obtained an easy way out of a possibly troublesome situation without having offended or hurt anyone. To his astonishment, Weil received a letter. This letter was preceded by a brief explanation from which I recently realized that Weil treated me with more courtesy than I remembered. He did not seem able to understand me, but he had the courage or the kindness to suggest that I visit him in his room at the Institute on a later occasion. I had decided myself that a written account of my discoveries and speculations would be more productive. It turned out that way in the end.

While I was not overly excited about my Christmas thoughts, they certainly represented progress. Obviously, I took the opportunity to write them down with enthusiasm. It is also clear that I was aware of their immaturity. Partly because of Weil's position in mathematics at the time, the letter later achieved fame that was not intended and could not have been intended. Over time, this fame has probably somewhat distorted my own assessment of the letter and its content.

Since, after the letter was written, I happened to meet Harish-Chandra, whom I had got to know better in the meantime, I asked him to hand the letter to Weil. Afterwards the proposed visit to Weil's office took place. As far as I could see, he found the letter and its contents not particularly convincing. Rather, he explained two important things that were previously unknown to me. First was the content of his work on determining Dirichlet series by functional equations, a further development of the Hecke theory; either he explained it to me or gave me a reprint. The second is unlikely, as we probably spoke together again in January and the work was only published in 1967. He also gave me a reprint of his work on what many of us now call the Weil group without really thinking about it. The development of the theory of this group and related cohomological questions is described in detail in a historical article of Helmut Koch *The history of the theorem of Shafarevich in the theory of class formations* that appeared in the collection *Class Field Theory — Its Centenary and Prospect*. These two works of Weil influenced me in the next development of my thought.

My first concern in the weeks after this conversation was probably preparing for some lectures at Yale University, which took place in April, and in which I defined the L-functions $L(s,\pi,\rho)$ and described the examples I could at least partially treat because of the theory of Eisenstein series. We also had, my wife and I, to deal with the upcoming trip to Turkey because we had decided to spend at least a year there at the Orta Doğu Teknik Üniversitesi. I still spent the evenings in my office at the University, where I mainly occupied, or wanted to occupy myself, with my new ideas. Unfortunately, my thoughts were often interrupted by the chairman of the department, who was obsessed with the idea that the department had become weaker, and that the first step in improving it was not to fire various young mathematicians, which he or the department could not, but to chase them out by tormenting them so that they left voluntarily. At first I did not take him seriously, but he visited me constantly in my room and described to me a future in which the value of the currency fell, but my salary remained constant, so that, despised by my colleagues and impoverished, I would lead a miserable life. Although I did not take him very seriously, I saw no countermove on the part of my colleagues and was a little worried and undecided whether it would be better to just give up my job and not only to leave Princeton for a year, but forever. My wife quickly realized that this indecision on my part was pointless and recommended that I quit my job immediately. Still hesitating, I wanted to speak to a dean anyway to explain the situation to him. Unfortunately, the dean was a stupid guy and, instead of listening to me, he just wanted to know if my wife agreed with my hasty decision. I quit right away.

It was a happy decision in every way. I immediately accepted the offer of a position at

Yale, where, after the year in Turkey and before finally coming to the Institute in Princeton, I had four happy and profitable years. In the course of this story, a colleague who undoubtedly meant well to me told me that the chairman had discussed my case with Weil, who said that my reputation was “overblown”. Oddly enough, I did not take that assessment very seriously. I was already aware at the time that Weil’s assessments could be arbitrary. Also, I had no idea of my reputation at the time, and the term ”overblown” is relative.

What is very clear to me is that the chairman, and perhaps also Weil, did me a great favor. Under the rules in force at the time, the Institute was not allowed to appoint a mathematician who was employed at Princeton University, so that if I had stayed at the University I would never have been appointed to the Institute and, along with my wife, the position at the Institute has been one of the two great blessings of my life.

I owe this blessing above all to Harish-Chandra. I thank him even more, because sometime in the two years 1965/66, 1966/67, probably in the fall of 1966, when my self-confidence was still not very strong, although its low point was behind me, he already had, to my surprise, mentioned to me that he had suggested to his colleagues that I be appointed a professor at the Institute. They were not particularly enthusiastic about his proposal. That was not important to me. It was more important to me that Harish-Chandra had such a positive opinion of me. Although I admired many of the permanent members of the Institute, it had never occurred to me that I could be one of them, nor did I expect Harish-Chandra to take up his proposal again in the future. The main thing was the idea that Harish-Chandra took me seriously as a mathematician. When he repeated the suggestion four years later, the topologists still hesitated. The others had then been more pleased with me, Weil apparently not because of the letter, but because I had appropriated the Weil group.

Although not without flaws, Weil was someone I liked when I was young and later when we were colleagues at the Institute. My first encounter with him was impressive for unexpected reasons. During my first years at the University, the current literature seminar took place every Wednesday under Weil’s leadership. Each time in this seminar a lecture was given, either by a mathematician at the Institute, which could be a temporary guest or a permanent member, or by a mathematician at the University, who could also, but rarely, be a student. Weil sat alone in the front row of the large lecture hall with a newspaper in hand, apparently reading it, and, with no good intent, often interrupted the lecturer. Some, even students, did well under these attacks, and some did not. I took part in the seminar every week and learned a lot about the mathematics of the day.

The lecture hall was on the ground floor opposite to my first office, which I shared with others, as well as to my later office, next to the entrance, where I was alone. One Wednesday, during either my first year or my second year, someone knocked on the door and immediately came in to talk to me. To my astonishment, it was André Weil, with whom I had not spoken before. He immediately sat down on a chair and at the same time threw one leg over the armrest. We talked about mathematics, and I, inexperienced as I was, immediately expressed myself about various topics in the theory of modular forms, which I had just begun to study. My remarks, as I later realized, were often stupid. He did not react to my stupidity. Why he came, for what purpose, has never been entirely clear to me. I assume he heard my name from someone, probably Bochner, and instead of asking me to come to him in his office, which would have been more common, he came to me. That amazed me then and still amazes me today. Weil was certain of his value in the world and rarely forgot it. On the other hand, he did not insist on his dignity.

Over the next few years I met him now and then, hardly regularly. He twice invited me to a series of private seminars where he lectured on the problems he was working on. The second time was some time after the letter. He wanted to extend his variant of Hecke theory to a general algebraic number field, especially to a field with complex places. When I was looking for a generalization of Hecke theory to general groups, I myself had often considered the case SL(2) or GL(2), so that I had a solution to the problem already at hand more or less. This is the theory of Whittaker functions for SL(2), which is actually an exercise in the theory of ordinary differential equations with irregular singular places, a theory that impressed me when I was studying Coddington and Levinson's book. I wrote down my solution and sent it to him, perhaps even before we flew to Turkey. The subsequent letters, which were then sent to Jacquet, were obviously written in Turkey.

It is noteworthy that these letters, the content of which was included in the book with Jacquet, were not directly inspired by the questions in the first letter itself, but by a question of Weil, who wanted to develop his Hecke theory. This was not very difficult for archimedean fields, but for non-archimedean fields I obviously benefited from Kirillov theory, which I came across in the library of the University of Ankara. Nonetheless, inspired by the two letters to Weil/Jacquet, I began to think about the local form of the conjectures in the original letter. This letter dealt almost nowhere with a local theory, neither a local correspondence nor local functoriality. The local correspondence came only after I became acquainted with the local Weil group. But when I wrote these letters, that is, during the first few months in Turkey, the form of the local correspondence was clear to me in almost every detail. I spent my time there working through these details and by the end of the visit I understood various things, as one can see from the letters to Weil, Jacquet, Harish-Chandra, Serre, and Deligne cited in Parts 5 and 6 of `http://publications.ias.edu/rpl`. The other writings and works that I will refer to in the following are also located here. The progress made is briefly summarized here:

a) a clear idea of the possible local and global correspondence for the group GL(2);

b) the role of the special representations in this correspondence;

c) the existence of the ϵ-factors.

The special representation was a mystery to me at first, so I was happy when I read the work by Serre, that I mention in the letter to him, because from this work it was immediately clear to me that the special representations were indispensable because they were attached to elliptic curves having non-integral j-invariants or, better, to their ℓ-adic representations. Serre and Deligne were involved in investigating the corresponding phenomenon for general ℓ-adic representations, and, thanks to their results, it became clear what to expect in general. However, I would have preferred, and would still prefer, if, for this purpose, the Weil group had not been replaced by what is now called the Weil–Deligne group, but by a direct product $\mathrm{SL}(2) \times W_F$. According to the Jacobson–Morosov theorem, the two forms are equally useful. This second possible form of the Weil–Deligne group has the advantage of being at home in a semisimple, completely reducible, world.

The existence of the ϵ-factors presented and still presents difficulties. I had proven their existence, and the proof was complete if I took two difficult lemmas, or theorems if you will, from Dwork. Even if I accepted these lemmas, it was hard to prove. The lemmas were much more substantial than the rest of the proof and their proofs were probably longer. I tried to create a complete proof with the help of the dissertation of K. Lakkis, to whom Dwork had sent his notes. Unfortunately, Lakkis used them, not to prove the two lemmas of Dwork, but only to prove them up to the sign. Signed or unsigned, I have not been able to find a complete proof of these lemmas whose length was not impossibly extended. Fortunately or unfortunately, Deligne noticed that the existence of the ϵ-factors was easy to prove by global methods, once one had seen their validity, which in my opinion was not at all trivial. I was happy to give up my attempts, which had lasted too long. However, it continues to be important to find a local proof for this local theorem. Although I am not certain, because strong convictions are not appropriate without proof in mathematics, I believe nonetheless that if we ever manage to find a proof of functoriality in general, then we are almost certain to find at the same time a local proof for the existence of the ϵ-factor. The global proof is very welcome, but as a temporary aid. Without it we would have been stuck at this point for a long time.

In the letter to Weil, there was another point that was always important to me, but whose importance has only recently been generally recognized, namely, the central role of the quasi-split groups. In the letter, it was not particularly emphasized. In the notes that I wrote together with Jacquet, the choice of the Weil representation, as it appears in Weil's treatment of Siegel's work, as a tool was initially a little arbitrary, because it gave the impression that this representation was of fundamental importance in the theory, which was certainly not my opinion. The letter already mentioned that it was probably possible to reduce the theory of automorphic forms and representations for a general reductive group to the case of its quasi-split form. For reasons that are no longer clear to me, while the lecture notes were being written, it appeared possible to prove a global assertion of this kind for forms of GL(2), if only one knew that the local characters of the representations of two forms of GL(2), the group GL(2) on the one hand and the multiplicative group of a division algebra on the other hand, that are associated with each other using the symplectic group, are equal up to sign. Jacquet succeeded in proving this claim. This correspondence between automorphic forms for the two groups certainly did not appear for the first time in our lecture notes. Others had proven special forms of the proposition before us. But it was formulated there for the first time as a theorem that could be expressed briefly and simply, and which in all likelihood was also universally valid and generally fundamental. The idea for proving Weil's conjecture on the volume of a quotient $G(F)\backslash G(\mathbb{A}_F)$ by first showing it for quasi-split groups using the theory of Eisenstein series, and then using the trace formula to transfer it to all semisimple groups, occurs in these notes for the first time. These ideas have now been incorporated into the theory of endoscopy, initially in the work of Kottwitz. Others later contributed significantly to the topic. There is still much to be done, however. To what extent Weil read my letters is not clear to me. I am convinced that he did not try to understand the first letter for a long time. He must have given Jacquet the second letter, which concerned the group GL(2), and Jacquet then explained at least part of this letter to him. Perhaps even a part of what is further contained in our lecture notes. He then probably gradually became aware that there is a relation between automorphic forms for GL(2) and two-dimensional Galois representations, without realizing that this was part of what was in my first letter and what I proved in my later letters, and what Jacquet had at least partially explained to him. Only later, when it became clear to me that certain misunderstandings had arisen in this regard, at least among some influential number theorists, and that the content of my letters and the lectures *Problems in the theory of automorphic forms* had been overlooked by many, did I bring to Weil's attention again the contents of the first letter. Thereupon, with Borel's help, he looked at the letter and understood it sufficiently well to see that he ran behind [hinterherlief]. I do not believe, however, that he ever later tried to understand the scope of functoriality. My own views have, indeed, matured only gradually over the years.

I admit that I am somewhat ambiguous on Weil. As a colleague I got on well with him, although we were very different in age, education, and childhood environment. I also found that he lacked a sense of gratitude, if not to people, then to institutions. His personality was difficult in other ways too. But he was also charming. Although he laid much emphasis on his education and could be pedantic, he also showed a real interest in smaller, more modest things, for example, he was happy to talk with a mathematician from Quebec in order to learn the local idioms of the language, and he wanted also to attend my wife's sculpture lessons as a pastime and solace after the death of his wife. Unfortunately he was far too clumsy. As a substitute, my wife created his portrait for him, and he liked to talk to her about everyday things while the sculpture came into being under his eyes. As far as mathematics is concerned, I need hardly repeat that Weil had a great influence on algebraic geometry and arithmetic and, as hardly any other mathematician of his time, saw the depths of their mutual relationships. His broad knowledge of the history of mathematics and his ability to use that knowledge to introduce entirely new points of view are also unique. I admire and envy his gifts and achievements very much. Nevertheless, I found that he was rather weak as an analyst, and also as an algebraist.[Ich fand nichtsdestoweniger, daß er als Analytiker, und auch als Algebraiker, eher schwach war.] He was aware of this but, since in his opinion it did not correspond to his place in mathematics, he did not want to admit it, neither to himself nor to the rest of the world. In my opinion, this weakness in analysis, which he denied, was inherited by his admirers, and the mathematics of today, which otherwise owes him so much, was influenced in an unfortunate way

7 Yale und Bonn (Yale and Bonn)

I returned from Turkey in the fall of 1968 and, forever I thought, resumed my normal life in New Haven. As a first task I wanted to write down the results of my correspondence with Jacquet and complete the proof of the existence of the ϵ-factors by finding a shorter proof of the two lemmas of Dwork. I was convinced after this year in Turkey and the results achieved there that the conjectures described to Weil in the first letter had something to them, and that the time had come to offer them to the world. The lecture notes with Jacquet came about without major problems. A complete proof of the existence of local ϵ-factors would be today only possible with the help of Dwork's estate, which is kept in the library of Princeton University, but I have never looked at it. Almost all of my contributions can be found at https://publications.ias.edu/rpl/section/22. My first public exposition of functoriality was contained in a lecture I gave in Washington in 1969 and which was published shortly afterwards. In addition to the letter, the lecture contains part of the progress made in the meantime. I went into more detail on the local theory and emphasized the special role of quasi-split groups. The role of the special representations is also indicated. I also explain how functoriality, if it existed, could be used to prove Ramanujan's conjecture, also in a general form. Oddly enough, I barely touched on the related question of the Sato–Tate conjecture and its generalization. When I read the lecture, I have the impression that the lecturer was aware of this conjecture and its possible generalization, but that, due to a lack of knowledge of arithmetic and the theory of automorphic representations, he did not dare to suggest a general statement. That was wise of him.

The remark about the consequences of functoriality for the Frobenius-Hecke conjugacy classes of an automorphic representation, namely, that their eigenvalues are frequently all of absolute value of 1, occurred to me on a train platform in Philadelphia, as I was thinking about the famous Selberg–Rankin estimate. Since it is precisely this remark that is essential to Deligne's proof of the fourth Weil conjecture, I thought for a moment when I learned how he proved it, "If I had only known Grothendieck's theorem on the continuation of the L-function attached to an ℓ-adic representation ... " Now I know better. Into his proof there went a lot of experience and an extensive theory, the beginnings of which I have hardly mastered even today.

It was only during my Yale years that I began to think about such things, initially when I was in Bonn for the 1970/71 academic year. I made up my mind to learn German, and at the same time to familiarize myself with Shimura's work on what I later called Shimura varieties, by lecturing on his work in German. My listeners were very polite and very patient. I am grateful to them to this day.

My sources were the many works of Shimura on this subject. I initially started with the simplest case, modular curves, but probably at the same time incorporated the work of Shimura's. An incident was decisive.

When the Shimura variety is defined by a group G, then there is attached to each element $\pi = \pi_\infty$ of the group, or rather of the connected component of the group $G(\mathbb{R})$, a discrete series representation on a subspace of the cohomology of a holomorphic vector bundle on the Shimura variety. The dimension of the corresponding cohomology is the multiplicity with which π_∞ occurs in $L^2(\Gamma\backslash G(\mathbb{R}))$. It is better, as we do today, to consider an adèlic space $G(\mathbb{Q})\backslash G(\mathbb{A}_\mathbb{Q})/K$. At the time, before Deligne's Séminaire Bourbaki talk, no general notion of a Shimura variety was available. Shimura had considered different possibilities for G individually, as had Siegel in different ways. Neither the one nor the other had tried to examine all possibilities. Siegel's aims were anyway quite different from those of Shimura. I had to rely on the work of Shimura. Obviously, I was looking for a generalization of Eichler–Shimura theory. What I had available other than the work of Shimura, was the existence of discrete series; their existence had been recognized a few years earlier by Harish-Chandra, and then proved when he developed the representation theory of semisimple Lie groups. In addition, at Boulder, I had learned of the relationship between $(\mathfrak{g}, K)$-cohomology and the cohomology of various vector bundle on $\Gamma\backslash G/K$. As the last essential element, I had a theorem that Wilfried Schmid had proved a few years earlier, namely, the existence of a geometric realization for every discrete series representation.

Unfortunately, I cannot go into the existence of the discrete series here. I repeat, however, what I have emphasized on other occasions, that the knowledge of the existence of these series and its proof was one of the great mathematical events of the middle of the last century. In particular, their existence and properties were essential to the discovery and development of endoscopy. I have noted with regret that many number theorists and many geometers are not even aware of this. It is also very important not to forget here that, for a given group $G(\mathbb{R})$, or again better its connected component $G^0(\mathbb{R})$,[2] discrete series only exist if the rank of G is equal to the rank of its maximal compact subgroup K or its connected component K_0.

Then every finite-dimensional irreducible holomorphic representation σ of the group $G(\mathbb{C})$ corresponds to a finite number of irreducible representations in the discrete series of $G(\mathbb{R})$. The set of representations associated with a given σ is often called an L-packet. The number of representations in a packet is the same for all σ, and it is important to know this number. Let K_0 be the connected component of K and T a Cartan subgroup of K_0. Since all Cartan subgroups in K_0 are conjugate under K_0, K is the normalizer of K_0 in K or in $G(\mathbb{R})$. Let Ω_G be the Weyl group, equal to the quotient $N_T(\mathbb{C})/T(\mathbb{C})$, and Ω_K the quotient $N_T(\mathbb{R})/T(\mathbb{R})$, where N_T is the normalizer of T as an algebraic group. There are $[\Omega_G : \Omega_K]$ elements in each L-packet. We can also introduce a number $[\Omega_K : \Omega_{K_0}]$ in the same way.[3]

[2]Better notation, $G(\mathbb{R})^0$.

[3]I don't understand the actual sentence.

For the groups GL(2) and PGL(2), $[\Omega_K:\Omega_{K_0}] = [\Omega_G:\Omega_K] = 2$ and $[\Omega_G:\Omega_K] = 1$. For SL(2), which, however, does not directly occur in the context of the Shimura varieties, $K = K_0$, so that $[\Omega_K:\Omega_{K_0}] = 1$ and $[\Omega_G:\Omega_K] = 2$. In general, we usually have $[\Omega_K : \Omega_{K_0}] = 1$.

Initially, we imagine the Shimura variety S to be defined by a quotient $\Gamma\backslash G(\mathbb{R})/K$. A finite-dimensional irreducible representation σ of $G(\mathbb{C})$ defines a vector bundle on this algebraic variety. Since my knowledge of algebraic geometry was inadequate at the time — and remains inadequate today — I intially simply assumed that there corresponds to this vector bundle an ℓ-adic representation whose L-function $L(s,\tau)$ I wished to investigate. For reasons to be explained, I limited myself to the cohomology of the bundle in the middle dimension. As with the Eichler–Shimura theorem, I wanted to try to prove that, except for a finite number of factors, this equals a product of the automorphic L-functions that I had introduced. If one imagines the Langlands correspondence to be an identification of a tannakian category defined by motives with a subcategory of a category defined by automorphic representations, then it was only in Bonn and only for Shimura varieties that I seriously dealt with this correspondence. My first steps in this direction, for example in the book with Jacquet, were rather modest, either local or global applications of Hecke's method in the Weil's formulation, both for the group GL(2), or, occasionally, various hesitant remarks. In particular, I had previously hardly come into contact with algebraic geometry.

What I realized in the autumn of 1970, when I was standing on Wegelerstrasse in Bonn, no more than 200 meters away from Schmid's parents' house, was that as a result of the theorem he had proved, every element of the discrete series associated with the representation σ provides a one-dimensional contribution to the cohomology of the corresponding vector bundle in the middle dimension, which is equal to the dimension of the Shimura variety. More precisely, because of the intertwining of several elements of the L-packet of the connected component $G^0(\mathbb{R})$ of $G(\mathbb{R})$ into a single one, the dimension of the contribution is equal to $[\Omega_K : \Omega_{K_0}]$. In principle, then, all together the contribution from an L-packet is $[\Omega_G : \Omega_{K_0}]$-dimensional.

If we want to better understand this statement, which has so far remained fairly imprecise, we need to think adèlically. Then the corresponding Shimura variety, which need no longer be connected, is a quotient $G(\mathbb{Q})\backslash G(\mathbb{A})/K_0K_{\text{fin}}$, where K_{fin} is an open compact subgroup of the group $G(\mathbb{A}_{\text{fin}})$. The set $\mathbb{A}_{\text{fin}}$ is the ring of finite adèles. For simplicity, we also assume that the quotient $G(\mathbb{Q})\backslash G(\mathbb{A})$ is compact. Then the corresponding Shimura variety is complete. The contributions to the cohomology are contributions from a representation $\pi = \bigotimes \pi_v = \pi_\infty \pi_{\text{fin}}$, where π_∞ is an irreducible representation of $G(\mathbb{R})$ that lies in the L-packet associated with σ, and π_{fin} is an irreducible representation of $G(\mathbb{A}_{\text{fin}})$. Let $m_\pi(K_{\text{fin}})$ be the multiplicity of the trivial representation of K_{fin} in π_{fin}. The same σ defines the vector bundle whose cohomology we are investigating. Let m_π be the multiplicity with which π occurs in $L^2(G(\mathbb{A})\backslash G(\mathbb{A}))$. The contribution of π to the cohomology in the middle level now has dimension equal to

$$m_\pi m_\pi(K_{\text{fin}})[\Omega_K:\Omega_{K_0}]. \tag{1}$$

The representation[4] π_f determines the L-function attached to the representation π except for its Γ-factors. Let $\pi_\infty, \pi'_\infty, \pi''_\infty, \dots$ be the elements of the L-packet associated with σ and let $\pi' = \pi'_\infty \otimes \pi_v$, $\pi'' = \pi''_\infty \otimes \pi_v, \dots$ Then, for all holomorphic representations ρ, the L-functions $L(s,\pi,\rho)$, $L(s,\pi',\rho)$, ... are the same, so that if all the multiplicities $m_\pi = m_{\pi'} = \cdots = m_{\pi_{\text{st}}}$ are the same, where π_{st} is now the common name for the packet

[4]Should be π_{fin}

$\{\pi, \pi', ...\}$, then it is obvious that the part of the cohomology in the middle dimension that corresponds to this L-packet, whose dimension must be

$$m_{\pi_{\mathrm{st}}} m_\pi(K_{\mathrm{fin}})[\Omega_G : \Omega_{K_0}], \tag{2}$$

must be motivically defined. Then it would be nice, I said to myself at the time, if there were a representation ρ of the group ${}^L G$, whose dimension is exactly $[\Omega_G : \Omega_{K_0}]$, so that one might conjecture that the L-function of this motivically defined part is

$$L(s, \pi, \rho)^{m_{\pi_{\mathrm{st}}} m_\pi(K_{\mathrm{fin}})}. \tag{3}$$

I then diligently calculated for each group G that defines a Shimura variety and found each time that the desired representation was present. Not much later it turned out that the highest weight involved was an essential element from the start in Deligne's general definition of a Shimura variety. For me at first, that was not the essential thing. The disturbing thing about these considerations was whether the assumption $m_\pi = m_{\pi'} = \cdots = m_{\pi_{\mathrm{st}}}$ was reasonable. For PGL(2), as already noted, it is true because the L-packet consists of a single element. Otherwise, I easily found, it does not always apply.

That was not so bad. One could imagine that, for this or that reason in the motive, only part of the expected ℓ-adic representation occurred and consequently only part of the function (3). Such considerations quickly lead to endoscopy, albeit not in its current form, but to the question of how the multiplicities m_π in an L-packet depend π. But L-packets are introduced not only at infinity, there are also global packets and local packets at every place v. The first case to be examined was obviously the group SL(2) where the problem was much simpler than in general. In addition, the trace formula was already available for this group. When Labesse came to Bonn for a short visit in the second semester, I made him aware of these problems. If I am not mistaken, at the time I had already asked Shelstad, who was then a student at Yale, some questions about groups over $\mathbb{R}$, for which Harish-Chandra's general invariant harmonic analysis for real groups was available. Her research, then and later, has been very instructive to me.

During my visit to Bonn, I met and got to know Deligne in Bonn and also in Paris. Not long after my return to Yale, only two years later, the somewhat unfortunate Antwerp conference took place. Out of a kind of loyalty, I initially hesitated to attend, but eventually agreed. Deligne then suggested to me that I should take the geometric part of the theory, namely the Eichler–Shimura theory as he had developed it, and he the representation theory part, as it is in my book with Jacquet. For me that was a suggestion like Bochner's suggestion to lecture on class field theory. I accepted trembling. Influenced by an idea of Ihara, I wanted to use the trace formula. I did it, with appreciable success, but my lack of knowledge of algebraic geometry left some gaps in my exposition. The conference organizer,[5] who was unfamiliar with representation theory, did not like the lecture and left the room. The fixed-point formula that I proposed at the lecture was quickly proven by Illusie, and I believe that it still today plays a role in the advanced theory of Shimura varieties.

8 The Institute for Advanced Study.

I came to the Institute for Advanced Study almost forty years ago, right after the Antwerp conference, and have remained here as a permanent member of the faculty, now retired and

[5] Serre.

emeritus. The topics that I started or continued at the Institute still occupy me, in large part because I have not achieved my goals. Since it is hardly excluded, rather very likely, that I will never achieve these goals, I would like to describe the goals here and not so much the few achievements, especially as the goals are, in my opinion, sometimes misunderstood. There are four topics: (i) endoscopy; (ii) Shimura varieties and motives; (iii) universality; (iv) beyond endoscopy. All but the third are related to each other. I will start with the others and only later come to the third, where I not only failed to achieve my goal, but also never made it comprehensible.

What I learned about endoscopy for general groups during my years at the Institute was explained in my 1980 Paris lectures. The two later works on endoscopy that I wrote with Shelstad, are not unimportant, but the basic idea in these papers came more from her and her investigations of real groups than from me. However, I find endoscopy amazingly beautiful and stable invariant harmonic analysis also very important, and I am happy with all the progress in this area, where much still remains unknown to us.

For the Shimura varieties, which have become very popular in recent years, I have more conjectured than proven, especially in the articles for the conference on Hilbert's problems in DeKalb and the conference in Corvallis. The development of the theory of Shimura varieties is closely related to the development of endoscopy. Kottwitz contributed a lot to both, introducing or improving important elements right at the start that I had either overlooked or incorrectly expressed. When developing Ihara's method, as I presented it in Antwerp, a comparison of the trace formula with an enumeration of the points modulo p is fundamental. In my work on this comparison, I overlooked that the combinatorial objects involved in this enumeration are essentially orbital integrals. They should therefore be treated in the same way, namely with the help of endoscopy. The description of the action of the Galois group on the points of a Shimura variety modulo p that I explained during the conference in DeKalb, was also not entirely correct and had to be improved by Kottwitz. He has demonstrated the improved form. The present day development of the comparison of automorphic L-functions with the L-function attached to a Shimura variety is strongly influenced by him.

In the Corvallis article there were other conjectures, which were mainly taken up by James Milne and his coworkers, in particular the conjugation of Shimura varieties and the conjectured relationship of the Taniyama group, introduced there, to motives of potential CM-type, which he along with Deligne proved. Borovoi's final general construction of all Shimura varieties was also influenced by this report.[6]

With the help of Shimura varieties mathematicians have certainly answered one, for me, main question: will it be possible to express all motivic L-functions as products of automorphic L-functions? The answer is now beyond any doubt, "Yes!". Although no general proof is available, this response is fully justified from the examples and evidence available. To what extent it is necessary or useful to pursue the theory of the Shimura varieties in order to prove the corresponding general theorem, however, is not clear to me, and I note with some disappointment that many younger mathematicians are eagerly studying the theory of the Shimura varieties without having seriously asked this question. I will come back to it.

First, almost incidentally, I make a small remark that I should have have made earlier because my assertion concerning the function (3) was not quite right. We have a number of

[6]Langlands's conjecture on the conjugation of Shimura varieties was proved by Milne and Shih for Shimura varieties of abelian type, and by Milne and Borovoi in general. The conjectured relation between Langlands's Taniyama group and motives of potential CM-type was proved by Deligne. The existence of canonical models for general Shimura varieties follows from the conjugation conjecture.

arrows:

$$\text{representation } \pi \longrightarrow \text{cohomology} \longrightarrow \ell\text{-adic representation} \longrightarrow \tilde{\pi}.$$

The last arrow is that of the (Langlands) correspondence. We do not need to say here what this correspondence is. Certain properties are desired. But it is quite possible that π and $\tilde{\pi}$ are not isomorphic representations, although they will be closely related. I should have said this in connection with the formula (3). But it is only a very slight modification. There is in some circles a strong stubborn tendency to define away this amendment, which shows a gross misunderstanding.

I have already said in various places that, in my opinion, it is necessary to prove functoriality first and then afterwards, with the help of the knowledge and tools obtained, to develop a theory of correspondences and, at the same time, of motives over $\mathbb{Q}$ and other global fields, as well as to justify it over $\mathbb{C}$. It is certainly the case that much research in recent years, in particular the study of p-adic theories, has been indispensible for the construction of a theory of motives and of their relationship to automorphic forms. The sources of these p-adic theories very often lie in the theory of Shimura varieties or in related cohomological questions. To what extent the further development of the general theory of Shimura varieties itself is necessary or useful for this purpose is not clear to me. This theory is technically very demanding. Now that it has been developed, it attracts many experts.

I hope that before I have to give up mathematics for good, I will have the time and strength to contribute not so much to the development of a p-adic theory — with or without Shimura varieties — but rather an overview of its purpose and possibilities. I emphasize here, though this will be hardly necessary for most readers, that the construction of a theory of motives may be no easy matter. It requires, for example, a proof of the Hodge conjecture. I also emphasize that so far no one has resolved to undertake this construction simultaneously with the development of the correspondence.

We are not yet done with the endoscopy. For years, the main obstacle was the fundamental lemma with which I and some students, Rogawski, Hales and Kottwitz, occupied ourselves over the years, I for a relatively short time, and Hales and Kottwitz for years. Thanks to their contributions and those of Waldspurger, Laumon, and ultimately Ngô, the fundamental lemma is now proven, so that we can now turn to functoriality and its proof. We could have done that beforehand. For SL(2) or GL(2) the fundamental lemma was either easily provable or not necessary at all. It is true that Ngô's approach and the introduction of the Hitchin basis offer us, even for these two groups, a significantly new point of view. Within the framework of the groups this basis is different from the framework of Lie algebras, and it is called the Steinberg-Hitchin basis. It seems that this basis will allow us to treat analytically the result of the trace formula — only the stable (or more precisely stabilized) — with the help of Poisson's formula, which was not possible before.

Under the collective name "beyond endoscopy", methods are brought together, for the purpose of proving functoriality, especially for homomorphisms from a finite Galois group $\mathrm{Gal}(K/F)$ to the L-group ${}^LG$ of a group G defined over a finite algebraic number field F. These methods are similar to those found in Hasse's report published before World War II. This means in particular that they are analytic, and that the study of L-series near $s = 1$ is fundamental. To use them, we need analytical methods that are sufficiently strong, and that we did not have before. Whether these new methods are strong enough, is still an open question, which is currently being investigated, in, for example, two works published in the Ann. Sci. Math. du Québec. The first was written with Edward Frenkel and Ngô Bao Chau.

But the attempt must go on. Serious problems have not yet been resolved, including for the group SL(2).

In order to reach the edge, even more is required, at least a count, as with Kummer extensions in the abelian theory, of extensions with a given Galois group and given ramification. That will be the crux of the matter, and also probably, in some ways, a turning away from the cohomology. I hope to confine myself in the next few years mainly to this point, and only for extensions whose Galois group can be embedded in GL(2).

Although at the moment it is advantageous in some respects to deal only with the groups SL(2) or GL(2), it would also be very useful and very encouraging to treat in general the local problems that have already been solved for SL(2) or GL(2). This seems to me to be possible over $\mathbb{R}$ or $\mathbb{C}$ using the methods Shelstad used for endoscopy. Over other local fields it may be that the solution of the problem is much more difficult and requires the methods of algebraic geometry, which Ngô used. This is also a question that I would like to consider myself. I cannot hope to solve it. I lack the knowledge and the time.

Among the four themes that I mentioned, the geometric correspondence has not occurred. Thanks to Drinfeld and others, the correspondence has had three forms for several decades: over algebraic number fields, over function fields of dimension 1 over a finite field, and over Riemann surfaces. I ignored the third one here, although it is certainly this form that makes the correspondence so famous. I would like to understand this third theory better, not in order to work in the field myself, although I think there is much to be done there, but because I am curious to understand its relationship to physics and, more importantly, because it is closely related to the theory of ordinary differential equations with essential singular places. I have already mentioned that, as a young mathematician, I first became acquainted with this theory in the book of Coddington and Levinson, and that I have studied it occasionally since then. In a mathematical sense, little has been done with the third form of the correspondence so far, but I have found attractive the relation with algebraic geometry and perverse sheaves described in the collection *Singularités Irreguliéres: Correspondance et Documents* of letters and other writings of Deligne, Malgrange and Ramis.

The third theme, universality, has almost no relation to the others, and corresponds to the fact that basically I would rather have become an analyst, and that I just happened to spend so much of my time on number theory. In the last fifty years scientists from various fields of physics and applied mathematics, from quantum theory, from statistical mechanics, from hydrodynamics, have offered mathematicians a problem whose central position in mathematics is beyond question. In my opinion, this problem could, without exaggeration, be compared to the great problems that arose in the 17th, 18th, and 19th centuries and were solved with the theory of differential and integral calculus, with the theory of ordinary and partial differential equations, and with the theory of Fourier series and Fourier integrals. Obviously, one cannot hope to contribute seriously to solving this problem without a deep understanding of the relevant scientific fields and a broad knowledge of the mathematics involved. This I do not possess. I will still try to understand the essence of the problem. Although I have not got very far, I still cherish the hope of being able to describe this creature to other mathematicians. That is also a goal for the coming years. It is clear to me that my failure in this attempt is highly likely.

The general mathematics problem can be described briefly. One seeks to prove for infinite-dimensional dynamic systems, which are either given or constructed artificially, that there are fixed points at which the functional matrix has only finitely many eigenvalues with absolute value greater than 1, and for which the absolute values of the infinitely many other eigenvalues converges rapidly to 0. Even with simple artificially defined systems, it is not easy to prove such a thing. A fine example has been treated by Oscar Lanford using a mixture of numerical and theoretical methods. This research is presented in his work *A computer assisted proof of the Feigenbaum conjectures* (BAMS, vol. 6, 1982). I myself was made aware of the general problem through informal conversations with the physicist Giovanni Gallavotti. While reading the relevant literature, I came across percolation. It is a rather artificial notion that requires much less knowledge of this or that scientific field than the very deep questions of quantum physics, thermodynamics, or turbulence.

The behavior of the eigenvalues of the functional matrix is an expression of the universality of the corresponding fixed point. Universality means that in the infinitely dimensional tangent space only a finite number of directions are unstable. The fixed point is often independent of the details of the physical system in question. If there is no such independence, there is no university either.

The first problem is that initially one does not know, even for percolation, with which coordinates the fixed point is to be described. In the context of the problem, at first view, there are usually no obvious coordinates. Without such coordinates you have no handle on the problem.

For two-dimensional percolation, Harry Kesten had proven that the crossing probabilities [Überquerungswahrscheinlichkeiten] exist. When I read his book *Percolation Theory for Mathematicians*, it seemed to me that these probabilities could be independent of the shape of the percolation, that is, of the lattice or of the various probabilities. It is only important that the model is critical. They could thus be used as coordinates to investigate universality. I expressed this opinion to Yvan SaintAubin. He was skeptical, so we decided to experiment with the question. I later discussed the results over lunch at the Institute with Thomas Spencer and Michael Aizenman. Aizenman asked whether these universal crossing probabilities were conformally invariant. Saint-Aubin and I subsequently examined this question numerically. Spencer immediately talked to John Cardy to learn his opinion. Cardy initially suggested that they were not conformally invariant, but after a moment's thought, he found the conjecture reasonable. He was able to look at the problem on a higher scientific level than we could. He assumed conformal invariance, and with its help he very quickly offered his now well-known formula. This formula agreed with the numerical results that we already had in hand and that we had calculated with a different goal. We wanted to answer questions about universality. Our later numerical results, which I still find beautiful today, have further confirmed Aizenman's question or conjecture in other respects. We published these results in a lengthy paper in the BAMS, which was read by some probability theorists. In particular, I discussed it with Oded Schramm and Stanislas Smirnov, who had read the paper. Could it be that we are the legendary physicists whose experiments some ignorant mathematicians point to when they describe the origin of the question of conformal invariance in percolation? Conformal invariance, as a general problem in quantum field theory, is something else, on a different level, and was introduced much earlier.

The works of Schramm and Smirnov mainly relate to conformal invariance. Their findings are important. I find Schramm's SLE theory particularly beautiful. I am nonetheless convinced that the depth of the problem lies in universality. If one can master universality, even just for percolation, then, in my opinion, the conformity invariance will probably be a relatively easy consequence. However, this is no reason not to consider conformal invariance, because the results are beautiful, and conformal invariance is better known to mathematicians than universality. Nevertheless, I find it disappointing that so many mathematicians of today are unable to recognize or even understand where the true problems lie, those that determine the history of mathematics.

9 Die Mathematik als Zugang zur geistigen Welt. (Mathematics as access to the intellectual world)

When I started this essay, I was tempted to add a few words on this subject, because my life as a mathematician has given me many opportunities to get to know this colorful world and its past up close that I would otherwise never have had. But I finally denied myself. It would have been a lament about the current situation that nobody wants to hear. I very much admire the contributions and achievements of many mathematicians today. Nevertheless, I find that the mathematical background, as an intellectual background, no longer offers what it offered in the past, in my youth.

Finally, I would like to thank Volker Heiermann, Helmut Koch, and Joachim Schwermer for carefully reading the first version of this article. Also I would like to thank Wilhelm Zink for his reading of the second version. Here and there traces of my mother tongue remain in the wording. I hope, however, that almost all real errors have now been removed.