PROBLEMS IN THE THEORY OF AUTOMORPHIC FORMS: 45 YEARS LATER

ROBERT P. LANGLANDS

This video is being prepared at the suggestion of Ivan Fesenko as an informal non-technical discussion of various mathematical topics that occupy me now and that have occupied me over my many years as a mathematician. As an introduction I offer a brief autobiography.

I began my mathematical, or better my university, studies more than sixty years ago just before my seventeenth birthday, after a childhood and adolescence that were in no sense a preparation for an academic career, or for a career in any sense. Various influences came together that suggested that I attend university not so much to prepare myself for a profession but to become what one might think of as a savant. A major influence was perhaps Ernst Trattner's *The story of the world's great thinkers,* a book that was very popular in the late thirties, so that there are still many copies available on the used-book market at very modest prices. What I envisioned, after enrolling in the University of British Columbia in 1953, when its campus, which 100 years earlier had been virgin forest, was still relatively unspoiled with almost no cars and almost no concrete buildings, was myself, in a jacket with leather elbow-patches and perhaps a pipe in my mouth, gazing over the lawns and the trees while reflecting on a still undetermined, but certainly abstruse, topic. That topic became mathematics not out of a strong preference for that subject, but because it pretty much required no preparation, only native ability.

It did not disappoint me. My undergraduate years were pleasant and quiet. Afterwards, in part because of the circumstances of the period, in part because of my own expectations and my own personality, there was no disappointment, although there were difficulties and there was discouragement. There was a year as a graduate student at UBC, then I went on to Yale, a fortunate choice: the quality was high, although somewhat specialized; I had no obligations as any kind of teaching assistant, so that my time was my own; and, in contrast with, say, Princeton or Harvard, there were no fellow students with superior preparation eager to intimidate me. The few courses or seminars I had were helpful and instructive, so that I had a great deal of time on my own to spend in the library. My thesis, on semi-groups and partial differential equations, written on my own, grew out of the various courses on functional analysis, semi-groups, and partial differential equations. E. Hille's book on semi-groups was, in particular, a useful and attractive introduction to a number of topics in classical analysis. A second independent paper on Eisenstein series grew out of a seminar by I. Gaal on Selberg's work and a seminar on functions of several complex variables under the direction of F. Browder and S. Kakutani that never took place. Luckily I read some of the pertinent literature nevertheless. Neither the thesis nor the paper were ever published in the strict sense, but the thesis was incorporated in a book by Derek Robinson, *Elliptic operators and Lie groups,* the paper in my own subsequent writings.

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None the less, more by good luck than by good management, both were important in my career. The first came to the attention of Edward Nelson, who, then an assistant professor at Princeton, found enough merit in the first that I was offered, sight unseen and with no documentation, a position as instructor at Princeton. I had wanted to stay at Yale, and a number of the faculty would have been content to offer me a similar position, but fortunately the resident probabilist had taken a dislike to me, and he blocked the appointment. So I went to Princeton, where, asked to speak in a small analysis seminar, I spoke on the Eisenstein series paper. Bochner was favourably impressed, largely by the circumstance that I had already been thinking about a topic with no relation to my thesis, but also by the topic, Dirichlet series. Bochner, the first part of whose career had been spent in Germany, had had close relations with members of the Hasse-Noether school, and thus was more familiar with class field theory than his own work might suggest. Not only was he, so far as I know, the reason for my rapid promotion at Princeton, which Artin had left only a couple of years earlier, but he had also encouraged, one could even say forced, me to give a graduate-course on class field theory. I knew nothing about it and was scared stiff, but could not refuse. I learned a great deal. So did a few students. It is impossible to overestimate the debt I owe to Bochner.

After a few years at the University, with an intermission of a year at the Institute and another at Berkeley, I had—in the shape of the department chairman, also a probabilist—another stroke of luck. Fancying himself as a Hercules whose task was to cleanse the department of the deadwood accumulated under the influence, in particular, of Bochner, he took it upon himself to drive a number of us out. The story is complicated, but the upshot was that I returned to Yale, where I was very happy, but I could not resist the offer of an appointment to the Institute, an offer that at the time would not, because of a gentleman's agreement between the two institutions, have been possible if I had remained at the University. This offer I owe, I am certain, principally to Harish-Chandra. It was a blessing that gave me a life that nothing else could have brought. We shall meet probabilists again. They seem to have often been a thorn in my side, but there are also many whom I admire and to whom I have reason to be grateful. I recall, in particular, the well-known probabilist William Feller, who was on the faculty at Princeton during my stay and for whose presence I was always grateful.

So these early years were free of anxiety, but not of discouragement. Mathematics, if one is at all ambitious, is difficult. I was free to give it up, free to ignore any constraining demands, from deans or chairmen, free for example not to apply for grants, to write or publish only what I cared to write or publish and only when I felt it appropriate, willing to continue in modest circumstances in out-of-the-way places, but not willing to abandon the goals I had set for myself, or that, say, Bochner had encouraged me to set.

Recall the period in which I began and the circumstances. In the early post-war period, thus even in the mid-fifties, the memory of European mathematics was still vivid. A number of my teachers in Canada were newly arrived from Europe or had pursued their own post-graduate studies in Göttingen and elsewhere. European learning, European languages had not yet lost their importance in the curriculum. I learned them or, rather, acquired some rudiments, either on my own or in a university class, principally with the intention of using them professionally. The possibility of using them abroad, even the possibility of visiting England, whose history played, in principle, a large role in our secondary education, and which was a common destination for Canadian university students of my time, was not something I entertained. I had enough to occupy me at home.

It was only when completely discouraged by my attempts to find the elusive non-abelian class field theory or the elusive automorphic L-functions that I began to think that the time had come to abandon mathematical research. Largely as a consequence of a chance acquaintance with a Turkish visitor to Princeton, the economist Orhan Türkay, unfortunately recently deceased, that I decided, as a first, mildly adventurous, step to spend a year or more in Turkey with my family. He suggested in particular that I teach at the Orta Doğu Teknik Üniversitesi, a recently created, presumably with American encouragement and support, English language university. My visit was supported, as I recall, by the Ford Foundation. The choice of this university was in part a mistake, in part not. I had recovered from my discouragement and had a great many mathematical ideas to deal with, so that it was just as well that my family and I were not dealing with too difficult an adaptation. There were difficulties, but they were overcome. My ambition was to learn Turkish, but as I had had no previous experience abroad and no real training in any language, this was easier said than done. With the help of an amiable and generous Turkish-speaking immigrant from Bosnia whose relatives owned the neighbourhood grocery store and of Orhan Türkay himself, I made some progress and, with sufficient preparation—especially in regard to the mathematical terminology—by the time I left I was able to deliver, at least to a sympathetic and tolerant audience in Izmir, a mathematics lecture in Turkish. English had in the late sixties not yet reached the provinces. Since then I have improved, but not really enough. I never spent any further long periods there. Nevertheless, in recent decades some short visits and considerable application at home have allowed me to profit from the country's twentieth-century literature, fictional and historical.

A major benefit of the stay in Turkey was to have discovered not only a taste for the foreign, a desire to acquaint myself with other places and their past, but also how, as an anglophone, to go about learning other languages in a world, more precisely a professional world that was already converting itself to a single linguistic currency, Anglo-American. Although my greatest efforts have gone to mathematics, acquiring some familiarity with the heterogeneous linguistic and intellectual heritage, largely of Europe but also of some other regions, has been one of my principal pleasures during and since my time in Ankara, and an incidental, but in retrospect decisive, advantage of my profession as a mathematician, offered in large part by friendship or acquaintance within it, and allowing me to become in the end a "savant malgré moi"—at least in my own eyes. It troubles me greatly to see this advantage disappearing. It is an impoverishment of the profession, which offers today, I believe, much less in the way of intellectual richness than it did in my time, both within mathematics itself and without.

When inviting me to prepare this video, Ivan Fesenko did not, I am certain, intend so much in the way of personal reminiscences. Nevertheless, I wanted to express clearly my sentiment that thanks to circumstances of time and place I was never constrained by my profession as a mathematician, never forced to any kind of submission. I could always, without great reserves of moral courage, do as I wanted. I would like to begin the mathematical remarks by describing an attempt in which, to my disappointment, I had little success. This attempt is described in the reference:

`https://publications.ias.edu/rpl/section/27`

I learned, as I describe there, about the problems posed by renormalization from Giovanni Gallavotti. The essence of the matter is that for many and various problems from mathematical physics—fluid flow, statistical mechanics, and quantum field theory—a central feature seems to be the existence of fixed points of dynamical systems in an infinite number of dimensions with a very special property: in all but finitely many directions the fixed point is stable. The mathematical problem is two-fold—to construct such systems in a rigorous fashion and to establish their physical relevance. Following my conversation with Gallavotti, I began to learn about these systems. The difficulty, or so it seemed to me, is that although physicists have some rough, and very penetrating, understanding of how and where these systems arise, they have no solid tools for treating them, nothing like the tools available for various kinds of partial differential equations. I concluded that the task of a mathematician was to provide these tools.

After some reflection and after a reading of Harry Kesten's *Percolation theory for mathematicians,* I began to think that percolation was the place to begin. An initial problem with an infinite-dimensional system and its fixed points is that the parameters in which the problem or the system presents itself, may not be ones which permit an identification of the fixed point. For percolation, the initial problem is on a lattice and discrete, with probabilities of occupation prescribed. Renormalization entails a passage to finer and finer lattices, where the initial parameters, the probabilities, disappear and new, at first unknown, parameters appear. As I recount in the reference cited, my notion, after a study of Kesten's book, was that the transition probabilities were the appropriate parameters for describing the fixed points. I constructed, together with Marc-André Lafortune, some simple geometric models to test this notion and then, with Yvan Saint-Aubin, began to calculate the crossing probabilities for various models of percolation to see whether, in fact, crossing probabilities were universal, in other words, whether they could possibly appear as the coordinates of a *stable* fixed point. This was by no means obvious and not implied by Kesten's conclusions, important as they are, for the *existence* of crossing probabilities does not imply their *universality*, which so far as I know is not yet established. Our numerical experiments confirm, however, that they are indeed universal. When I explained these conclusions to Michael Aizenman, he suggested that they might also be conformally invariant. Saint-Aubin and I tested this as well, and the results, which I found very pretty, confirmed this too.

Oddly enough, the geometric model and the universality, namely those numerical results related to the general and fundamental problem of understanding the mathematics of renormalization, were ignored, but conformal invariance was not. It drew the attention of a number of probabilists or at least of mathematicians, some very strong, whose interests included probability, Oded Schramm on the one hand, a group of mathematicians in Stockholm on the other. As I recall in the reference, two Fields Medals have been awarded for work related to conformal invariance in percolation, but in the laudatios, one by Kesten himself, both by probabilists, there is no reference to the problem of universality in percolation, which is I believe a more difficult problem, although still specific, nor to the major problem of creating a mathematical theory of renormalization. Since Fields medals are taken quite seriously by a large number of mathematicians and by whatever part of the general public takes an interest in mathematics, the lack of perspective is regrettable.

I would very much have liked to continue reflecting on renormalization, but to attack it seriously requires not only enormous mathematical strength but also broad, concrete experience in the various domains mentioned, fluid mechanics, statistical mechanics, quantum field theory. The first only God can give; the second requires a lifetime to acquire.

The theory of automorphic forms, especially the analytic theory, is more circumscribed, but difficult and broad. In a message to Peter Sarnak reproduced in

`https://publications.ias.edu/rpl/section/25`.

I described briefly the problems that seem to me central. Their solution, unlikely to be the work of one or even of a few mathematicians, will not be easy. There have been at least three Fields medals awarded for work on some aspect of them, but curiously enough all three were given to specialists in algebraic geometry, although the analytic and number-theoretical problems otherwise met, and sometimes overcome, are at least as deep and at least as important as the algebraic ones. These specialists are all excellent mathematicians, but one wonders again whether the influence of these medals, and other prizes with wide recognition, on the course of mathematics and on our understanding of it is entirely beneficial.

In the message, I first draw attention to two problems that are more focussed than the others: endoscopy and the representation theory of reductive groups over nonarchimedean local fields. There are local and global forms of endoscopy. The local theory is a part of the representation theory over a local field, but it also has a special flavour and charm of its own for which, over an archimedean field, one can consult the papers of Diana Shelstad or, over a nonarchimedean field, those of, for example, Mark Reeder and Tasho Kaletha. A genuine and complete theory of local representations over nonarchimedean fields has not been easy to come by and is still waiting for its author. The model is presumably Harish-Chandra's theory over archimedean fields. One can expect that nonarchimedean fields of positive characteristic will be easier than p-adic fields and that etale cohomology will be an essential tool. I would expect that to develop the theory over p-adic fields, it would be necessary to borrow ideas from Waldspurger's reduction of the fundamental lemma over these fields to the fundamental lemma for local fields of positive characteristic.

My concern in the message was not with them. It was with the following five questions:

(i) the trace formula and functoriality;
(ii) the geometric theory;
(iii) mirror symmetry;
(iv) reciprocity;
(v) advanced theory of automorphic L-functions.

In the first question the word functoriality is used in a fashion particular to the theory of automorphic forms. It is a notion not yet developed, but can be regarded as one form of the reciprocity of class field theory in say, as I prefer, its pre-war form. Like the older theory, it has a local and a global form. It is the global form to which the first question refers. A few forms can be established by other methods, but I am persuaded that the final form will be proved by the methods intimated in the paper *A prologue to "Functoriality and Reciprocity"*. The intimations of that paper will only be developed slowly and with great effort. First the trace formula itself, that of Arthur-Selberg, will have to be combined with methods from analytic number theory, in particular, the approximate functional equation. The best reference for this may be the thesis of Ali Altuğ and various papers that he is preparing. They will have to be followed not only by a good deal of further analysis but also by an intensive algebraic or diophantine study of, on the one hand, certain limits it is expected to offer and, on the other hand, of formulas for the number of Galois extensions of a given type of a given number field. The two will need, in my view, to be compared. As a place to start with this, I usually recommend reading Hasse's *Klassenkörperbericht,* rather than any modern treatment of class field theory. Some suggestions for starting on a non-abelian theory are made in the papers at the internet address above, but anyone wishing to attack these problems will be on his own, and will need to find the necessary courage in himself or herself.

The geometric theory as treated in my Mostow lecture is modelled on the arithmetic theory. Although I expect it to be much easier than the arithmetic theory, it itself will, I believe, demand a good understanding of a number of topics: spectral analysis; classical differential geometry—the Gauss-Bonnet theorem and the index theorem, connections; maybe Hamilton-Jacobi theory; classical algebraic geometry and classifying spaces; as well as some modern sheaf theory—direct images and stacks. There are parts of the lecture as it stands where I was too naive. The notion of an "equidimensional whole" was ill-conceived. Nevertheless we are not, I believe, dealing with difficulties as formidable as those of (i). A complete theory will presumably include both the sheaf-theoretic and the spectral-theoretic structures, but in an integrated form with the passage from eigen-functions to connections and from connections to local systems clear.

I am still puzzled by the notion of mirror symmetry and, in general, am curious what "theoretical physicists" have in mind when they speak of the "Langlands program." I do not suppose that the program is of any interest at all to the bulk of the members of the usual physics department. In (i), (ii), (iv) and (v) we are dealing with genuine mathematics, with indisputably serious historical roots in analysis, algebraic geometry, analytic number theory, and arithmetic. In (iii), we are, so far as I know, dealing with questions with considerable interest for differential geometers and topologists, but perhaps for lack of any experimental pertinence, of little or no interest to the majority of physicists. Whether these questions are at the mathematical level, current or potential, of (i) and (iv), or even (v), is a question that I am happy to leave to others. I, personally, would simply like to have a clearer notion of (iii)—of the new elements it contains from gauge theory or elsewhere and of the nature of its mathematical or physical interest.

My initial impression is that the topic has been added to the "program" because the two groups, G and ${}^LG$, are of the same type, namely reductive groups over the field F, the field of meromorphic functions on a Riemann surface, and play dual roles. I recall that in the geometric theory the Hecke eigenfunctions appear to be classified by connections or their integrals. The purpose of the still provisional Mostow lecture is to clarify this, but there are many signs already in, for example, several expositions of Frenkel. These integrals, or perhaps the integral of the unitary parts of the connections, define conjugacy classes in the various Cartan subgroups of ${}^LG(F)$. Dually, Hecke eigenfunctions for ${}^LG$ are classified by conjugacy classes in $G(F)$. This would be the source of the duality appearing in (iii), but I am still struggling with the notions and constructions necessary to an adequate formulation of the ideas in the Mostow lecture. So my ideas continue to be uncertain. I add that, so far as I know, the word "dual" appears when describing the two groups G and ${}^LG$ and their relation, because their Cartan subgroups T and ${}^LT$ are dual, in the sense that the groups of characters, $\Delta = \mathrm{Hom}\big(T, \mathrm{GL}(1)\big)$, ${}^L\Delta = \mathrm{Hom}\big({}^LT, \mathrm{GL}(1)\big)$, are dual—${}^L\Delta = \mathrm{Hom}(\Delta, \mathbf{Z})$.

There is an additional element in (iii), whose understanding, unlike that of the geometrical theory itself, demands a good deal of what can, I believe, be regarded as extra-mathematical knowledge, the Maxwell equations or gauge theory. So, in putting (i), (ii) and (iii) under the same rubric we are perhaps mixing apples and oranges. It is all very hard to understand!

The topic (iv) is another matter. It is perhaps the most difficult of all, but it is an integral extension of (i). In the letter to Weil of 1967, I envisaged functoriality as reducing to a form of a non-abelian class field theory, thus a generalization of the classical theory, which is abelian, when in the pair H, G whose representations are associated by the functoriality we take $H = \{1\}$. I turned to reciprocity only later, when I became familiar with the Weil conjectures and the Hasse-Weil L-functions. The hypothesis that they were all given, in some way, by the automorphic L-functions of the Weil letter was suggested by the Eichler-Shimura theorem and the obvious first cases on which to test it were the generalization of the modular varieties studied by Shimura, which I called, for evident reasons, Shimura varieties. Although their theory and the relation between their L-functions and automorphic L-functions is not, so far as I know, fully investigated, the evidence they provided was overwhelming. They were not, however, intended to be an end in themselves.

What the evidence of all sorts suggests is a generalization of class field theory not just to varieties of dimension zero, thus a non-abelian class field theory, but a generalization to all varieties over any number field of finite degree over the rationals. The difficulties to be surmounted are, however, daunting. Whether one is confident or sceptical depends on one's temperament and on one's interests. I explain the central notions or hypotheses. When a number field F is given, one introduces, in principle, as in the *Prologue* referred to earlier, two groups or, better, two families of groups, one $\mathfrak{A}$ associated to automorphic forms over F by the functoriality to be defined in that theory, one $\mathfrak{M}$ associated to "motives," over F, presumably in the sense of Grothendieck, but there are difficulties to be overcome. As Grothendieck has explained, the construction of a theory of motives is intimately related to some outstanding, and very difficult, conjectures in algebraic geometry. Reciprocity, if it exists, will come from a homomorphism from $\mathfrak{A}$ to $\mathfrak{M}$, which will define a transfer from motives to automorphic forms. Its existence will provide a proof of the analytic continuation of the Hasse-Weil L-functions.

To establish the existence of such a theory will not be a task for just a few mathematicians nor—if we dare to think in terms of the future—for just one generation. The principal tool available at present seems to be the theory of ℓ-adic representations and their deformation. It is a powerful tool and there are a good number of extremely competent specialists. I find, however, that they are excessively focussed. We can, I am sure, not do without them; they, however, appear to prefer problems with circumscribed, more modest goals! I fear that those with a larger view of the subject will have to learn from them; on the other hand, they themselves, so far as I have observed, have no desire to learn anything *was ihnen nicht in den Kram paßt.*

The fifth topic is not something with which I, myself, have had much to do, but the message to Sarnak was suggested by a message from him to me, in which he included a paper by Templier, Shin and himself. I had always been aware, but not acutely aware, that in the background of a general theory of Euler products lay the Riemann hypothesis and questions about the distribution of zeros, central problems in analytic number theory. It had, however, never occurred to me that it might be profitable to look at them in this larger frame. The joint paper sent to me suggests, at least at first glance, that it would. So

I added, without much reflection, the fifth topic to the other four, with which I have been preoccupied for years.

Topics (i), (ii) and (iv) can be regarded as flowing largely, but not entirely, from the analytic theory of automorphic forms, created largely by Hecke and Siegel, and from class field theory, which was given, to some extent, its final shape by Artin. In view of this, it may be useful to add the comments he made at the Princeton University Bicentennial Conference on the Problems of Mathematics held in 1946. They begin on p. 5 of a pamphlet published on that occasion.

> Brauer reported his proof of Artin's conjecture about induced characters, which asserts that characters known to be rational combinations of certain special characters are in fact integral rational combinations... Brauer's result represents a decisive step in the generalization of class field theory to the non-Abelian case, which is commonly regarded as one of the most difficult and important problems in modern algebra.
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> Artin stated that "My own belief is that we know it already, though no one will believe me—that whatever can be said about non-Abelian class field theory follows from what we know now, since it depends on the behaviour of the broad field over the intermediate fields—and there are sufficiently many abelian cases." The critical thing is learning how to pass from a prime in an intermediate field to a prime in the large field. "Our difficulty is not in the proofs, but in learning what to prove."

It is evident that Artin's notion of a non-abelian theory is not ours. His notion is closely tied to contributions of the initial creators of the abelian theory; ours lies more in Artin's own contributions.

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