REFLEXIONS ON RECEIVING THE SHAW PRIZE

ROBERT P. LANGLANDS

To receive the Shaw Prize is of course a great honour, but it was also an occasion to discover, or to be reminded, that a number of mathematicians have a perception of the development of the theory of automorphic forms over the last four decades that differs from mine if not in a radical, certainly in an essential way. Some of the differences are a result of misapprehensions that are a natural consequence of the variety of the theory's relations to fields practiced by mathematicians with many different temperaments and training. With a little explanation these misapprehensions can be dissipated. The prize is an opportunity to do so. Others are the result of conflicting methodological stances, mostly unrecognized and certainly unresolved. Their resolution will certainly demand a deeper understanding of the subject than is yet available. In this lecture I attempt to describe the current, unresolved situation. My emphasis will be on my own stance, although my purpose here is not to advocate but to explain it.

My own views are best explained with reference to the accompanying diagram, in which there are five circles of different sizes, the sizes reflecting nothing more than the space the associated fields of mathematics occupy in my own mind. The upper left-hand corner is the analytic theory of automorphic forms, a theory that came into prominence in the fifties and sixties, as the legacy of mathematicians like Erich Hecke, C. L. Siegel, Atle Selberg and, as it became more and more appropriate to employ the language of infinite-dimensional representations, Harish-Chandra. It is an analytic theory. In the mid-sixties, as a young mathematician there were several serious questions that I tried to broach, not all in this area and for most of them with little success. With two I was lucky, simultaneously and as a result of my own earlier work on the general theory of Eisenstein series, basically the study of the spectra of specific commuting families of differential operators on certain noncompact Riemannian manifolds. The spectra are highly structured and their qualitative properties difficult to establish. To my surprise, their study ultimately led to a conjectural response to two of the questions or problems: the definition of a natural family of analytically—at least potentially—tractable L-functions associated to automorphic forms and the possible structure of a nonabelian class field theory. The second came immediately after the first, more the result of inspiration than of effort.

I recall here that not long before, in the proceedings of a mathematical conference celebrating the second centenary of Princeton University in 1956, Artin had suggested that such a theory might not exist, or at least might not contain any new elements. So I may well have been the only one who was searching for it in the 1960's.

The suggested answer took the form of a construction and a conjecture. The basic object in the theory of automorphic forms is, today, an automorphic representation of the adelic points $G(\mathbf{A}_F)$ of a reductive algebraic group G over the algebraic number field F, all objects

Date: September, 2007.

Appeared in Clay Mathematics Proceedings, volume 13: *On certain L-funtions,* Conference in honor of Freydoon Shahidi, July 23–27, 2007, Purdue University, pp. 297–308.

that need not be defined here [1]. For many expository purposes, the representation can be replaced by an element of the function space on which it acts. If, in addition, the group is taken to be GL(2), and, for simplicity, the adeles replaced by the real numbers, this element is often just a classical elliptic modular form. This simplification entails, however, a real possibility of misunderstanding the import of the construction and the conjecture.

The first step in the construction is to attach to G a complex algebraic group ${}^LG$, or better ${}^LG_K$, usually referred to as the L-group. K is a sufficiently large finite Galois extension of the ground field F, itself a finite-dimensional extension of $\mathbf{Q}$ at the time of the group's initial introduction. The L-group has a connected component $\hat{G}$ of the same dimension as G and its group of connected components comes with an isomorphism with $\mathrm{Gal}(K/F)$. So there is an exact sequence

$$1 \to \hat{G} \to {}^LG_K \to \mathrm{Gal}(K/F) \to 1.$$

The second step is to attach to each automorphic representation π and to each finite-dimensional algebraic representation r of ${}^LG$ an L-function defined by an Euler product, at first partial,

$$L_S(s,\pi,r) = \prod_{v \notin S} L(s,\pi_v,r). \tag{1}$$

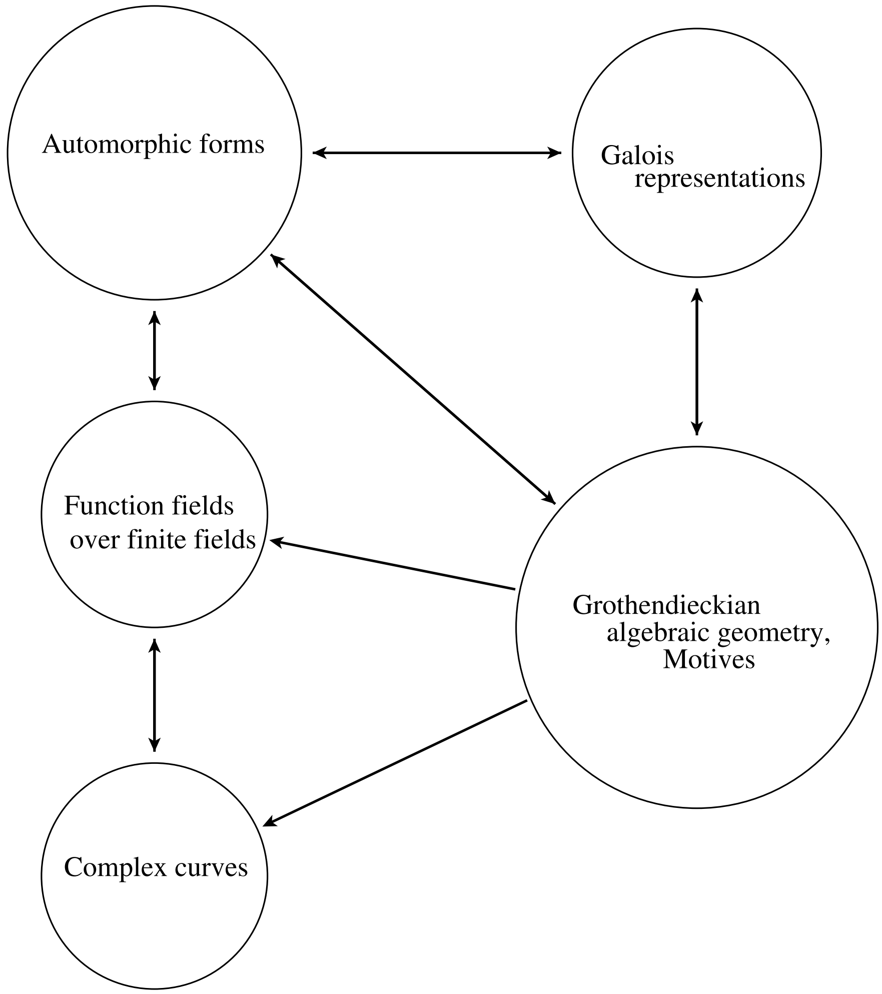
The set S is a finite set of places of F containing all infinite places, and $L(s,\pi,r)$ has the form

$$\frac{1}{\det\left(I - \frac{r(A(\pi_v))}{\mathrm{Nm}\,\mathfrak{p}_v^s}\right)},$$

where $\{A_{\pi_v} = A_v(\pi)\}$ is a conjugacy class in ${}^LG_K$ attached to π or its local representative π_v. These products converge in a half-plane. Of course, the L-functions introduced by Hecke, and more generally by H. Maaß, for GL(2) were the source of the impulse to search for such general L-functions.

The definition of the L-functions (1) was inspired by the general theory of Eisenstein series, for it was there that a substantial number of them emerged and could be continued to the whole complex plane. The first problem that presents itself is the continuation of all of them, not just as meromorphic functions but as meromorphic functions with a very limited number of poles. If G is $\mathrm{GL}(n)$ and $r = r_0$ the standard representation of $\mathrm{GL}(n)$ it was pretty clear that this could be done, using ideas already proposed, as I recall, in their first form by T. Tamagawa [2]. The final theory was developed by Godement-Jacquet.

Artin's proof of the analytic continuation of abelian Artin L-functions came quickly to my mind and a conjecture simple to state presented itself immediately with great force. Suppose H and G are two groups over F and ϕ is a homomorphism $\phi : {}^LH_K \to {}^LG_K$ compatible with the projections onto the Galois group. Then for any automorphic representation π_H of $H(\mathbf{A}_F)$ there is an automorphic representation $\pi_G = \phi(\pi_H)$ of $G(\mathbf{A}_F)$ such that $\{A_v(\pi_G)\} = \{\phi(A_v(\pi_H))\}$ for almost all v. The informed reader will notice that for simplicity all problems related to L-packets have been passed over in silence.



It is immediately clear that this conjecture is already deep and pregnant with consequence even for $H = \{1\}$ and $G = \mathrm{GL}(n)$. For suppose ρ is a representation of the Galois group $\mathrm{Gal}(K/F)$ in $\mathrm{GL}(n, \mathbf{C})$. Then taking advantage of the freedom in the choice of K—an inevitable consequence of the initial freedom in the choice of G—we take ${}^LH = \mathrm{Gal}(K/F)$, ${}^LG = \mathrm{GL}(n) \times \mathrm{Gal}(K/F)$, $\phi(\sigma) = \sigma \times \rho(\sigma)$, π_H the unique one-dimensional representation of the trivial group $H(\mathbf{A}_F) = \{1\}$ and $\pi_G = \phi(\pi_H)$, and conclude that

$$L(s, \rho) = L(s, \pi_H, \rho) = L(s, \pi_G, r_0'), \tag{2}$$

r_0' being the product of the standard representation of $\mathrm{GL}(n, \mathbf{C})$ with the trivial representation of $\mathrm{Gal}(K/F)$. As a consequence of (2) and the Tamagawa-Godement-Jacquet theory for $\mathrm{GL}(n)$, $L(s, \rho)$ can be extended to the entire complex plane.

The general conjecture that $\phi(\pi_G)$ always exists I began after some time to call *functoriality*. I was amazed by it at the time and remain so today. It has, I believe, to be regarded as a striking historical fact that the solution—still itself in large part conjectural, but no longer entirely—to the Artin conjecture (for the first of the very few available cases, see [3]) appeared as part of a much larger conjecture with implications of a much broader compass. To deny this context and this historical origin by referring to the conjectured existence of the π_G attached to ρ as in (2) as the strong Artin conjecture seems to me wrong-headed. It lends an unmerited legitimacy to clearly limited methods. The denial can charitably be ascribed to ignorance and a fear of the analytic theory of automorphic forms.

For number theorists in the 1960's and subsequent decades, Galois cohomology and elliptic curves were much more intensively cultivated than algebraic number theory as such. Legions of practitioners were produced in these domains for whom, by and large, the analytic theory of automorphic forms, especially nonabelian harmonic analysis, was anathema. The use by A. Wiles of some simple cases of functoriality that could be proven by such means in the proof of the Shimura-Taniyama conjecture and therefore of Fermat's theorem was at first simply overlooked [4]. Even now that it has been generally noticed, there is among many number theorists a reluctance to accept the imbrication of number theory and other domains entailed by a systematic reference to functoriality and nonabelian harmonic analysis and a failure to recognize the possibilities that this offers.

Once the general conjecture was formulated, the first order of business was to examine its simpler consequences and to verify in so far as possible that they could be proved or were compatible with what was then known. There were also over the years some accretions to the original conjecture. I would now be inclined to add to the conjectured existence of $\phi(\pi_H)$ just described a second one and to label the two together *functoriality.* Functoriality as such applies to all automorphic representations, even to those that, like most of the representations associated to Maaß forms, probably have no strictly diophantine significance.[1]

There are some fine points concerning the second conjecture for which I would hesitate to lay my hand in the fire and that I pass over in silence here, but I describe it nonetheless because something like it has certainly to be proved in any theory that aspires to completeness. To describe it, I have to assume a notion adumbrated by Arthur [5] that would be a consequence of any complete theory of the trace formula, namely the notion of *Ramanujan type* for an automorphic representation π, essentially the type for which the Ramanujan conjecture would be true. Functoriality offers of course the possibility of proving the Ramanujan conjecture for these representations, which will be in the majority, and of disproving it for the rest. If π is of Ramanujan type, the critical strip for $L(s, \pi, r)$ will have the same significance as for Dirichlet L-functions, thus lie between $\Re s = 0$ and $\Re s = 1$. Moreover the order $m(\pi, r)$ of the pole of $L(s, \pi, r)$ at $s = 1$ will be greater than or equal to 0. Call π thick if $m(\pi, r)$ is always equal to the number of times the trivial representation of ${}^L G$ is contained in r. The second conjecture is that for any $\pi = \pi_G$ there always exists an H, a thick π_H and a $\phi : {}^L H \to {}^L G$ such that $\pi_G = \phi(\pi_H)$. For a thick π the distribution of the conjugacy classes $\{A_v(\pi)\}$ would, basically by definition, be given by the usual Weyl distribution on conjugacy classes of ${}^L H$.

So functoriality contains a very general form of the Sato-Tate conjecture. Here, in contrast to any work on the Artin conjecture, the Sato-Tate conjecture was formulated before functoriality. So there are historically sound reasons for singling it out. Its early formulation is, like that of

[1]Peter Sarnak observed to me that this view is too narrow and referred me, in particular, to the work of Cogdell, Piatetski-Shapiro and himself on the number of representations of integers by ternary quadratic forms.

the Taniyama-Shimura conjecture, no doubt a reflection of the strong early interest in elliptic curves and their zeta-functions.

The two conjectures of functoriality are in themselves related to Artin's conjecture, largely through their application to the trivial group $H = \{1\}$, but, as formulated here, their purely arithmetic content is otherwise still limited. Not only do they have a validity extending beyond those automorphic forms strictly related to diophantine problems but also there is not yet in them any reference to diophantine problems for varieties of dimension greater than zero, for example no reference to the Taniyama-Shimura conjecture.

A good deal of work has been done on functoriality by F. Shahidi, I. Piatetski-Shapiro, and others without any pretence that the methods would ever offer the ultimate insights, but which, in my view, was nevertheless of great importance because it persuaded many analytic number theorists of the relevance of functoriality to their problems [6]. This is, in some sense, quite separate from any interest that functoriality may have as a tool for more purely diophantine problems. The trace formula was developed—in higher dimensions created—by J. Arthur and used as a tool by him and many others in the treatment of specific cases of functoriality, largely those accessible to endoscopy, especially twisted endoscopy. The book [7] will be a valuable introduction to the results of many years of effort.

The techniques referred to in the field as *endoscopy* had, however, from the beginning an obvious and important limitation. They could provide cases of functoriality that have been widely used and in quite different contexts, base change or the Jacquet-Langlands correspondence in various guises, but functoriality in general was not within their range. At the same time, there was a severe technical difficulty that caused me, and others, to despair: the *fundamental lemma*. It was a simply stated general combinatorial lemma and I expected that as such I would be able to prove it with time. Matters turned out quite differently.

Endoscopy, a feature of nonabelian harmonic analysis on reductive groups over local or global fields, arose implicitly in a number of contexts, in its twisted form both implicitly in the early work of Saito-Shintani on what was later called base change, and somewhat more explicitly in suggestions of—I believe—Jacquet for functoriality from orthogonal groups or symplectic groups to $\mathrm{GL}(n)$. It arose for me in the context of the trace formula and Shimura varieties.

Over the years a number of my students were introduced to the fundamental lemma and its difficulties, R. Kottwitz, J. Rogawski and T. Hales. Some went on, as is well known, to quite different things, but Kottwitz continued to reflect not only on it, but also on Shimura varieties and the number-theoretical difficulties attached to them and on applications of the trace formula. It was he, in the beginning alone and then later together with M. Goresky and R. MacPherson, who first had some genuine insight into the topological nature of the lemma.

In the hands of J-L. Waldspurger, G. Laumon and most recently B. C. Ngô, the lemma and the associated problems took on quite different features. Notice that in the diagram under the large circle in the upper left-hand corner, there are two slightly smaller circles, the size reflecting, as I observed, my own predilections. These are theories that were inspired by the theory for automorphic forms over number fields: first of automorphic forms over the second examples of global fields, namely function fields over finite fields, and then in the very lowest circle over the complex numbers. By the time we arrive at this third circle the theory has quite a different flavour. Two names associated to the second circle are V. Drinfeld and L. Lafforgue [8]. There is a whole school, strongly influenced by Drinfeld and largely a Russo-American school, associated to the third circle.

The fundamental lemma is a local lemma, over p-adic fields. The recognition informing recent work is first that to prove it over a p-adic field, it is enough to prove it for the second type of local fields, fields of Laurent series over finite fields, and secondly that to prove it over such fields it is best to work not with local orbital integrals but with the corresponding global objects as they appear in the trace formula. The first step is far from easy but was taken by Waldspurger in an important paper; for the second we pass naturally from the first of the three circles on the left of the diagram to the second.

Before passing to the third, I have to indulge in a good deal of somewhat reckless speculation, but I am growing old and the need to correct false impressions is growing more urgent. I may no longer have enough time to pursue any insight slowly to the point of genuine understanding and conviction. So, in the face of what seem to me the serious misunderstandings that have emerged, I must take my chances and state my case without delay as clearly as I can. The reader is warned that prudence is expected of him. He will have to take a great deal of what follows with a grain of salt until he has reflected on it himself.

I have been troubled for years and often discouraged by my failure, indeed by the general failure, to broach functoriality in any decisive way. Not so long ago, I suggested a different approach to the question with which I began to amuse myself [9, 10] but it was all very tentative. At the same time, I resolved to learn more in general about the various researches referred to often in a blanket way by the catch phrase *Langlands program,* a phrase that can mean many things.

I also had occasion to listen to lectures of Ngô (supplemented by the report of J-F. Dat [11]) and to try to understand them. In particular, I had to attach for myself some meaning to the notion of stack and algebraic stack. It was a revelation. I discovered that I had been thinking for decades of orbital integrals in an incorrect way. I had separated the local from a global part. With the notion of stack, with the suppleness of the etale cohomology, the two parts are, over global fields of the second kind, thus over function fields, to be fused and regarded as yielding the number of points on a stack, a number that can be calculated cohomologically. The problems encountered in [9, 10] suddenly appeared in an entirely new light. I try to explain this, although I am still dealing with concepts that I may have misunderstood.

In [9] a tentative method for approaching functoriality by taking limits in various trace formulas over an appropriate sequence of functions was introduced. The global field was taken to be $\mathbf{Q}$, the group $\mathrm{GL}(2)$. The following difference, written in a notation that is not quite the same as that of [9], was encountered

$$\frac{\sum_{p<X} \ln(p)\theta_m(p)}{X^{m/2+1}} - c_m X^{m/2}. \tag{3}$$

I define neither the constant c_m nor the expression $\theta_m(p)$. The question of whether the limit of this difference exists as $X \to \infty$ was discussed, but inconclusively. In [10], I passed to the rational function field over a finite field with q elements. Then the sum (3) is replaced by

$$\frac{\sum_{\deg \mathfrak{p}=n} n\theta_m(\mathfrak{p})}{q^{nm/2+n}} - c'_m q^{nm/2}. \tag{4}$$

The limit is to be taken for a fixed m but with $n \to \infty$ and the constant has changed. There would be something similar for the function fields of curves of positive genus. The divisor $\mathfrak{p}$ is here prime.

We write (4) as

$$\frac{\sum_{\deg \mathfrak{p}=n} n\theta_m(\mathfrak{p}) - c'_m q^{mn+n}}{q^{nm/2+n}}. \tag{5}$$

We need to show that this expression has a limit as n approaches infinity. The first term of the numerator is a fused orbital integral, and thus can—I suppose—be calculated cohomologically. Thus, the dimension of the associated stack being $mn+n$, it will be of the form

$$\sum_{k=0}^{2(mn+n)} (-1)^k \sum_{j=1}^{d_k} \gamma_{j,k} q^{k/2},$$

where $|\gamma_{j,k}| = 1$. The kth term is the contribution of the cohomology with compact support in degree k, thus of the cohomology in degree $2(mn+n)-k$. So what is necessary is to show that after the cohomology in degree 0 or at least very small degrees, which will just contribute the term $c'_m q^{mn+n}$, there is no cohomology in positive dimension less than (approximately) the intermediate dimension $mn+n$ and that the dimension of the cohomology in all degrees can be bounded. Then the cohomology in degrees around $mn+n$ can contribute to the limit, and the cohomology in higher degrees will contribute 0 because of the denominator.

All this looks far-fetched. It is suggested by a simple phenomenon, first described to me by N. Katz, that is discussed in [10]. In the naive reflexions of that paper, the stack is replaced by the moduli space of hyperelliptic curves of some large genus, thus by the space of monic polynomials of a given degree with distinct roots. This space has cohomology over $\mathbf{Q}$ only in degrees 0 and 1. It is an Eilenberg-MacLane space for a braid group, itself fairly closely related to congruence subgroups. For congruence subgroups, the phenomenon of concentration of cohomology in only a few dimensions, in particular those around the middle dimension, seems to have presented itself in other contexts [12], but all this is still very new to me.

I have, of course, passed rather glibly from function fields over finite fields to ordinary topology. This is the passage from the second circle on the left to the third. For vanishing theorems, this is perfectly natural because there are comparison theorems between etale cohomology and other cohomologies and between etale cohomology for a variety (for stacks!) and their reductions. Moreover, it is quite likely that as the theory over complex curves progresses, the stacks that appear for orbital integrals when we are examining the trace formula over function fields over finite fields will, as a variant of the stacks $\mathcal{H}ecke_\lambda$ of E. Frenkel's report on recent advances [8], also appear there.

If so, a gratifying unity will appear. Functoriality, as in the first circle, is to this point in this presentation largely analytic, the only link to algebraic number theory being the Artin L-functions. In both geometric forms of the theory, the reciprocity, both local and global, between Galois representations or representations of the fundamental group on the one hand and, on the other, automorphic forms for curves over finite fields, or $\mathcal{D}$-modules and perverse sheaves over the complex field, is the focus of attention. In these two cases, the functoriality is a consequence of the reciprocity. Over number fields, functoriality is, as I have stressed, also applicable to automorphic representations for which there is no reciprocal Galois representation and there is no real sign that it can be deduced in any generality except from the trace formula. The possibility that the topological study of the varieties (or stacks) appearing in the purely geometric theory will be pertinent to the trace formula is appealing.

There will be at least two major problems. The cohomology of braid groups is difficult and not well understood. That of the stacks $\mathcal{H}ecke_\lambda$ and their variants may be even more challenging. In addition, even if this strategy works, it is limited at first to global fields associated to curves over finite fields. On the other hand, a well-defined technique with a well-defined structure that was successful for the trace formula over function fields would certainly stimulate the search for related techniques over number fields. It is apparent from [10] that the difficulties, even for function fields, are related to the behaviour of class numbers, so that it is not impossible that questions like those raised by the heuristics of Cohen-Lenstra [13] will be relevant when we turn to number fields. I expect, however, that for number fields there will be very large, still unforeseeable difficulties that will make great demands on the inventive powers of analytic number theorists.

We could continue down the circles on the left and examine the relation of the third circle to various aspects of ordinary differential equations or to conformal field theory, but that is not, so far as I know, where the misunderstandings lie. They lie largely in a failure to appreciate the autonomous merit of functoriality, but also in a misapprehension of its relation to motives and to Galois representations.

I myself am inclined to regard the Galois representations as instruments, and the central relation between the left and the right sides of the diagram as the diagonal arrow between automorphic forms and motives, not the horizontal and vertical arrow passing through Galois representations. What the diagonal arrow provides, as in the proof of Fermat's last theorem, is passage from a context where a given, critical assertion is difficult, even impossible, to one in which it is almost transparent. It may, for example, not be possible to prove directly that there is no free-standing elliptic curve with various constraints on its ramification, but when the curve or an isogeneous curve is assumed to be contained in the jacobian of a modular variety the same conclusion can be immediate.

Grothendieck appears to have been grievously disappointed when his cherished notion of motives and the theorems needed to establish it turned out to be unnecessary for the proof of the last of the Weil conjectures. Perhaps he could have drawn a different conclusion. As explained in N. Katz's report [14], the last of the Weil conjectures was proven, by Deligne, in essence on the basis of a profound understanding of the etale cohomology theory accompanied by an observation arising in the theory of automorphic forms, namely that Ramanujan's conjecture, in its original or in its generalized forms, is an immediate consequence of functoriality and the resulting knowledge of the analytic properties of the family of all L-functions associated to the corresponding automorphic form or representation. In the context of the Weil conjectures, there are only the Galois representations, where functoriality is almost formal, and so no need for unproved assertions, just for a complete mastery of the etale cohomology theory. The conclusion to be drawn from this might have been that the theory of motives will have to be founded simultaneously with functoriality.

At the moment, I cannot make too much of this suggestion. There is, however, one point to which I shall return. Reflections on Shimura varieties led to the introduction of the Taniyama group [15]. This Taniyama group was then shown [16] to be the motivic Galois group of a restricted family of motives, not in the sense of Grothendieck but in a different sense, that defined by absolute Hodge cycles, thus the family of motives of potentially CM-type. It is likely that the two senses will be shown ultimately to coincide. Since the Taniyama group was shown at its introduction to be closely related to automorphic forms on tori, this is a genuine connection between automorphic forms and motives—or Galois representations—whose interest should not be overlooked.

I had already observed that the Taniyama-Shimura conjecture, like the Sato-Tate conjecture, preceded the introduction of functoriality for automorphic forms. I myself only became aware of it after my letter to Weil, when he drew my attention to his paper on the Hecke theory in which he mentions it. With this conjecture and the large number of L-functions introduced in connection with functoriality at hand, it was natural to suppose that they would account for all the L-functions attached to algebraic varieties—in the sense associated in a general way to the pair of names Hasse-Weil.

Given the Eichler-Shimura theory and the extensive researches of Shimura on what I later referred to as Shimura varieties, these were the clear context in which to test the supposition. As I already observed, there were difficulties associated with endoscopy and therefore with the fundamental lemma. There were also—what seemed to me at first until Kottwitz enlightened me—independent combinatorial obstacles. Finally there was a serious problem connected with the action of the Galois group on abelian varieties over finite fields that was finally clarified by Kottwitz and by Reimann-Zink [17]. At the time (1992), the general fundamental lemma still missing, Kottwitz was able to develop a reasonably complete theory only for a limited class of varieties [18], but these are in themselves of considerable importance.

We can now hope that, with the recent work of Laumon-Ngô, it can be established in general that the L-functions attached to Shimura varieties are automorphic. It is, however, not yet clear to me what pertinence this will ultimately have for the general reciprocity between motives and that large but special class of automorphic representations (sometimes called arithmetic) to which motives are thought to correspond. The final structure of the arguments can hardly be certain at this stage.

The proof of the Taniyama-Shimura conjecture, first for semi-stable curves by Wiles (with the help of R. Taylor) and then in general, introduced an entirely new element into the correspondence between automorphic representations π and Galois representations σ or, if one immediately passes to the diagonal arrow, motives M. Here there are many things with which I am completely unfamiliar and many more that I barely understand. So we are leaving the domains in which I have any claim whatsoever to authority. In particular, the theory of Galois representations as it developed in the hands of, say, B. Mazur and J-M. Fontaine is a subject that is not easily mastered and that I neglected in favour of other interests for too long a time. This makes it difficult to understand not only the work of Taylor but also the p-adic local reciprocity, which I am only beginning to learn.

Whether it is the horizontal arrow or the diagonal arrow from motives to automorphic forms that is being considered, there is also a necessity to establish an independent stance. My own first impressions were described in my review [19] of Hida's book [20]. There is a seeding and there are deformations, apparently of two kinds: the first are moves from a $\mathbf{Q}_l$ representation to a $\mathbf{Q}_{l'}$ representation, but for the same motive; the others simultaneous deformations of automorphic representation and Galois representations. The change from l to l' is some wondrous phenomenon at the heart of the etale theory that I have not yet been able to internalize. There is nothing I can add at present to the comments of M. Harris and R. Taylor on deformations of both kinds and on the p-adic local reciprocity that are contained in the text supplementary to my review.

My view of the seeding is different from that of, say, Taylor, perhaps largely because I am so attached to functoriality, which has a wider scope than the arithmetic automorphic representations alone. This attachment suggests to me that the best seeding is that given by motives of potentially CM-type, a class that includes all motives of dimension 0, thus all Artin representations. As I observed, for motives of CM-type the correspondence can be established thanks to the Taniyama group and its properties.

It is, on the other hand, almost an explicit demand of the approach described here for establishing functoriality for function fields that motives in whose cohomology arbitrary Galois representations with finite image can be isolated. Something similar will have to be available for number fields, and at the moment it is not clear to me where to look. So it is best to keep an open mind.

Recall what the correspondence is to associate to what. We are trying to establish the isomorphism of two Tannakian categories, perhaps with a fibre functor. That for automorphic forms will be defined by its group, which will necessarily be over $\mathbf{C}$. Apart from some obscurities and difficulties caused by centres that I prefer to disregard at present, this will be essentially the product over all thick π_H of the groups ${}^LH$. There is, of course, a restriction to elements with the same image in $\mathrm{Gal}(K/F)$ and an inverse limit over K. Notice that H is freely varying. Thus the analogue of a motive, better a motive with values in ${}^LG$, corresponds to a choice of a thick π_H and a homomorphism $\phi : {}^LH \to {}^LG$. Motives are of course defined quite differently and the associated group defined by a categorical construction that still presents severe problems. There is also a fibre functor to be introduced by an imbedding $\bar{\mathbf{Q}}_l \to \mathbf{C}$. As a consequence the correspondence will be $M \to \{\pi_H, \phi : {}^LH \to \mathrm{GL}(n)\}$. So the complete construction does not seem to be possible without functoriality. This is a point on which to reflect!

The simplest example of the seeding provided by the Taniyama group is of course that for the trivial group, thus for the trivial representation π_H, $H = \{1\}$. Supplemented by functoriality, this would mean that every motive of Artin type, thus essentially every linear representation of $\mathrm{Gal}(\bar{F}/F)$ would have its automorphic correspondent. It would mean as well that any base change was possible. It would also mean I suppose, when the relation between automorphic representations of Ramanujan type and the remaining ones was taken into account, that induction to include various motives whose Galois representation is not irreducible would be possible. This is the kind of information available to R. Taylor and his collaborators in their recent papers, except that the base change and, in general, the functoriality at hand are extremely limited, largely solvable base change, some form of the Jacquet-Langlands correspondence, and the functoriality provided by the converse method.

So it is startling to me, initially even somewhat disturbing, that Taylor is able to deduce from their results the Sato-Tate conjecture. This conjecture is, as observed, just one case of a statement expected to be valid for all automorphic representations (of Ramanujan type of course—but these are typical and all others are deduced from them). Nevertheless because it anticipated the general assertion and refers to one of the simplest and most studied classes of diophantine objects, elliptic curves, a proof of it, even if turns out to be of limited import for the conjecture in general, is of special interest. Taylor's proof lies, in part, outside the strategy described in this lecture for it does not work with automorphic forms alone and does not rely solely on functoriality, but combines some special cases of functoriality already at hand with deformation.

The strategy of this lecture, in spite of a large conjectural element, is coherent and has a solid record of proved predictions. A major departure from it is at least a methodological challenge. Moreover, that two different strategies will succeed in such a highly structured subject seems to me unlikely. Perhaps that described here is correct and hidden somewhere in the arguments of Taylor is a method that, say, surmounts the analytic difficulties for number fields, about which I have been able to suggest very little. Maybe a way will be found to handle with the deformations not only other automorphic representations of arithmetic type but even all automorphic representations; on the other hand the Sato-Tate conjecture, even in its general form for all automorphic representations, may turn out to be only a weak consequence of functoriality and not lead back to it. The relation of the conjecture, in its original or in its general form, to functoriality appears, on reflection, to be like that of the Tchebotarev theorem to the Artin conjecture. Although of importance in its own right, it is a weaker, more accessible assertion.

Until more insight into these questions is acquired, there will remain a serious intellectual, or methodological, gap between my stance and that of Richard Taylor. Although we have been yoked by the Shaw Prize, we are to some extent pulling in different directions. Perhaps that is not so bad. There is still a long way to go and the road uncertain.[2]

References

[1] A. Borel and W. Casselman, *Automorphic forms, representations, and L-functions,* Proc. of Symp. in Pure Math., American Mathematical Society, 33, 1977.
[2] T. Tamagawa, *On the ζ-function of a division algebra,* Ann. of Math. 77, 1963.
[3] R. P. Langlands, *Base change for* GL(2)*,* Ann. of Math. Study, 96, Princeton Univ. Press; J. Tunnell, *Artin's conjecture for representations of octahedral type,* BAMS 5, 173–175, 1981.
[4] Alf van der Poorten, *Notes on Fermat's Last Theorem,* Wiley-Interscience.
[5] J. Arthur, *Unipotent automorphic representations: conjectures,* Astérisque, 171–172, 1989.
[6] Peter Sarnak and Freydoon Shahidi, *Automorphic Forms and Applications,* Amer. Math. Soc., 2007.
[7] J. Arthur, *On the endoscopic classification of representations: orthogonal and symplectic groups,* Colloquium Publications, American Mathematical Society, 61 2013.
[8] E. Frenkel, *Recent advances in the Langlands program,* BAMS, 41 2004.
[9] R. P. Langlands, *Beyond endoscopy, Contributions to automorphic forms, geometry, and number theory,* Johns Hopkins University Press, Baltimore, MD 2004.
[10] R. P. Langlands, *Un nouveau point de repère dans la théorie des formes automorphes,* Can. Math. Bull., Can. Math. Soc., 50, 234–267, 2007.
[11] J-F. Dat, *Lemme fondamental et endoscopie, une approche géométrique,* Sém. Bourbaki, 2004–05.
[12] J-S. Li and J. Schwermer, *On the cuspidal cohomology of arithmetic groups,* American Journal of Mathematics, 131, no. 5, pp. 1431–1464, 2009.
[13] H. Cohen and H.W. Lenstra, *Heuristics on class groups,* Number theory (New York 1982), SLN, 1052 Springer, Berlin, Heuristics on class groups of number fields, Number theory (Noordwijkerhout 1984), SLN, 1068 Springer, Berlin, 1984.
[14] Nicholas M. Katz, *An overview of Deligne's proof of the Riemann hypothesis for varieties over finite fields,* Proc. of Symp. in Pure Mathematics, Developments arising from Hilbert problems, Amer. Math. Soc., 1976.
[15] R. P. Langlands, *Automorphic representations, Shimura varieties and motives, Automorphic forms, representations, and L-functions,* A. Borel and W. Casselman, Proc. of Symp. in Pure Math., American Mathematical Society, 33 1977.
[16] Pierre Deligne, James S. Milne, Arthur Ogus, Kuang-yen Shih, *Hodge cycles, motives and Shimura varieties,* SLN 900, Springer, Berlin, 1982.
[17] Robert Kottwitz, *Points on some Shimura varieties over finite fields,* Jour. of the AMS, 5 1992.

[2]Over the past few months, my reflexions on the fate of functoriality and the methods proposed here have profited from conversations and communications, both sometimes very brief, with several mathematicians: Nicholas Katz, Peter Sarnak, Mark Goresky, Dipendra Prasad, C. Rajan, Şahin Koçak and Joachim Schwermer. I am grateful to them all.

[18] Robert Kottwitz, *On the λ-adic representations associated to some simple Shimura varieties,* Inv. Math. 108, 1992.
[19] R. P. Langlands, *Review of* [20], Bull. Amer. Math. Soc., 44 291–308, 2007.
[20] Haruzo Hida, *p-adic automorphic forms on Shimura varieties,* Springer-Verlag, New York, 2004.

Compiled on February 11, 2020 6:01pm -05:00