Introductory remarks

The theory of automorphic forms is much like the proverbial elephant. How it looks depends very much on where one stands. At Princeton University, they are very familiar with the trunk, but many over there turn red and blue if there is any mention of its ears. At Harvard, they are quite pleased to talk of the ears, but just not sure whether they are used to fly through the air or to swim in the sea. Here at the Institute we may have an adequate notion of the trunk and the ears, but suppose, on the other hand, that the tail is of equal importance with the legs and the trunk for the elephant's locomotion and nourishment.

This is not satisfactory. If the subject is to achieve its promise, the younger mathematicians attracted to it not only have to understand in some depth what that promise is but also have to acquire the necessary techniques, techniques drawn from many and varied parts of mathematics.

So for our own instruction and for that of the participants, we have made an effort to organize a conference in which each of the elephant's more important appendages is given its due and the beast is seen, in so far as this is possible in fourteen hours, whole. Since most other members of the organizing committee will have occasion during the course of the conference to present, either implicitly or explicitly, their different views of what we are trying to achieve, I would like to take a few minutes to offer some frankly biased comments. They will probably be a little off-base as well. I became conscious as we organized this conference that I was out of touch with many things and falling behind the times.

For me the subject has a structure, functoriality, from which a great deal else, Artin's conjecture and the general Ramanujan's conjecture, will all flow once it is established. They will be seen to be, although at a much deeper level, like Weil's conjecture on the Tamagawa number, which now follows, almost unnoticeably, from the most basic understanding of Eisenstein series and the trace formula, so that it is no longer even a theorem just an incidental fact. Of course Eisenstein series and the trace formula, at least the general trace formula, would not be at our disposal had it not been for Siegel's creation, as he was studying quadratic forms and the volumes of fundamental domains on orthogonal and other groups, of the theory of automorphic forms on a general group.

It goes almost without saying that in the structure expressed by functoriality, as in that of class field theory, which forms a part of it, a good deal of concrete number theory is implicit. So the reasons for the structure are not going to reveal themselves without long, patient searching for the revealing analytic or algebraic clues. I'll come back to this later.

Functoriality established, in whole or in part, the next goal is simply to deal with the Hasse-Weil conjecture, thus to identify motivic L-functions, the factors of Hasse-Weil zeta-functions, with automorphic L-functions. The most famous achievement along these lines so far, an achievement that may remain for a long while the high-point of the subject, is the proof of the Taniyama conjecture, also known, in various more precise formulations, as the Shimura-Taniyama-Weil conjecture, whose proof not only invokes some of the little hard information on functoriality available but also relies in various ways on our knowledge both ancient and modern of the modular curves, the simplest examples of what are called Shimura varieties. These are varieties that are defined over the complex numbers as quotients of homogeneous spaces by discrete groups and whose importance Shimura was the first to appreciate. He was also the first to begin to demonstrate, as a generalization of classical complex multiplication, that they could be defined in a natural way over number fields.

It is by no means clear to what extent the ultimate general treatment of motivic L-functions will pass through Shimura varieties, but as a start we would like to understand the arithmetic of these varieties, at least in so far as it affects the motivic L-functions attached to them. There are promising starts for varieties associated to, for example, unitary groups, but basic general problems remain: to define them correctly over appropriate rings, to understand their compactifications, and to understand how the points at infinity contribute to their zeta-functions. From that point on – granted the fundamental lemma that remains to be explained and the trace formula – there is a fairly well defined path to the comparison of their zeta-functions with automorphic L-functions. Rather than explaining these general problems in any detail, the organizing committee opted for a closer examination of one case, a case to which the number theorists attach, for reasons about which I am hoping Skinner or Wiles will give us a hint, particular importance, namely the group of symplectic similitudes in four variables. This is the lecture of Laumon. Since the zeta-function of the corresponding Shimura variety cannot be understand without an understanding of the endoscopy of this group, two additional lectures are needed, one by Arthur on multiplicities and L-packets for this group and one by Hales on the fundamental lemmas needed to deal with it. Arthur's lecture will, by the way, probably be the only one in which the trace formula is alluded to. In a more serious, more systematic conference much more time would be devoted to it.

Although it is very likely that the best way to encourage interest in the study of Shimura varieties is to persuade number theorists of the arithmetical importance of the theory for specific varieties, the general theory has in itself a great deal to be said for it, and I am uneasy that we were unable to devote at least one lecture to it. Its development requires skills and techniques that with the departure of Grothendieck are cultivated less than they once were, so that I sometimes fear that if it does not occur soon, there will be no-one left to carry it out.

Of course, the identification of motivic L-functions with automorphic L-functions is only to be a beginning. One wants to use the knowledge acquired in establishing it to deduce hard diophantine or geometric information, for example that supposed to be implicit in the values of L-functions at various points. Rather than overstep the bounds of my competence, I will say nothing more about this but simply wait to hear the lectures of Skinner and Wiles, expressing again my hope that they will not be excessively reticent and that they will be generous and give us some insight into the arithmetical consequences that they foresee as possibly resulting from an adequately developed theory of automorphic forms.

As you will see from Hales lecture, the fundamental lemma, even in its simplest manifestations, is difficult. In its general form, which appeared when studying Shimura varieties and their zeta-functions with the help of notions from representation theory and from the trace formula, it seems to have great combinatorial and topological depth. Once again, prudence suggests that I say no more, but refer you now to the lecture of MacPherson, who will with any luck give us some idea as to whether that depth has yet been plumbed.

This accounts for six of the lectures. Before going on to the others, I want to say a little about functoriality. I recall that automorphic representations are attached to groups G over number fields or, as we shall see in the lecture of Lafforgue, over function fields. Attached to G is a complex group ${}^LG$ or, more precisely, a family of complex groups ${}^LG_K$, one for every sufficiently large Galois extension K of the basic field F. The connected component does not change with K, only the group of connected components which is the Galois group of K/F. For many purposes K can be fixed. To an automorphic representation π is associated a family of semisimple conjugacy classes in ${}^LG$, the family

$$\{A_{\mathfrak{p}}(\pi)\},$$

in which $\mathfrak{p}$ runs over the complement of a finite set in the set of primes of F. I am inclined to call these classes Frobenius-Hecke classes as they are generalizations, introduced at the time functoriality was first discovered, of objects introduced by Frobenius and by Hecke. They are sometimes called Satake classes, although this terminology reflects a misunderstanding of their purpose and of what Satake discovered. If G if *quasi-split* and *unramified* then the homomorphisms of the Hecke algebra of $G(F_{\mathfrak{p}})$ into $\mathbb{C}$ are parametrized by the semisimple classes in ${}^LG$ which project to the Frobenius class at $\mathfrak{p}$ in Galois group. Satake on the other hand was concerned, so far as I know, with the Hecke algebra for an arbitrary local group, for which such a parametrization is not available. The existence of the parametrization does of course – and did – follow readily from the structure theorems for quasi-split groups over local fields and from Satake's structure theorem for the Hecke algebra, itself modeled on a theorem of Harish-Chandra for real groups.

This parametrization was in fact suggested by the need for a compact general expression for the Euler products appearing as constant terms of Eisenstein series and functoriality, in its first form, was suggested by the parametrization and by the Artin reciprocity law of class field theory. It predicts that if there is a homomorphism of complex groups

$$\varphi : \quad {}^LG' \to {}^LG''$$

and an automorphic representation π' of G' with associated classes $\{A_{\mathfrak{p}}(\pi')\}$ then there is an automorphic form π'' of ${}^LG''$ such that

$$\{A_{\mathfrak{p}}(\pi'')\} = \{\varphi(A_{\mathfrak{p}}(\pi'))\}$$

for almost all $\mathfrak{p}$.

With experience we can formulate it better, in a way that contains more information and that is more likely to lead to a proof. Basically every automorphic representation on G should be attached to a distinguished subgroup ${}^{\pi}H$ of ${}^{L}G$ that – at the cost of taking a sufficiently large extension K – we can pretty much assume is an L-group. For example, if π is of Galois type, thus associated to an admissible homomorphism of the Galois group into ${}^{L}G$, then ${}^{\pi}H$ will be the L-group of the trivial group $\{1\}$ over F whose L-group is the Galois group of K/F. In general, the group ${}^{\pi}H$, which at first one should only try to associate to a representation of Ramanujan type, a notion that I take from ideas of Arthur, would have the property that for any representation ρ of ${}^{L}G$ the multiplicity of the trivial representation of ρ restricted to ${}^{\pi}H$ is the order of the pole of $L(s, \pi, \rho)$ at $s = 1$.

I am becoming more and more convinced now that we are beginning to understand, thanks to Arthur, what the trace formula can do for us in the context of endoscopy, that we should turn to this general problem, for which we will need to develop not only the techniques we are learning from Arthur, spectral-theoretic and representation-theoretic, but also analytic techniques more familiar from classical analytic number theory. In the early years of the general analytic theory of automorphic forms we had enough to do to understand the consequences of the spectral theory and the representation theory. Indeed this is a development that is far from over, but it is sufficiently mature that we can now move ahead, taking it to some extent for granted and combining the insights already acquired with others still to be discovered. So for me, one major purpose of the conference is to continue and deepen the dialogue already begun between the two different approaches to the analytic theory.

On the other hand, there is another point of view, cogently argued on many occasions by Peter Sarnak. Various techniques have been used to establish partial results for functoriality, and it is certainly the results they provided that have persuaded any substantial number of mathematicians that there is something to it. I, on the other hand, who, for obvious reasons, was more easily convinced than many others, came to look on them, conviction once acquired, with reservations because of their lack of ultimate promise. On the principle, I suppose, that a bird in the hand is worth two in the bush, Sarnak argues, however, and there I believe is much to be said for his arguments, that the results they yield are already good enough for many of the traditional purposes of analytic number theory. So it is valid, indeed laudable, to exploit them to this end and unwise to abandon them prematurely.

This will be abundantly clear from the lecture of Shahidi, who plans, I believe, to describe some, at least, of the available methods, many of them representation-theoretic, for dealing with special cases of automorphic L-functions and special cases of functoriality. So far as I know, it is these special cases, not the general structure, that have made functoriality appealing to analytic number theorists.

The lectures of Duke and Iwanieç will certainly not address my favorite question, the existence of ${}^{\pi}H$, and they will seem to be a long way from, say, the lecture by Arthur on multiplicities for the symplectic group in four variables. I am persuaded, however, that to penetrate the theory of automorphic in any depth, we, or rather you – for me it is too late – cannot content yourself with learning the techniques of Duke and Iwaniec on the one hand or of Arthur on the other, but will have to make an effort to master both techniques. So far, I see very few young people who are taking this challenge seriously. This last sentence was written in a pessimistic mood. Just after writing it, I went to a seminar, and down the row from me was a young fellow with two books in his lap, Knapp's voluminous tome on representation theory and another book on the Hardy-Littlewood method. My pessimism vanished.

In a slightly different vein, the refinement of global functoriality proposed by Arthur and suggested by his analysis of the trace formula has led to a much clearer concept of Ramanujan's conjecture and thus to a much clearer notion of its possible consequences. In particular the notion of an automorphic form of Ramanujan type is taken, although the terminology is not, from him. In addition this refinement has clarified the very difficult local problem of classification of unitary representations, for both real and p-adic groups. If I am not mistaken, we shall hear more about Arthur's ideas and their consequences in the lecture of Moeglin.

The local representation theory is of course an essential aspect of the theory of automorphic forms. In the solution of many number-theoretical problems, an analysis of the ramification is, surreptitiously or not, an important ingredient. Moreover sooner or later, we will want local demonstrations of local results, but we have a long way to go. Even so, there is a lot one could discuss even now. Unfortunately our time is limited. So there will be just one lecture on the local theory, that of Taylor. The results are a striking application of global results on Shimura varieties and encourage a great deal of confidence in the existence of a local correspondence for general groups, but, like so many of the other results described in the conference, are also an illustration of the difficulty in the theory of automorphic forms of not merely obtaining the right results but in addition obtaining them with the right methods. Here as elsewhere in the subject – except perhaps, to give vent to another personal bias, in class-field theory itself or in the theory of tempered representations of real groups – we are still looking for the right methods. So it would have been good to devote some time to efforts to construct purely local theories, but that time was just not available.

There is, for better or worse, something known as the "Langlands program" although it is not always clear what that is, whether, as for me, it would be functoriality or whether, as often seems to be the case, it is what was for me an afterthought, namely the problem of showing that all motivic L-functions are automorphic L-functions. What is certain is that in so far as the phrase means something to a public outside of mathematics or even outside the theory of automorphic forms, within topology, say, or within physics, it means often neither the one nor the other, but refers rather to the geometric theory, created by Drinfeld and referred to by him as the "geometric Langlands theory", the theory over function fields over finite fields, about which, as I said, we shall hear from Lafforgue, lying, both historically and conceptually, somewhere between this and the original purely number theoretic ideas. Both theories, the theory over function fields and especially the strictly geometric theory, are of interest at this conference because they were inspired by the theory of automorphic forms. The theory over function fields over finite fields is, in many ways, a part of the theory of automorphic forms although it has, I believe, broader implications in, say, sheaf theory. The geometric theory is, to an even greater degree, significant in ways that transcend the confines of automorphic forms and of which few of us here have any notion. So Gaitsgory and Beilinson, wittingly or unwittingly, have assumed a titanic task: to make this geometric, perhaps even physical, significance comprehensible to an audience, many members of which do not have a great deal of geometrical experience, much less experience with theoretical physics. We have a lot to learn and do not expect to become perfectly conversant in two hours with all ramifications of the theory or to become fluent in, say, the connections with mirror symmetry, but I confess that I myself am hoping to know a little more on Friday evening about these things than I do now. Those of us who have been in Princeton during the past two weeks have a head-start; we have been treated to an exellent introduction to the subject by Gaitsgory; but we have stressed to the two speakers that all of you were not that fortunate, and that there are a large number of young people here with backgrounds largely in algebraic number theory or analytic number theory who would greatly appreciate an explanation of the geometric theory tailored to their needs. At least I hope there are, for functoriality as it appears in the geometric theory carries a conceptual conviction that is not, and may never be, present in functoriality over number fields, where the proofs, even in the simplest cases, demand estimates of greater or less difficulty. But conceptual insight is not always what number theorists, especially analytic number theorists, are looking for or what persuades them.

There are many other things, some important, that we did not fit in. The theta-correspondence is an example. It and its connections with functoriality have always been a mystery to me and I still think it a genuine challenge – a challenge that, in spite of much progress, has not yet been met – to formulate clear, conceptual principles that subsume all essential phenomena to which the theta-correspondence gives rise.

Before stopping, I want to stress once again how important it is for those who want a clear view of the subject to have some insight into the trace formula and into the general theory of Shimura varieties, what the trace formula can give with the help of endoscopy, thus through the direct comparison of the geometric sides of the trace formulas available at present, what role it plays for Shimura varieties, what the study of Shimura varieties itself needs from algebraic geometry or from the theory of algebraic numbers, what the fundamental lemma is, how it is used, and why it is difficult, and of course what the limitations, and they are severe limitations, of these ideas are, although they are far from exhausted.

The study of particular varieties and particular groups plays itself out to a large extent against the background of general insights. On the other hand, without the desire to learn about specific groups for specific reasons, most mathematicians will have no incentive to learn anything about the general theory. So some balance has to be struck. Moreover the methods favored at present in the general theory, spectral methods, thus in particular familiar analytic tools like differential equations and Fourier analysis, and representation theory, whatever their power and whatever their necessity, are probably not enough. So a balance has to be struck in another sense, or rather a bridge has to be built, between these methods, used by Selberg, Harish-Chandra and Arthur and those of the analytic theory of numbers.

Whether you will feel on Saturday that the organizers and the lecturers have managed to strike the balance and build the bridges, I don't know; but I hope that you will at least be persuaded of the need for them

As most of you will know, the Director of the Institute, Phillip Griffiths, is a mathematician whose contributions to, for example, variations of structure and to vanishing theorems for cohomology have influenced the development of automorphic forms. So I am pleased that he agreed to drop by this morning to welcome the participants in the conference to the Institute.