An appreciation

Qing Zou asked me to contribute some remarks introductory to the book that follows. I have not read it. I have indeed only seen a relatively small number of pages. Moreover, although in the course of my career as a mathematician and, in general, in the course of my life, I have acquired a working knowledge of several languages, Chinese in any of its forms is not one of them. Consequently I have not read even the pages received and can only guess at their content and that of the text as a whole. So this appreciation consists of jottings suggested by my own reflections over the years, but not entirely unrelated to the pages at hand.

Since the phrase *Langlands programme* makes me uneasy, I would like to think of the appreciation as a collection of partly autobiographical but largely historical comments on those parts of pure mathematics, of which there are surprisingly many, that appear in the contemporary theory of automorphic forms or automorphic representations. They have been embraced by it — or coupled to it — over the course of the nineteenth, twentieth, and now of the twenty-first centuries with, I believe, an inevitability that is hard to deny. My comments here are not unrelated to those in the lecture *Is there beauty in mathematical theories?* (see *The many faces of beauty*, ed. Vittorio Hösle) but are perhaps more mature. Nevertheless, they still remain in good part provisional. In contrast to those earlier remarks, what follows is directed to individuals with an active interest in mathematics, in particular to the readers of Qing Zou's book. So I do not hesitate to suggest topics on which they might reflect.

The current theory of automorphic representations, has a number of aspects that it is important to distinguish from each other, either because one is at a higher conceptual level than another or because — their resemblance, which is often compelling, notwithstanding — they refer to mathematical domains with different techniques and different histories. On some I have reflected for long periods, on others for short periods, and on some of the most important not at all. Their classical manifestations appeared with: (i) the quadratic reciprocity law and its development as class-field theory in the hands of Weber, Takagi, Artin and others; (ii) with Cartesian geometry either in the form of algebraic geometry or of differential geometry, where Lefschetz, Hodge or Chern are familiar names, not to speak of Riemann or the discoverers of non-Euclidean geometry; (iii) with the theory of groups and their representations, to which are attached, among others, the names of Galois, Frobenius, and in recent times Harish-Chandra; (iv) algebraic geometry over finite fields, in which two representative names are Weil and Grothendieck; (v) the developments arising from the classical theory of elliptic modular functions, especially p-adic deformations, whose importance Serre has emphasized and which feature prominently in the proof of Fermat's theorem by Wiles, and the theory of Shimura varieties; (vi) the theory of automorphic forms on general groups as developed, even rescued, by Siegel largely as a continuation of the work on quadratic forms, by a large number of prominent nineteenth-century mathematicians, Eisenstein, Minkowski and others — this is not the place for a careful history and I am not the one to prepare it — and greatly enriched by Maaß and then Selberg and others as the analytic theory of automorphic forms. There are other currents appearing simultaneously with those in this list, perhaps even inseparable from them: (vii) algebraic number theory as created by Gauss, Kummer, and Dedekind and others; (viii) the theory of L-functions, in which Dirichlet is a prominent name, as is Hecke, whose work strongly influenced Maaß, and who, along with Siegel may be regarded as one of the founders of the general theory of automorphic forms. A systematic history would demand the inclusion of many more names and a much more elaborate chain of succession.

All of these theories or initiatives were current as I began my mathematical career in 1960, ending my student days with a doctorate from Yale and beginning my career as a professional mathematician at Princeton. The years at Yale and those at Princeton were among the most profitable of my mathematical life. I had the good fortune to be largely master of my own time and from reading or from various courses was introduced not only to functional analysis and analysis as presented in the first edition of Zygmund, in Hille's book on analytic semi-groups, and in Stone's book *Linear transformations in Hilbert space* but also to the Hecke theory and to Selberg's lecture on Eisenstein series and the trace formula, almost his only publication on the subject. I cannot say that I understood all these books as well as I might have, but they, with the addition of the book of Coddington-Levinson on ordinary differential equations, certainly provided me with a knowledge of basic linear analysis and of functional analysis that was a big help as I was trying to find my way through Siegel's papers in my first years at Princeton. It also meant that I was able to follow Selberg's oral presentation of the proof of the analytic continuation of the Eisenstein series for $SL(2)$ in my first, and surprising as this may be, my last mathematical conversation with him, since our offices were, many years later, essentially side-by-side for a good long time. Selberg was not always a taciturn man, but, in my experience, not given to talking about mathematics. For groups of rank-one, the analytic continuation is a chapter in the spectral theory of differential equations on a half-line. It was a great pleasure to speak, for the first time in my life, with a strong mathematician about serious mathematical matters, or rather to listen to him. Since there was no published version of his arguments available, it was also very important as I developed the arguments that appeared in the notes *On the Functional Equations Satisfied by Eisenstein Series* that I was familiar with the argument for $SL(2)$.

Selberg, I am sure, had invited me to his office at the suggestion of Salomon Bochner, whose encouragement and suggestions also played a decisive role in my first years at Princeton. In the first months, I was as a very junior mathematician invited to speak in the analysis seminar. Although my thesis, later incorporated by Derek Robinson into his book, *Elliptic Operators and Lie Groups*, had been on semi-groups and partial differential equations, the talk was about a theorem that I had proved as a graduate student while studying Selberg's paper. I believe that Bochner, whose early years as a mathematician had been spent in Germany and who, various clues suggest, moved in the same mathematical circle as, say, Hasse and Emmy Noether, was not only very fond of Dirichlet series but also very favorably impressed by my independence. I had worked alone as a graduate student and, more importantly, on at least two different subjects. He encouraged me in a number of ways, above all by suggesting that I give a course on class field theory. This was a terrifying suggestion. In the early 1960's class field theory was unknown outside of Germany and the circle of Artin's students in Princeton, and not regarded as otherwise accessible. He insisted, and on the basis of Hecke's book, *Vorlesungen über die Theorie der algebraischen Zahlen*, and Chevalley's thesis I managed to give a course to a small number of very tolerant students. It is astonishing to reflect how much and how many varied topics I became familiar with in those years. In particular, thanks to a suggestion of David Lowdenslager, who died very young, I turned while studying Siegel and Selberg to the papers of Harish-Chandra.

Two problems attracted me: the construction of a non-abelian class field theory; the correct(!) definition in the context of the general theory of automorphic forms of the notion of an L-function. They were discouragingly difficult, and I did abandon them for some time, but suddenly as the result of idle computations with the functional equations of Eisenstein series a road to the simultaneous solution of both of them was opened. It is not that other suggestions were not being made, one, at least, of some use but most not, but I had and have today even more the conviction that the definition implicit in the theory of Eisenstein series reveals the essence of the theory of automorphic forms or representations. As the result of a chance encounter, I described these ideas, at the beginning of 1967 and in an inchoate form, in a letter to André Weil, which is still, in spite of its raw form, largely valid. The subject has nevertheless changed. Not only has it progressed, but its goals, influenced by the reflections and contributions of many people, have changed. Although many very difficult problems remain to be solved and many difficult notions remain to be clarified, the conceptual structure of the theory is becoming clearer. Whether this clarity is generally accepted is another matter.

I have tried and continue to try, in for example the Rogawski paper and the Mostow lecture,[1] to explain my stance. There are three central issues — functoriality, the geometric theory, reciprocity, of which the first and the last will be perhaps of most interest to the readers of this book. There are also special aspects, fascinating in themselves, all difficult, some extremely challenging, but which can, and probably should, be examined as independent issues. There are, for example, local questions: (i) the representation theory of reductive groups over non-archimedean fields; (ii) endoscopy, especially over non-archimedean fields; (iii) Arthur packets over all types of local fields. There are good reasons for an individual mathematician to focus on any one of these areas.

The geometric theory is, if not conceptually independent of the other issues, technically independent. The base field in the usual theory of automorphic forms is an algebraic number field of finite degree over $\mathbb{Q}$; in the geometric theory, the base field is the field of meromorphic functions on a compact Riemann surface. In it, there is a connection to theoretical physics, provided by, for example, the duality associated to the names Montanen-Olive. This connection may be the principal source of the notoriety of the Langlands programme, but interesting as it is, it does not seem to me the major reason for mathematicians to concern themselves with the geometric theory. For them, the major reason is rather the presence of functoriality, thus of a central feature of the arithmetic theory, in the geometric theory where it appears to draw on a large number of the central notions of pure mathematics, especially of differential geometry, namely connections and curvature. It is not

[1] *A prologue to functoriality and reciprocity, Part* I and *Notes for a lecture at the Mostow conference*, October, 2013 both available in the section Beyond Endoscopy of `publications.ias.edu`.

that functoriality in itself is essential to the geometric theory, but it adds a differential-geometric richness that is easily obscured by the impulse, not, I think, entirely happy, to treat the geometric theory as a topic in sheaf-theory. That is, I feel, too confining.

The first and the third topics, functoriality in the arithmetic context and reciprocity, will be arithmetic or algebro-geometric. There is no reason to think that reciprocity has a geometric form. Functoriality precedes reciprocity and they share (or are expected to share) what might be called a non-abelian class field theory: functoriality when the source group is the group with one element, namely $\{1\}$; or reciprocity when the algebraic variety (or motive) is the cohomology of a variety of dimension 0, thus a set of points.

I remind the reader that functoriality is not yet available either at the local level or at the global level. At the local level, it is expected to be a feature of the representation theory of reductive groups. At the global level, it is, as explained in the Rogawski paper, expected to be a consequence of the trace formula, but by no means an immediate consequence. It is an expression of the introduction of the L-groups into the theory. A basic ingredient of functoriality and of the letter to Weil is the assignment to each reductive group G over a field F, local or global, of an L-group ${}^LG$, which is a group over $\mathbb{C}$. Questions of endoscopy aside, local functoriality assures that the representation theory of a group G defined over a local field is cogredient in ${}^LG$, thus when there is a homomorphism ${}^LH \to {}^LG$, there is a set-theoretic mapping of the collection of (appropriate equivalence classes of) irreducible admissible representations of $H(F)$ to those of $G(F)$. Functoriality, another designation for this cogredience is a substitute for, or an adjunct to, the Galois group. As such it is a complicated notion and needs to be explained in detail, but much has been done. So there is no doubt of its pertinence. Over a global field the appropriate property is a cogredience for automorphic representations.

Locally the existence of functoriality is regarded as a problem in the theory of the representations of $G(F_v)$, F_v the local field, and as such only partially solved. In the Rogawski paper a programme for establishing the existence of functoriality in a global context is sketched. It will be very difficult and will demand the use and development of methods from analytic number theory, methods whose focus is zeta-functions, L-functions, and Dirichlet series. Some progress has been made since that text was written, but much, much more is required. It will have many consequences.

The third issue, reciprocity, has not yet been seriously addressed. Arithmetically it is at a different level than functoriality. Global functoriality, like global class-field theory is an arithmetic theory and thus will demand the study of diophantine problems. This is intimated in the Rogawski paper and elsewhere. The diophantine problems are, however, discrete and are not encumbered by the geometry of varieties of positive dimension. Nevertheless global functoriality will provide a solution to the problem of reciprocity for a limited class of motives, those of dimension 0. Moreover functoriality implies that the collection of (stable classes of) automorphic forms has a linear structure, or rather a tensor structure, analogous to the Tannakian structure that was introduced in the context of motives by Grothendieck. The problem posed by reciprocity is to show that the second structure, the motivic structure, is a substructure of the first, the automorphic structure. Here, however, a major preliminary problem appears: establish the existence of an appropriate tensor structure for motives. Grothendieck and his collaborators attempted to so but with only partial success. This means that we — or rather the youthful among us, for there is little, I fear, that we elderly can offer, except encouragement — are faced with the problem of establishing two theories simultaneously, a theory of motives and a theory of reciprocity for motives. It may be easier, perhaps even necessary, to deal with both at once.

In the previous paragraph, I have used five times a word *motives* that I cannot expect the reader to understand or necessarily to have encountered. It expresses, at the moment, more a hope, a hope based on considerable experience, than a genuine theory. The hope, or the expectation, is that a theory of diophantine equations, thus of equations less over $\mathbb{Z}$ as one could expect from the allusion to Diophantus than over $\mathbb{Q}$, or perhaps over a finite algebraic extension of $\mathbb{Q}$ is, as in the study of Galois groups and their representations, for some purposes, best expressed by linear structures of some kind associated to the equations, thus to the algebro-geometrical objects — sets of points, curves, surfaces, or higher-dimensional varieties — they define and to the not easily understood topological structures attached to these, especially subspaces of their cohomology. These linear structures are not readily come by and the applications, although important as mathematical achievements, rare and not easily accessible.

Before we come to motives and reciprocity, it is best to recall that once again, as at the end of the nineteenth century and beginning of the twentieth when class field theory was developed, the link between the new objects, automorphic representations, or to be more precise L-packets of automorphic representations, and motives, remains the L-functions: Hecke L-functions associated to automorphic representations; Hasse-Weil L-functions associated to motives. The properties of the L-functions and of their local factors, for they are both given as Euler products, is a consequence of the relation, referred to here as reciprocity, between the tensor structures. Concretely it comes to statements affirming that such and such an L-function attached to an algebraic variety, according to principles first formulated in all generality by Weil and developed by Grothendieck and others, is equal to such and such an L-function attached to an automorphic form by principles to which the name of Hecke, among others, is attached. A coarse, even very coarse, assertion of the utility is that the Hasse-Weil or Artin L-functions have clear arithmetic properties although their analytic properties are unknown, while Hecke L-functions have clear analytic properties but whatever arithmetic they directly reflect appears only initially in their construction and is followed by a great deal of difficult analysis. For mathematicians attached to mathematics that can, in spite of or because of its profundity, be expressed in a more elementary fashion, the proof of Fermat's theorem is perhaps a more persuasive demonstration of the importance of reciprocity, but our concern here is the search for a general demonstration of its validity.

It was Grothendieck and his collaborators who first attempted to create a theory of motives, but without reflecting that reciprocity might be relevant, almost certainly without even an inkling of its extent. Whether for this or other reasons, they appear not to have been successful. He, or they, had a clear idea of the algebro-geometric information necessary to the construction of the theory. These were the *standard conjectures*, an expression of some deep properties of algebraic varieties. I shall review them briefly here because I feel that reciprocity is unlikely to be established until there are mathematicians who understand not only modern algebraic geometry but also modern representation theory.

At the moment, it can hardly be said that there are any who do so. My own knowledge of algebraic geometry is, unfortunately, superficial from both an intuitive point of view and a technical point of view. My basic source for this appreciation is the two articles of Kleiman, *Algebraic cycles and the Weil conjectures*, 1968, and *The standard conjectures*, 1991, but I have also consulted the brief essay of Grothendieck, *Standard conjectures on algebraic cycles*. My suggestion, for which I have no good grounds, except that there seems no alternative, is that it might be best to attack these conjectures and related conjectures, such as the Hodge conjecture, simultaneously with reciprocity, thus to move forward on the two fronts simultaneously.

This would mean in particular that we would develop the theory of motives at first for varieties over number fields of finite dimension over $\mathbb{Q}$ and only afterwards for varieties over general subfields of $\mathbb{C}$. This does not strike me — perhaps as a result of ignorance — as an outrageous idea. Over number fields, there are a number of techniques, for example p-adic deformation, of which my command is shamefully weak, that are developed, have been applied, and are undoubtedly important. How difficult it might be to pass from the standard conjectures over number fields to the standard conjectures over fields with transcendent elements I cannot say at the moment. I can imagine that it would, for a competent geometer, be an accessible problem.

I do not know what the theory of Shimura varieties, from which I have profited tremendously, can offer. Their study suggested notions like endoscopy and the Taniyama group, not to mention reciprocity itself, for which the Shimura varieties provide in a number of ways the best evidence available. Nevertheless their attraction for many mathematicians looks sometimes to me like the attraction of the lamp-post for the drunkard who has lost his keys, although sometimes the lamp-post is, in spite of its possible futility, the most promising of the available choices.

As far as I can see, from a brief and superficial examination, there are three levels at which one can work, but which it is best to distinguish:
(i) the Hodge conjecture;
(ii) the two standard conjectures, the Lefschetz standard conjecture and the Hodge standard conjecture, which is not the same as the Hodge conjecture;
(iii) the Weil conjectures and the work of Deligne.

Long ago, in the spring of 1967, just a few months after I wrote the letter to Weil, I was returning to Princeton from a day in Philadelphia, and on the way to the platform at the railroad station, I began to reflect on the Ramanujan conjecture, the theorem of Rankin-Selberg, and the possibilities suggested by the introduction of the general automorphic L-functions of the letter. I had never examined the paper of Rankin, written in 1939, when he was 24 years old, nor the paper of Selberg, written in 1940, when he was 23 years old, but I had some idea from hearsay or thought I had some idea, not only of what they had done, but also why they were able to obtain their estimates toward the Ramanujan conjecture. If the Hecke L-function associated to a classical automorphic form was

$$\prod_p \frac{1}{(1-\alpha_p p^{-s})(1-\beta_p p^{-s})},$$

then

$$\prod_p \frac{1}{(1-\alpha_p\bar{\alpha}_p p^{-s})(1-\alpha_p\bar{\beta}_p p^{-s})(1-\beta_p\bar{\alpha}_p p^{-s})(1-\beta_p\bar{\beta}_p p^{-s})},$$

can be expanded in a Dirichlet series whose logarithm is

$$-\sum_p \{\ln(1-\alpha_p\bar{\alpha}_p p^{-s})+\ln(1-\alpha_p\bar{\beta}_p p^{-s})+\ln(1-\beta_p\bar{\alpha}_p p^{-s})+\ln(1-\beta_p\bar{\beta}_p p^{-s})\}$$

or

$$\sum_p\sum_n \frac{|\alpha_p+\beta_p|^{2n}}{n^s}.$$

So Landau's theorem on Dirichlet series with positive coefficients, as on p. 10 of the book of Hardy-Riesz, *The general theory of Dirichlet series*, would be applicable. As I remember, it occurred to me on the escalator taking me to the platform that once the general theory described in the letter was available, thus functoriality, this argument would apply to the general form of Ramanujan's conjecture and, even for $GL(2)$, yield the full result, not just a weaker estimate. Two to three years later, in a lecture in Baltimore, published as *Problems in the theory of automorphic forms, §8*, I described the argument briefly, although rather glibly, largely because no-one had yet reflected on the nature of Ramanujan's conjecture in the context of a general reductive group. The Arthur packets were not to be introduced for some time.

Katz in his Dekalb lecture *An overview of Deligne's proof of the Riemann hypothesis for varieties over finite fields* suggests that the observation was a clue for Deligne to his proof of the Weil conjecture. Deligne himself does not mention it, not even as a suggestion that it might be profitable to consult Rankin. In a conversation more than thirty years later with me and Peter Sarnak, he was quite adamant that the clue came entirely from Rankin's paper.[2] I, myself, was in no position in the 1970's to read Deligne's papers on the Weil conjectures or on Hodge theory, nor had my ideas about automorphic forms matured to their present stage. It seems to me now evident that for a possible simultaneous development of the theory of motives and the theory of automorphic representations some familiarity, perhaps even a great deal, with these papers — combined with a mastery of the necessary analysis — would be advised.

I recently came across a brief essay by Illusie, *Pierre Deligne et la géométrie arithmétique*, in which he recounts Grothendieck's reaction on hearing that the last of the Weil conjectures had been proven. Apparently Grothendieck "était content, mais en même temps, évidemment, un peu déçu que les conjectures standardes elles-mêmes n'aient pas été démontrées." The close relation between automorphic representations or forms, thus of the Ramanujan conjecture in its first guise, and the Weil conjectures was familiar to a number of mathematicians, in particular, to Weil. So rather than being disappointed, Grothendieck, if he had responded with what I suppose — for although I had been in the same room with him twice, I had never spoken to Grothendieck — was his former

[2]Rankin, of course, does not otherwise cast such a long shadow as Selberg.

confidence and vigour, might have concluded that the correct way to attack the theory of motives was through the theory of automorphic forms and proceeded accordingly. He may, of course, have been unaware of its possibilities. It is difficult, especially from the outside, to weigh the importance of all the elements that a complete theory of algebraic geometry and arithmetic might contain, but it may be that the Weil conjectures should be considered a waymark and not the goal. We are perhaps not yet at a stage where we can assure Grothendieck that the items (ii) and (iii) are indeed closer and more entwined than he thought, but we can hope that his disappointment was premature. Experience with, say, the theorem of Fermat suggests that their relation with each other or with reciprocity will not be established easily or rapidly.

I promised that this appreciation would be short. It may already be too long. I would like to finish with some words, as few as possible, that provide both the reader and the author with some understanding of what is needed to introduce the notion of a motive and prepare them for distinctions like that between the *Hodge conjecture* and the *Hodge standard conjecture*. There are, as already observed, two standard conjectures, distinguished by the names of Hodge and Lefschetz. The two together are, apparently, sufficient for the creation of a theory of motives in Grothendieck's sense and they are both implied by the Hodge conjecture itself. Before describing them roughly, let me describe informally the conjectural construction of motives.

They are diophantine objects, or rather categories formed from diophantine objects. They are, at first glance, intuitive enough. One begins with a category whose objects are smooth projective algebraic varieties X and whose homomorphisms consist of the space $\mathrm{Hom}(X, Y)$ of numerical equivalence classes of algebraic cycles on $X \times Y$ of dimension $r = \dim X$, thus formal integral (or rational) finite linear combinations $\sum a_i U_i$, of subvarieties of $X \times Y$ two being equivalent if the intersection numbers $\sum_i a_i U_i \cdot V$ are equal for all subvarieties V of $X \times Y$ of dimension $\dim Y$. So their definitions require a sophisticated knowledge of algebraic geometry. There is a product $\mathrm{Hom}(X, Y) \times \mathrm{Hom}(Y, Z) \to \mathrm{Hom}(X, Z)$. For the motivic structure, there are two elaborations. First of all, the category just defined has a linear and a multiplicative structure. So we can add to the objects by formally introducing the images of all idempotent endomorphisms. There is also an additional object to be introduced, the Tate motive, whose function will be explained after the central issue is described. Adding it, as in Kleiman's 1991 essay, and fixing the field F of definition of the varieties and of the cycles we arrive — I believe — at the category of motives over F.

According to Grothendieck's essay, the theory of these motives is "a systematic theory of 'arithmetic properties' of algebraic varieties, as embodied in their groups of classes of cycles for numerical equivalence". One can fix the field of definition of these cycles, for the moment a number field F of finite dimension over $\mathbb{Q}$. Once global functoriality is proved, we can use it to associate a similar category to the theory of automorphic representations over F. This will be much larger, but the hope is to prove that it contains the motivic theory as a subcategory — a much smaller subcategory. This would be the theory to which I refer, anticipating its development, as *reciprocity*. I regard it as a possible generalization — clearly at an altogether different conceptual level — of the quadratic reciprocity of Euler, Legendre and Gauss.

I observe that there may be a complication that the provisional "theory" of motives described above does not explicitly envisage, namely a form of *endoscopy.* I do not care to speculate what form it would take. It might be instructive to examine the possibilities that appear in the theory of Shimura varieties and their conjugates.

The category or categories defined by functoriality will be Tannakian, thus, in particular, there will be a notion of contragredient object, provided by the contragredient of the underlying representation of $G(\mathbb{A})$. The introduction of the Tate motive ensures that the contragredient exists in the motivic context.

The Hodge conjecture itself is discussed in many places. Its statement is fortunately easy. If X is an n-dimensional algebraic variety and $p \leq n$ then a class in $H^{p,p}(X) \cap H^{2p}(X, \mathbb{Q})$ is realized as a rational linear combination of the classes associated to subvarieties of dimension p. Anyone attempting to establish the standard conjectures might also want to decide whether the Hodge conjecture was true or false. The standard conjectures are more technical, but that they are associated to the names of Lefschetz and Hodge suggests that they have a strong geometric flavour none the less. All of these conjectures are statements about rational or integral linear combinations of subvarieties of X that are not necessarily smooth and are viewed as topological objects. The appropriate notion of equivalence (τ-equivalence in the two papers of Kleiman) for such linear combinations is almost the same as algebraic equivalence, two such linear combinations being regarded as algebraically equivalent if one can be algebraically deformed to the other. A proper appreciation of both these notions requires experience. To gain that, it is necessary to consult not only the papers of Kleiman but also standard texts.

In addition to the Lefschetz standard conjecture, there are two Lefschetz theorems, the weak and the strong. Both state, in one form or another, that the section of a smooth irreducible projective variety X by a linear space carries about all the cohomology of X that it possibly can. One starts from a smooth projective variety X and considers a smooth hyperplane section $Y \subset X$. If the dimension of X is r, and the dimension of Y is $r-1$, then Y defines a cohomology class $\gamma_X(Y)$ of degree 2 on X. We can multiply by this class $x \to \gamma_X(Y)$ and obtain a homomorphism $L : x \to x \cdot x \to \gamma_X(Y)$ from $H^i(X)$ to H^{i+2}. It can be iterated: $L^r : H^i(X) \to H^{i+2r}(X)$. The strong Lefschetz theorem, which is valid in the classical context of cohomology over $\mathbb{C}$ and for etale cohomology, where it is regarded as deeper than the last Weil conjecture, affirms that the $(r-i)$-fold iterate

$$L^{r-i} \,:\, H^i(X) \to H^{2r-i}, \quad i \leq r \tag{1}$$

is an isomorphism. The Lefschetz standard conjecture states that this continues to be so when i is replaced by $2i$ and the cohomology by τ-equivalence classes of algebraic cycles.

The Hodge standard conjecture is not any easier to state, and, as for the Lefschetz conjectures with which it is logically intertwined, has variants according to the cohomology theory used. It is not so easy to grasp its significance. There seem to be at least two purposes to the conjectures: (i) to establish the cohomological properties, whether in the ordinary theory or, in positive characteristic, in the etale theory, that are necessary to establish a theory of motives — say one adequate to the construction of a theory of "reciprocity"; (ii) to allow, as with the Hodge conjecture itself, us to recognize when two possibly distinct "motives" are the same, as with two rational classes of type (p, p). These tasks are not made easier by the unavoidable multiplicity of possible cohomology theories. Kleiman's second paper is written simply and clearly, but has a complexity that for me, at least, is very difficult to master. That may be because the hierarchy of the lemmas and theorems to be proven was, and is, uncertain. I have, as yet, no feeling for either the Lefschetz standard conjecture or the Hodge standard conjecture. The vector space of algebraic cohomology classes, thus the classes in degree $2i$ represented by algebraic cycles of dimension i, is denoted $A^i(X)$. The Hodge standard conjecture is related to a theorem of Hodge, the index theorem, for algebraic surfaces and concerns the intersection of the spaces $A^i(X)$ with the primitive cohomology.

The Lefschetz theorems imply that cohomology of X can be largely reconstructed from the kernels of

$$L\,:\,H^i(X) \to H^{i+2}, \quad i = 1, \dots, r.$$

The elements of these kernels are referred to as the primitive cohomology, and denoted $P^i(X)$. Of particular interest is that part of the primitive cohomology formed from algebraic cycles, thus $A^i(X) \cap P^{2i}(X)$, $i \le r/2$. The Hodge standard conjecture affirms that the $\mathbb{Q}$-valued pairing on $A^i(X) \cap P^{2i}(X)$, given by $(-1)^i \langle L^{r-i} s \cdot y \rangle$ is positive-definite for $i \le r/2$. For surfaces, thus for $r = 2$, this is a theorem of Hodge.

I have yet to master the ins-and-outs of the standard conjectures and their consequences for the theory of motives, but the beginnings are there and the first challenge is to succeed, on the basis of the theories, theorems, and methods available, in constructing a theory of motives for algebraic cycles, as well as a theory, presumably p-adic, of automorphic forms or representations in which some kind of deformation appears that is compatible with functoriality. The exact meaning of compatibility will have to be made exact, but it is to be defined by the L-functions. I hope that with the help of the deformation theory it will be possible to establish that the motivic category is a sub-category of that given by the functoriality of (stable classes of) automorphic representations.

To summarize: young mathematicians who are familiar with the theory of automorphic representations can study, in addition, either differential geometry with the hope of attacking the geometric theory, as would be easier; or they can study algebraic geometry with the hope of attacking reciprocity, as would be, I believe, much more difficult. For myself I have become less ambitious; I simply want to appreciate more deeply all the implications, or all the possible implications, of the modern theory of automorphic representations, and to complete my education in pure mathematics by acquiring a concrete familiarity with some important classical and modern topics of which my knowledge has remained regrettably superficial.

These introductory notes bear the heading, *An appreciation*. It may be asked, "an appreciation of what?" I have no certain response to this question, but my remarks, like, if I am not mistaken, the book of Qing Zou that contains them, are an attempt to explain how the theory of automorphic representations is related, in a concrete, precise, and enriching way to a large collection of central themes in pure mathematics, classical and contemporary. Whether they will continue to be central is uncertain. The attention of mathematicians may in the future be focussed on entirely different matters; mathematics as such may lose its appeal. The world is much different now than it was when I was young; it may, indeed it will certainly, change even more. We can look back at least as far as Euclid, or even Pythagoras, and perceive in mathematics, or, in the present context better, pure mathematics, a unity or, in spite of interruptions, a continuity of purpose over millennia. The continuity of pure mathematics may, like that of any other human undertaking, be broken and the accomplishments themselves forgotten. That is a chance we take!

I should confess as well that, even in the book of Qing Zou, the theory of automorphic forms and automorphic representations may be treated in a perspective broader in some respects than the one I offer. I chose to focus on the issues in the theory of automorphic representations that have preoccupied me and that I consider central to pure mathematics, both historically and conceptually.

Robert Langlands