A MATHEMATICAL DREAM AND ITS INTERPRETATION

Michael Harris

UFR de Mathématiques
Université Paris 7
2 Pl. Jussieu
75251 Paris cedex 05, FRANCE

On sabbatical from my position as professor at Brandeis, I spent the 1992-1993 academic year in France, visiting colleagues and teaching courses at two universities, in preparation for a possible move to Paris. Boston was then and still is one of the world's great mathematical centers, and by attending Harvard's number theory seminar and the MIT representation theory seminar I kept in touch with all the most important developments relevant to my own work in automorphic forms, on the border between these two subjects. Paris, however, was not only actively and consciously exercizing its role as the natural headquarters of mathematical research in Europe, with the most extensive seminar schedule anywhere. It was also home to the world's largest concentration of specialists in automorphic forms, most of them roughly my own age. This meant that for nearly twenty years we had followed developments in the field and neighboring fields in the same sequence, had witnessed the same breakthroughs and had met one another repeatedly at the same international meetings, and were deeply familiar with one another's complementary contributions to a highly active, influential, and competitive branch of mathematics.

A year after I wrote a thesis in pure number theory in 1977, I switched to the field of Shimura varieties and in this way gradually became a specialist in automorphic forms. My interests, reflecting my start in number theory, were somewhat peripheral from the standpoint of most of my Paris colleagues, who were for the most part guided by the priorities of the Langlands program. This was one of the great research programs of our time, in Lakatos' sense, and benefited from Langlands' meticulous elaboration not only of the program's ultimate goals, too distant to serve as more than motivation, as well as a remarkably precise vision of accessible intermediate goals and the steps needed to attain them. In both these respects the Langlands program, developed in several stages during the 1970s, and thus part of my generation's collective memory, resembled the program promoted by Grothendieck in the 1960s, whose ultimate goal was to prove the Weil conjectures. All but one of these had been settled by Grothendieck himself, but the last one was

Institut des Mathématiques de Jussieu, U.M.R. 7586 du CNRS. Membre, Institut Universitaire de France.

Typeset by $\mathcal{A}\mathcal{M}\mathcal{S}$-TEX

scheduled as the final chapter of a book in thirteen parts, most of which were speculative in the extreme. Our teachers had been trained in Grothendieck's program, as ours had been in the Langlands program, but Grothendieck's vision was doubly short-circuited: first by Grothendieck's own withdrawal from active mathematical research, and then from society altogether, in the early 1970s, then by Deligne's unexpected proof of the last of the Weil conjectures, decades ahead of schedule, using ideas he had learned by studying the Langlands program.

Of all the possible techniques for proving his conjectures, Langlands preferred those connected with the Selberg trace formula and its vast generalization by Arthur. In this Langlands has been consistent through most of his career, frequently to the point of criticizing proofs of his conjectures that avoid use of the trace formula. The general idea of the trace formula is not hard to explain, and indeed is quite similar to the classical Lefschetz fixed point formula in algebraic topology, whose vast generalization by Grothendieck and his collaborators was central in their approach to the Weil conjectures. Both formulas can be seen as examples of *index formulas* which arise in one form or another in most branches of pure mathematics as a means of deriving often unexpected global consequences – for automorphisms of manifolds, differential operators, algebraic varieties, solutions of diophantine equations, or representations of finite groups – from purely local data that are in principle amenable to calculation. It was one's attitude to this "in principle" that determined where one stood with respect to Langlands' program. To begin calculating the local data one needed to assimilate a mass of complex specialized notation and terminology that had mostly been developed by Langlands and his closest collaborators, much of it in articles by Langlands himself that were notoriously difficult to read. Arthur's invariant generalization of the Selberg trace formula involved not only local data but also presumed the solution of global problems in simpler situations, hence introduced an inductive structure into the problem with its own complications. Finally, the heart of Langlands' plan involved use not of Arthur's trace formula but a hypothetical *stable* version thereof, whose construction relied on yet another series of difficult articles by Langlands, as well as the so-called *fundamental lemma*, originally expected to be proved by a routine if tedious calculation, and only resolved this year by Ngô, following the latter's work with Laumon based on a new version of Grothendieck's trace formula.

For all these reasons, and also because my original interest in Shimura varieties derived from number theory and geometry, rather than from group theory, those parts of the Langlands program based on the trace formula were not to my taste, and I had avoided learning the relevant techniques, although inevitably I was exposed to the methods in international conferences, and to the phenomena they were designed to explain in my own work. Langlands had been a frequent visitor to Paris, however. This was where he had first presented his vision of the stable trace formula, in a series of lectures I had attended in 1980. I had understood nothing whatsoever, but the Parisian specialists had studied these ideas over the years in their weekly automorphic forms seminar; some of these specialists were recognized internationally as experts in the Langlands program, but nearly all had made direct use of Langlands' techniques in one way or another in their own work.

Even had I wished to join the Langlands program, I had no hope of catching up with specialists who had a fifteen-year head start. I preferred remaining on the margins, where my work would not be judged by comparison with a pre-established set of goals and milestones. By 1992 I had spent fourteen years using Shimura varieties to study special values of L-functions. I entered this field by accident, because I knew how to put together two kinds of methods, one from geometry and one from group theory, whose conjunction I had recognized under another form in an article by Shimura himself. During the intervening years I had learned to vary the specific ingredients, initially as a close reader of Shimura's work, and in this way I learned a good deal more geometry and group theory, but the kind of combination was invariably the same. I did not so much set myself specific goals as discover new problems similar to those I had already considered in the articles I read while pursuing my education, or in conversations with my collaborators. Already in 1989 I was getting tired of this and when I spent a year in Moscow I hoped to switch fields, or at least to look at a class of special values of L-functions I hadn't previously considered. Five years earlier this had been a major priority for the Russians with whom I was expecting to work, but in the interim most of them had switched fields entirely , and although I returned from Moscow with a host of new ideas, they were very much along the lines of what had been on my mind when I had arrived, and owed little or nothing to my interactions with my Russian colleagues.

The projects I had begun in Moscow were nearly exhausted by the time I arrived in Paris three years later. I was looking for something new, not only because I was tired of the old subject but because it was tired of me, and had no new problems to offer. My most promising new departures of the previous years had turned out to lead to problems far beyond my powers to solve. While waiting in Paris for inspiration to strike I had found the key to completing the last of my Moscow projects. Once again it was a matter of aligning geometric and group-theoretic ideas, and although I knew clearly enough what was involved, I could tell it would be long and tedious to write out the details and I was not eager to begin. I must have been complaining to my friends, because one of them wrote at that time

Dear Michael,

It is time for a good book on special values of L-functions but I dont know if you really want to write one. On the other hand, it does seem perfectly rational to be dismayed at the prospect of another 30 years of results about period relations in the setting of Shimura varieties. So I understand your desire for change. . . . I dont think it can be that satisfying to be thought of as Mr. Coherent Cohomology and I can see the desire to get beyond that pigeonhole.

My only other project was a long shot, an attempt to understand yet another kind of special value of L-functions by comparing a version of the Arthur-Selberg trace formula – in some respects a refinement of the standard trace formula – called the *relative trace formula* with a sort of refinement of the Grothendieck-Lefschetz trace formula in the setting of arithmetic intersection theory (Arakelov theory). The goal was to find an abstract framework to explain, and ultimately to generalize, the constructions in the celebrated Gross-Zagier formula, in which two infinite collections of terms (one geometric, one group-theoretic . . .) are shown at the end of nearly one hundred pages of computations to match miraculously, term by term, with striking consequences.

This is the sort of vague idea that inevitably occurs to a number of people when they have nothing better to do, and though I had nothing very precise in mind, two talks I had heard the previous summer had renewed my interest in the question. At a conference in Jerusalem I had heard Rallis talk about his new and abstract approach to the relative trace formula; at a conference in the Black Forest in Germany I had heard Michael Rapoport explain his new work with Zink on the p-adic properties of Shimura varieties, which I hoped would explain the geometric side of the Gross-Zagier formula.

Rapoport, at the time a professor at Wuppertal, was going to be in Paris for a week or so in December, in connection with his participation in a jury overseeing the Orsay thesis defense of a graduate student named Alain Genestier. Or maybe he was visiting Paris for a month, which he did regularly in those days, usually to collaborate with Genestier's thesis adviser Gérard Laumon. My memory of the precise sequence of events is somewhat cloudy, and what follows is a reconstruction based on the few documents I managed to retrieve from that period. I do know that I made a lunch appointment with Rapoport early in the month to ask questions about the work he had described in Germany. Most likely I had seen him at the Tuesday seminar at Orsay – he may even have been the speaker – and had proposed that we meet for lunch. The lunch would have been on Thursday December 10, and I have confirmed that Genestier's thesis defense took place the following afternoon.

Rapoport and I were both satisfied with the quality of lunch, which was unusual in itself. I raised my question, Rapoport expressed interest, and had even brought some relevant documents to the table, but after a brief discussion we agreed that neither of us was sufficiently prepared to go more deeply into the question, and decided to postpone further consideration of the problem until we had had time to read the relevant background material. We made tentative plans for me to visit Wuppertal in January, The conversation then turned to matters connected with Genestier's thesis. Genestier was studying an analogue of Shimura varieties called *Drinfeld modular varieties*, and he was doing so from the point of view of their p-adic uniformization. This was a property they shared with certain Shimura varieties, a fact that had only recently been established by Rapoport and Zink as an application of the ideas Rapoport had explained in Germany. This is not quite right. Actually, Genestier was not thinking about p-adic uniformization but about the *Drinfeld upper half space*, an object with both geometric and group theoretic properties that could be used to give a relatively simpler model of either a Drinfeld modular variety or a Shimura variety, and this model, called p-adic uniformization, had not yet been established in the situation considered by Genestier. Instead, Genestier was interested in the properties of the Drinfeld upper half space itself. A primary motivation for studying the Drinfeld upper half space, Rapoport explained, was that it was widely expected to give a geometric model (cohomology) for a group-theoretic object – the local Langlands correspondence for $GL(n)$ of a non-archimedean local field. For the kind of Drinfeld upper half-space treated in Genestier's thesis, this local Langlands correspondence had been established by Laumon and Rapoport in collaboration with Rapoport's Wuppertal colleague Stuhler. But there was no geometric model for this correspondence, and the correspondence was still a major open problem for p-adic fields, where p-adic uniformization was known.

If these ideas were familiar to me at all, it was only in the vaguest way. Four years earlier I had attended a conference in Ann Arbor (as "Mr. Coherent Cohomology") in which Carayol had given several lectures setting out the conjectures Rapoport sketched to me over lunch. I remembered Carayol's lectures and the notes he had distributed at the conference, but I could not relate what I remembered to what Rapoport was telling me. Rapoport advised me to reread the article based on Carayol's lectures in order to prepare for Genestier's thesis defense. I believe I did so that night, motivated by the potential relevance of this work to my understanding of the Gross-Zagier formula. Carayol's article was written very clearly, and although the notions he treated were unfamiliar, I could see a parallel with other geometric constructions of group representations, specifically Schmid's proof of a conjecture of Langlands to the effect that the discrete series of real Lie groups occur in L_2 cohomology of period domains – an object for which Rapoport and Zink claimed to have found an analogue for p-adic groups – and the Deligne-Lusztig construction of representations of finite groups of Lie type, originally inspired by work of Drinfeld. A curious feature of the conjectures outlined by Carayol was that they involved the actions of *three* groups: the group $GL(n, F)$, where F is a p-adic field; the multiplicative group J of a certain division algebra over F, and the Weil (or Galois) group W of F. Drinfeld had proved in a very difficult paper that his upper half spaces had a family of unramified coverings, and Carayol's conjecture, an elaboration of earlier conjectures of Deligne and Drinfeld, was that the natural action of $W \times J \times GL(n, F)$ on the cohomology of this family simultaneously realized the (conjectural) local Langlands correspondence between representations of W and $GL(n, F)$ and the (known) Jacquet-Langlands correspondence between representations of J and (certain) representations of $GL(n, F)$.

Genestier's thesis was a difficult piece of work, and his defense was professional but intended primarily for experts, and I was not an expert. Nevertheless, I was struck by the resemblance of one of his main results – the irreducibility of the unramified coverings of Drinfeld's upper half spaces – to a theorem of Ribet I had studied as a graduate student. I wondered whether Genestier might not be able to derive his irreducibility result as Ribet had, by studying the action on the unramified coverings of the stabilizers of certain natural points on the base space. Genestier's thesis defense was followed by the customary champagne reception, and I remember insisting on this idea in conversations over champagne with Laumon, with Rapoport, and with Genestier himself. It is more than likely that at the reception I drank more than four glasses of champagne, which I have learned in the course of many thesis receptions to mark the border beyond which my remaining capacity for coherent thought can no longer be taken for granted.

The following morning – Saturday morning, if my reconstruction is correct – my wife had an early appointment, and we must have set the alarm early. I drifted into consciousness with the certainty that I had just dreamt about the cohomology of unramified coverings of Drinfel'd upper half spaces, and that the dream had brought me an insight I could not quite recover but that I was certain I should not let slip away. Warding off my wife's attempts to rouse me completely, I remained at the edge of wakefulness for several minutes, until the insight attained sufficient coherence to be expressed in words, or more precisely a combination of words and images to which I could associate mathematical content. Over the next few weeks my ideas grew clearer as I reread Carayol's article and discussed the problem with colleagues in Paris and Orsay, so that by December 29 the insight that came to me in my dream had taken the form of a research program that I described in detail in a letter to Rapoport. I will need to refer to the letter and have reproduced the mathematical argument in its entirety, though I don't think it will be of much interest to most readers.

Dear Michael–

It is probably a good time to think about organizing my visit to Wuppertal, if this is going to happen. Since I saw you I started thinking about a related but very different problem, namely the one you mentioned of trying to construct the discrete series of $GL(n)$ by imitating Atiyah-Schmid for the Drinfeld modular varieties. I came up with a crazy idea that is impossible (for the moment) to put into practice but which is probably right nonetheless. It is inspired by Schmid, rather than Atiyah-Schmid, and actually more by Zuckerman's algebraic version of Schmid, and even more by recent work of Schmid and Vilonen on realizing the characters of discrete series by a Lefschetz fixed point formalism. The basic idea is the following. We start with a representation ρ of D^* (D the division algebra with invariant $1/n$) and use it to construct a local system $L(\rho)$ on the Drinfeld upper half space Omega. We assume ρ is associated to a supercuspidal representation of $GL(n)$ (I'm thinking in terms of mixed characteristic, by the way) and we make the following two hypotheses:

(a) $H^*(\Omega, L(\rho))$ is concentrated in degree $n-1$;

(b) $H^*(\Omega, L(\rho))$ is discrete series.

Verifying these hypotheses may turn out to be the hard part. In any case, (b) guarantees that the representation of $GL(n)$ on $H^*(\Omega, L(\rho))$ is determined by the values of its character on the elliptic regular set. Now any elliptic regular element g in $GL(n)$ has exactly n fixed points in Ω. The following hypotheses involve the development of technology but the technology shouldn't depend on the specific situation.

(c) The character of g on $H^*(\Omega, L(\rho))$, i.e. the value of the function representing the distribution character at g, is given by the Lefschetz fixed point formula. (This is actually a series of hypotheses, including something about the summability of the character and something about ℓ-adic cohomology – or whatever cohomology – of rigid analytic spaces not of finite type.)

Assuming (c), one can calculate the character of g as the sum over the fixed points x of $Tr(g; L(\rho)_x)$. Assuming (a), we get

$$(1)^{(n-1)}Tr(g; H^{n-1}(\Omega, L(\rho)) = \sum_x (Tr(g; L(\rho)_x))$$

Note that the $(-1)^{n-1}$ (I lost the $-$ sign somehow) is exactly the sign relating the characters of ρ and the corresponding representation of $GL(n)$. Sorry about the *missing parentheses. We need another hypothesis:*

(d) At each fixed point x of g, $Tr(g; L(\rho)_x) = Tr(\rho(g'))$ where g' is an element of $D^\times$ corresponding to g.

Assuming all four hypotheses, we then conclude that the representation of $GL(n)$ on $H^{n-1}(\Omega, L(\rho))$ is isomorphic to n copies of the representation corresponding to ρ; alternatively, is isomorphic to (representation corresponding to ρ)$\otimes\sigma$ where σ is some n-dimensional representation of the Weil group ...

Hypothesis (d) would follow from the identification of the action of g on the fiber of the Drinfeld cover of Ω with the action of g', a sort of reciprocity law at the fixed points, which ought to extend to the Weil group as well. I suggested that the fixed points ought to be obtained as follows: let K = field generated by g (or by its eigenvalues), and let LT be the Lubin-Tate formal group with CM by K. For some embedding of K in D, we can construct

$$D \otimes_K LT$$

and this should be a special formal O_D module (the tensor should involve rings of integers). This should also provide an alternative proof of Genestier's theorem on the irreducibility of the Drinfeld cover (by letting the elliptic torus vary). Genestier has been working on this and he seems to have verified what is needed (it isn't clear whether the indicated method would also imply geometric irreducibility; Genestier thinks it doesn't). Of course the association of g' to g depends on some choices, but it seems that these choices are already implicit in the moduli problem, just as in the classical theory of elliptic curves, the relation to the upper half-plane depends on the choice of a basis for the homology.

I have some vague ideas about (a) and (b) involving Poincare duality and a cohomological interpretation of the Jacquet module, but I realize that I can't go anywhere until I actually learn the theory. The article of Boutot-Carayol is clear but it isn't easy. Drinfeld's original article is not so helpful. But of course if I want to think about intersection theory I will have to read everything. Laumon has made a number of suggestions regarding (c); he doesn't like the shape of the cohomology (inverse limits vs. direct limits) but he thinks it may be possible to rewrite everything in terms of vanishing cycles on the special fiber, where the cohomology theory really exists. Problems (a) and (b) are closer to things I have thought about in the past, but as Laumon points out, they are both false when the coefficients are trivial (Schneider-Stuhler) so they can't be easy to prove in the supercuspidal case. On the other hand, any proof would have to find a characterization of the supercuspidals in terms of the representations ρ of D^*.

I think I can come to Wuppertal during the week of January 18 ...

Rereading this letter, it is clear to me that I learned a great deal of mathematics from my colleagues in Paris during the last two weeks of December. At the time of my lunch with Rapoport, many of the notions described here were only familiar to me in the vaguest way, or at least I would not have written about them with such confidence. I mention Laumon's suggestions in the letter, and specifically his preference for a cohomology theory that "really exists" as opposed to the one, implicit in my letter, recently developed by Vladimir Berkovich, which did indeed exist but which did not obviously have all the properties needed to prove the a Lefschetz formula. The reference to Laumon's suggestions must refer to conversations that took place the week after Genestier's thesis defense, mostly with Genestier in attendance. The paragraph beginning "*Hypothesis (d) would follow*" is an almost verbatim account of what, after the dream itself, was the most uncanny incident of the whole experience: when Genestier asked me how I hoped to carry out the comparison in (d), I proceeded to explain the argument involving Lubin-Tate groups, of whose possible relevance to the problem Genestier, Laumon, and I learned simultaneously from the unconscious source that had already communicated to me in the course of the dream, and that had not bothered to provide this detail until it was specifically requested.

By chance, that same week Laumon's Orsay colleague Guy Henniart was hosting two visitors, Phil Kutzko of the University of Iowa and Colin Bushnell of London's King's College. Bushnell, Henniart, and Kutzko were the three leading experts in the representation theory of $GL(n)$[1]; they had been interested for years in the local Langlands correspondence for p-adic fields, and had begun writing a long series of articles on the subject, following Henniart's proof of the "numerical Langlands correspondence" (the counting argument mentioned in the footnote) and the monumental book of Bushnell-Kutzko.

I had known Kutzko for some time, and met Bushnell at a conference in the former East Germany in December 1989, when the two of them presented the results that soon appeared in their book. Due to a misunderstanding, I had not realized this was the main point of the conference – which Rapoport also attended – and my own presentation was interrupted before the end by the main East German organizer, who protested that it was not only incomprehensible to everyone else in the room but that it was irrelevant to the proceedings. In 1989 I had nothing to tell Bushnell and Kutzko, nor was I in any way able to appreciate their work. Three years later, though, I eagerly followed the two of them, and Henniart, to a brasserie in Montparnasse, where I spent much of the meal asking their opinions of what I had learned in my dream.

The story of the dream is only halfway done, and although I will spare you most of it, I have yet to tell you whether or not it has a happy ending, nor whether or not it is the one the text thus far seems to have prepared. But I already want to stress

[1]They still are. Although I eventually went on to write several articles on the local Langlands correspondence for $GL(n)$, and with Richard Taylor wrote a book containing the first proof of this correspondence, it is significant that our work used only the most formal properties of representations of $GL(n)$. The representation theory of the group J only plays a role, and a minor one at that, in a counting argument due to Henniart and quoted in a crucial way in every proof of a local Langlands correspondence, including the one found by Henniart soon after my work with Taylor. Henniart's proof is certainly the most natural one, but it also uses very little of the detailed representation theory developed by Bushnell, Henniart, and Kutzko. The relation between the local Langlands correspondence and representation theory remains an important open question. Incidentally, a counting argument of the kind developed by Henniart had first been used in this context by Jerry Tunnell to give a nearly complete proof of the local Langlands correspondence for $GL(2)$; the first complete proof in this case was discovered by Kutzko. Tunnell studied with me at Harvard and will reappear towards the end of this story.

the point of this story, which is that it does *not* follow the standard account of the role of the unconscious in scientific thinking, as exemplified by Kekulé's (possibly apocryphal) dream about the benzene ring or Poincaré's celebrated discovery of the relation between Kleinian groups and non-Euclidean geometry as he stepped onto the omnibus, or the dream of Robert Thomason on which my article [DAPTS] is based. Kekulé and Poincaré and Thomason and dozens of others have recounted the dreams and unconscious interludes that helped them solve problems that had troubled them for some time. The contrast with my situation could not be more striking: the dream I have described provided a strategy for solving a problem I had considered altogether irrelevant to my interests one week earlier. And though I was unable to bring the dream argument to a successful conclusion, the dream and the interest it inspired in this question did bring about a radical change in my mathematical priorities and a dramatic rise in my standing within the community of mathematicians.

For three months I thought intensely about how to transform the research program proposed in my dream into rigorous mathematics. Concretely, this meant I read widely and spoke to all the colleagues I could reach in an attempt to solve the problems (a)-(d) described in my letter to Rapoport. I did visit Wuppertal in January, and explained my ideas at length to Stuhler as well as Rapoport.[2] I accepted an invitation to Strasbourg a few weeks later, and although my lecture was on another topic, most of my conversations were again about the ideas of my dream. Carayol was in Strasbourg, as was Boutot, and their papers on the subject were my main source of inspiration. Carayol's Ann Arbor lectures clearly made the connection between his conjecture and Shimura varieties, and his article with Boutot was the main reference for Drinfeld's Ω other than Drinfeld's original and ferociously difficult short note. I spent half my time looking back and forth between Genestier's thesis, which had developed a new way to calculate with the coverings of Ω, and and the Boutot-Carayol article, unable to apply Genestier's methods to the problems (a) and (b), especially the latter, but hoping that inspiration would strike. The other half of my time I spent catching up on fifteen years of work on the trace formula.

In the spring I was teaching an undergraduate course at Orsay and sharing an office with Luc Illusie, who had been Laumon's thesis adviser and, by that token, Genestier's mathematical grandfather. Early in May Luc arrived at the office one morning and announced, in English, "You're cooked!" He showed me a message he had received from a colleague in California named Ogus: the German algebraic geometer Gerd Faltings, one of the most overpowering mathematicians of his generation, had just given a lecture in Berkeley on the cohomology of Drinfeld's coverings of Ω based on an approach apparently very similar to mine, but had claimed much more than I could dream of proving. "You and Genestier are both cooked!" I entertained hopes that there had been a misunderstanding until notes

[2]Most of my time with Rapoport in Wuppertal was spent trying to understand the Gross-Zagier formula in the framework of his book with Zink. On this we made no progress whatsoever, but Rapoport continued to think about the question. A few months later he began a collaboration with Steve Kudla, who had been developing a much more detailed and flexible framework for understanding the Gross-Zagier formula in a very general setting. This collaboration continues to this day.

taken at Faltings' lecture arrived in Paris, including a calculation roughly equivalent to (c) and (d) of my message to Rapoport, and – much more importantly, to my mind – the announcement of a proof of a version of (b) at the level of group characters, especially that the character of the cohomology of coverings of Ω on the Euler characteristic was concentrated on elliptic elements. Question (a) was left as a conjecture except in the case of $GL(2)$. The details appeared a few weeks later when Ogus mailed photocopies of his notes, most notably: Faltings' effective use of the Berkovich cohomology theory, in which he was able to make sense of the Lefschetz trace formula; his ingenious (partial) solution of problem (b); and his very difficult solution to problem (a) in the first non-trivial case, when $n = 2$.

In the meantime I had written to Carayol. In my files my message is dated May 12, 1993. The original was in French, but it loses nothing by in translation to English:

Henri–

It seems that Ogus is at the origin of a rumor according to which Faltings has proved "Drinfeld's conjecture" on the cohomology of coverings of the non-archimedean half-plane. You must be aware of this. No one has seen Ogus' notes, so we don't know what this is about. I can hardly imagine he has proved the local Langlands conjecture.

If by "Drinfeld's conjecture" one means the conjecture that all discrete series representations can be realized in the cohomology of the coverings, without specifying the multiplicity, nor possible non-discrete components, then the claim seems strange to me, because I had the impression that you had given a more or less complete argument in your Ann Arbor talk. In any case, I think I can complete your argument for supercuspidal representations, using the results of Kottwitz and Clozel on twisted unitary group, since the spectral sequence on page 30 of your article degenerates for supercuspidals. Moreover, by using a (global) argument of Taylor, one obtains a Galois representation of the right dimention in the cohomology of the Shimura variety (but independent of the global realization; more precisely, its restriction to the decomposition group at p depends only on the original supercuspidal representation). But I suppose you already knew how to do this. Nevertheless, if Faltings is really in the process of proving the conjecture (by local methods, perhaps), it would be useful to make the global proof public (for example, the people at Orsay don't know it). ...

There follows a final technical paragraph in which I sketch my approach to the "global proof". In the original message I wrote "local" instead of global, but this is obviously a slip, which has here been corrected.

Rediscovering this message, I find myself a little surprised by the timing. I had thought the conversation with Illusie had taken place in March, and that the ideas described in my message to Carayol were developed in the two intervening months, as a way of channeling the disappointment at learning the news about Faltings. My message to Rapoport described what I hoped would be a completely new research project to occupy me for five years or more; a chance to give "Mr. Coherent Cohomology" a rest. Apparently the reality was quite different. Between March and May I had convinced myself of the possibility of a global argument. But my dream's appeal lay precisely in the absence of global techniques. Devotion to an ideal of methodological purity led me to prefer a purely local approach to a problem that was itself purely local; and the problem offered what looked like the prospect of a five-year vacation from Shimura varieties.[3]

The news about Faltings put an end to these daydreams, and I reluctantly resolved to save what could be saved from the six-month apprenticeship. A few weeks later, Carayol came to Paris for a day or so, to see his dentist if I remember correctly. In the interim I had described my global approach to Henniart, who quickly showed that my results implied that the cohomology of the coverings of Drinfeld's Ω gave a canonical "numerical correspondence"[4] I met Carayol at a café on the Place d'Italie and explained how the global argument to which I had referred in my message led to a natural candidate for the local Langlands correspondence. He had predicted as much in his Ann Arbor talk, but at the café he denied he had thought through the consequences of the global argument.

The story lasted another eight years, and in a significant sense it has not yet ended. The ideas I worked out with considerable help from my French colleagues that spring (and many others later, not only French) were finally published four years later, as "an elaboration of Carayol's program" [H]. Several articles, several ideas[5], and several years later, Taylor and I wrote a book containing, among other results, the first proof of the local Langlands conjecture for p-adic fields. But the one we solved was only one of the many local conjectures, those formulated by Langlands himself and those proposed by analogy, and there are currently several active branches of number theory that derive in part from the ideas I first encountered in my dream in 1992.

Just over midway through my career to date, that dream set in motion developments that changed my life in more ways than I care to name. But I have recorded this story because I want to understand its uncanny side, and this particular incident is more uncanny, I believe, than the typical intervention of the unconscious in science. The literature on the role of the unconscious in creativity contains many striking examples of dreams providing solutions that had long resisted the persistent efforts of scientists' conscious minds. I know of no other example of a dream providing a strategy to solve a problem that had never previously laid serious claim to the dreamer's attention. The dream did help me solve what I may well have felt to be my most pressing scientific problem, escaping the role of "Mr. Coherent Cohomology."

[3]In reality, the local and global approaches were not so different. The global approach was based on the Arthur-Selberg trace formula rather than the Lefschetz trace formula, but it is well known that the two are closely related and in many cases are different ways of saying the same thing. As I had already hinted in my December message to Rapoport, one of the principal sources of my intuition in this unfamiliar subject was Wilfried Schmid's work, some twenty years earlier, on the representation theory of real Lie groups. During the period described in this essay I was quite conscious that Schmid had made use of global as well as local trace formulas, perfectly analogous to the ones to which I allude here, in his articles of the 1970s. More recently, he had discovered a new geometric approach to the local trace formula in joint work with my Brandeis colleague Kari Vilonen that I cited explicitly in the first paper written on the basis of my dream. More about Schmid below.

[4]This refers to Henniart's counting argument, mentioned in the previous footnote.

[5]Not only our own: an observation made by Pascal Boyer in his thesis was crucial, as was the constant support of Vladimir Berkovich, already mentioned above.

But that hardly suffices to explain the dream's specific content.

Not being inclined to seek supernatural explanations for life-altering events, I have been wondering for years how that marvelous idea found its way into my dreams, and stayed long enough for me to remember it. Just recently I have begun to piece together a tentative explanation. My theory does not show me in a particularly flattering light, but it is highly plausible. I suspect the unconscious drive behind my dream was, in a word, jealousy. Long-forgotten jealousy, more precisely, directed at one person with whom I overlapped only briefly and a second who was a total stranger during the period in which the jealousy was experienced.

As a graduate student I might not even have become aware of the local Langlands conjecture, were it not for the fact that Jerry Tunnell's Harvard thesis containing the first proof of the conjecture in a non-trivial setting was written the same year as my Harvard thesis on a totally unrelated subject. Tunnell's thesis overshadowed all others that year, and being naturally competitive I suppose that must have made me uncomfortable. When I occasionally made use of Tunnell's work in subsequent years I don't remember any conscious residue of the jealousy of my last year as a Harvard graduate student. But it's certain that the incident remained as an unconscious memory, and it's plausible that it was triggered by the allusions to the local Langlands conjecture in my lunchtime conversation with Rapoport.

Even earlier, as an ambitious undergraduate math major in Princeton, I had been exposed to the local folklore of Princeton undergraduates who had realized their ambitions. The star shining on the distant horizon was John Milnor, the 1962 Fields Medalist who at the time was a professor at the nearby Institute for Advanced Study but whose Princeton senior thesis was still being quoted by knot theorists. A more recent landmark was the undergraduate career of Wilfried Schmid. I no longer remember the stories told about him, but I was aware that, only a few years past his Ph. D. he was already recognized as a leader in two fields. While writing my own senior thesis I looked up Schmid's in the Princeton archives, although the subjects had no relation whatsoever. Even earlier, I had attempted to read his article entitled *On a conjecture of Langlands* [S]. This was the same Langlands but the conjecture was not the one that started to interest me in 1992 but rather the one I mentioned earlier as an analogue of the conjecture in Carayol's Ann Arbor article.

As an undergraduate I was not able to make much sense of Schmid's article, but ten years later I studied it very carefully when I started working on coherent cohomology. I have already mentioned that Schmid's article made explicit use of local as well as global trace formulas, and if the strategy outlined in my dream struck me immediately as believable it was precisely because of the analogy with Schmid's work, specifically with the article I just mentioned. I can be even more specific. My five-year plan was to find a p-adic analogue of Schmid's calculation of the distribution characters of discrete series of real Lie groups as "Lefschetz numbers of certain complexes" (p. 10 of [S]). This is in an obvious sense absurd, because Schmid's calculation is apparently dependent on the use of Hilbert space methods, and there is nothing of the sort available in the setting of the Berkovich cohomology theory. But my letter to Rapoport already invokes "Zuckerman's algebraic version of Schmid"

– meaning Schmid's Ph.D. thesis as well as the article [S] – and I had strong hopes that I could define an algebraic version of the Berkovich cohomology of the Drinfeld coverings to which Schmid's methods could be applied.[6]

Psychoanalytic dream interpretation is based as much, I understand, on the dreamer's subsequent associations as on the content of the dream itself. It seems reasonable to conclude that my dream was motivated in part by an entirely unconscious but deeply buried wish to write an article like [S], about a conjecture of Langlands. It's hard to deny that, from a conscious point of view, publication of [H] represented a satisfying epilogue to the story that began with my dream in December 1992, but it is strange to imagine that my unconscious mind had begun preparing this outcome more than twenty years earlier.

Bibliography

[H] M. Harris, Supercuspidal representations in the cohomology of Drinfel'd upper half spaces; elaboration of Carayol's program, *Inventiones Math.*, **129** (1997) 75-119.

[DAPTS] M. Harris, Do androids prove theorems in their sleep? Article to appear in A. Doxiadis and B. Mazur, eds., *Mathematics and Narrative.*

[S] W. Schmid, On a conjecture of Langlands, *Annals of Math.*, **93** (1971) 1-42.

[6] I still suspect this is possible, and in a more general setting. But I have no idea how to carry this out.