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MSRI Summer School, Harder project.

I guess one can formulate p-adic Langlands conjectures in some cases, and even prove them sometimes. This project aims to guide you through the formulation and proof of such conjectures in the case of a general torus. There are still some loose ends in my understanding of the details, so I reserve the right to update this pdf as the summer school progresses.

Let T be a torus defined over a number field K. The torus is not necessarily split or anything like that – it is just the torus associated to some random finite free $\mathbf{Z}$-module with a random action of G_K. Here is a statement which is surely "known to the experts": if π is an algebraic automorphic representation for T, then there's an associated p-adic Galois representation taking values in the $\overline{\mathbf{Q}}_p$-points of the L-group of T. Sounds plausible, right? This is not in the literature as far as I know. An analogous statement attaching representations of the global Langlands group to not-necessarily-algebraic automorphic representations was proved by Langlands a long time ago in "Representations of abelian algebraic groups", and this remains the only paper of Langlands I have ever made it through.

In fact, the statement above about p-adic Galois representations is a consequence of a more general statement that given a p-adic automorphic representation for T there's an associated p-adic Galois representation. For this to make sense we need a definition of a p-adic automorphic representation of T, so here it is: if T is a torus over a number field K then a p-adic automorphic representation of T is a continuous group homomorphism $T(\mathbf{A}_K)/T(K) \to \overline{\mathbf{Q}}_p^\times$. The proof of this involves taking Langlands' paper and attempting to change all the $\mathbf{C}$'s to $\overline{\mathbf{Q}}_p$'s. Of course some are easily changed, and others need more thought. I once got a Masters student to try and write up the proof, and they did it, but they never published it. Perhaps someone wants to attempt to summarise the write-up, which I've put on the website. I've also put the notes I wrote when I read Langlands' paper, or at least the local part of the paper, online on the course web pages.

When trying to formulate p-adic versions of Langlands' philosophy, one runs into an additional bonus which one does not really see classically. Langlands' conjectures relate automorphic representations to representations of Galois groups. Now Galois groups are profinite, and complex representations of Galois groups are hence rather rigid (in the sense that it's hard to deform them in interesting ways). However p-adic Galois representations deform much more easily, presumably because the p-adic integers are also profinite. Do p-adic automorphic representation deform just as easily? Indeed they often do – and this is the theory of eigenvarieties. It's very easy to build an eigenvariety for p-adic automorphic representations of a torus, because this fancy-sounding statement just boils down to the assertion that the 1-dimensional representations of an abelian group, under some finiteness assumptions, can be given a geometric structure.

Once one has these geometric objects, representing p-adic automorphic representations of T and p-adic Galois representations into the L-group of T, one could attempt to write down a map between them. The associated map on coordinate rings is often referred to as a map from an "R" to a "T", and in some cases one can prove that this map is an isomorphism – this would be called an "$R = T$ theorem".

Here's an interesting twist though! If T is a random torus then a global Galois representation is *not* determined by its local behaviour at all places! This visible "contradiction to Cebotarev density" is explained by the observation that if G and H are groups (the point being that H might not be $\mathrm{GL}_n(\mathbf{C})$ and ρ_1, $\rho_2 : G \to H$ are group homomorphisms, then it is *not* true that local conjugacy implies global conjugacy. More precisely, it could be the case that for all $g \in G$, $\rho_1(g)$ and $\rho_2(g)$ are conjugate, and yet ρ_1 and ρ_2 are not conjugate (so what is happening is that the conjugating element M such that $M\rho_1(g)M^{-1} = \rho_2(g)$ depends essentially on g). This can even happen for H the L-group of a torus. This is a phenomenon which is distinct from failure of multiplicity one for automorphic representations – it is some global Tate-Shaferevich group thing. The Langlands people have known about this for decades – they put an equivalence relation on global "parameters" (which one can think of as representations of Galois groups) and the relation is "everywhere locally isomorphic". The Langlands correspondence is between automorphic representations and parameters modulo equivalence (and then there's another issue involving L-packets).

Here's a consequence. It seems to me that for kooky groups like a random torus, standard "$R = T$" theorems will not in some sense hold – they need to be modified a little. Notational note: unfortunately for the rest of this note, "T" will now refer to a Hecke algebra rather than the torus. In fact, in stark contrast to GL_n, there isn't even a map from R to T in this setting, because an automorphic representation gives rise not to a Galois representation but to an equivalence class of Galois representations, so actually there is a map from a T to an R here.

What is going on here, it seems to me, is that R is really parametrising representations of the Galois group up to global isomorphism, and T is parametrising Hecke eigenvalues and in particular p-adic automorphic representations up to everywhere local isomorphism. In particular there should be some sort of "R eigenvariety" E_R (parametrising Galois reps) and a "T eigenvariety" E_T (parametrising p-adic automorphic reps) and there's a map E_R to E_T which is finite etale and a torsor for this Tate-Schaferevich group. In particular, E_R and E_T are etale locally isomorphic, but not globally isomorphic.

An explicit example of this phenomenon where the Sha was non-trivial was worked out for me by Lenstra and de Smit in an email sent to e in January 2008; I finish by appending Lenstra's email.

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KB said:

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...if G is a connected reductive group
over a global field F and pi is an automorphic representation for G
(whatever that is) then there should be a representation of some kind of
huge "global Langlands group of F" into the L-group of G. However, on
reading Langlands' paper, I see that Langlands actually constructs a map
the other way: given a representation rho of a certain global Weil group,
Langlands constructs an automorphic representation pi, and furthermore he
asserts that his dictionary from rho's to pi's is a surjection with finite
fibres. He does not assert that the fibres have size 1 and indeed does
not, it seems to me, make any comment about the size of the fibres,
although my understanding is that the fibres all have the same size; each
pi is associated to N rho's, where N only depends on G and F. Let me
choose a pi. I get N rho's and these rho's are global representations
which locally will be isomorphic but which globally will not be. If N>1
then this almost sounds like a counterexample to the Cebotarev density
principle, but I am not so sure that it is. I am wondering whether
N=1 always. That is a very wordy way of asking the question---what I am
implicitly saying is "did you read this paper, and if so then did you
resolve this issue?". I could have asked the question in a completely
different way: I could have set up notation and asked a
precise mathematical question about representations of Weil groups, but I
was too lazy. Can you shed any light on this, and if not then do you want
me to ask the concrete question about representations of Weil groups?
My feeling is that this is not the sort of question that should be worked
on, this is the sort of question where you should just find the right
person who knows the answer already.

Kevin

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[second email from KB]

I took the trouble to translate Q2 into lower-level mathematics. Here is a case of what it says. By a "complex torus" I mean a group isomorphic to (C^*)^n for some n, and by an action of a finite group G on a complex torus I mean G acts on the left via algebraic automorphisms, that is, the action of G is induced from a right action of G on the character group of the torus (the associated finite free Z-module).

One instance of the question Langlands leaves open is: does there exist a finite group G acting on a complex torus T, such that the kernel of the product of the maps H^1(G,T)-->H^1(C,T) is non-zero, as C runs through all the cyclic subgroups of G? That is: can there be a "global 1-cycle" that is "locally trivial" but not "globally trivial"? (thinking of G as a global Galois group and C as running through the decomposition groups).

Kevin

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[reply from Lenstra:]

...but your more elementary version I could do myself. That is, I just walked into Bart de Smit (to whom cc)'s office and then the answer came by itself.
The answer is YES. To construct G and T, the strategy is (1) do it first for H^2, and (2) do a sensible dimension-shift.
I take G = V4. First take the one-dimensional torus U, acted upon trivially by G. With Q the quaternion group of order 8, we have an exact sequence 1 -> {1,-1} -> Q -> G -> 1. Also there is an inclusion {1,-1} -> U, and the push-out with {1,-1} -> Q, say E, fits into an exact sequence 1 -> U -> E -> G -> 1, representing an element of H^2(G,U). Since E is non-abelian, this element is non-trivial. But restricting it to any cyclic subgroup C of G gives an abelian subgroup of E, making the sequence split as U is divisible. So much for (1).
For (2), there is an exact sequence 0 -> Z -> Z[G] -> M -> 0 of G-modules (with G acting trivially on Z), with M Z-free of rank 3. The sequence is Z-split, so it remains exact upon tensoring with U over Z. That gives an exact sequence 1 -> U -> U tensor Z[G] -> T -> 1 of tori acted upon by G, where T is of rank 3 and U tensor Z[G] is induced and hence cohomologically trivial. So all H^1(..,T) are H^2(..,U), including the restriction maps. Done.
I hope this survives your scrutiny.
Happy New Year!!
Hendrik

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[email from Buzzard to Lenstra]

> I hope this survives your scrutiny.

It does---and more: I even explicitly wrote down a 1-cocycle that was not
a 1-coboundary and which was in the kernel of all the restriction
maps. I was close to finding this example myself! I had G the group V4
and I had a 3-dimensional torus, but it was the subgroup of (C^*)^4
consisting of (a,b,c,d) with abcd=1; you used the quotient. My
attempt doesn't work but yours does. Thank you both very much.

So this is what you guys have done---you've given an explicit example
of a phenomenon that was known to Langlands 30 years ago but about
which he said nothing in the relevant paper. You've given a very
interesting concrete obstruction to a "natural" formulation
of a global Langlands conjecture.

Let me start at the beginning. Almost from my mathematical birth I have
been told that one could associate a 2-d Galois representation to
a cuspidal modular eigenform, and that this was "a generalisation
of Class Field Theory". Starting from the mid-1980s more general
statements of this nature have been proved and/or conjectured.
Carayol and then Taylor constructed Galois representations attached
to holomorphic Hilbert modular forms. Clozel made a conjecture
in 1990 or so about the existence of n-dimensional Galois representations
attached to
certain automorphic representations of GL_n over a number field.
Gross also made conjectures about Galois representations attached
to automorphic representations for a connected reductive group G
which was "compact at infinity". Clozel attached Galois representations
to certain self-dual automorphic representations for GL_n.
Weissauer and Laumon attached
Galois representations to automorphic representations for Sp_4
over Q. Taylor did something for GL_2 over imaginary quadratic field.
In this century Clozel, Harris and Taylor
constructed Galois representations attached to certain automorphic
representations of unitary groups. There were other results too.
I vaguely knew all of this
stuff existed but had never tried to fathom it out properly.

So about a year ago I decided it was time to fathom it out properly,
and so I picked up these Corvallis procedings and tried to work out
what a reductive group was and what Langlands philosophy was, and
it's really something quite different to the above. I found that the first rule
of Langlands Reciprocity Conjectures is that no-one talks about
Langlands Reciprocity Conjectures. You get survey articles
that stick to GL_n, and vague pictures of how it all works, but
no-one really ever sticks their neck out without saying "now
assume G is split and semisimple" or "now assume G is GL_n" or something.
At least there's a conjecture about L-functions, that Borel
explains in his Corvallis paper---this is one of the papers
in Corvallis that is worth reading, at least if you can get over the
fact that Borel doesn't really know what a scheme is, so for
him an algebraic group over Q is an algebraic group over Q-bar

with a "Q-structure". It turns out that if G is a connected
reductive algebraic group over Q (for example GL_n or SL_n
or Sp_n or a split torus or a non-split torus or...) then
there's something called an automorphic representation of G,
which is a typically infinite-dimensional representation of G(adeles).
There's also something called the L-group of G, which is
a semidirect product of a complex Lie group by a finite group.
The way L-functions are supposed to work is that given an
automorphic representation of G, and a representation of the L-group
of G, i.e. an algebraic (or holomorphic) map from the L-group of G
to GL_N(C), you can attempt to define a function of a complex variable s
via an Euler product and it's a deep theorem of Langlands that the attempt
succeeds in the sense that the infinite product converges for Re(s)
sufficiently large. One can conjecture that this function has
a meromorphic continuation and a functional equation. So there
at least is a well-formed conjecture, but it's analytic and
not really to my taste. It's also wide-open, even for GL_2---the problem
is that the L-group of GL_2 is GL_2(C) but if you don't choose
the obvious two-dimensional representation, you choose its 12th
symmetric power, then no-one has analytically continued the L-function
yet. In fact I think that what Taylor did was managed to get it
to Re(s)>=1 or something and this was enough for Sato-Tate.

But the real "meat" is these Langlands functoriality conjectures, which turn
out, as far as I can see, to be quite vaguely formulated. My
student Toby Gee met Langlands recently and he actually tried
to get him to formulate a meaningful conjecture, and Langlands made
some noises about how some attempts were obviously going to
be true and very deep, but you had to be careful because some other
attempts were going to be obviously false, and he completely took
Toby in with his profound observations on how things work, and Toby left
the conversation and only realised 5 minutes
later when Langlands had gone that he'd dodged the question.

So the big question is: can one give an explicit conjecture
relating automorphic representations to Galois representations?
Because this is what people are proving. And this turns out to
be tricky. There are two kinds of conjectures one can try to make.
Let F be a number field and G a connected reductive group over F,
and let pi be an automorphic representation of G over F,
that is, a huge representation of G(adeles of F) satisfying a big
list of properties. Firstly one can conjecture that associated to pi
there is a map from the "Galois group of F" into the L-group of G.
Well, no, this isn't right because this doesn't even work for GL_1
over Q; an automorphic representation of GL_1 over Q is just
an idele class character Q^* \ A_Q^* --> C^* and the left hand
side isn't Gal(Q-bar/Q), it's something bigger---it has a real
analytic component. So you have to perhaps change "Galois group"
to "Weil group of F". But then even this isn't right because
locally the story is understood for GL_n and there, even for GL_2, you have to
use the Weil-Deligne group (scheme), and there is, as far as I know,
no global Weil-Deligne group attached to a global field.
So now people launch into some kind of "Tannakian formalism"
and talk about the "Langlands group of F" or whatever---but

this is a complete disaster; one can't apply the Tannakian formalism to anything because a Tannakian category is a category with a whole bunch of axioms and I would say that the current state of the art has managed to prove about 5 percent of these axioms so far, for the "category" of "all automorphic representations". It's just a nonsense. Even if one did have a definition of a global Langlands group, Langlands' philosophy would seem to say that associated to a modular form there is a representation of this group to GL_2(C), not to GL_2(Q_p-bar). So I'll give up on this strand until someone tells me what a global Weil-Deligne group is.

But on the other hand there *are* theorems and conjectures attaching p-adic Galois representations to certain automorphic representations. And here I really do mean the absolute Galois group of the number field in question. So this is the second idea: Galois groups have meaning, so let's try and formulate a conjecture involving Galois groups. Well, one looks in the literature for at least a concrete conjecture. For the same reason as above (GL_1) one now has to throw away some automorphic representations---those that really don't correspond to Galois representations. I should say that any conjecture of this form would not be one approved of by Langlands---Langlands sees no reason to "cut down the space of automorphic representations" to the meagre subset of those representations that might be related to Galois groups---recall that there are plenty of automorphic representations of GL_2 corresponding to non-holomorphic (Maass) forms and the non-Euclidean Laplacian might have a transcendental eigenvalue on such a form, and if this is the case then it seems like all the Hecke eigenvalues are random complex numbers which aren't even algebraic (I wrote down an explicit example of such a thing, in fact), so there is no chance of a p-adic Galois representation. Langlands thinks that these forms are just as interesting as holomorphic cusp forms. Well I don't, and I want a proper conjecture that makes sense.

So about a year ago Toby Gee and I embarked on a program to formulate a meaningful and general conjecture of the following form: Let p be a prime number. Let G be a connected reductive group over a number field F. Let pi be an automorphic representation of G. Assume that pi is "algebraic" in a sense that can be made precise. Then there should be a representation rho:Gal(F-bar/F)-->L(Q_p-bar), where L is the L-group of G but interpreted as an algebraic group over Q (this can be done), so its Q_p-bar-points made sense. Furthermore, at every prime q not p at which pi is unramified, I want rho to be unramified at q and I want to be able to explicitly say what rho(Frob_q) is. The idea is that we want to formulate a general conjecture that implies all known theorems and conjectures.

This seemed to me at the time to be an eminently reasonable thing to do and to be honest it rather shocked me that one couldn't find such a statement in the literature. Clozel makes such a conjecture when G is GL_n. Gross makes such a conjecture under some other restrictive hypotheses. But what is the general conjecture that implies all these other conjectures? Toby and I nearly have it. The other day I embarked on one part of our programme: I started to check our conjecture for G a torus, because in this case it should of course follow from class field theory.

And the funny thing is that it does seem to follow from class field theory, but in fact rho is not well-defined in general :-/ One can give an explicit example of a G and a pi where there is more than one rho with the required properties. This surprised me! But you've shown that it can happen. I'm sure this was well-known but remember the first rule of Langlands Reciprocity Conjectures; you have to discover these things yourself, and then you must be careful not to let anyone else know about them. Let F be the field Q of rational numbers. Let K be the field Q(sqrt(13),sqrt(17)). There's a norm map K^*-->Q^* and this is the Q-points of a map of tori Res_{K/Q}(GL_1)-->GL_1 over Q. Let G be the kernel of this map of algebraic groups. Let pi be the trivial 1-dimensional representation of G. Then pi is an automorphic representation for G. The candidates for a Galois representation attached to pi can be interpreted as elements of a cohomology group H^1(Gal(K/Q),the complex torus you wrote down), and my assertion that I know the representation everywhere locally can be interpreted as conditions on the restriction of this cocycle to Gal(K_w/Q_v) for w a place of K lying above the place v of Q. But all decomposition groups in Gal(K/Q) are cyclic! So knowing the representation everywhere locally is not enough to determine it globally. This is yet another spanner in the works and believe me I've already seen a few. **But I go on, undeterred!**

Kevin