Notes of Panel discussion on Open problems, Gross Conference, June 2010

Panelists were:

- Jordan Ellenberg
- David Kazhdan
- Barry Mazur
- Sophie Morel
- Fernando Rodriguez-Villegas

Gross 2010 Conference, Harvard University, June 3, 2010.
$http://www.math.harvard.edu/conferences/gross\_10$

1. Jordan Ellenberg

I'm not going to attempt to present what I view as the deepest or most important questions arising from Dick's work (such as how to construct rational points on elliptic curves when the L-function of a curve vanishes to order > 1). Instead, I'm going to talk about something related to Dick's work that I am particularly interested in and that seem ripe for investigation.

My topic is structures on integral orbits. Take a variety X with an action of a reductive group G, which we assume to have a dense orbit. Suppose these things have models over $\mathbb{Z}$. What can you say about the orbits of $G(\mathbb{Z})$ on $X(\mathbb{Z})$ (i.e. the set $X(\mathbb{Z})/G(\mathbb{Z})$)? We're just beginning to see that there are often some interesting non-trivial structures on this set. This has been studied in the recent work of Manjul Bhargava, but even before that in the work of Gross and Gan.

For example, Gauss studied the case of X the set of binary quadratic forms of discriminant d, with $G = SL_2$. In this case you have for example the structure of Gauss' composition law.

Another example: take V a 2-dimensional space, $X = \mathrm{Sym}^3 V$ (this is a 4-dimensional space which can be viewed as the space of binary cubic forms). $SL(V)$ acts on X_d, which is the subspace of forms of discriminant d and the orbits are isomorphism classes of binary cubic forms of discriminant d. You see again a group law, corresponding to the identification of $SL(V)\backslash X_d$ (OR: of the orbits) with the 3-part of the class group of a quadratic imaginary field.

Take now V to be a 3-dimensional space, $X = \mathrm{Sym}^2 V^* \otimes \wedge^3 V$, $G = GL(V)$. The theorem of a recent paper of Gross and Lucianovic (generalized by Voight) is that $X(\mathbb{Z})/G(\mathbb{Z})$ parameterizes quaternion rings over $\mathbb{Z}$. This whole space doesn't have a group law but there is a group law on the closely related set of Azumaya algebras.

JSE's question: understand what is producing all these group structures. One thing you can say: an orbit corresponds to a torsor for the stabilizer of a given element, so you can relate $G(\mathbb{Z})\backslash X(\mathbb{Z})$ to $H^1(\mathrm{Spec}\,\mathbb{Z}, H)$, where H is the stabilizer of an element. When the stabilizer is abelian you tend to get group laws. In the final case above, however, the stabilizer is a form of SO_3.

So: are there more interesting cases with abelian H? Are there any interesting cases where the stabilizer is not abelian but has a natural abelian finite cover? (Thanks to Manjul Bhargava we now understand the case when X is a prehomogeneous vector space.)

2. David Kazhdan

The first thing I would like to discuss is a basic question related to the formulation of Langlands' conjectures. I feel some essential piece is missing in the formulation.

Consider automorphic representations $V \subset L^2(G_{\mathbb{A}}/G_F)$ giving rise to Galois representations $\rho : \mathrm{Gal}(\overline{F}/F) \to {}^L G$. V can be written as a tensor product of local representations V_p. On the Galois side, we think about the set of Frobenius elements $\rho(\mathrm{Frob}_p)$ in G^L. This is a set of conjugacy classes of elements. For classical groups such as GL_n this is fine, since it is enough to give knowledge of the representation ρ.

On other groups (such as SL_n) there is a discrepancy here. Fixing the conjugacy classes, there can be multiple representations $\rho_i, i \in I$ which give rise to the same local data. These give rise to an automorphic representation (π, V) which is determined (abstractly) by the local data; and we can think about $\dim \mathrm{Hom}_{G_{\mathbb{A}}}(V, L^2(G_{\mathbb{A}}/G_F))$. It should be equal to $|I|$.

This is not enough: fixing ρ_i, one should be able to pick out a particular homomorphism in this space. In the geometric Langlands program the analogue of this statement does hold, since you can pick out a particular perverse sheaf, but we're missing this in number theory.

The second thing I would like to discuss is perhaps less important. The local basis of Langlands' conjectures has two components. The first is the Satake isomorphism; the other is the Gindikin-Karpelevich formula, which explains where the L-function comes from. One can ask: is there an extension of Langlands' automorphic forms for affine Kac-Moody groups?

Langlands' theory is one-dimensional: it deals with one-dimensional global fields. It would be nice to have two-dimensional analogues. One can think of one-dimensional Kac-Moody groups as "one and a half"-dimensional analogues.

Does there exist an interesting theory of automorphic forms for Kac-Moody groups?

3. Barry Mazur

The mood of the question asking is general structures. One could also ask some specific, smaller problems. I think they're focuses in the sense that the ones I want to talk about are magnets for other types of questions.

Theorem 1. *[Heath Brown] Consider the family*

$$E_d : dy^2 = x^3 - x.$$

Let d be odd and square-free. Consider the function $d \mapsto$ rank $E_d(\mathbb{Z}) + \dim_{\mathbb{F}_2} Ш[2] = s_d$. Consider the density

$$D(s) = \lim_{x \to \infty} \frac{\#\{|d| \le X | s_d = s\}}{\#\{|d| \le X\}}$$

The theorem states that $D(s) = 2^s / \prod_{i \le s}(2^j - 1) \cdot \alpha$ for some normalizing constant α independent of s.

Swinnerton-Dyer gave a similar theorem, ordering the curves in a different (and slightly odd) way. He gave essentially the same answer for the density. This theorem is the result of a Markov process.

One can wonder how this interacts with Manjul Barghava's work, which gives more precise answers for ever more precise questions.

Thus: replacing E_1 with a general abelian variety A and $\mathbb{Q}$ with a general number field K, what answers do we get for similar questions?

Second problem: this is related to Serre's lecture earlier today. One takes an elliptic curve $E/\mathbb{Q}$ and considers the numbers $|E(\mathbb{F}_p)|$. Consider the set of primes $p \le X$ such that this number is $> p+1$. Consider also the set of primes with the number $\le X$. The ratios of the orders of these two sets tends to 1 as $X \to \infty$, which makes it interesting to consider the difference $D(X)$, i.e. the Chebyshev bias.

This bias should be related to the rank of the curve. This is one of the things that prompted Birch and Swinnerton-Dyer to make their conjecture. What happens if one takes the average of the function $D(X)$ as $X \to \infty$? If one assumes various conjectures, including GRH, one can compute this average in terms of the orders of vanishing of the L-functions of all of the symmetric powers of the modular form associated to the elliptic curve.

A natural minimalist conjecture is that except for finitely many symmetric powers, this order of vanishing is 0 or 1, corresponding to the sign of the functional equation (in fact for Δ, there is no known symmetric power for which there is a higher order of vanishing).

4. Sophie Morel

I have to apologize because my open problem is not really open. I want to talk about functoriality, staying on the automorphic side, namely in a case that follows simply from Lafforgue's work.

Let $F = k(X)$ be a function field, and n a positive integer. Let E/F be a separable everywhere unramified extension of degree n. We want to discuss functoriality between $G = E^\times$ and $H = GL_n$. Let

$$\mathcal{H}_G = C_c^\infty(K_G \backslash G(\mathbb{A}_F)/K_G),$$

where $K_G = \hat{\mathcal{O}}_E^\times$, and

$$\mathcal{H}_H = C_c^\infty(K_H \backslash H_(\mathbb{A}_F)/K_H),$$

$K_H = GL_n(\hat{\mathcal{O}}_F)$. These Hecke algebras are commutative.

Let's say that the automorphic representations are the ones appearing in $L^2(G(F)\backslash G(\mathbb{A}_F)/K_G)$, resp. $L^2(H(F)\backslash H(\mathbb{A}_F)/K_H)$.

The special case of functoriality known as automorphic induction says that there should be a map from automorphic representations of $mathcal H_G$ to automorphic representations of $\mathcal{H}_H$, which I am now going to describe locally. We can write $\mathcal{H}_G = \otimes_{x\in|X|}\mathcal{H}_{G,x}, \mathcal{H}_H = \otimes_{x\in|X|}\mathcal{H}_{H,x}$, where the product is over the closed points $x \in |X|$. We want to write the Satake isomorphism very explicitly. Let us write $F_x, E_x = E \otimes F_x$ for the local fields.

Writing T for the diagonal torus in GL_n, Satake says

$$\mathcal{H}_{H,x} \xrightarrow{\sim} C_c^\infty(T(\hat{\mathcal{O}_{F_x}})\backslash T(F_x))^{W(T,GL_n)} \cong \mathbb{C}[Y_1^{\pm 1}, \ldots, Y_n^{\pm 1}]^{\sigma_n}.$$

$$\mathcal{H}_{G,x} \xrightarrow{\sim} C_c^\infty(\mathcal{O}_{E,x}^\times \backslash E_x^\times) \cong \otimes_i \mathbb{C}[X_i^{\pm n_i}],$$

where $E_x \cong E_1 \times \cdots \times E_r$, and $[E_i : F_x] = n_i$.

There's an explicit map $\rho : {}^L G \to {}^L H$ inducing $\rho^* : \mathcal{H}_{H,x} \to \mathcal{H}_{G,x}$,

$$Y_j \mapsto \zeta_{n_i}^k X_i,$$

if $j = n_1 + ... + n_{i-1} + k$ with $1 \leq k < n_i$.

Automorphic induction: start with a character $\chi : \mathbb{A}_E^\times / \hat{\mathcal{O}}_E^\times \to \mathbb{C}^\times$, a local product $\chi = \otimes_x \chi_x : E_x^\times \to \mathbb{C}^\times$.

χ_x gives rise to $\rho_{x,*}(\chi_x) = \rho_x^* \circ \chi_x : \mathcal{H}_{H,x} \to \mathbb{C}^\times$.

Then we can define $\rho_*(\chi) = \otimes_x \rho_{x,*}(\chi_x) : \mathcal{H}_H \to \mathbb{C}^\times$.

Theorem: If χ is automorphic, then so is $\rho_*(\chi)$.

This is proved using heavy machinery from algebraic geometry, a subject that is a priori not connected to the problem. Can one give a simpler (perhaps group-theoretic) proof for this group-theoretic statement?

5. Fernando Rodriguez-Villegas

I'd like to focus on some computational aspects. Cassels said that number theory was an experimental science; I very much subscribe to that.

Some general remarks: I feel that as mathematicians we do not use the available technology as much as possible. This is a plea to use this technology to greater effect.

For example, there was Gowers' communal paper-writing experiment, which was a great success due to the ease of modern communications.

I would like to talk about such communal collaboration and how to organise such things. For example, one thing I use on almost a daily basis is Sloane's wonderful encyclopedia of integer sequences.

My concrete suggestion is to do something along these lines using L-functions. This goes back to ideas Brian Conrey has suggested very often. Part of the problem is how to organise this. The computations of L-functions where the degree of the L-factor is bigger than 2 is so small compared to where the degree is less that or equal to 2, partly due to the lack of energy and dedication. I'd like to propose to greater effort in this direction.

I imagine L-functions as the genetic material of mathematics. So this is like a mathematical genome project.

Partly with this goes the problem of how to do this in an efficient way. How do we co-ordinate the efforts of so many people in different contexts? This seems like a non-trivial problem. You often find for example that things you are doing have been done before. For example, recently I had the experience that someone was looking for points on a given variety. I wrote a program to search for points on this variety and found some points of large height.

I had a visitor who came by and searched for people who had searched for similar points. He quickly found via this meta-search a number of people who had indeed found my points and many more.

I once wanted to do some calculations on L-functions using the p-adic Gamma function (about 8 years ago). I thought I'd go on the web and find some implementations of this function. But there were none! This function had been around for 50 years but nobody had every computed a single value (theoretical computations aside).

There seems to be a lot more that we could do to help people who would find access to such data valuable.

To summarize: my plea is to put more co-ordinated effort into calculations related to L-functions, in a such a way that the data could be used to make connections between different problems.

6. Nicholas Katz

It was recently struck by how little we know about things we think we should know about.

In 1957 Taniyama wrote a paper where the notion of compatible system of l-adic representations was first introduced. We know now that if X is a smooth proper variety over $\mathbb{Q}$, the étale cohomology $H^i(X)$ gives rise to such a family, the Frobenii having integral traces. After Faltings, we can recover the Hodge numbers of the corresponding complex variety. This information is contained in the ρ_l restricted to I_l. In terms of Tate twists, this means you only get Galois invariants after twisting H^i in the 'positive' direction.

So suppose one has a compatible system of l-adic representations, where the Frobenii have integer traces, and suppose even that they are pure of weight i. It's reasonable to imagine that applying B_{HT}, the Hodge-Tate weights are all positive (i.e. there are no Galois invariants after twisting in the negative direction).

Why is this interesting? One application to the étale cohomology of a variety would run as follows. Suppose we knew this, and that for all p sufficiently large, $\mathrm{trace}(\mathrm{Frob}_p|H^i(X))$ is divisible by p. This means that taking the first Tate twist $H^i(X)(1)$, Frob_p still has integer coefficients. Applying the given result, one would obtain that $H^{0,i}$ and $H^{i,0}$ were both zero.

If one takes a Calabi-Yau quintic threefold, trace of Frob_p on H^3 is congruent modulo p to Frob_p acting on $H^3(\mathcal{O})$, where $\mathcal{O}$ is the structure sheaf. Could this 'Hasse invariant' always be zero? The above result in the affirmative would say that this cannot happen.

7. Further discussion

This is a question to Barry Mazur: do you know an example of a modular form of weight > 2 such that the L-function has a double zero at the critical point.

Barry Mazur: I talked about this in Texas a year or two ago. A number of people who had methods for computing tried it out on a number of symmetric powers and nobody found anything other than 0 or 1.

The discussion I gave corresponds only to the non-CM case. I can give you an answer in the CM case.

We haven't made too many computations though. It would be nice to do more computations and/or apply the random-matrix heuristics to going up the symmetric power tower, and to the family of twists of a given form.

Noam Elkies to Barry Mazur: in the particular family of elliptic curves you wrote down, my student Rogers found in 2003 a rank 7 twist, the only one known of rank >6. We never found one of rank >7. This is regarding the actual rank - one can easily make examples where the Selmer group becomes very large.

Matthew Emerton says: suppose ρ_1, ρ_2 are non-isomorphic representations which are the same locally. These give rise to π_1, π_2 which are abstractly isomorphic. Suppose we work on GSp_4, and these contribute to H^3 of a Siegel threefold: $H^3(\pi_1) \oplus H^3(\pi_2) \subseteq H^3(X)$. One question is how the ρ_i appear in this H^3. The H^3 has a Hecke-equivariant symplectic pairing. So one way to think about the question is whether the geometry of the threefold forces the classical group structure on the Galois representations, which would give a way of picking out particular subspaces on the automorphic side.

Another audience member says: in the situation where you have high multiplicities for SL_3, the automorphic representations do become non-isomorphic after moving to GL_3. If one could make the base change explicit in general, this might give a way of understanding Kazhdan's problem.

Notes taken by Ana Caraiani and Jack Thorne.