Bures, 8.16.1964

My dear Serre,

Thank you for the copy of your letter to Ogg, and your letter of the 13th, to whose questions I will now return.

1) "My" proof of the Shimura-Koizumi theorem (which is actually inspired by theirs). Let B be an abelian variety over K (the fraction field of a discrete valuation ring V), which has good reduction, i.e. comes from an abelian scheme $\overline{B}$ over V, and let A be an abelian variety over K which is isomorphic to a quotient (or to an abelian subvariety, which boils down to the same thing) of B, that is $A = B/N$; then A has good reduction. Indeed, let $\overline{N}$ be the scheme-theoretic closure of N in $\overline{B}$, namely the unique closed flat sub-prescheme of B whose general fiber is N (this is where dim 1 is used); then, since $\overline{B}$ is projective over V (another Japanese theorem, using Weil's ampleness criterion, and valid over a regular base), it follows by the theory of passage to the quotient (written up by Gabriel in the SGAD seminar, for example) that $\overline{A} = B/\overline{N}$ is representable by a projective scheme over V, and it is trivial that this is an abelian scheme extending A, qed. Of course, N is not in general smooth over K, i.e. it can have nilpotent elements; moreover, the Japanese did not have a good theory of passage to the quotient, and that is why they are forced to twist and turn every which way (I believe they construct an $\overline{A}$ by generalizing Weil's theorem on the definition of a group by birational data, rather like Mike's SGAD talk).

2) My allusions to "vanishing cycles" were indeed a little vague. To begin with, the only result which appears in the literature (which is probably proved in Igusa's secret papers) is in Igusa's note n the Proceedings (if I remember rightly) which starts with a regular scheme X and a projective morphism $f : X \to Y = \mathrm{Spec}(V)$ whose generic fiber is smooth and geometrically connected of dimension 1, and whose special fiber is geometrically integral and has only one singular point which is an ordinary double point. In this case, the Galois action is given by the Poincaré formula. I think it should be possible to analyse what happens for several double points (and perhaps for more complicated points?) and in higher dimensions, but I have not written up anything on this (it is in my short-term program, but has not yet been done). Hopefully, the information obtained this way will be precise enough to make it possible to prove your conjecture with Tate, in the case of the Jacobian of a curve whose reduction is "not too bad"*. To pass to arbitrary Jacobians, one would need to construct a "not too bad" model for an arbitrary non-singular projective curve over K, after finite extension of K if necessary. In a letter a few weeks ago, Mumford more or less said that given C, it is possible to find a model X whose special fiber has only ordinary singularities (if the genus is ≥ 2); in any case, he has apparently proved this for a residue field of characteristic 0. Once Jacobians are in the bag, the passage to arbitrary abelian varieties raises a question which I have actually already come across elsewhere, and which looks very interesting to me: does every abelian variety (over an algebraically closed field, say, that will suffice) have a "*finite* resolution" by Jacobians, at least up to isogeny? Alternatively, on forming a "K group" from abelian

* N.B. I have not thought about the case where the residue field is finite.

varieties up to isogeny (a free group generated by simple abelian varieties up to isogeny), is the subgroup generated by the Jacobians the whole group? It would actually suffice if this were true up to torsion, to be able to pass from the result for Jacobians to the case of general abelian varieties, if in your conjecture with Tate we settle for rational and not integral traces. In any case, the case of Jacobians would imply that for an arbitrary abelian variety, the traces you have in mind are algebraic integers — but I do not see how to get any further without using an auxiliary result of the kind mentioned above.

3) Moreover, this question is related to the following one, which is probably far out of reach. Let k be a field, which for the sake of argument is algebraically closed, and let $L(k)$ be the "K group" defined by schemes of finite type over k with relations coming from decomposition into pieces (the initial L is of course suggested by the link with L-functions). Let $M(k)$ be the "K group" defined by "motives" over k. I will say that something is a "motive" over k if it looks like the ℓ-adic cohomology group of an algebraic scheme over k, but is considered as being independent of ℓ, with its "integral structure", or let us say for the moment its "$\mathbf{Q}$" structure, coming from the theory of algebraic cycles. The sad truth is that for the moment I do not know how to define the abelian category of motives, even though I am beginning to have a rather precise yoga for this category, let us call it $\mathbf{M}(k)$. For example, for any prime $\ell \neq p$, there is an exact functor T_ℓ from $\mathbf{M}(k)$ into the category of finite-dimensional vector spaces over $\mathbf{Q}_\ell$ on which the pro-group $\mathrm{Gal}(\overline{k_i}/k_i))_i$ acts, where k_i runs over subextensions of finite type of k and $\overline{k_i}$ is the algebraic closure of k_i in $\overline{k}$; this functor is faithful but not, of course, fully faithful. If k is of characteristic 0, there is also a functor T_∞ from $\mathbf{M}(k)$ into the category of finite-dimensional vector spaces over k (this is the "de Rham-Hodge functor", whereas T_ℓ is the "Tate functor"). In any case, taking for granted the two ingredients (Hodge and Künneth) of the Riemann-Weil hypothesis that you know about, I can explicitly construct (and indeed I can do this over more or less any base prescheme, not only over a field) the subcategory of *semi-simple* objects of $\mathbf{M}(k)$ (essentially as direct factors defined by classes of algebraic correspondences of some $H^i(X, \mathbf{Z}_\ell)$, where X is a non-singular projective variety). This is all that is needed to construct the group $M(k)$ (and I think it would be possible to give a description of it that would be independent of the conjectures mentioned above, if one wanted to). Hence, for any ℓ, there is a homomorphism from $M(k)$ to the "K group", namely $M_\ell(k)$, defined by the $\mathbf{Q}_\ell$-G-modules of finite type over $\mathbf{Q}_\ell$, where G is the pro-group defined above, or, if you prefer, the associated pro-Lie algebra (which has the advantage over the group of being a *strict* pro-object, i.e. with surjective transition morphisms). This being said, on taking alternating sums of cohomology with compact support, one obtains a natural homomorphism

$$L(k) \to M(k),$$

which is actually a ring homomorphism (with the Cartesian product on the left and the tensor product on the right). The general question which then arises is what can be said about this homomorphism; is it very far from being bijective? Note that the two sides of this homomophism are equipped with natural filtrations, via dimensions of preschemes, and the homomorphism is compatible with these filtrations. The above question on Jacobians can then be formulated as follows: is $L^{(1)} \to M^{(1)}$ surjective? (Indeed, up to a trivial factor of $\mathbf{Z}$ which comes from dimension 0, $M^{(1)}$ is nothing other than the K group defined by the abelian varieties defined over k).

I will not venture to make any general conjecture on the above homomorphism; I simply hope to arrive at an actual construction of the category of motives via this kind of heuristic considerations, and this seems to me to be an essential part of my "long run program". On the other hand, I have not refrained from making a mass of other conjectures in order to help the yoga take shape; for example, that $M(k) \to M_\ell(k)$ is injective, or more precisely that two simple non-isomorphic (perhaps I should rather say non-isogenous) motives give rise to simple ℓ-adic components which are pairwise distinct. Tate's conjecture can be generalized by saying that for non-singular projective X, the "arithmetic" filtration on the $H^i(X)$ (via the dimension filtration on X) is determined by the filtration on $M(k)$ mentioned above, or alternatively that the filtration on $H^i(X, \mathbf{Z}_\ell)$ is determined by the Galois (or rather pro-Galois) module structure via the corresponding filtration on $M_\ell(k)$. For example, in odd dimension, the maximal filtered part of $H^{2i-1}(X, \mathbf{Z}_\ell(i))$ is also the largest "abelian part", and corresponds to the Tate module of the intermediate Jacobian $J^i(X)$ (defined by the cycles of codimension i on X which are algebraically equivalent to 0).

I should also mention that I do indeed have a construction of such intermediate Jacobians (whose dimension is bounded by $b_{2i-1}/2$ as it should be). Unfortunately, I do not yet even conjecturally understand the link between Hodge-style positivity and the Néron-Tate formula on self-duality of J^i for $\dim X = 2i-1$, and I would like to discuss this with you some day before you leave. For surfaces, one does indeed get a proof of the Hodge index theorem using the Néron and Tate stuff, essentially by reducing the problem to the positivity of the self-duality of the Jacobian of a curve, and I continue to suspect that this principle of proof by reduction to dimension 1 is actually applicable to more general situations.

4) At the bottom of page 8, I think $\mathcal{O}_X$ should read $\mathbf{Z}_\ell$; it is because of this slip that I had the impression that you had forgotten a properness condition!

5) I have no feeling for your question on the variation of the Néron model under unbounded extensions of the base field. You should ask Néron if he knows anything.

6) The editors of the Bulletin are F. Browder, W. Rudin, E.H. Spanier, 190 Hope Street, Providence (Rhode Island).

Regards,
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