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6 August, 1958

Paris, August 6, 1958

Dear Professor Zariski,

I am very sorry to be obliged to tell you about a most silly misadventure which has happened to me: a letter with documents concerning my visa application, sent to the American embassy on July 19, has got lost in the mail, devil knows how. Therefore I would need again two papers which were already sent me by the University: my certificate of eligibility to an exchange program (which, according to instructions of the embassy, I should have in *two* copies); a statement that I will have a sufficient salary paid to cover all costs. I am very sorry to bother you once again with these trivial details, and I am convinced it will be the last time before coming to Harvard at last.

Writing down the theory of schemas, I got what seems to me now the definitive form of your theorem on holomorphic functions<sup>1</sup> proved by the same standard arguments (implying, in particular, a *decreasing* induction on the dimension on cohomology, so that the statement implying  $H^0$  is proved last!) as the general finiteness theorem for proper maps. The statement is as follows: if  $f : X \rightarrow Y$  is a proper morphism of noetherian schemas (think for instance of a proper morphism of algebraic varieties),  $\mathcal{F}$  an algebraic coherent sheaf on  $X$ , and the  $R^n f_*(\mathcal{F})$  the “higher direct images” of  $\mathcal{F}$  by  $f$  (the sections of  $R^n f_*(\mathcal{F})$  on an affine open set  $U$  of  $Y$  being the group  $H^n(f^{-1}(U), \mathcal{F})$ ); if  $y$  is a point of  $Y$  which for convenience of statement we assume closed, and if we consider the sheaf of ideals  $\mathcal{J}$  in  $\mathcal{O}_X$  defined by the fibre  $f^{-1}(y)$ , then (i) the  $R^n f_*(\mathcal{F})$  are coherent sheaves (ii) the completion of  $R^n f_*(\mathcal{F})_y$  for the  $\mathfrak{m}_y$ -adic topology is isomorphic to  $\varprojlim_k H^n(f^{-1}(y), \mathcal{F} \otimes_{\mathcal{O}_X} \mathcal{O}_X/\mathcal{J}^k)$ . Taking for instance  $\mathcal{F} = \mathcal{O}_X$ ,  $n = 0$  we see that  $f_*(\mathcal{O}_X)$  is a coherent sheaf of commutative  $\mathcal{O}_Y$ -algebras and the set of maximal ideals of  $f_*(\mathcal{O}_X)_y$  is in one to one correspondance with the

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<sup>1</sup> Grothendieck eventually published a different proof, of a fancier theorem, in EGA III 4. Something like the simple-minded proof outlined in this letter, of a less general theorem, may be found in section III.11 of R. Hartshorne, *Algebraic geometry*, Grad. Texts in Math. 52, Springer-Verlag 1977.

set of connected components in the fiber  $f^{-1}(y)$ . This gives a reinforcement of your connectedness theorem; namely if  $X, Y$  are both irreducible, their sheaves without nilpotent elements,  $f$  surjective, and the field of  $Y$  quasi-algebraically closed in the field of  $X$ , then  $f_*(\mathcal{O}_X)_y$  is contained in the integral closure of  $\mathcal{O}_y$  in  $k(X)$ ; if we know that  $\mathcal{O}_y$  is “unibranch”, that is if there is only one maximal ideal in the integral closure of  $\mathcal{O}_y$  in its quotient field, then there can be only be one maximal ideal in  $f_*(\mathcal{O}_X)_y$  as well: this gives the connectedness theorem, without analytic irreducibility needed. Besides, using the connectedness theorems and the same standard techniques, one gets as a consequence your “main theorem” for *arbitrary* noetherian rings. So a “local” result is proved by global means.

As an application of the general connectedness theorem, I give the following example (which was given to me as a problem by Serre). If  $\mathcal{O}$  is a noetherian local ring,  $S$  the associated graded ring,  $X$  the projective algebraic set (over the residue field) defined by  $S$ , then  $X$  is connected provided  $\mathcal{O}$  is unibranch.

I have some hope also of solving your two open problems on holomorphic functions by these methods, using perhaps a general duality theorem, which I am now developing, and which holds for arbitrary complete schemas (the singularities do not matter). I will tell more about it in the seminar at Harvard.

Sincerely yours  
(signed) A Grothendieck

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5 October, 1960

Paris Oct 5, 1960

My dear Mumford,

I beg very much your pardon for not having replied to your last letter nor acknowledged receipt of your manuscript. Unfortunately, it is too late now to publish it this very year; besides the referee was prevented from rereading the manuscript, and I will have to give it to another one. I appreciate your effort to give complete proofs and hope that you will let us publish your paper in spring 1961—which is certainly possible if there are no other gaps of importance in the proofs.

I am glad to hear you are interested in the existence theorem for Picard schemata, yet I did not prove it with the generality you believe. I have to assume:

- 1)  $X$  flat and *projective* over  $S$
- 2) The fibers  $f^{-1}(s)$  are “*absolutely irreducible*” and *without embedded primes*
- 3) For every fiber,  $H^0(f^{-1}(s), \mathcal{O}_{f^{-1}(s)}) \xrightarrow{\sim} k(s)$

To go much further will presumably demand a considerably greater effort, which I do not intend to go into myself, but which I expect to be very much worthwhile.

Sincerely yours,

(signed) A Grothendieck

P.S. I will send you a copy of Chapter I of the Elements. For other issues of the “Publications IHÉS” you should write to the publisher, I believe.



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20 April, 1961

A. Grothendieck  
23 Boul. de Levallois  
Neuilly (Seine)

Paris April 20, 1961

Dear Mumford,

I am much interested by a result of yours on passage to the quotient by semi-simple algebraic groups, which Zariski has reported to me. Would it be possible for you to send me an outline of the proof? Even for the group  $PGL(2)$ , or the reductive group  $\mathbb{G}_m$ , I am not able to solve the problem, and I begin to have doubts even if such general results (as yours, which looks very like the conjecture 8.1 in the Bourbaki talk III on construction techniques)<sup>1</sup> really exist. I just found various counterexamples to my conjecture as it was formulated. For instance (as I wrote Tate) even for very standard operations on  $PGL(n)$ , the graph may not be closed and then even a non separated quotient may not exist; thus in 8.1. 1° one has at least to assume a closed graph (as indeed people generally do). Moreover conjecture 8.1. 2° seems hopelessly false, take for instance  $Y = \mathbb{P}^1$ , and  $X$  the principal projective bundle on  $Y$  associated to the standard ample line bundle  $\mathcal{O}_Y(1)$  and the natural homomorphism  $\mathbb{G}_m \rightarrow PGL(2)$ ; it is easy to see that  $X$  is “quasi-affine” i.e., an open subset of an affine space, i.e.  $\mathcal{O}_X$  is ample, yet the sheaf  $\mathcal{O}_Y$  of which it is the inverse image is of course not ample! Thus seems to escape the hope of any “general” construction of an ample sheaf on a quotient  $X/G$ , knowing one on  $X$ .

I knew the existence of varieties of moduli (for curves, or polarized abelian varieties) over  $\mathbb{Q}$ , for all “levels” (Stufe), but using Baily’s transcendental results,<sup>2</sup> guaranteeing the existence over  $\mathbb{C}$  and hence over  $\mathbb{Q}$  of the quotient

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<sup>1</sup> TDTE III. Préschémas quotients. Séminaire Bourbaki 1960/61, n°212.

<sup>2</sup> See W. L. Baily, On the theory of  $\theta$ -functions, the moduli of abelian varieties, and the moduli of curves. Ann. of Math. 75, 1962, 342–381.

10      3 20 April, 1961

variety you know. I am now able to perform directly the same constructions over  $\mathbb{Z}$ , but only for high levels  $n$ . As I do not know by now if the corresponding schemes are quasi-projective over  $\mathbb{Z}$ , and hence if the groups  $\Gamma = \mathrm{Sp}(2g, \mathbb{Z}/n\mathbb{Z})$  have orbits contained in affine sets, I cannot yet pass to the quotient by  $\Gamma$  to construct  $\mathcal{M}_n$  for smaller levels. If your arguments are correct, they may yield the lacking proof for the quasi-projectivity(?).

Sincerely yours,  
(signed) A Grothendieck

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25 April, 1961

April 25, 1961

Dear Mumford,

I thank you very much for your letter, and would like to congratulate you on your results. Still I would appreciate very much getting a sketch of your key-theorem (theorem 1 of your letter). It is of course obvious that the quotient  $U/G$  exists and  $U$  is a locally trivial principal bundle over  $U/G$ , even if  $U$  means the bigger open set of all  $(x_1, \dots, x_d)$  such that, for at least one choice of  $n + 2$  distinct indices, we get a projective basis of  $\mathbb{P}^n$ . What is not clear to me is what you define to be the ample sheaf on  $U/G$ , or probably rather how you prove that the obvious sheaf you get on  $U/G$  (say by descending the inverse of the sheaf of highest differentials on  $(\mathbb{P}^n)^d$ ) is ample. Is your hypothesis on  $U$  really necessary?

I had obtained in the meanwhile the same counterexamples as you based on Hironaka's construction,<sup>1</sup> and was all the more afraid your proof was erroneous, as the theorem Zariski read to me from your letter resembled very much to my false conjecture. I am glad to know you are as skeptical as I about general criteria for passing to the quotient by the projective group, and feel more confident now. Besides, my construction of schemata of moduli for high levels (as defined axiomatically in my Cartan Seminar talks<sup>2</sup> or in an older letter to Tate) resembles very much to yours, except that I did not observe that the suitably embedded polarized abelian varieties are completely determined by their sets of points of order  $n$ , ( $n$  big enough), which then leads you to a rather specific situation for passing to the quotient.

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<sup>1</sup> For this counterexample see Chap. 4, §3 of [D. Mumford, *Geometric Invariant Theory*, *Ergeb. Math. u. Grenzgebiete* 34, Springer-Verlag 1965]; see also H. Hironaka, *An example of a non-Kählerian complex-analytic deformation of Kählerian complex structures*, *Ann. Math.* 75 (1962) 190–208.

<sup>2</sup> Séminaire Henri Cartan 13, 1960/61, Exposé 9–16.

It seems to me that, because of your lack of some technical background on schemata, some proofs are rather awkward and unnatural, and the statements you give not as simple and strong as they should be. Therefore I suggest you to wait for writing a detailed paper till August, where I would appreciate very much discussing these matters with you. It is much to be desired, at last, to have on these questions a paper having the conceptual clarity in statements and proofs they deserve (especially after work like that of Igusa, which is most discouraging to read!). As for the statement of the results, I believe my Cartan talk is a good model, and scarcely anything needs to be changed. This of course would not prevent you to announce your results at once in a random way, before writing your detailed paper.

Can you prove, as is plausible from the transcendental approach, that for modular spaces of level  $n \geq 3$  (over which therefore the modular family of curves and jacobians is defined), the invertible sheaf on the modular scheme defined by the highest degree differentials on the jacobians is ample? (Indeed, this really should stem from the corresponding result for schemata of moduli for abelian polarized varieties). In fact, there are quite a few candidates for ample sheaves on  $\mathcal{M}_{g,n}$ , and it would be interesting to know about their relations.

In your “appendix”, you refer to a result of Matsusaka I did not hear of before, namely the connectedness or irreducibility of the variety of moduli for curves of genus  $g$ , in any characteristic. I did not know there was any algebraic proof for this (whatever way you state it). Yet I have some hope to prove the connectedness of the  $\mathcal{M}_{g,n}$  (arbitrary levels) using the transcendental result in char. 0 and the connectedness theorem; but first one should get a natural “compactification” of  $\mathcal{M}_{g,n}$  which should be simple over  $\mathbb{Z}$ .<sup>3</sup> I would like to know what is known to you concerning connectedness. I insist once more that the most interesting objects are not the classical  $\mathcal{M}_g$ ’s, but the schemata with operators  $\mathcal{M}_{g,n}$ , which have much nicer properties and achieve much preciser aims than  $\mathcal{M}_g$  alone. For instance  $\mathcal{M}_{g,n}$  ( $n \geq 3$ ) is simple over  $\mathbb{Z}$ . Of course the strongest connectedness theorems will be concerned with the  $\mathcal{M}_{g,n}$ ’s, big  $n$ , or (still better) with their Teichmüller analogues.

I indeed wrote a precise theory of the so-called “Hilbert schemata” which are to replace Chow coordinates, but are in fact rather different. They contain as open subsets the non-multiple parts of symmetric products, but the points corresponding to multiple cycles are blown up there, because an ideal at a point, primary for the maximal ideal, is not known by telling the multiplicity (except on a non singular curve!). I will give a rather detailed account in my next Bourbaki talk,<sup>4</sup> alluded to in talk III.

<sup>3</sup> A proof of the connectedness, along these lines, was eventually published by Deligne and Mumford; see [69c].

<sup>4</sup> A. Grothendieck, TDTE IV. Les schémas de Hilbert. Séminaire Bourbaki 1960/61, Fasc 3, Exposé 221. A better and more elegant treatment of Hilbert schemes was found later, by Mumford; see Chap. 14 of *Lectures on Curves on an Algebraic Surface*, Annals of Math. Studies 59, Princeton U. Press 1966. See also [1964Aug31].

4 25 April, 1961 13

Sincerely yours  
(signed) A Grothendieck



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10 May, 1961

Paris May 10, 1961

Dear Mumford,

I thank you very much for your letter and the proof of your key theorem. I think I will be able in the next days to read it thoroughly.

Please excuse me if I omitted to write you some time ago that I received the revised version of your MS on the blowing down of a surface, which has been given to the printer. You will probably get the proofs from the printer during this month. I hope you will not be too dissatisfied with the delay of publication!

It occurred to me that I had sent you only a copy of my Bourbaki talk on quotients,<sup>1</sup> but none of my Cartan talk.<sup>2</sup> This is done now; I will send you the following ones within the next weeks.

The formation of the modular schemas  $\mathcal{M}_{g,n}$  ( $n \geq 3$ ), representing contravariant functors, is obviously compatible with base extension. But I doubt the same be true for  $\mathcal{M}_{g,1}$ , which is the sub-product of  $\mathcal{M}_{g,n}$  obtained by dividing by the finite group of automorphisms  $G = \mathrm{Sp}(2g, \mathbb{Z}/n\mathbb{Z})$  (at least when restricting to the part of  $\mathcal{M}_{g,1}$  lying over the open subset of  $\mathrm{Spec}(\mathbb{Z})$  complement of the set of primes dividing  $n$ ), and does not represent any reasonable contravariant functor (but, as you remarked, a covariant one). Such a commutation would mean that for every open affine set of  $\mathcal{M}_{g,n}$ , stable under  $G$  and with affine ring  $A$ ,  $H^1(G, A) = 0$ . This can be expressed equivalently by introducing for every  $x \in \mathcal{M}_{g,n}$  the inertia group  $G_x$  of  $x$  (which is the group of automorphism of the corresponding algebraic curve), and demanding that  $H^1(G_x, \mathcal{O}_x) = 0$ , where  $\mathcal{O}_x$  is the local ring of  $x$  in  $\mathcal{M}_{g,n}$ . You can also replace the latter by its completion, which is nothing else but the local ring describing the “formal variety of moduli” of the given curve, in the sense of my Bourbaki talk II,<sup>3</sup> and the consideration of which is independent of the global theory.

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<sup>1</sup> Séminaire Bourbaki 1960/61, n° 212

<sup>2</sup> Séminaire Henri Cartan 13, 1960/61, Exposé 9–16

<sup>3</sup> Séminaire Bourbaki 1960/61, n° 195

16      5 10 May, 1961

Although I did not make any effective computation, I do not see why such a relation should hold (even for genus  $g = 2$ ); it does however for  $g = 1$ , because an equivalent formulation of the question is whether the fibers of  $\mathcal{M}_{g,1}$  over the different points of  $\text{Spec}(\mathbb{Z})$  are normal, which is indeed true for genus 1. In the same direction, there is the question whether the natural morphism from  $\mathcal{M}_{g,1}$  into the corresponding modular space for polarized abelian varieties is really an embedding; a priori one can say only that  $\mathcal{M}_{g,1}$  is the normalisation of a (non closed) subschema of the latter, which may not be normal.

Sincerely yours  
(signed) A Grothendieck

---

29 January, 1962(a)

Paris Jan 29, 1962

Dear Mumford,

Thanks for your letter, and best wishes to you and your wife for your son!

Your ampleness criterion looks nice indeed. I would appreciate to have an outline of the proof some time.

I am afraid you will not convince me of the usefulness of Chow coordinates, in fact your example shows again that the wrong method will lead to prove statements under unnatural assumptions (such as normality). Although I did not check it, I am convinced that the method I used for the theorems of passage to the quotient in my Bourbaki talk III<sup>1</sup> will yield:

Let  $X$  quasi-projective over  $S$  loc. noeth.,  $\mathcal{R} \subset X \times_S X$  a closed subscheme such that

- (i)  $\mathcal{R}$  is “set-theoretically an equivalence relation”,
- (ii)  $\text{pr}_1 : \mathcal{R} \rightarrow X$  is proper (hence projective) and universally open.

Then  $Y = X/\mathcal{R}$  exists,  $X \rightarrow Y$  is proper (and universally open),  $\mathcal{R} \rightarrow X \times_Y X$  is a bijective closed immersion. [I checked this long ago when  $X$  is finite over  $S$ ; no openness conditions are then required.]

If such a statement should be of use somewhere, I can include it in Chap. V. However, I never needed it, as it is much too coarse for the kind of problems I was considering. In fact, it should be considered rather as a statement of a theory of schemes “modulo  $\mathcal{M}$ ”, where  $\mathcal{M}$  is the set of all morphisms which are “universal homeomorphisms”, which one wants to consider as isomorphisms in the new category (obtained by adjoining formally their inverses). N.B. under the usual finiteness assumptions, “universal hom.” = “finite surjective radicial morphism”. If one sticks to, say, algebraic groups, one gets Serre’s “quasi-algebraic groups” = groups mod. purely inseparable isogeny.

Tate wrote me you are talking in your seminar on your theorem about passage to the quotient. I would appreciate to know when you obtain results

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<sup>1</sup> Séminaire Bourbaki 1960/61, n°212

18 6 29 January, 1962(a)

on existence of Picard schemes (I am giving a Bourbaki talk on Picard on Feb 18).

Sincerely yours,  
(signed) A Grothendieck

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29 January, 1962(b)

Paris Jan 29, 1962

Dear Mumford,

I have been too rash in my reply to your last letter: in effect, I was thinking of a reduction of the general case (concerning passage to the quotient under the conditions you know) to the case where  $\mathcal{R} \rightarrow X$  is *finite*. However, in the latter, I have no means of attacking the problem, which in fact meets with a few unsolved problems on equivalence relations I still had in store. Therefore I grant you that, for the time being, in your example Chow coordinates do give mathematical information about existence of quotients which is not obtained by other means. I do not expect this situation to hold for long still! Besides, Chevalley had non-trivial unpublished results on quotients, of course never using Chow coordinates, which may well cover the cases we have in mind. Unfortunately he is very sick at the moment, with a so-called “pancreatite”<sup>1</sup>, and there is no asking him about anything now. One more comment: it seems that, for the application of Chow coordinates, your regularity assumptions:  $X$  normal,  $\mathcal{R} \rightarrow X$  univ. open, are not the right thing exactly, unless you assume  $\mathcal{R}$  irreducible. What is needed, in effect, seems that all components of all fibers of  $\mathcal{R} \rightarrow X$  have the same dimension (which *does not* follow from the assumptions as you stated them).

It also appears to me that in my Bourbaki talk III,<sup>2</sup> on quotients, in th. 6.1. (i), the assertion that  $X/\mathcal{R} = Y$  is *quasi-projective* is proved only if  $\mathcal{R}$  is really an *equivalence* relation (or  $\mathcal{R} \rightarrow X$  *finite*), and not only a *preequivalence* (this excludes the case of groups operating with fixed points!). I do not know at present if there may be a counterexample in the general case. I guess you are right to say that “the last word has not been said” at all in the theory of

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<sup>1</sup> pancreatitis

<sup>2</sup> Techniques de descente et théorème d’existence. III, Préschémas quotients, Séminaire Bourbaki 1960/61, n°212.

20 7 29 January, 1962(b)

quotient schemes!

Best regards

(signed) A Grothendieck

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5 February, 1962

Paris Feb 5, 1962

Dear John,<sup>1</sup>

In connection with my Bourbaki talk<sup>2</sup>, I pondered again on Picard schemes. For instance, as I told Mumford, I proved that if  $X/S$  is projective and simple,<sup>3</sup> then  $\mathcal{P}ic_{X/S}$  is of finite type over  $S$ . More generally, the decomposition of  $\mathcal{P}ic_{X/S}$  according to the Hilbert polynomials (in fact, the first two non trivial coefficients of the polynomial suffice) consists of pieces which are of finite type, hence projective over  $S$ . Another way of stating this is to say that a family of divisors  $D_i$  on the geometric fibers of  $X/S$  is “limited” iff the projective degrees of the  $D_i$  and  $D_i^2$  are bounded.

Another result, of interest in connection with your seminar, is a proof of the fact that, for an abelian scheme  $A/k$ ,  $k$  a perfect field, the absolute formal scheme of moduli over  $\mathbb{W}_\infty(k)$  is simple over  $k$ . This comes from the following general fact: Let  $X_0/S_0$  be simple,  $X'_0/X_0$  étale,  $S_0$  subscheme of  $S$  defined by an ideal  $\mathcal{J}$  of square 0. Let  $\xi_0 \in H^2(X_0, \mathcal{G}_{X_0/S_0} \otimes_{\mathcal{O}_{S_0}} \mathcal{J})$  and<sup>4</sup>  $\xi'_0 \in H^2(X'_0, \mathcal{G}_{X'_0/S_0} \otimes_{\mathcal{O}_{S_0}} \mathcal{J})$  be the obstruction for lifting. Then  $\xi'_0$  is the inverse image of  $\xi_0$  under the obvious map. As a consequence, if  $X_0/S_0$  is abelian, taking  $X'_0 = X_0$ ,  $X'_0 \rightarrow X_0$  multiplication by  $n$  prime to the residue characteristic, we get  $\xi_0 = n^*(\xi'_0)$ . If  $S = \text{Spec } A$ ,  $A$  local artin, and  $\mathfrak{m}\mathcal{J} = 0$ , then we are reduced to an obstruction in the  $H^2$  of the reduced  $X_0 \otimes_{A_0} k = A$ , satisfying  $\xi = n^*(\xi)$  for  $n$  prime to  $p$ . Using the structure

$$H^*(A, \mathcal{G}_{A/k}) \simeq \bigwedge^* H^1(A, \mathcal{O}_A) \otimes t_A,$$

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<sup>1</sup> Letter to John Tate.

<sup>2</sup> Referring to Séminaire Bourbaki 1960/61, n°232 and n°236, V. Les schémas de Picard. Théorèmes d'existence. VI. Les schémas de Picard. Propriétés générales.

<sup>3</sup> The standard terminology has changed from “simple” to “smooth”.

<sup>4</sup> Here  $\mathcal{G}_{X_0/S_0}$  and  $\mathcal{G}_{X'_0/S_0}$  denote the relative tangent sheaves for  $X_0/S_0$  and  $X'_0/S_0$  respectively.

we get  $n^*(\xi) = n^3\xi$ , hence  $(n^3 - 1)\xi = 0$ . Taking  $n = -1$  we get  $2\xi = 0$ , hence  $\xi = 0$ , and we win!

I just noticed<sup>5</sup> the proof does not give any information for residue char. = 2! Here is a simple proof valid in any char.: Consider the obstruction  $\eta_0$  for lifting  $X_0 \times_{S_0} X_0$ , then  $\eta_0 = \xi_0 \otimes 1 + 1 \otimes \xi_0$ , and  $\eta_0$  is invariant under the automorphism  $(x, y) \rightsquigarrow (x, y + x)$  of  $X_0 \times_{S_0} X_0$ . Thus we get an element  $\xi = \sum_{i,j} \lambda_{i,j} e_i \wedge e_j$  in  $H^2(A, \mathcal{O}_A) = \bigwedge^2 t$ , s.th.  $\eta = \sum_{i,j} \lambda_{i,j} e'_i \wedge e'_j + \sum_{i,j} \lambda_{i,j} e''_i \wedge e''_j$  in  $\bigwedge^2(t \oplus t)$  is invariant under  $(x, y) \rightsquigarrow (x, y + x)$ , carrying  $e'_i \rightsquigarrow e'_i + e''_i$  and  $e''_i \rightsquigarrow e''_i$ , hence trivially  $\xi = 0$ !

As a consequence, we get that the scheme of moduli for the polarized abelian schemes, with polarization degree  $d$ , is simple over  $\mathbb{Z}$  at all those primes  $p$  which do not divide  $d$ . This comes from the fact that the obstruction to polarized lifting lies in a module  $H^2(A, \mathcal{E})$ , where  $\mathcal{E}$  is an extension (the "Atiyah extension")

$$(*) \quad 0 \rightarrow \mathcal{O}_A \rightarrow \mathcal{E} \rightarrow \mathfrak{G}_{A/k} \rightarrow 0$$

whose class  $c$  in  $H^1(A, \Omega^1_{A/k})$  is just the Chern class  $\frac{d\mathcal{L}}{\mathcal{L}}$  of the invertible sheaf  $\mathcal{L}$  on  $A$  defining the polarization. Now in the exact sequence of cohomology for  $(*)$ , the map

$$\begin{array}{ccc} H^i(\mathfrak{G}_{A/k}) & \xrightarrow{\partial^{(i)}} & H^{i+1}(\mathcal{O}_A) \\ \simeq \downarrow & & \simeq \downarrow \\ \bigwedge^i t' \otimes t & & \bigwedge^{i+1} t' \end{array} \quad t = t_A, t' = t_{\hat{A}}$$

is trivially described in terms of

$$c \in H^1(A, \Omega^1_{A/k}) \simeq \text{Hom}(t, t'),$$

where the homomorphism  $c: t \rightarrow t'$  is just the tangent map for  $\varphi: A \rightarrow \hat{A}$  defined by the polarization. This map being surjective by assumption,  $\partial^{(i)}$  is surjective, hence  $H^i(\mathcal{E}) \rightarrow H^i(\mathfrak{G}_{A/k})$  is injective, in particular

$$H^2(\mathcal{E}) \rightarrow H^2(\mathfrak{G}_{A/k})$$

is injective. As the obstructions obtained in  $H^2(\mathfrak{G}_{A/k})$  are zero, the same holds for the polarized obstructions in  $H^2(\mathcal{E})$ , hence the assertion of the simplicity. (If however  $p|d$ , simplicity does not hold at any point of  $\mathcal{M}$  over  $p$ !)

Using the simplicity for the formal scheme of moduli of abelian varieties, I can prove the following:

Let  $X/\Lambda$  be flat, proper,  $H^0(X_0, \mathcal{O}_0) \xleftarrow{\sim} k$ , where  $\Lambda$  is local artin with residue field  $k$ . Assume  $\mathcal{P}ic_{X_0/k}$  exists, and is simple over  $k$ , i.e.  $\dim \mathcal{P}ic_{X_0/k} = \dim H^1(X_0, \mathcal{O}_{X_0})$  (always true in char 0). Then

a)  $\mathcal{P}ic^0_{X/\Lambda}$  exists and is an abelian scheme over  $\Lambda$ .

<sup>5</sup> This paragraph was penned on the left margin vertically.

- b) The “base extension property” holds for  $R^i f_*(\mathcal{O}_X)$  in dimension 1, and more generally in any dimension  $i$  such that

$$\bigwedge^i H^1(X_0, \mathcal{O}_{X_0}) \rightarrow H^i(X_0, \mathcal{O}_{X_0})$$

is *surjective*, and  $H^1(X, \mathcal{O}_X)$  is free over  $A$ .

Idea of proof:

- a)  $\mathcal{P}ic_{X/k}^0$  is constructed stepwise. Having  $\mathcal{P}ic_{X_{n-1}/k}^0 = A_{n-1}$ , to get  $A_n$  we first lift *arbitrarily*  $A_{n-1}$  to an abelian scheme  $A'_n$ . We then try to construct the can. invertible “Weil sheaf” on  $X_n \times_{A_n} A'_n$ , extending the given Weil sheaf on  $X_{n-1} \times_{A_{n-1}} A_{n-1}$ . The obstruction lies in

$$H^2(X_0 \times A_0, \mathcal{O}_{X_0 \times A_0}) \simeq H^2(\mathcal{O}_{X_0}) \times H^2(\mathcal{O}_{A_0}) \times H^1(\mathcal{O}_{X_0}) \otimes H^1(\mathcal{O}_{A_0})$$

and in fact, as easily seen, in the last factor  $H^1(X_0, \mathcal{O}_{X_0}) \otimes H^1(A_0, \mathcal{O}_{A_0}) \simeq t_{A_0} \otimes H^1(A_0, \mathcal{O}_{A_0}) \simeq H^1(A_0, \mathfrak{G}_{A_0/k})$ . This space is exactly the group operating in a simply transitive way on the set of all extensions of  $A_{n-1}$ . Thus we can *correct*  $A'_n$  in just one way to get an  $A_n$  with a “Weil sheaf” on it! This does it.

- b) Let  $\omega$  be the conormal sheaf to the unit section of  $A = \mathcal{P}ic_{X/S}^0$ , thus  $\omega$  is *free* because  $A/S$  is simple, and by definition of  $\mathcal{P}ic_{X/S}^0$  we have

$$H^1(X, \mathcal{O}_A) \simeq \text{Hom}(\omega, \mathcal{O}_S)$$

This description holds also after any base extension, hence the fact that  $H^1(X, \mathcal{O}_X)$  is free over  $A$  and its formation commutes with base extension. This implies also  $H^1(X, \mathcal{O}_X) \rightarrow H^1(X_0, \mathcal{O}_{X_0})$  surjective, hence  $H^i(X, \mathcal{O}_X) \rightarrow H^i(X_0, \mathcal{O}_{X_0})$  is surjective for the  $i$ 's as in the theorem, ok.

*Corollary.* Let  $A/S$  be any abelian scheme, then the modules  $R^i f_*(\mathcal{O}_A)$  on  $S$  are locally free and in fact  $\simeq \bigwedge^i R^1 f_*(\mathcal{O}_A)$ . If  $\mathcal{P}ic_{A/S}$  exists, then  $\mathcal{P}ic_{A/S}^0$  is open and is an abelian scheme over  $S$ .

(Moreover, biduality holds, as follows easily from the statement over a field ...).

*Corollary.* Let  $f : X \rightarrow S$  be flat, proper,  $k(s) \xrightarrow{\sim} H^0(X_s, \mathcal{O}_{X_s})$  for every  $s$ , let  $s \in S$  be such that  $\dim H^1(X_s, \mathcal{O}_{X_s}) = \dim \mathcal{P}ic_{X_s/k(s)}$ , (the latter defined, if  $\mathcal{P}ic_{X_s/k(s)}$  is not known to exist, in terms of the formal Picard scheme). Then  $R^1 f_*(\mathcal{O}_X)$  is free at  $s$ .

This is always applicable if  $\text{char } k = 0$ .

I do not know if, in the case considered, the  $R^i f_*(\mathcal{O}_X)$  or even  $R^i f_*(\Omega_{X/S}^j)$  are also free at  $s$ , even in char 0. It is true for  $f_*(\Omega_{X/S}^1)$  whenever we know that  $\dim H^1(X_s, \mathcal{O}_{X_s}) = \dim H^0(X_s, \Omega_{X_s}^1)$ , for instance if  $\text{char } k(s) = 0$  and  $f : X \rightarrow S$  is projective and simple. (If *moreover*  $S$  is reduced, Hodge theory implies *all*  $R^i f_*(\Omega_{X/S}^j)$  are free at  $s$ ; but if  $S$  is artin, I have no idea!)

I now doubt very much that it be true in general that  $\mathcal{P}ic_{X/S}^\tau$  is flat over  $S$ , or even only universally open over  $S$ , when  $X/S$  is simple. Here is an idea of an example, inspired by Igusa's surface. Let  $A/S$  be an abelian scheme,  $G$  a finite group of automorphisms of  $A$ . If  $G$  operates without fixed points on  $B/S$  projective and simple over  $S$ , with  $\mathcal{O}_S \xrightarrow{\sim} g_*(\mathcal{O}_B)$ , we construct  $X = B \times_G \hat{A}$  which is an abelian scheme over  $Y = B/G$ , and one checks

$$\mathcal{P}ic_{X/S} \simeq \mathcal{P}ic_{Y/S} \times_S (\mathcal{P}ic_{\hat{A}/S})^G$$

(where upper  $G$  denotes the subscheme of invariants), hence

$$\boxed{\mathcal{P}ic_{X/S}^\tau \simeq \mathcal{P}ic_{Y/S}^\tau \times_S A^G}$$

Hence for getting examples of bad  $\mathcal{P}ic_{X/S}^\tau$ , we are led to study schemes of the type  $A^G$ , with  $S$  say spectrum of a discrete valuation ring  $V$ . Thus we are led to the questions:

- a) Can it occur that there are components of  $C = A^G$  which do not dominate  $S$ ? For instance,  $A_1^G = \text{unit subgroup}$  (set theoretically, or even scheme-theoretically) and  $A_0^G \neq \text{unit subgroup}$  set theoretically—where  $A_0, A_1$  are the special and the general fibers.
- b) If  $C_1 = A_1^G$  is connected (for instance is the unit subgroup), and hence  $C^\circ = C_0^\circ \cup C_1^\circ$  is open, can it occur that  $C^\circ$  is non flat over  $S$  [for instance  $C_1 = \{e\}, C_0^\circ \neq \{e\}$ ]?
- c) Same questions for  $H^1(A, \mathcal{O}_{A/S})^G = t_{\hat{A}}^G$  and  $H^0(A, \Omega_{A/S}^1)^G = t_A^G$  (in order to get examples where the dimensions  $h^{01}$  and  $h^{10}$  for the fibers make a jump in the case of *equal characteristics*).

The trouble is I have no idea how to get non trivial ways of letting a finite group operate on an abelian variety. It seems that starting with products of elliptic curves and using only endomorphisms of the factors, for instance letting a finite subgroup of  $\text{GL}(n, R)$  operate on  $E^n$ , where  $R$  is the ring of endomorphisms of the elliptic curve  $E$ , won't give a counterexample (I more or less proved this latter statement). If  $p$  is the residue characteristic, one sees easily that the only trouble against flatness can come from a Sylow  $p$ -subgroup of  $G$ . For instance, in a) the question is equivalent to getting an example where  $T_p(\overline{A_0}) \rightarrow T_p(\overline{A_1})$  (where  $T_p$  is the contravariant Tate functor,  $T_p(M) = \text{Hom}({}_p M, \mathbb{Q}_p/\mathbb{Z}_p)$ , and  $\overline{A_0}$  and  $\overline{A_1}$  are the *geometric* fibers) induces

$$\hat{H}^{-1}(G, T_p(\overline{A_0})) \rightarrow \hat{H}^{-1}(G, T_p(\overline{A_1}))$$

which is *not injective*. I am convinced such things can happen. Perhaps you or Mumford are cleverer than I and find a counterexample? What I did get easily was an example of an *abelian* scheme  $X/S$  [product of two elliptic curves over  $S$ ] such that multiplication  $p : \mathcal{P}ic_{X/S} \rightarrow \mathcal{P}ic_{X/S}$  is *not* universally open, i.e. such that there exists an irreducible component  $C$  of  $\mathcal{P}ic_{X/S}$  not dominating

$S$ , but such that  $pC$  is contained in a component dominating  $S$ . [N.B. if  $n$  prime to all residue char., multiplication by  $n$  in any  $\mathcal{P}ic_{X/S}$  is *étale*.]

Best regards to Karin, kids etc.

(signed) Schurik

P.S. I just proved: If  $X \rightarrow S$  is *simple* and *projective*, then  $\mathcal{P}ic_{X/S}^\tau$  is *projective* over  $S$ . Method:

- a) From the fact that the fibers of  $\mathcal{P}ic_{X/S}^0$  are proper, follows that  $\mathcal{P}ic_{X/S}^0$  is proper over  $S$ , hence closed in  $\mathcal{P}ic_{X/S}$ , hence easily that  $\mathcal{P}ic_{X/S}^\tau$  is *closed* in  $\mathcal{P}ic_{X/S}$ . It remains to prove it is of *finite type* over  $S$ —hence proper over  $S$ , and quasi-projective over  $S$ , hence projective.
- b) For every  $n > 0$ , the kernel of  $\mathcal{P}ic_{X/S} \xrightarrow{n} \mathcal{P}ic_{X/S}$  is of finite type over  $S$  [and even more: the multiplication  $\mu$  by  $n$  is of finite type, hence finite]. If  $n$  is prime to the residue characteristics, this follows from the fact that  $\mu$  is *étale* and has finite fibers. This reduces to the case  $S$  of char  $p > 0$ ,  $n = p$ . Then I use a technique of descent involving the “relative  $p$ -power scheme”  $(X/S)^{(p)}$ , following a suggestion of Serre.
- c) For variable  $s \in S$  ( $S$  noetherian), the Néron-Severi torsion group of  $X_s$  remains of bounded order. This can be shown using the method of Matusaka’s proof for the finiteness of the “torsion group”. From a), b), c), the theorem follows.

*Remark:* Using the Picard-Igusa inequality for  $\rho =$  rank of Néron-Severi, and Lefschetz type theorems I told you about, one gets also that  $\rho(X_s)$  remains bounded for  $s \in S$  ( $S$  noetherian).

*Question:* Is  $\mathcal{P}ic_{X/S}^\tau$  always of finite type over  $S$ , under merely the usual assumptions for existence of  $\mathcal{P}ic_{X/S}$ ? I have no proof even if  $X \rightarrow S$  is normal! Same question for  $\rho$ . This seems related to the question of uniform majorization of the Mordell-Weil-Néron-Lang finiteness theorem, for a *variable* abelian variety.



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 31 March, 1962

31.3.1962

My dear Mumford,

I was quite interested by your letter. Concerning your example of a non flat  $\mathcal{P}ic^\tau$ ,<sup>1</sup> I am convinced there should be still stronger counterexamples, insofar as 1°)  $\mathcal{P}ic^\tau$  need not be flat over  $S$  even at points of the connected component of the identity 2°)  $\mathcal{P}ic^\tau$  need not even be universally open over  $S$ , except at points corresponding to the part of the torsion of Néron-Severi prime to the characteristic; i.e., in the case where  $S$  is the spectrum of a valuation ring,

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<sup>1</sup> See TDTE VI (= Séminaire Bourbaki 1961/62, n°236), Remarque 2.9, where AG described this example as a deformation of an Igusa surface over an Artinian local ring.

Such an example can be provided as follows. Let  $E_1, E_2$  be two ordinary elliptic curves over an algebraically closed field  $k$  of characteristic 2, and let  $a \in E_2(k)$  be a non-trivial 2-torsion point of  $E_2$ . Let  $X = (E_1 \times E_2)/(x, y) \sim (-x, y + a)$ . The Hodge numbers of such an Igusa surface  $X$  is the same as that of an abelian surface, and the Hodge-to-De Rham spectral sequence degenerates. Let  $I$  be the  $k$ -linear dual of  $H^1(X, \Theta_X)$ , and let  $R := k \oplus I$  be the Artinian local  $k$ -algebra with  $I^2 = (0)$ . Let  $X_1 \rightarrow \text{Spec}(R)$  be the universal first order equi-characteristic deformation of  $X$ , i.e. its Kodaira-Spencer class  $\gamma \in I \otimes H^1(X, \Theta_X)$  is the identity map for  $H^1(X, \Theta_X)$ . Then  $\mathcal{P}ic^\tau(X_1/S)$  has two connected components, the neutral component  $\mathcal{P}ic^0(X_1/S)$  and another component  $\mathcal{P}'$ . The structural morphism  $\mathcal{P}' \rightarrow S$  factors through a closed subscheme of  $S$  defined by a non-zero ideal of  $R$ . So  $\mathcal{P}ic^\tau(X_1/S)$  is not flat over  $S$  at points of  $\mathcal{P}'$ . The key facts are: (a) For any non-trivial line bundle  $\mathcal{L}$  on  $X$  with  $\mathcal{L}^{\otimes 2} \cong \mathcal{O}_X$ , the Chern class  $c_1^{\text{dR}}(\mathcal{L})$  is element in  $\text{Fil}_{\text{hodge}}^1 H^2(X, \Omega_X^\bullet)$  whose image  $\overline{c_1(\mathcal{L})} \in \text{gr}_{\text{hodge}}^1 = H^1(X, \Omega_X^1)$  is non-zero. (b) The natural map  $H^1(X, \Omega_X^1) \times H^1(X, \Theta_X) \rightarrow H^2(X, \mathcal{O}_X) \cong k$  is a non-degenerate pairing.

An example of a non-flat  $\mathcal{P}ic^\tau$  where the base scheme is the spectrum of a discrete valuation ring with mixed characteristics  $(0, p)$  is published in Prop. 4.2.4 on p. 138 of of M. Raynaud, *p-torsion du schéma de Picard*, Astérisque 64 (1979) 87–148.

there may be components of  $\mathcal{P}ic$  which do not dominate  $S$ . Even in case 1°; examples with  $S$  the spectrum of a valuation ring (discrete of course) should exist. I wrote to Tate about the matter about two months ago, telling him how one could adapt Igusa's example so as to reduce oneself to producing suitable examples of finite groups of automorphisms on abelian schemes (which should give also examples where the  $H^{10}$  and  $H^{01}$  of fibers of simple morphism make jumps. . .) and begging him for help, but that unnice chap never answered a word. Besides, did the same tell you that I proved the simplicity of moduli for abelian schemes (either formal moduli, or polarized moduli with polarization degree prime to the char)? Using this, I can prove that  $H^{0,1}$  behaves decently whenever the  $\mathcal{P}ic$  of the special fiber is simple. . .

I am particularly happy with your simple example of a non existing  $\mathcal{P}ic$ .<sup>2</sup> I still naively surmised descent of Picard schemes would not cause any difficulty, and had felt satisfied in your very example with proving the existence of  $\mathcal{P}ic$  in the case where the irreducible components of the special fiber are geometrically irreducible.<sup>3</sup> Still there remains the hope that  $\mathcal{P}ic^\tau$  exists in great generality, or at least (in case the Pic groups of the fibers are simple)  $\mathcal{P}ic^0$ , obtained by taking the subfunctor of the Pic functor corresponding to invertible sheaves inducing on the special fibers sheaves that are algebraically equivalent to 0;<sup>4</sup> (in case  $\mathcal{P}ic$  exists and its fibers are simple, I proved that  $\mathcal{P}ic^0$  is open).

It is quite mysterious to me how from your general remarks on Severi-Brauer schemes you will deduce the existence of  $\mathcal{P}ic$  in the case you claim, fibers separable and irreducible components being geometrically irreducible. Whatever way you present technicalities, it seems to me you will need a theorem of the following type:  $f: X \rightarrow S$  being as before (of course, also projective and flat), there should exist a family  $(U_i, S_i)$  of finite étale multisections of  $X$  over open subsets  $U_i \subset S$ , such that, for every  $S'$  over  $S$  and every  $\xi \in \text{Pic}(X'/S')$  "sufficiently ample", corresponding to some immersion of  $X'$  into a  $\mathbb{P}_{S'}^N$ , as usual (we can in fact suppose without loss that the Brauer-Severi schema corresponding to  $\xi$  is trivial), and every  $s' \in S'$ , there exists a  $U_i$  be-

<sup>2</sup> See TDTE VI (= Séminaire Bourbaki 1961/62, n°236), 0.a. This example is discussed on p.210 of the book *Néron Models* by S. Bosch, W. Lütkebohmert and M. Raynaud, Springer-Verlag 1990, and in greater detail in 9.4.14 on p.267 of the article S. Kleiman: The Picard scheme, in *Fundamental Algebraic Geometry*, Amer. Math. Soc. 2005, 235–321.

<sup>3</sup> The representability theorems for the Picard functor come in two flavors, as schemes or as algebraic spaces. See 8.1 and 8.2 of S. Bosch, W. Lütkebohmert & M. Raynaud, *Néron Models*, Springer-Verlag, 1990. See also 19.4 of S. Kleiman, The Picard scheme, in *Fundamental Algebraic Geometry*, Amer. Math. Soc. 2005, 235–321. Mumford's existence theorem of the Picard scheme, stated on the first page of TDTE VI, Séminaire Bourbaki 1961/62, n° 236), and also on p. viii of *Lectures on Curves on an Algebraic Surface* in a slightly weaker form, is still unpublished; see also Remark 19.4.18 of Kleinman's article in loc. cit.

<sup>4</sup> See the last paragraph of [1965.Jan23] for a counterexample based on a remark of M. Raynaud.

low  $s'$  such that the multisection  $S'_i$  of  $X'$  over  $U'_i$  deduced from  $S_i$ , viewed as a family of zero cycles on the fibers of  $\mathbb{P}_{S'}^N$ , consists only of zero-cycles in your open set (where passage to the quotient by the projective group *and* the symmetric group in  $(\mathbb{P}^N)^m$  is possible). Did you prove anything such? I wonder how you will use the hypothesis on the irreducible components of the fibers of  $X/S$ !

You make an allusion to results of yours “over  $\mathbb{Q}$ ” for vector bundles over non singular curves. Do you just mean “in char. 0”—as just afterwards you assume the ground field algebraically closed. I confess the little you say about it does not suggest much to me! The reference in my notes on properness criteria which you did not understand was to III 5.5.1. (I guess you will get Chap III very soon, as it has appeared by now; I had copies sent to Hartshorne and Lichtenbaum too). This states that if  $X$  is separated of finite type over say a complete local noetherian ring, and if  $Z_0$  is an open and proper subset of the special fiber  $X_0$ , then there exists an open and closed subset  $Z$  of  $X$ , proper over  $S$ , whose special fiber is  $Z_0$  (and  $Z$  will in fact be the biggest closed subset of  $X$  proper over  $S$ ).

Lubkin’s result seems very unlikely to me too, but although I had a little thought of constructing a counterexample over  $S = \mathbb{P}^1$  (keeping in mind  $\pi_2(S) = \mathbb{Z}$ ), I did not succeed. I will keep the question in mind, and discuss it with Serre when he comes back from Bourbaki next week.

Sincerely yours  
(signed) A Grothendieck



**23 June, 1962**

Neuilly June 23, 1962

Dear Mumford,

I was of course quite interested by the results you stated in your last letter. If you want to explain the ideas of the proof to me when I come to Harvard this will take a while, as it turns out that I won't come this year, due to health troubles for my wife and children. If you have time to give me an idea of the proof for finite type of  $\mathcal{P}ic_{X/S}^P$  by letter, I would appreciate it. I wonder if you can prove the slightly stronger result I had in case  $f: X \rightarrow S$  is simple, and the fibers of pure dimension  $d$ , namely that it is sufficient that, in  $P(n) = a_0n^d + a_1n^{d-1} + a_2n^{d-2} + \dots$ , the coefficients  $a_1$  and  $a_2$  remain bounded [in terms of divisors,  $D$  and  $D^2$  have bounded projective degrees] in order for the invertible sheaves considered to belong to a quasi-compact subset of  $\mathcal{P}ic_{X/S}$ ? My proof, following Matsusaka, uses equivalence criteria and Riemann-Roch for surfaces, and is technically rather involved.

Sincerely yours,  
(signed) A Grothendieck

P.S. Do you know if  $\mathcal{P}ic_{X,S}^\tau$  is of finite type over  $S$ , when  $f: X \rightarrow S$  is separable (=flat with reduced geom. fibers)?



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6 July, 1962

23 Boul. de Levallois  
Neuilly (Seine)

Neuilly July 6, 1962

Dear Mumford,

I enjoyed very much the proofs you gave me in the last letter, your proof of finite type for  $\mathcal{P}ic^f$  is certainly much simpler than mine (which gives a more precise result, in a less general case). The main step in my proof (besides Matsusaka's method using Riemann-Roch, to deal with the case of non singular surfaces—a method now superseded by your proof) is the following:

**Theorem.** *Let  $f: X \rightarrow S$  be a projective and flat morphism whose fibers are of depth  $\geq 3$  at closed points,  $Y$  a Cartier Divisor on  $X$ , transversal to the fibers i.e., flat/ $S$ , and ample relative to  $S$ , assume  $\mathcal{P}ic_{X/S}$  and  $\mathcal{P}ic_{Y/S}$  exist, then the morphism*

$$\mathcal{P}ic_{X/S} \rightarrow \mathcal{P}ic_{Y/S}$$

is of finite type.

Idea of proof. Let  $Y_0 = Y$ , define  $Y_m$  ( $m \geq 0$ ) as usual, then using exact sequences of cohomology, it is not hard to show that  $\mathcal{P}ic_{Y_m/S} \rightarrow \mathcal{P}ic_{Y_0/S}$  is of finite type. (NB if  $S \neq \emptyset$ , there exists a non empty open subset  $U$  of  $S$  such that the restriction of the previous morphism over  $U$  is affine—a fortiori of finite type. This proves finite type for the morphism by noetherian induction on  $S$ ). This permits us to replace  $Y_0$  by  $Y_m$ , large  $m$ . Using the depth  $\geq 2$  assumption and the (easy part of) equivalence criteria as developed in my IHÉS Seminar 1962, one gets that, for large  $m$ ,  $\mathcal{P}ic_{X/S} \rightarrow \mathcal{P}ic_{Y_m/S}$  is a monomorphism. One is reduced to proving that, for any section of  $\mathcal{P}ic_{Y_m/S}$  over  $S$ , its inverse image in  $\mathcal{P}ic_{X/S}$  is of finite type over  $S$ , and using that its projection to  $S$  is a monomorphism, one is reduced to proving the following: Assume  $S$  irreducible with generic point  $s$ , let  $\mathcal{M}$  be an invertible sheaf on  $Y$  such that  $\mathcal{M}_s$  does not come from an invertible sheaf on  $X_s$ , then there is an open neighbourhood  $U$  of  $s$  such that  $t \in U$  implies that  $\mathcal{M}_t$  does not come from an invertible sheaf on

$X_t$ . To prove this, using  $\text{depth} \geq 3$  and the “existence” part of the equivalence criteria, one gets that the assumption on  $\mathcal{M}_s$  means that either (i) there exists  $m' \geq m$  such that  $\mathcal{M}_t$  does not come from an invertible sheaf on  $Y_{m'}$  or (ii) there exists a coherent sheaf  $\mathcal{L}_s$  on  $X_s$ , invertible in a neighbourhood of  $Y_s$  but not invertible on the whole of  $X_s$ , having  $\text{depth} \geq 2$  at all closed points, and inducing  $\mathcal{M}_s$ . It is now easy to see that either property (i) or (ii) will still hold in a neighbourhood of  $s$ .

Unfortunately this proof involves a considerable technical background. The theorem just stated, together with your finiteness theorem, proves the following:

**Theorem.** *Let  $f: X \rightarrow S$  be flat projective with geometrically integral fibers, assume the fibers are of depth  $\geq d$  at closed points, and let  $d' = \text{Sup}(0, d - 2)$ . Then, in order to ensure quasi-compactness for a subset of  $\text{Pic}_{X/S}$  in terms of the coefficients of the Hilbert polynomials, one can neglect the  $d'$  last coefficients.*

If for instance the fibers of  $f$  are Cohen Macaulay and of dimension  $n$ , this means that one needs to look only at the first three coefficients (the first one being inessential anyhow, being the projective degree of the fibers). Does this statement become false if the fibers of  $f$  are not Cohen-Macaulay, and (say) normal of  $\dim 3$ ?

I doubt if there will be an occasion for me to expound the theory of formal moduli in a seminar, before Chap V is published. (Next year I will run a seminar together with Demazure on semi-simple group schemes, whereas my main interest will lie in developing (at last) Weil cohomology for schemes). I had noticed also, at the very start of my ponderings on the subject, that, in the case where “all obstructions vanish”, i.e. the functor one wants to represent is “simple”, the existence of formal moduli is immediate; the main point of the theory is of course to construct also *singular* formal modular varieties.

I include a copy of a letter to Hironaka, containing various questions. I would appreciate any comments you would make; Mike Artin has perhaps an idea on some of them. Besides, is Mike still in Cambridge? I wrote him lately to ask him to write us now a firm answer if he wants to come to Paris in 63/64, but did not get any answer. Perhaps you could give him a call about it, if he is still there.

Sincerely yours  
(signed) A Grothendieck

P.S. I have a few comments on Picard of a projective scheme over a field  $k$ , (which we may assume algebraically closed). First, if  $X$  is any scheme, denote by  $K(X)$  the usual group constructed with locally free sheaves on  $X$ , this is augmented into  $H^0(X, \mathbb{Z}) = \mathbb{Z}^{\pi_0(X)}$  (by rank), let  $I(X)$  be the kernel. Serre proved (in a very elementary way) that when  $X$  is quasi-compact and has an ample sheaf, and  $\dim X = d < +\infty$ , then  $I(X)^{d+1} = 0$ . This has various applications, for instance: let  $P$  be the group of invertible elements of  $K(X)$ ,

these are of the type  $1 + y$ ,  $y \in I(X)$ , consider the inclusion map  $P \rightarrow K(X)$ , this as a map of  $\mathbb{Z}$ -modules is a *polynomial map of degree  $\leq d$* . Therefore the natural map  $\text{Pic}(X) \rightarrow K(X)$  has the same property, and of course keeps it if we follow it by any linear map. Thus, if again  $X$  is projective over a field  $k$ , and if  $\mathcal{F}$  on  $X$  is coherent, then  $\mathcal{L} \rightsquigarrow \chi(\mathcal{F} \otimes \mathcal{L})$  is a polynomial map on the group  $\text{Pic}(X)$ . As Serre remarked some time ago (before Riemann-Roch was proved), from this follows that  $\chi(\mathcal{F} \otimes \mathcal{L})$  does not change if we replace  $\mathcal{L}$  by a sheaf which is congruent to it mod  $\text{Pic}^\tau(X)$ , in other words, for any  $\mathcal{F}$  and  $\mathcal{L} \in \text{Pic}^\tau(X)$ , we have  $\chi(\mathcal{F} \otimes \mathcal{L}) = \chi(\mathcal{F})$ . (Use the fact that  $\text{Pic}^\tau(X)$  has a composition series where the factors are divisible, or torsion groups. One can also prove this invariance of  $\chi$  by a direct argument, using still  $I^{d+1} = 0$  but not the polynomial type of  $\chi$ ). This proves for instance that under the conditions of your finiteness theorem for  $\mathcal{P}ic_{X/S}^P$  for a projective flat morphism  $f: X \rightarrow S$  (integral geometric fibers),  $\mathcal{P}ic_{X/S}^\tau$  is contained in one  $\mathcal{P}ic_{X/S}^P$  (provided  $S$  connected), and therefore of finite type over  $S$ , and moreover the pieces  $\mathcal{P}ic_{X/S}^P$  are stable under translation by  $\mathcal{P}ic_{X/S}^\tau$ .

Besides, there is a converse to the previous result, to the effect that “ $\tau$ -equivalence” is in fact *equivalent* to “numerical equivalence”, namely if  $\mathcal{L}$  invertible is such that for every coherent  $\mathcal{F}$ ,  $\chi(\mathcal{F} \otimes \mathcal{L}) = \chi(\mathcal{F})$  i.e.  $\chi(\mathcal{F}(\mathcal{L}-1)) = 0$ , then  $\mathcal{L} \in \text{Pic}^\tau(X)$ . Indeed, it is sufficient ( $\mathcal{O}(1)$  denoting as usual an ample sheaf on  $X$  relative to  $k$ ) to assume  $\chi(\mathcal{O}(n) \otimes \mathcal{L}^{\otimes m}) = \chi(\mathcal{O}(n))$  for any integers  $n, m$ . This means in fact that the sheaves  $\mathcal{L}^{\otimes m}$  have same Hilbert polynomial, hence remain in a quasi-compact subset of  $\mathcal{P}ic_{X/S}$ , which by definition means  $\mathcal{L} \in \text{Pic}^\tau(X)$ . (NB the argument supposes  $X$  integral, but it is easy to get rid of this assumption in the original statement). One interesting consequence of the last criterion, for a projective morphism  $f: X \rightarrow S$  as above: the subscheme  $\mathcal{P}ic_{X/S}^\tau$  is not only open, but also closed!

There seems to be another characterisation of  $\tau$ -equivalence to 0 for  $\mathcal{L}$  on a projective  $X/k$ . With the previous notations, note first that if  $\mathcal{L}$  is  $\tau$ -equivalent to 0 then, for any ample sheaf  $\mathcal{M}$  on  $X$ ,  $\mathcal{L} \otimes \mathcal{M}$  is again ample, therefore  $\mathcal{L}(1)$  and more generally the sheaves  $\mathcal{L}^{\otimes n}(1)$  must be ample. (This fact is well known and an easy consequence of the fact that for every neighbourhood  $U$  of 0 in  $\mathcal{P}ic^\circ = G$ ,  $U \cdot U = G$ ; a still simpler proof—in fact a trivial one—is obtained using your ampleness criterion, and the fact that  $\mathcal{L}$  is numerically equivalent to 0). I believe the converse should be true. Let  $V$  be the Néron-Severi group of  $X$  tensored by the reals, which is a finite dimensional vector space over  $\mathbb{R}$ , endowed with an open convex cone  $P$  (generated by ample sheaves), let  $\bar{P}$  be its closure. The previous conjecture would follow from the fact that  $\bar{P}$  does not contain any line (i.e.  $P$  does not come from a cone in a smaller quotient space...). Another way of stating this is that for any  $x \in P$ , the set  $P \cap (x - P)$  is relatively compact (which would yield an interesting finite-type criterion in  $\mathcal{P}ic$ ). Generally speaking, what facts are known to you concerning the shape of  $P$ ?

A last question about finiteness criteria. Consider the map  $\chi: V \rightarrow \mathbb{R}$  (polynomial of degree  $\leq d$ ) deduced from  $\chi: \text{Pic}(X) \rightarrow \mathbb{Z}$  by ring extension. Select an  $a \in P$  (corresponding to the choice of an ample sheaf  $\mathcal{O}(1)$ , for instance), then for many  $\xi \in V$ ,  $\chi(a + n\xi)$  is a polynomial with respect to  $n$ , say  $P_\xi(n)$  (the Hilbert polynomial of  $\xi$  with respect to  $a$ ). Let  $c_i(\xi)$  be its coefficients, which are polynomial functions in  $\xi$ . If  $\xi$  varies in  $V$  in such a way that the coefficients  $c_i(\xi)$  remain bounded, does  $\xi$  remain bounded (we now assume  $X$  irreducible)? Perhaps this is just a formal consequence of your finiteness result, (which corresponds to taking  $a, \xi$  in the original lattice of  $V$ ); this should be considered as a generalisation of the known fact that on the Néron-Severi space of a non singular surface, the intersection form has just one positive square. Of course, under suitable assumptions on the depth of  $X$  at closed points, one should be able to disregard some of the last coefficients  $c_i(\xi)$ , in the criterion of boundedness.

12 July, 1962

12 July, 1962

Dear Mumford,

I had a little more thought about finiteness questions for Pic, and have finally come to a solution of about all the questions I had met with.<sup>1</sup> The key facts I will state in a

**Theorem 1.** *Let  $S$  be a noetherian prescheme.*

- (i) *Let  $f: X \rightarrow Y$  be a surjective morphism of proper  $S$ -schemes, suppose  $\text{Pic}_{X/S}$  and  $\text{Pic}_{Y/S}$  both exist, then  $f^*: \text{Pic}_{Y/S} \rightarrow \text{Pic}_{X/S}$  is of finite type.*
- (ii) *Let  $Y$  be a projective  $S$ -scheme,  $X$  a “hyperplane section” i.e., the subscheme of zeros of a section of an invertible sheaf  $\mathcal{L}$  on  $Y$  ample relative to  $S$ . Assume again  $\text{Pic}_{Y/S}$  and  $\text{Pic}_{X/S}$  exist, then  $f^*: \text{Pic}_{Y/S} \rightarrow \text{Pic}_{X/S}$  is of finite type, provided all irreducible components of the geometric fibers of  $X/S$  are of dimension  $\geq 3$ .*

*(NB in other words, in both statements, a subset of  $\text{Pic}_{Y/S}$  is quasi-compact iff its image in  $\text{Pic}_{X/S}$  is. It is evident how to state these theorems so that they make sense without the assumption of existence for the Picard schemes, and the proofs work as well. The same remark holds for all other statements which seem to make use of the existence of certain Picard schemes. The proof shows also that in cases (i) and (ii), if  $S$  is the spectrum of a field, the morphism  $f^*$  is even affine).*

<sup>1</sup> An account of the finiteness theorems along the lines in this letter and the previous letter [1962July6] is in two expositions in SGA6, LNM 225, Springer-Verlag, 1971: Exposé XII, M. Raynaud, Un théorème de représentabilité relative sur le foncteur de Picard, p. 595–615. Exposé XIII S. Kleinman, Les théorèmes de finitude pur le foncteur de Picard, p. 616–666. Results on the Picard functors are explained in Chap. 8 of S. Bosch, W. Lütkebohmert & M. Raynaud, *Néron Models*, Springer-Verlag, 1990, and also in S. Kleinman, The Picard scheme, in *Fundamental Algebraic Geometry*, Amer. Math. Soc. 2005, 235–312.

- (iii) Let  $X$  be a projective  $S$ -scheme, with integral geometric fibers all of dimension  $n$ , endowed with a sheaf  $\mathcal{O}_X(1)$  very ample over  $S$ . In order for a subset  $M$  of  $\text{Pic}_{X/S}$  to be quasi-compact it is necessary and sufficient that, in the Hilbert polynomials  $a_0x^n + a_1x^{n-1} + \dots$  of the elements of  $M$ , the coefficients  $a_1$  and  $a_2$  remain bounded.  
 (NB It can be shown also that if we express the invertible sheaves on geometric fibers stemming from  $M$  in terms of Cartier divisors  $D$ , the condition is also equivalent with asking that  $D$  and  $D^2$  should have bounded projective degrees—this statement makes a sense even when the fibers are singular, because if  $D$  is a Cartier divisor one can give a meaning to  $D^k$  and  $\deg D^k$  for every  $k \dots$ )
- (iv) Let  $X$  be a proper  $S$ -scheme such that  $\text{Pic}_{X/S}$  exists. Then, for every integer  $n \neq 0$ , multiplication by  $n$  in this group prescheme is a morphism of finite type.

As a corollary of (i) and (iii) we get the following

**Corollary 1.** *Let  $X$  be proper over  $S$  such that  $\text{Pic}_{X/S}$  exists, then  $\text{Pic}_{X/S}^\tau$  is of finite type over  $S$ .*

Also, as a trivial consequence of (i) and (ii):

**Corollary 2.** *Under conditions (i) or (ii), if  $\mathcal{L}$  is an invertible sheaf on  $Y$ , then  $\mathcal{L}$  is  $\tau$ -equivalent to 0 iff its inverse image on  $X$  is. In other words, if  $k$  is an algebraically closed field and  $S$  its spectrum, denoting by  $LN(Y)$  the Néron-Severi group of  $Y$  mod torsion,  $LN(Y) \rightarrow LN(X)$  is injective, a fortiori for the Picard numbers  $\rho(Y) \leq \rho(X)$ .*

In the same way, using (i) to reduce to the projective case, (ii) to cut down the dimension of fibers to be  $\leq 2$ , then again (i) and resolution of singularities for a surface (over an algebraically closed field) to reduce to the case of a simple morphism, and lastly Néron's theorem, the Igusa-Picard inequality, and corollary 1 we get:

**Corollary 3.** *Let  $X$  be proper over  $S$ . Then the Néron-Severi groups of the geometric fibers of  $X/S$  are of finite type, and of bounded rank and bounded order for the torsion subgroups.*

I will give the idea of the proof of theorem 1. Logically, (i) comes first, (ii) uses a weaker version of (iii) and is needed itself to prove (iii) in full strength, (iv) uses (i) and corollary 1 (in the case  $X/k$  normal, to ensure that the kernel of multiplication by  $n$ ,  $n$  prime to the residue characteristic, is of finite type over  $S$  if  $S$  is the spectrum of a field  $k \dots$ ) hence to a certain extent (ii) and (iii) or some other known information as Néron's theorem, or finite generation of fundamental group.

The proof of (i) relies heavily on the ideas of non flat descent (expounded roughly in my Bourbaki talks), it is pretty natural although cumbersome in

details. At first sight, there seems to be a drawback because of the lack of criteria for effectiveness of descent data in the case of a non flat morphism (assumed to be of descent with respect to locally free sheaves say); if we had always effectivity we would be able to conclude that, if  $S \neq \emptyset$ , there exists in  $S$  a non empty open set  $U$  such that over  $U$  the morphism  $\mathcal{P}ic_{Y/S} \rightarrow \mathcal{P}ic_{X/S}$  is affine. I do not know if this is a true statement in general (as the schemes involved are not of finite type over  $S$ , it does *not* follow from the corresponding known fact over a field, when applied to the generic fiber. . .). However, having only in mind the finite type property, one gets along by remarking (for the simple types of morphisms  $f$  one can reduce to) that  $\mathcal{P}ic_Y \rightarrow \mathcal{P}ic_X$  can be factored through an  $S$ -prescheme  $Q$ , with  $Q \rightarrow \mathcal{P}ic_X$  an *affine* morphism, and  $\mathcal{P}ic_Y \rightarrow Q$  a *monomorphism*. Indeed,  $Q$  expresses the classification of invertible sheaves on  $X$  with descent data relative to  $f$ , (to give such descent data on a given  $\mathcal{L}$  on  $X$  is expressed in taking a section of a suitable scheme *affine* over  $S$ ), the fact that  $\mathcal{P}ic_Y \rightarrow Q$  is a monomorphism comes from the fact that we assume  $f$  a morphism of descent for invertible sheaves, (universally with respect to base changes  $S' \rightarrow S$ ). Now we are reduced to proving that  $\mathcal{P}ic_Y \rightarrow Q$  is of finite type, which amounts to verifying that if a descent datum on a given invertible sheaf  $\mathcal{L}$  on  $X$  induces on the generic fiber a non effective one, it is non effective on the neighbouring fibers as well—a very easy fact indeed.

For (ii) I use your finiteness theorem. However, it seems to me that your proof is incomplete at one point, namely when you conclude that (granting  $H^0$  of  $\dim > 1$  for all invertible sheaves considered) the effective divisors  $D$  yielding the sheaves  $\mathcal{L}$  remain in a quasi-compact subset of  $\mathcal{D}iv$ . In fact, we know only that the Hilbert polynomial for  $D$  is  $P_{\mathcal{O}_X} - P_{\mathcal{L}-1}$ , now it does not seem obvious to me that, from the assumption that the Hilbert polynomials  $P_{\mathcal{L}}$  remain bounded, the same is true for the polynomials  $P_{\mathcal{L}-1}$ . Therefore it seems that your argument applies only, a priori, if you know that the sheaves  $\mathcal{O}_D$  can be chosen in a way so as not to have embedded primes, (at least no embedded primes of dimension 0); indeed, from the induction assumption it follows at least that *except for the constant terms*, the coefficients of the Hilbert polynomials for the divisors  $D$  remain bounded, (and the case where the relative dimension of  $X/S$  is 0 or 1 does not offer any difficulty). If however the fibers of  $X/S$  satisfy Serre's property  $(S_2)$ , for instance are normal (the only case I will use in the proof of (ii)), then the  $\mathcal{O}_D$  are without any embedded primes, and we get through. Once (ii) is proved, one can recover your original statement without restriction, and in the stronger form of the theorem 1 (iii), as follows. By criterion (ii) one reduces easily to the case when  $X/S$  is of relative dimension 2 (in which case your version and mine agree). Of course, we can always reduce to proving quasi-compactness when restricting over some non empty open subset of  $S$ ,  $S$  integral. But with this restriction in mind, it is easily seen that we can find a finite morphism  $f : X' \rightarrow X$ , such that  $X'/S$  satisfies to the same conditions as  $X/S$ , but has moreover fibers satisfying  $(S_2)$ , and such that  $f$  induces on every geometric fiber an isomorphism except

at isolated points. (NB of course, the verification of this fact reduces to the case when  $S$  is the spectrum of a field; then the set  $Z$  of points where  $X$  is not  $S_2$ , i.e. not Cohen-Macaulay, is finite because  $X$  is integral of dim 2, and, denoting by  $i$  the inclusion  $U = X - Z \rightarrow X$ , we take  $X' = \text{Spec}(i_*(\mathcal{O}_U))$ . Moreover, an invertible sheaf on a fiber of  $X$  and its inverse image have the same Hilbert polynomial, except for a *fixed constant* (namely length of  $\mathcal{O}_{X'_s}/\mathcal{O}_{X_s}$ ). This way we are reduced to proving your criterion for  $X'$  instead of  $X$ , for which it is already known.

To prove (iv) we can assume  $n$  a prime, and are reduced to proving that, for every section of  $\mathcal{P}ic_X$  over  $S$ , its inverse image by  $n$  is of finite type over  $S$ . Now this inverse image is a formally homogeneous principal space over  ${}_n\mathcal{P}ic$ , which is of finite type over  $S$  by corollary 1. This reduces us to proving the following: if the fiber of this prescheme at the generic point  $s$  of  $S$  (assumed integral) is empty, so are the neighbouring ones. If  $n$  is distinct from the characteristic of  $k(s)$ , we can assume it is prime to all residue characteristics, then the scheme considered is étale over  $S$ , hence easily the conclusion. (NB in fact, a universally open morphism which is locally of finite type and has finite fibers is of finite type—thus we need only the part of corollary 1 stating that, over a field, the torsion of Néron-Severi killed by  $n$  prime to the characteristic is finite). If  $n$  is equal to char  $k(s)$ , we can assume  $S$  to be of characteristic  $n = p > 0$ , and then, using the Frobenius functor relative to  $S$ , we get a canonical factorisation of multiplication by  $p$  as

$$\mathcal{P}ic_{X/S} \xrightarrow{g} \mathcal{P}ic_{X^{(p)}/S} = (\mathcal{P}ic_{X/S})^{(p)} \xrightarrow{f^*} \mathcal{P}ic_{X/S} \quad (+)$$

where

$$f : X \rightarrow X^{(p)}$$

is the Frobenius morphism, and the first map in (+) is the Frobenius morphism for the prescheme  $P = \mathcal{P}ic_{X/S}$  over  $S$ . As the latter is locally of finite type over  $S$ , it follows that  $g$  is finite. Moreover  $f$  is finite and *surjective*, and therefore, by (i),  $f^*$  is of finite type. Hence  $f^*g$  is of finite type and we are through. (NB I proved first (iv) for a *simple* morphism, a few months ago, in this case  $f : X \rightarrow X^{(p)}$  is flat, and the theory of flat descent implies easily that  $f^*$  is *affine*, without using the more delicate theorem 1 (i)).

I did not solve in full generality the following problem: Let  $X/S$  be projective over  $S$ , such that  $\mathcal{P}ic_{X/S}$  exists, let  $M$  be a subset of  $\mathcal{P}ic_{X/S}$ , then prove  $M$  is quasi-compact iff there exists  $n$  such that

$$\mathcal{O}_X(-n) \leq M \leq \mathcal{O}_X(n)$$

(inequality with respect to the order relation on all fibers defined by the cone of ample sheaves). Using (i) and (ii), one can reduce to the case where  $X/S$  is of relative dimension 2 and with normal irreducible fibers. However, if  $S$  is the spectrum of a field (which we may assume alg. closed) the answer is affirmative, as results at once from the more general:

**Theorem 2.** *Let  $X/k$  be proper,  $k$  alg. closed. There exists a finite number of integral curves  $C_i$  in  $X$ , with normalisations  $C'_i$ , such that, for a subset  $M$  of  $\text{Pic}_{X/k}$  to be quasi-compact, it is necessary and sufficient that the numbers  $\deg \mathcal{L}_{C'_i}$  ( $\mathcal{L} \in M$ ) remain bounded.*

Proof: using (i) and (ii) we are reduced to the case where  $X$  is a normal irreducible surface. Using resolution of singularities for a surface, we can assume  $X$  non singular. In this case, the fact is known and results from (a) Néron's finiteness theorem (b) the fact that the fundamental bilinear form on  $LN(X)$  is non degenerate. [The latter results formally, besides, from the weak RR for surfaces and your finiteness theorem (which yields also, besides, the fact on the signature of the quadratic form): using RR, your criterion is equivalent with: putting  $D' = 2D + K$ , ( $K$  the canonical divisor), if  $D'^2$  and  $D'E$  remain bounded,  $D$  remains in a finite subset of  $LN(X)$ . This excludes the possibility of the bilinear form being degenerate, because the set of  $D \in LN(X)$  in the kernel of the bilinear form satisfies the finiteness criterion. Also (b) implies directly that  $LN(X)$  is free. Then Igusa's argument applies to yield the Igusa-Picard inequality, without using Néron's result. Thus corollary 3 of Theorem 1 (a common generalisation of Néron's and Igusa's result) is now proved without reference to Néron's result, (using heights etc.). I wonder if you are able to give a direct proof of Néron's theorem from your finiteness criterion, without using Igusa's involved argument (using the structure of the fundamental group of a curve), and to get rid in the proof of Theorem 2 of resolution of singularities of a surface. I would expect that this is possible, using the following argument. Let  $X$  be any complete surface over  $k$  (not necessarily normal), then using Serre's remark that  $I(X)^3 = 0$ , we get a canonical bilinear form in  $LN(X)$ , ( $X$  need not be projective, see below proof of (i)  $\implies$  (ii) in corollary 1), by setting

$$B(\mathcal{L}, \mathcal{L}') = \chi(\mathcal{O}_X) - \chi(\mathcal{L}) - \chi(\mathcal{L}') + \chi(\mathcal{L} \otimes \mathcal{L}')$$

(this definition, and the whole of intersection theory, generalizes to varieties of arbitrary dimension. . .). Now, if  $X$  is integral and proj., this form is non degenerate, and has just one positive square. (This statement of non degeneracy + Néron of course implies theorem 2). This is an easy consequence of resolution of the singularities of  $X$  and of theorem 1 (i), taking into account that the canonical bilinear form is compatible with the maps  $LN(X) \rightarrow LN(X')$  stemming from morphisms of degree 1,  $f : X' \rightarrow X$ . Do you have any idea of how to get rid, in the proof of non-degeneracy, of the resolution of singularities? What happens if  $X$  is not projective? (I used projectivity through the fact that there is at least one positive square for the bilinear form, and that any subspace, in a quadratic space of signature  $(1, s)$ , which contains one positive square, is non degenerate).]

As an easy consequence of Theorem 2, we get:

**Corollary 1.** *Let  $X/k$  be proper,  $\mathcal{L}$  an invertible sheaf on  $X$ . The following conditions are equivalent:*

- (i)  $\mathcal{L}$  is  $\tau$ -equivalent to zero.
- (ii) For every coherent  $\mathcal{F}$  on  $X$ ,  $\chi(\mathcal{F} \otimes \mathcal{L}) = \chi(\mathcal{F})$ .
- (ii bis) As before, with  $\mathcal{F} = \mathcal{O}_Y$ ,  $Y$  an integral curve contained in  $X$ .
- (iii) For every integral curve contained in  $X$ , letting  $Y'$  be its normalisation,  $\deg \mathcal{L}_{Y'} = 0$  (NB with notations of theorem 2, it is enough to take for  $Y$  one of the  $C_i$ ).  
And for completeness, if  $X/k$  is projective, I state the following equivalent conditions:
- (iv)  $\mathcal{L}^{\otimes m}(1)$  ample for every integer  $m$ .
- (v) (If  $X$  is integral)  $\chi(\mathcal{L}^{\otimes m}(n)) = \chi(\mathcal{O}(n))$  for every  $m, n$ , i.e. the sheaves  $\mathcal{L}^{\otimes m}$  all have same Hilbert polynomial.

Proof: (i)  $\Rightarrow$  (ii). By a devissage argument and Serre's result  $I(X)^{d+1} = 0$  for a quasi projective  $X$  of dimension  $d$ , one proves (without projectivity assumption, for any prescheme of finite type over  $k$ )  $I(X)^{d+1}K_{\bullet}(X) = 0$ , where  $K_{\bullet}(X)$  is the Grothendieck group for the category of all coherent sheaves on  $X$  (not only locally free ones as in the definition of  $K^{\bullet}(X)$ ;  $K_{\bullet}$  behaves covariantly for proper morphisms,  $K^{\bullet}$  contravariantly for arbitrary morphisms). It follows again that the map  $\mathcal{L} \rightsquigarrow \mathcal{L} \otimes \mathcal{F}$  from  $\text{Pic}(X)$  into  $K_{\bullet}(X)$  is polynomial of degree  $\leq d$  if  $\mathcal{F}$  is a coherent sheaf on  $X$ ; hence, if  $X$  is complete,  $\mathcal{L} \rightsquigarrow \chi(\mathcal{F} \otimes \mathcal{L})$  has the same property. From this, by Serre's remark, follows that the function is constant on classes modulo  $\text{Pic}^{\tau}(X)$ .

(ii)  $\Rightarrow$  (ii bis)  $\Rightarrow$  (iii) is trivial, (iii)  $\Rightarrow$  (ii) follows trivially from theorem 2, (i)  $\Rightarrow$  (iv) is known and (ii)  $\Rightarrow$  (v) trivial, (iv)  $\Rightarrow$  (i) results trivially from theorem 2, and (v)  $\Rightarrow$  (i) from your finiteness theorem. As I remarked in my previous letter, the criterion (v) is useful in order to prove the

**Corollary 2.** *Let  $f : X \rightarrow S$  be flat projective with integral geometric fibers then  $\text{Pic}_{X/S}^{\tau}$  is open and closed in  $\text{Pic}_{X/S}$ .*

This raises some questions: does the result remain true if we drop the projectivity assumption? Of course one is reduced to the case where  $S$  is the spectrum of a valuation ring, and one would like to apply the corollary 2 to theorem 1, and Chow's lemma; but there is a difficulty, as in Chow's lemma  $X'/S$  will not have integral geometric fibers, therefore the conclusion to be proved for  $X/S$  may be false for  $X'/S$  (example: a conic degenerating into two lines). In the previous corollary, is it enough to assume the geometric fibers of  $X/S$  irreducible (not necessarily reduced)? If  $X/S$  is normal i.e. flat with normal geometric fibers, is it true that  $\text{Pic}_{X/S}^{\tau}$  is proper over  $S$  (as is  $\text{Pic}_{X/S}^0$ )? This is equivalent with asking that  ${}_n\text{Pic}_{X/S}$  (kernel of  $n$ th power) should be proper over  $S$ , and I doubt it is true. In characteristic 0, this is equivalent with stating that the Néron-Severi torsion groups of the geometric fibers are of the same order (in fact, isomorphic) if  $S$  is connected; I doubt very much that this is true. (Of course, the point is that I do not assume  $X/S$  simple).

I now become aware I forgot to give indications for the proof of theorem 1 (ii). First, using (i), one can assume  $Y/S$  to be normal relative to  $S$ ,

with irreducible geometric fibers, and  $X$  equally flat over  $S$ , and distinct from  $Y$ , therefore a relative Cartier divisor. Moreover, replacing  $X$  by a suitable multiple and using (i), we can assume the ample sheaf  $\mathcal{L}$  (whose section gives  $X$ ) to be *very ample*, i.e.  $X$  is really a hyperplane section. Moreover, we can now assume (again by suitable base change) that there exists another hyperplane section  $X'/S$  which is normal over  $S$ , and is a relative Cartier divisor. Let  $M$  be a subset of  $\text{Pic}_{Y/S}$  whose image  $M_X$  in  $\text{Pic}_{X/S}$  is quasi-compact, then the Hilbert polynomials of the elements of  $M_X$  remain bounded, therefore the same is true for the Hilbert polynomials of the elements of  $M_{X'}$ . By your criterion *in the normal case*, it follows that  $M_{X'}$  is also quasi-compact. Then we can replace in the argument  $X$  by  $X'$ . Now as the fibers of  $Y/S$  are assumed of dimension  $\geq 3$ , these of  $X/S$  are of dimension  $\geq 2$ , and as the fibers of  $X/S$  and  $Y/S$  are normal, they are of depth  $\geq 2$  at their closed points. This is enough to use the “equivalence criteria” I alluded to in my last letter (the assumption that the geometric fibers of  $X$  be of depth  $\geq 3$  at their closed points being stronger than actually needed!), and to carry through the argument I indicated there. Ouf!

I hope my sketchy indications are clear enough to convince you, modulo the IHÉS seminar of this year. Of course I will send you a copy of the seminar as soon as everything is written up.

Sincerely yours  
(signed) A Grothendieck



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 18 July, 1962

July 18, 1962

My dear Murre,

I recently had some thoughts on finiteness conditions for Picard preschemes, and substantially improved on the results stated in the last section of my last Bourbaki talk.<sup>1</sup> The main result stated there, for a simple projective morphism with connected geometric fibers (namely that the pieces  $\mathcal{P}ic_{X/S}^P$  are of finite type over  $S$ ), has been extended by Mumford to the case where instead of  $f$  simple we assume only  $f$  flat with integral geometric fibers (at least if these are normal). Using his result (the proof of which is quite simple and beautiful) I could get rid of the normality assumption, and even (as in theorem 4.1. of my talk) restrict to the consideration of the two first non trivial coefficients of the Hilbert polynomials. The key results for the reduction are the following (the proofs being very technical, and rather different for (i) and (ii), except that (ii) uses (i) to reduce to the normal case; moreover (ii) uses Mumford's result and the equivalence criteria as developed in my last Seminar):

- (i) Let  $X, Y$  be proper over  $S$  noetherian, let  $f: X \rightarrow Y$  be a *surjective*  $S$ -morphism, assume, for simplicity of the statement, that the Picard preschemes exist, then  $f^*: \mathcal{P}ic_{Y/S} \rightarrow \mathcal{P}ic_{X/S}$  is of finite type (and in fact affine if  $S$  is the spectrum of a field), i.e. a subset  $M$  of  $\mathcal{P}ic_{Y/S}$  is quasi-compact iff its image in  $\mathcal{P}ic_{X/S}$  is.
- (ii) The same conclusion holds for a canonical immersion  $X \rightarrow Y$  if  $Y/S$  is projective, with fibers all components of which are of dimension  $\geq 3$ , and if  $X$  is the subscheme of zeros of a section over  $Y$  of an invertible sheaf  $\mathcal{L}$  ample with respect to  $S$ .

A connected result is that, for any  $X/S$  proper and integer  $n \neq 0$ , the  $n$ th power homomorphism in the Picard prescheme is of finite type.

I tell you about this, namely (i), because of the method of proof, involving of course considerations of non flat descent. The fact that I do not have any

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<sup>1</sup> Referring to TDTE VI (= Séminaire Bourbaki 1961/62, n°236).

good effectivity criterion does not hamper, by just recalling what the effectivity of a given descent datum means. Now it turns out that, by a slightly more careful analysis of the situation, one can prove the following theorem, of a type very close to the one you have proved recently, and to some you still want to prove as I understand it.

**Theorem.** *Let  $S$  be an integral noetherian scheme,  $X$  and  $X'$  proper over  $S$ , and  $f : X' \rightarrow X$  a surjective  $S$ -morphism. Look at the corresponding homomorphism for the Picard functors  $f^* : \text{Pic}_{X/S} \rightarrow \text{Pic}_{X'/S}$ . Assume:*

- a) *the existence problem  $A$  defined below for  $X/S$  has always a solution (this is certainly true when  $X/S$  is projective).*
- b) *the morphism  $f_s : X'_s \rightarrow X_s$  induced on the generic fiber is a morphism of descent, i.e.  $\mathcal{O}_{X_s} \rightarrow f(\mathcal{O}_{X'_s}) \rightrightarrows h(\mathcal{O}_{X'_s})$  is exact.*

*Then, provided we replace  $S$  by a suitable non empty open set, the homomorphism  $f^*$  is representable by a quasi-affine morphism, more specifically in the factorisation of  $f^*$  via the functor representing suitable descent data,  $f^* = vu$  with  $u$  affine and  $v$  a monomorphism (as you well know),  $v$  is in fact representable by a finite direct sum of immersions.*

**Corollary.** *Without assuming b), but instead in a) allowing  $X/S$  to be replaced by suitable other schemes  $X_i$  finite over  $X$ , the same conclusion holds, namely  $f^*$  is representable by quasi-affine morphisms.*

This follows from the theorem, using a suitable factorisation of  $f$ . For instance, using Chow's lemma and the main existence theorem in my first talk on Picard schemes,<sup>2</sup> one gets:

**Corollary 2.** *Assume  $X/S$  proper satisfies the condition: a') for every  $X'$  finite over  $X$ , there exists a non empty open subset  $S_1$  of  $S$  such that problem  $A$  for  $X'|_{S_1}$  has always solution (this condition is satisfied if  $X/S$  is projective). Then, provided we replace  $S$  by a suitable  $S_1$  non empty and open,  $\text{Pic}_{X/S}$  exists, is separated, and its connected components are of finite type over  $S$ .*

N.B. The proof does not give any evidence towards the fact that, in the theorem, one could replace "quasi-affine" by "affine". This is true however over a field, because a quasi-affine algebraic group is affine! It would be interesting to have a counterexample, say, over a ring of dimension 1 such as  $k[t]$ ,  $X$  and  $X'$  projective and simple over  $S$  and  $X' \rightarrow X$  birational, or, alternatively,  $X$  and  $X'$  projective and normal over  $S$  and  $f : X' \rightarrow X$  finite. A counterexample in the latter case would of course provide a counterexample to the effectivity problem for a finite morphism raised in my first talk on descent. . . .

"Problem A" is the following: given  $X/S$  and a module  $\mathcal{F}$  on  $X$ , to represent the functor on the category of  $S$ -preschemes taking any  $S'/S$  into a

<sup>2</sup> Referring to TDTE V (= Séminaire Bourbaki 1961/62, n°232).

one-element or into the empty set, according as to whether  $\mathcal{F}'$  on  $X'/S'$  is flat with respect to  $S'$  or not, where  $X' = X \times_S S'$ ,  $\mathcal{F}' = \mathcal{F} \times_S S'$ .

Given  $X/S$ , we say that "Problem  $A$  for  $X/S$  has always a solution" if, for every coherent  $\mathcal{F}'$  on some  $X'/S'$ , the previous functor on  $(\text{Sch})/S'$  is representable by an  $S'$ -scheme of finite type. The main step in my proof of existence of Hilbert schemes shows that this condition is satisfied when  $X/S$  is projective; in the proof, essential use is made of the Hilbert polynomial, in fact we get a solution as a disjoint sum of subschemes of  $S$  corresponding to various Hilbert polynomials. Still I would expect that the functor is representable as soon as  $X/S$  is proper. In view of the application we have in mind here, it would be sufficient (for any integral  $S$ ) to find in  $S$  a non empty open set  $S_1$  such that Problem  $A$  has always a solution for  $X_1 = X \times_S S_1$  over  $S_1$ . To prove this weaker existence result, it is well possible that a reduction to the projective case is possible, using Chow's lemma and some induction on the relative dimension perhaps. I also would expect that a proof will be easier when working over a complete noetherian local ring, hence the case of a general noetherian local ring by flat descent. And it is well possible that, putting together two such partial results, a proof of the existence in general could be obtained. (I met with such difficulties already some time ago in a very analogous non projective existence problem, which besides I have not solved so far!) This problem  $A$  has been met also by Hartshorne (a Harvard student), but I doubt he will work seriously on it. Thus I now write to you in the hope you may be interested to have a try at this problem. As a general fact, our knowledge of non projective existence theorems is exceedingly poor, and I hope this will change eventually.

Sincerely yours.  
(signed) A Grothendieck



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17 September, 1962

Bures Sept. 17, 1962

My dear Hartshorne,

I thank you very much for the notes on your work on Hilbert schemes. They strike me as very ingenious. The main result is striking, the methods of proof illuminating, the technical difficulties to be overcome quite serious—I am sure it will be a very good thesis indeed.<sup>1</sup> I did not check enough the hard part, namely Chap IV, but I am confident your constructions are all right. Moreover, I think your method should enable you to make a still closer analysis of the structure of  $\text{Hilb}^P$ , for instance to determine the irreducible components, their dimensions, and their mutual incidence relations. For instance, for every set of integers  $m_* = (m_1, \dots, m_r)$ , consider the locally closed subset  $M_{m_*}$  of  $M = \text{Hilb}^P$  of points having that invariant, then the irreducible components of  $M$  are among the closures of the irreducible components of the  $M_{m_*}$ 's. The first question one might try to solve is whether the  $M_{m_*}$ 's are irreducible, also to determine their dimension and the incidence relations between their closures, etc. [For given  $P, m$ , the results will probably be different according to the characteristic, for (according to Serre) there are components of  $\text{Hilb}_{\mathbb{Z}}$  lying over single primes.] Quite a few pieces of information along these lines seems already contained in your proof of the connectedness of  $M$  (cf. my P.S.). Perhaps such an analysis will lead you to solve (in the context of Hilbert schemes, replacing Chow varieties) Weil's problem whether the geometric irreducible components are already defined over the prime field; this would follow of course if you could prove that the  $M_{m_*}$ 's are geometrically irreducible. I recall the following remarkable implication of a positive answer to Weil's problem: if  $X$  is a non singular projective variety defined over the field  $\mathbb{C}$  of complex numbers,  $u$  an automorphism of  $\mathbb{C}$ , and  $X^u$  the variety over  $\mathbb{C}$  deduced from  $X$  by  $u$  (via the base change  $\mathbb{C} \xrightarrow{u} \mathbb{C}$ , or, equivalently, by applying  $u$  to the coefficients of the equations describing  $X$ ) then  $X$  and

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<sup>1</sup> This 1963 Princeton University thesis was published in R. Hartshorne, *Connectedness of the Hilbert scheme*, IHÉS Publ. Math. 29 (1966) 5–48.

$X^u$  are homeomorphic, hence have same homology and homotopy invariants. (At this moment, it is not known whether this statement is true.<sup>2</sup>)

Another type of problem to be investigated: consider the open subset  $M'$  of  $M$  corresponding to simple subvarieties of  $\mathbb{P}^r$ ; determine the irreducible components of  $M'$  (i.e. those of  $M$  that meet  $M'$ ), in particular determine for which Hilbert polynomial  $P$  we have  $M' \neq \emptyset$  i.e., there exists a *non singular* subvariety of  $X$  admitting this  $P$ . Here Borel's theorem will not help much, as  $M'$  is not complete.

One suggestion in case the invariants  $n_i$  are not enough to get hold of the components of  $M$ : there are various other invariants that may help, and have the same semi-continuity property, for instance the values of the Hilbert *function*, or even the integers  $\dim H^i(X, \mathcal{O}_X(n))$ , any  $i, n$ . I do not know anything about these, (except the semi-continuity and the fact that the alternating sum is continuous and a polynomial in  $n$ ), any other general information about this set of integers, for variable  $X$ , would be welcome.

Another problem, of a very different type, is to determine the category of locally free sheaves over  $M = \text{Hilb}_S^P$ , where  $S$  is for instance the spectrum of  $\mathbb{Z}$ , or of a prime field, or an algebraically closed field. This problem is trivially equivalent with finding all functors, associating to an  $S'$  over  $S$  and a subscheme  $X$  of  $\mathbb{P}_{S'}^r$ , with Hilbert polynomial  $P$ , a locally free sheaf  $\mathcal{E}$  on  $S'$ , in a manner compatible with base change. One way to get such functors is to take  $\mathcal{E} = f_*(\mathcal{O}_X(n))$ , with large  $n$ , (and also  $R^i f_*(\mathcal{O}_X(n))$  for suitable  $n$ , depending on  $P$  and  $i$ ), and those obtained from such sheaves  $\mathcal{E}$  by the usual tensor operations. For instance, taking exterior powers of maximal order, one gets various *invertible* sheaves  $\mathcal{E}_n$  ( $n$  large). A first question is to determine the relations between these  $\mathcal{E}_n$  (viewed as elements of the Picard group  $\text{Pic}(M)$  say), and to see if these generate the latter. Of course, in the above construction of  $\mathcal{E}$  by means of direct images of  $\mathcal{O}_X$  twisted by  $n$ , we could as well replace  $\mathcal{O}_X$  by any other sheaf  $\mathcal{F}$ , flat with respect to  $S'$ , and depending functorially on  $(S', X')$  (or what amounts to the same, a sheaf  $\mathcal{F}$  on the universal  $X = X_M$  over the modular scheme  $S' = M$ ,  $\mathcal{F}$  flat over  $M$ )—as one would get for instance starting with a locally free sheaf of  $\mathbb{P}_S^r$ , and inducing it on the subschemes  $X$  of  $\mathbb{P}_{S'}^r$ . In other words, one is led to investigate equally the category of locally free sheaves on  $X_M$ , and on  $\mathbb{P}_{S'}^r$ , and their various interrelations by means of direct and inverse images (and of course tensor operations). A complete picture (even for the category of locally free sheaves on projective space only) is probably quite out of reach for the time being. However if, instead of the full category of locally free sheaves, one is content to work with the ring  $K(M)$  generated by their elements (as studied in connection with the Riemann-Roch theorem in Serre-Borel's paper), it would be possible perhaps to achieve complete results. These would allow to determine at least the group  $\text{Pic}(M)$ , and presumably  $\text{Pic}(X_M)$ , in terms of  $K(M)$  and  $K(X_M)$ . Moreover, once one knows  $\text{Pic}(M)$ , one should determine

<sup>2</sup> See C. R. Acad. Sci. Paris 258 (1964) 4194–4196, for a counterexample by Serre.

for every  $\mathcal{L} \in \text{Pic}(M)$  the group  $H^0(M, \mathcal{L})$  (having an evident functorial interpretation, in terms of the functor corresponding to  $\mathcal{E}$ ), and of course the tensor operations  $H^0(\mathcal{L}) \times H^0(\mathcal{L}') \rightarrow H^0(\mathcal{L} \otimes \mathcal{L}')$ , in particular one should know the algebras  $\prod_{n \geq 0} H^0(\mathcal{L}^{\otimes n})$ , and have thus a complete insight into all possible projective embeddings of  $M$ . For the time being even  $H^0(M, \mathcal{O}_M)$  is not known, because, although  $M$  is geometrically connected, it is generally not reduced (even for curves in  $\mathbb{P}^3$  over a field of char. 0, according to Mumford), therefore it is not clear whether  $H^0(M, \mathcal{O}_M)$  as a ring has nilpotent elements or not! Of course, the knowledge of this ring alone implies your connectedness theorem; thus the questions raised here, which are concerned with  $M$  as a scheme and not only as a topological space, may turn out to be rather tough. It is not even clear whether or not  $\text{Pic}(M)$  is discrete. If  $S$  is the spectrum of an algebraically closed field, so that  $\text{Pic}(M)$  is the set of points rational over  $k$  of the Picard group-scheme  $\mathcal{P}ic_{M/k}$ , the question in char 0 amounts to the question if  $H^1(M, \mathcal{O}_M) = 0$  (in char  $p > 0$ , one can only say that the latter relation implies that  $\mathcal{P}ic_{M/k}$  is discrete, hence  $\text{Pic}(M)$  finitely generated). An argument in support of this conjecture (discreteness) would be that those invertible sheaves on  $M$  one gets from locally free sheaves on  $\mathbb{P}^r$  (by twisting with *large*  $n$ , inducing on  $X_M$ , taking the direct image, and highest exterior power) form a finitely generated group, as one sees using the fact that  $K(\mathbb{P}^r)$  is generated as a ring by the class of  $\mathcal{O}(1)$ . Thus at first sight I do not see a way of constructing a non constant continuous family of invertible sheaves on  $M$ !

I include in this letter some trivial comments on your notes. I am not sure I will find time very soon to work through the details of Chap 4, hoping that in your final version it will simplify a little? Is it possible to keep your manuscript?

Sincerely yours  
(signed) A Grothendieck

P.S.: It is known that Borel's fixed point theorem extends to an arbitrary ground field provided "solvable" is understood as 'solvable over  $k$ '; this applies to the triangular group in particular.



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2 October, 1962

Bures Oct. 2, 1962

Dear Mumford,

Thanks for your letter which has just arrived. I did not completely understand what you are after by looking at Chow points. However I can certainly help you in defining your map  $\mathcal{Hilb} \rightarrow \mathcal{Div Grass}$ .<sup>1</sup> As usual, I like to give a general setting. I confess I did not systematically write down all that I am going to state, but enough bits of it here and there, some time ago, to be sure it can be done without much effort.

Let  $X$  be a quasi-compact prescheme having an ample sheaf, so as to allow locally free resolutions of coherent sheaves. Let's denote by  $K(X)$  the ring of classes of locally free sheaves on  $X$ ; as well known this is also, as a group, the group of classes of coherent sheaves on  $X$  having finite projective dimension. Taking highest exterior powers of locally free sheaves, one gets a natural homomorphism:

$$\det : K(X) \rightarrow \text{Pic}(X)$$

which you called rightly the first Chern class. Thus  $\det(\mathcal{F})$  is also defined for any  $\mathcal{F}$  of finite cohomological dimension, and behaves multiplicatively with respect to exact sequences of such  $\mathcal{F}$ 's. To define it, take a locally free resolution of  $\mathcal{F}$  by  $\mathcal{L}_i$ 's, and take the alternating product of highest exterior powers. Besides, looking closer it turns out one can even define *functorially* (with respect to isomorphisms) in  $\mathcal{F}$  an invertible sheaf  $\det(\mathcal{F})$  on  $X$  this way, for instance an automorphism of  $\mathcal{F}$  defines one of  $\det(\mathcal{F})$ , i.e. a section of  $\mathcal{O}_X^*$ ! As a consequence of this remark, the definition of  $\det(\mathcal{F})$  does not really require global resolutions, and is valid on any locally ringed space whatever!

Next let  $f: X \rightarrow Y$  be a quasi-projective morphism, for simplicity choose a  $Y$ -immersion  $X \xrightarrow{j} \mathbb{P}_Y^r$ . A coherent sheaf  $\mathcal{F}$  on  $X$  is called "of finite projective

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<sup>1</sup> The proof sketched in this letter, with the details completed, appeared in J. Fogarty, *Truncated Hilbert functors*, J. Reine Angew. Math. 234 (1969) 65–88.

dimension relative to  $Y$ ” if it is of finite projective dimension on  $\mathbb{P}_Y^r$ . This is easily seen to be a purely local property on  $\mathcal{F}$  ( $\mathbb{P}_Y^r$  can be replaced by  $\mathbb{A}_Y^r$ ), independent of the chosen immersion. It is always satisfied if  $\mathcal{F}$  is flat with respect to  $Y$ . Assume  $\text{supp } \mathcal{F}$  proper over  $Y$ . Then one can define an element  $f_!(\mathcal{F})$  the following way,

$$f_!(\mathcal{F}) \in K(Y) :$$

The assumption implies that we can resolve  $\mathcal{F}$  on  $\mathbb{P}_Y^r$  by sheaves of the type  $g^*(\mathcal{E}_i)(n)$ , or rather sums of such, where  $\mathcal{E}_i$  on  $Y$  is locally free. (Indeed, we may assume  $\mathcal{F}$  free on  $\mathbb{P}_Y^r$ , and for  $n$  big, represent  $\mathcal{F}$  as a quotient of  $g^*(g_*(\mathcal{F}(n)))$ , but for such  $n$ ,  $g_*(\mathcal{F}(n))$  is locally free on  $Y$ ; going on this way, one shows one eventually gets a resolution of  $\mathcal{F}$  of the desired type). One then defines

$$f_!(F) = \sum_{\iota} (-1)^i \mathcal{E}_i \cdot g_!(\mathcal{O}_{\mathbb{P}_Y^r}(n))$$

$$\text{with } g_!(\mathcal{O}_{\mathbb{P}_Y^r}(n)) = \sum_{\iota} (-1)^i R^i g_*(\mathcal{O}_{\mathbb{P}_Y^r}(n)).$$

(NB  $g$  denotes the projection of  $\mathbb{P}_Y^r$  on  $Y$ ). Of course one verifies the independence of this definition from all choices performed. Besides  $f_!$  is characterized by the following properties:

- a) Additivity for exact sequences.
- b) Transitivity, if one has  $X \xrightarrow{f} X' \xrightarrow{g} Y$ , with  $g$  flat.
- c) If  $\mathcal{F}$  is flat with respect to  $Y$ , and all  $R^i f_*(\mathcal{F})$  are locally free, then

$$f_!(\mathcal{F}) = \sum_{\iota} (-1)^i R^i f_*(\mathcal{F})$$

Moreover, we have the following way to get  $f_!(\mathcal{F})$  up to torsion if  $\mathcal{F}$  is flat over  $Y$ :

- d) For big  $n$ , the locally free sheaf  $f_!(\mathcal{F}(n))$  on  $Y$ , as an element of the abelian group  $K(Y)$ , is a polynomial in  $n$ , whose constant coefficient is precisely  $f_!(\mathcal{F})$ . (In fact  $n \rightarrow f_!(\mathcal{F}(n))$  is a polynomial in  $n$  coinciding for big  $n$  with the previous function).

NB I convinced myself that the general Riemann-Roch theorem can be stated and proved for a morphism  $f: X \rightarrow Y$  which is quasi-projective and “a complete intersection” (i.e., such that  $X$  is a complete intersection in  $\mathbb{P}_Y^r$ ), for any sheaf  $\mathcal{F}$  which has the properties stated, allowing to define  $f_!(\mathcal{F})$ .

On the other hand, you have in mind how intersection theory can be phrased in terms of the ring operations of  $K(X)$ , which allow besides to get rid to a large extent of all regularity assumptions, provided we do not try to intersect any two cycles (because the sheaves they define will not be of finite cohomological dimension in general), but rather classes of sheaves instead.

Now your definition! Let  $X \hookrightarrow \mathbb{P}_Y^r$  be proper and flat over  $Y$ , with relative dimension  $\leq d$ , we want to associate to it a section of  $\mathcal{D}iv \mathcal{G}rass_{r-d-1}(\mathbb{P}_Y^r)$ , in

a functorial way with respect to base change, so as to have  $\mathcal{H}ilb \rightarrow \mathcal{D}iv\ Grass$ . We make the base change  $Y' \rightarrow Y$ , with  $Y' = \mathcal{G}rass_{r-d-1}(\mathbb{P}_Y^r)$ . Now in  $\mathbb{P}_{Y'}^r$ , we have canonically a projective subbundle  $M^{r-d-1} = M'$ , the structure sheaves of  $X'$  and  $M'$  can be viewed as coherent sheaves on  $\mathbb{P}_{Y'}^r$ , we can take the product of the elements they define in  $K(\mathbb{P}_{Y'}^r)$ , and take the image under  $g'_1 : K(\mathbb{P}_{Y'}^r) \rightarrow K(Y')$ , which is defined as  $\mathbb{P}_{Y'}^r$  is projective and flat over  $Y'$ . Now take the  $\text{dét}$ :

$$\mathcal{L} = \text{dét } g'_1(\mathcal{O}_{X'} \cdot \mathcal{O}_{M'})$$

to get an invertible sheaf on  $Y'$ . Let  $T = g'(X' \cap M')$ ,  $Y'_0 = Y' - T$ , then  $\mathcal{O}_{X'} \cdot \mathcal{O}_{M'}$  restricted to  $g'^{-1}(Y'_0)$ , is of course 0, therefore (as the definition of  $g'_1$  is local on the base), the sheaf  $\mathcal{L}|_{Y'_0}$  is trivial. Looking at it more closely, one even finds a *canonical* trivialisation of  $\mathcal{L}|_{Y'_0}$ ,  $\mathcal{L}$  being itself canonically defined as a sheaf (not only as an isomorphism class of sheaves). To make this precise, I should have defined more precisely  $g_1$  as associating, to an  $\mathcal{F}$  having the stated conditions, not only an element of  $K(Y)$ , but an object of the category of finite complexes of locally free sheaves on  $Y$ , with morphisms being the hyperext. Such an object of course defines an element of  $K(Y)$  by taking alternating sums of the components (which is the same if we replace the object by an isomorphic one), and thus taking the  $\text{dét}$  we get again an element of  $\text{Pic}(Y)$ ; but more precisely we have directly a functor  $\text{dét} : C \rightarrow \text{Inv}$  of the aforesaid category into the category of invertible sheaves (generalizing my remark on  $\text{dét}$  in the beginning). Moreover, the definition should be extended of course, assuming for simplicity  $X$  flat over  $Y$ , from a single  $\mathcal{F}$  to the category  $C(X)$  of finite complexes of locally free sheaves on  $X$ , getting thus *functors*  $C(X) \xrightarrow{g_1} C(Y) \xrightarrow{\text{dét}} \text{Inv}(Y)$ . Now in  $C(X)$  the tensor product is functorially defined, and going back to the situation with  $\mathcal{H}ilb$  etc, viewing  $\mathcal{O}_{X'}$  and  $\mathcal{O}_{M'}$  as defining objects of  $C(\mathbb{P}_{Y'}^r)$  (via resolutions), written  $\mathcal{O}_{X'}$  and  $\mathcal{O}_{M'}$  for simplicity, and taking their product and applying the functor  $\text{dét } g_1$ , we get  $\mathcal{L}$  in a functorial way. (Thus,  $X$  could be replaced by any sheaf  $\mathcal{F}$  on  $\mathbb{P}_Y^r$  flat with respect to  $Y$ , and  $\mathcal{L}$  depends functorially on such an  $\mathcal{F}$ ...). This then makes clear that we have a canonical section of  $\mathcal{L}|_{Y'_0}$ , coming from a canonical isomorphism  $\mathcal{O} \xrightarrow{\sim} (\mathcal{O}_{X'} \cdot \mathcal{O}_{M'}) |_{g'^{-1}(Y'_0)}$  and applying the previous functor to this. Now it is easy to verify (I hope) that this rational section of  $\mathcal{L}$  over  $Y' = \mathcal{G}rass$  is in fact everywhere defined, due to the fact that  $Y'$  is simple over  $Y$  and that  $Y'_0$  contains the generic points of the fibers of  $Y'$  over  $Y$ , and that one shows that, on every fiber of  $Y'$ , the section is regular and defines the usual Chow divisor (which of course I did not check). Of course, instead of taking  $\mathcal{G}rass$ , one could also take multiprojective space over  $Y$ , as does Chow, I do not know if the theory of Chow coordinates works the same using  $d+1$  hyperplanes, or a linear subspace of codimension  $d+1$  as you suggest, except that it is of course still true that a pure cycle of dimension  $d$  is determined by a Chow point in your version. Sticking to Chow's definition,  $M'$  would be an intersection of  $d+1$  hyperplanes which might at some points intersect excessively, therefore

we better keep the system of  $d+1$  divisors  $D_i$  and directly look at the product  $\mathcal{O}_{X'} \cdot \mathcal{O}_{D_1} \cdots \mathcal{O}_{D_{d+1}}$  instead of  $\mathcal{O}_{X'} \cdot \mathcal{O}_{M'}$ .

To compute  $\mathcal{L}$ , one can as well first induce  $\mathcal{O}_{M'}$  on  $X'$  (in the sense of course of the categories  $C(\mathbb{P}_{Y'}^r)$ ,  $C(X')$  i.e. first taking a locally free resolution of  $\mathcal{O}_{M'}$ ), then project on  $Y'$  by  $f'_!$ , where  $f': X' \rightarrow Y'$  is the projection, and take the *dét*. Working, for simplicity, with  $Y'$  the multiprojective scheme, the induced object in  $C(X')$  is a complex having as underlying graded module the exterior algebra of  $(\prod_{0 \leq i \leq d} M \otimes M_i)$  where  $M$  (respectively  $M_i$ ) is the

inverse image of  $\mathcal{O}_{\mathbb{P}_Y^r}(1)$  by the projection of  $\mathbb{P}_{Y'}^r \rightarrow \mathbb{P}_Y^r$  (respectively,  $\mathbb{P}_{Y'}^r \rightarrow Y' \rightarrow (i^{\text{th}} \text{ factor } \mathbb{P}_Y^r \text{ of } Y' = (\mathbb{P}_Y^r)^{d+1})$ ). Perhaps this may help to identify the sheaf you constructed on  $Y$  (as highest coefficient in the Hilbert polynomial expressing the function  $n \rightsquigarrow \det f_* \mathcal{O}_X(n)$  for big  $n$ , or equivalently  $n \rightsquigarrow \det f_! \mathcal{O}_X(n)$  for any  $n$ ), as the inverse image of a suitable ample sheaf of  $\text{Div}(Y')$  under the section we just defined of  $\text{Div}(Y')$ , as you suggest. I have no feeling whether such an interpretation is possible. These questions are of course related to the problem I proposed to Hartshorne a few weeks ago, namely to determine (over various ground schemes  $S$  such as  $\text{Spec}(\mathbb{Z})$ , the spectrum of a field or others) the complete structure of  $K(M)$  and  $K(X_M)$ , where  $M$  is a component of the *Hilb* scheme, and  $X_M \subset \mathbb{P}_M^r$  the universal flat subscheme of  $\mathbb{P}^r$ —together of course with the operations  $f^!$  and  $f_!$  coming from the projection  $f: X_M \rightarrow M$ . I made one or two wishful conjectures, including that  $K(M)$  is generated by invertible sheaves and that  $\text{Pic}(M)$  is “discrete”, but I grant I have no serious support for such conjecture. I wonder if you are able to compute  $H^0(M, \mathcal{O}_M)$  and  $H^1(M, \mathcal{O}_M)$  over a field, say  $k = \mathbb{C}$ . Is the first  $k$ , the second 0? Even the first question has no obvious answer, because of the existence of nilpotent elements in  $M$ . A good knowledge of  $M$  should even contain, not only  $\text{Pic}(M)$ , but also knowledge of the  $H^0(M, \mathcal{L})$  for  $\mathcal{L} \in \text{Pic}(M)$ , the tensor operations etc.

For the computations you have in mind it may be enough to know that, for a projective bundle  $\mathbb{P}$  associated to a locally free  $\mathcal{E}$  of rank  $r+1$  over a base  $Y$  admitting an ample sheaf,  $K(\mathbb{P})$  is completely determined (as a  $\lambda$  ring) by the fact that, as a module over  $K(Y)$ , it has a basis consisting of the classes of  $\mathcal{O}_{\mathbb{P}}(n) = \mathcal{L}^n$  with  $n_0 \leq n \leq n_0 + r$ . The most convenient is to take  $n_0 = -r$ , i.e., write uniquely an element  $x$  of  $K(\mathbb{P})$  as  $\sum_i c_i \mathcal{L}^i$  ( $-r \leq i \leq 0$ ), then  $f_!(x) = c_0$ . As for the ring structure of  $K(X)$ , known when  $\mathcal{L}^{-r-1}$  is known as a linear combination of the basis elements  $\mathcal{L}^{-i}$  ( $-r \leq i \leq 0$ ), it is obtained by writing simply  $\lambda_{-1}(\mathcal{E} - \mathcal{L}) = 0$ , i.e.  $\lambda_t(\mathcal{E})$  vanishes when substituting  $t$  by  $-\mathcal{L}^{-1}$ . From this, the  $K$  of various flag fiber spaces associated to  $\mathcal{E}$ , including grassmannians, can be determined in terms of  $K(Y)$ ,  $\mathcal{E} \in K(Y)$  in a purely formal way, as in the talks I gave in Chevalley’s Seminar. As *Div Grass* is essentially a projective fiber bundle over *Grass* (due to the fact that the Picard scheme of *Grass* is étale over the base, and any invertible sheaf on *Grass* is “cohomologically flat” over the base), the  $K$  of this scheme

is easily determined too. Of course, when you have an  $X \subset \mathbb{P}$  flat over  $Y$ , the invertible sheaf it defines on  $Y$  by our previous construction is easily computed in terms of the element  $\sum c_i \mathcal{L}^{-1}$  of  $K(\mathbb{P})$  defined by  $X$ . The fact that  $X$  has relative dimension  $\leq d$  over  $Y$  is then expressed by the fact that, if we take for  $K(\mathbb{P})$  the basis formed by the elements  $H^i$  ( $0 \leq i \leq r$ ), with  $H = 1 - \mathcal{L}$ , ( $H$  “hyperplane section”), then the coefficients  $c_i$  with  $i > r - d$  have augmentation 0. From this I guess your assertion about the  $\Delta^{k+1}$  should follow formally. Anyhow, I guess the story will be clearer if instead of an  $X$  you take a coherent sheaf on  $\mathbb{P}_Y^r$  flat with respect to  $Y$ , or complexes on  $\mathbb{P}_Y^r \dots$

To come back to your initial problem of moduli for projective invariants of varieties, I do not see the point in what you call “my best result so far”, concerning moduli for projective curves. Once you know the existence of a modular scheme for jacobian curves of level  $n$ , does it not follow trivially that there is a modular scheme for curves in  $\mathbb{P}^3$ , by taking the previous modular scheme, a suitable open subset of the Picard scheme of the modular curve over it, and a suitable open subset of some grassmannian scheme over the latter? I did not figure this out, therefore I wonder if there are some difficulties, or if you stick to your approach via “stable Chow forms” only because of the hope that some time that method might yield results on higher dimensional varieties?

I did not understand at all your suggestion concerning Murre’s theorem.<sup>2</sup> In fact, Murre has two theorems, one (the easier, the first he got) concerns a group functor which is embedded in a representable one; then his criterion does not need any Rosenlicht type condition. In this case your suggestion falls short, as Murre’s criterion applies also when  $H \rightarrow G$  is *not* a closed immersion, for instance is a monomorphism  $\mathbb{Z} \rightarrow G$ , where  $G$  is a group scheme of finite type over  $k$  such as  $\mathbb{G}_m$  say. On the other hand in his second criterion, concerning a functor which is not embedded in another, (this condition being replaced by the Rosenlicht condition), I do not see what your condition 4) could possibly mean. In any case, besides, the ground field need not be algebraically closed.

I guess you heard that Mike proved that, over  $\mathbb{C}$ , the Weil cohomology = usual cohomology. Pondering over his proofs this now appears almost trivial, moreover his method yields some basic results in arbitrary characteristic. The development of a large part of Weil cohomology now seems to me a mere routine matter, and I feel the complete equivalent of the classical theory, including Weil’s conjectures, should be obtained within the next one or two years. Just the typical Kähler-Hodge-Lefschetz type things will perhaps offer some serious difficulty.

Sincerely yours  
(signed) A Grothendieck

<sup>2</sup> The results were published in J. P. Murre, *On contravariant functors from the category of pre-schemes over a field into the category of abelian groups (with an application to the Picard functor)*, Publ. Math. IHÉS 23 (1964) 5–43.

P.S. I had an afterthought on the relative Cartier divisor you request on  $Y' = \text{Grass}$ ; I gave you one but no proof that it is a *positive* divisor. However as  $Y'$  is simple, hence *flat* over  $Y$ , it is enough to prove it is positive at points of  $Y'$  which are *divisorial* on the fiber of  $Y' \rightarrow Y$  (simple reasons of “depth”), and besides reduce to the case where  $Y$  is local artinian (and points  $y \in Y$  such that  $\dim \mathcal{O}_{Y,y} = 1$ ). This implies it is enough to look at what happens at generic points  $x'$  of the intersection  $\text{supp } \mathcal{F}' \cap M'$ . Now such a point will project onto a generic point of  $\text{supp } \mathcal{F}$ , hence  $\mathcal{F}'$  will have projective dimension  $r - d$  at it (i.e. will be Cohen-Macaulay). Moreover the elements of  $\mathcal{O}_{P',x'}$  defining  $M'$  as a complete intersection will form an  $\mathcal{F}'$ -sequence, so that the higher  $\text{Tor}_i^{\mathcal{O}_{P',x'}}(\mathcal{F}', \mathcal{O}_{M'})$  vanish, and  $\mathcal{F}' \otimes \mathcal{O}_{M'}$  is of projective dimension  $(r - d) + (d + 1) = r + 1$ . Moreover,  $\text{supp } (\mathcal{F}' \otimes \mathcal{O}_{M'})$  will be finite over  $Y'$  when localizing at such a point  $y'$ . From this it easily follows that  $f'_1(\mathcal{F}' \otimes \mathcal{O}_{M'})$  is (on this neighbourhood of  $y'$ ) just the usual  $f'_*(\mathcal{F}' \otimes \mathcal{O}_{M'})$ , and the latter is at  $y'$  of cohomological dimension  $(r + 1) - r = 1$  where  $r$  is the rel. dimension of  $P'/Y'$ . But, as you already noticed, such a sheaf on  $Y'$  *does* define a positive Cartier divisor at  $y'$ . This concludes the proof! I wonder if there is something simpler to do it?

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 18 October, 1962

Bures 18.10.1962

Dear Mumford,

Thanks for your letter. Unfortunately, I have no idea how to rigidify in general polarized varieties with no infinitesimal automorphisms in a discrete way. Anyhow, didn't you give me once an example with no automorphisms whatsoever (infinitesimal or finite) for which there was no reasonable local modular family? In this context, I never really developed (in the spirit of my talk on formal moduli) the question of a *modular field* for say an algebraic scheme  $X$  over some field  $k$ , s.t.  $H^0(X, \mathfrak{G}_X) = 0$ , and, for simplicity, the group of automorphisms  $\Gamma$  of  $X$  being finite (say, by imposing if needed a polarization on  $X$ , or some other extra structure). To such an  $X$  there should be associated something like

- a.) A field  $k_0$ , finitely generated over the prime field
- b.) A galois extension  $k_1$  of  $k_0$ , with group  $\Gamma$
- c.) A scheme  $X_1/k_1$ , such that for  $g \in \Gamma$ , then  $\exists k_1$ -isomorphism  $X_1^g \simeq X_1$   
(but of course, no descent data to  $k_0$ !)
- d.) An isomorphism  $X_{1,\Omega} \simeq X_\Omega$ , where  $\Omega$  is some common (big) extension of  $k_1$  and  $k$ .

This data a.), b.), c.), in terms of  $X$  should be canonically definable, independent from field extension or  $k$ ,  $k_0$  should be contained in any "field of definition"  $k$  of  $X$ , and, if  $k$  is algebraically closed,  $k_0$  should be something like the field of invariants of all  $\sigma \in \text{Aut } k$  such that  $X^\sigma \simeq X$ , at least up to inseparability. In the rigid case,  $\Gamma = e$ ,  $k_0$  will be just the *smallest* field of definition for  $X$ , and  $X$  comes in a unique way from an  $X_0$  over  $k_0$ . Did you ever try to work out these things? If you do not assume  $H^0(X, \mathfrak{G}_X) = 0$ ,

something should still be feasible, replacing Galois extensions by principal homogeneous spaces under an algebraic group of automorphisms.

Your result of finiteness on polarized non singular surfaces<sup>1</sup> is quite interesting. I would appreciate very much to have an idea of the proof.

It seemed to me, looking at Mike's arguments, that his lemma on divisor classes (using resolutions) can be completely eliminated. Unfortunately, for the time being, everything is tied to equal characteristics (in fact, even to *algebraic* schemes). The key lemma is the following one:

If  $X^n$  is non singular over a field  $k$ , alg. closed, then every point has an open neighbourhood  $\mathcal{U}$  having the following structure: There exists

$$\mathcal{U} = \mathcal{U}_n \xrightarrow{f_n} \mathcal{U}_{n-1} \longrightarrow \dots \longrightarrow \mathcal{U}_1 \xrightarrow{f_1} \mathcal{U}_0 = \text{Spec}(k)$$

where every  $f_i$  is an "elementary" morphism; namely obtained from a simple proper morphism  $g : V \longrightarrow W$  with geometric fibers connected of dim 1 ( $V$  a nice relative curve over  $W$ ) by removing an étale multisection  $Z$ . Thus, from the point of view of topology,  $\mathcal{U}$  is remarkably simple, its universal covering being *contractible* and its  $\pi_1$  a successive extension of free groups [hence  $\mathcal{U} \simeq B\pi_1$ , the classifying space of such a group  $\pi_1$ !]

Sincerely yours,  
(signed) A Grothendieck

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<sup>1</sup> This refers to the second main theorem in [64].

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14 February, 1963

Bures Feb 14, 1963

My dear Mike,<sup>1</sup>

I was just going to write you when I got your letter. First I want to ask you if you feel like refereeing Néron's big manuscript on minimal models for abelian varieties<sup>2</sup> (it has over 300 pages). I wrote to Mumford in this matter, who says he will have no time in the next months, do you think you would? Otherwise I will publish it as it is, as it seems difficult to find a referee, and the stuff is doubtlessly to be published, even if it is not completely OK in the details.

I started thinking on the cohomology of schemes, after reading your notes which I find quite useful. (As for comments of detail, we will discuss about it when you are here and we are organizing the seminar). I got a few results:

- 1) Let  $f$  be a *proper* morphism of locally noetherian schemes then, for any torsion sheaf  $\mathcal{F}$  on  $X$ , formation of  $R^i f_*(\mathcal{F})$  commutes with arbitrary base-extension. (It is equivalent to state that, for  $Y$  strictly local, i.e. the spectrum of a local hensel ring with separably closed residue field, the maps  $H^i(X, \mathcal{F}) \rightarrow H^i(X_0, \mathcal{F}_0)$ , where  $X_0$  is the special fiber, are isomorphisms).
- 2) Let  $f$  be as above, assume  $\mathcal{F}$  a constructible torsion sheaf (constructible means that, for any  $x \in X$ , the restriction of  $\mathcal{F}$  to the closure  $Z$  of  $x$  is given by an étale group-scheme over a non empty open set of  $Z$ . It is equivalent to say when  $X$  is noetherian that  $\mathcal{F}$  is a noetherian object of the category of sheaves on  $X \dots$ ). Then the sheaves  $R^i f_*(\mathcal{F})$  are constructible.

The same should hold if  $f$  is only assumed to be of finite type, provided  $\mathcal{F}$  is prime to the residual-characteristics. By virtue of 2°), it is enough to show it for an open immersion  $U \rightarrow Y$  and  $\mathcal{F} = \mu_n$ . Whenever resolution of singularities is available, one is even reduced to the case where  $Y$  is regular,

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<sup>1</sup> Letter to Michael Artin.

<sup>2</sup> Published in Publ. Math. IHÉS 21 (1964) 361–484.

and  $U$  the complement of a divisor having only normal crossings, and then it would follow from the conjectural statement about the  $R^i f_*(\mu_n)$  when  $U$  is the complement of a regular divisor. Thus, using your local result, we get:

- 3) Let  $f$  be a morphism of finite type of locally algebraic preschemes over a field of char 0,  $\mathcal{F}$  a constructible torsion sheaf on  $X$ , then the sheaves  $R^i f_*(\mathcal{F})$  are constructible.

The same technique yields the comparison theorem:

- 4) Under the conditions of 3), assume the ground field is  $\mathbb{C}$ , the field of complex numbers. Then formation of  $R^i f_*(\mathcal{F})$  is compatible with passing to the underlying “usual” topological spaces and sheaves.

The result 1) on base extension should be true if we drop the properness assumption, assuming instead that  $\mathcal{F}$  is prime to the residue characteristics, and that the base change  $Y' \rightarrow Y$  is “regular”, namely flat with geometrically regular fibers. This would be applicable to situations like  $Y$  a “good” local ring, and  $Y'$  its completion, or situations deduced from this one by base extension on  $Y$ , which would be a nice thing to have in order to know once for all that, for instance, for cohomological purposes, a “good” hensel ring can always be replaced by its completion. For the time being I cannot prove that general result, even in characteristic 0 (NB when resolution of singularities is available, it can be shown to be equivalent with the statement about the regular divisor in a regular scheme. . .). However, using the local Lefschetz techniques, I proved your “key lemma” without resolution of singularities, and from this:

- 5) The conclusion of 1) remains valid when dropping the properness assumption, assuming  $f$  of finite type,  $\mathcal{F}$  prime to the residue characteristics, and the base change morphism  $Y' \rightarrow Y$  *simple* (which means regular and locally of finite type).

This implies the usual result on the cohomological structure of a regular scheme and a regular divisor in it in various “relative” cases. Using this, and 1), one gets in a pretty formal way:

- 6) Let  $f: X \rightarrow Y$  be proper and simple,  $G$  a commutative group scheme over  $X$ , finite and étale over  $X$ , (we say that the sheaf defined by  $G$  is “locally constant”), prime to the residue characteristics. Then the sheaves  $R^i f_*(G)$  on  $Y$  are equally locally constant. The same holds true if we replace  $X$  by  $X - Z$ , where  $Z$  is a closed subscheme of  $X$  simple over  $Y$  (but of course  $G$  has to be defined on the whole of  $X$ ).

Truth to tell, I checked this only when  $Y$  is the spectrum of a discrete valuation ring, but I think from this and 2) the general result should follow. I think also that 1) will yield the Künneth formula for a product, over a field, of two preschemes one of which is proper; of course, the same should hold true without properness, sticking to coefficients prime to the characteristic. Of course, the main interest of 6) is to allow computations of cohomology in

characteristic  $p > 0$  from transcendental results in characteristic 0, just as for the fundamental group. Besides, the main steps in the key results 1) and 5) are the analogous statements on fundamental groups. The main techniques I developed so far in algebraic geometry have to be used: the existence theorem on coherent algebraic sheaves, non flat descent, Hilbert and Picard schemes (the latter for nice relative curves only), Lefschetz techniques. Thus it was not so silly after all to postpone Weil cohomology after all this.

Here is what I can say about the Brauer group  $\text{Br}(X)$  of a prescheme (more generally, a ringed space). We define it as the group of classes of Azumaya algebras over  $X$ , two such algebras  $A$  and  $B$  being considered equivalent if there exist locally free sheaves  $\mathcal{E}, \mathcal{F}$  on  $X$  and an isomorphism

$$A \otimes \text{End}(\mathcal{E}) \simeq B \otimes \text{End}(\mathcal{F})$$

or, what amounts to the same, if there is a locally free  $\mathcal{E}$  and an isomorphism

$$B^0 \otimes A \simeq \text{End}(\mathcal{E}) .$$

Viewing an Azumaya algebra of rank  $n^2$  as being defined by an element of  $H^1(X, PGL(n))$  (NB étale “locally finite” topology<sup>3</sup>), and using the obstruction (coboundary) map corresponding to the exact sequence

$$e \rightarrow \mathbb{G}_m \rightarrow GL(n) \rightarrow PGL(n) \rightarrow e ,$$

one obtains a homomorphism

$$c: \text{Br}(X) \rightarrow H^2(X, \mathcal{O}_X^*)$$

which is always injective (as results formally from the fact that the vanishing of the obstruction means the possibility of lifting the structure sheaf to  $GL(n)$ ). Denoting by  $X_Z$  the prescheme  $X$  with the Zariski topology, and using the map  $f : X \rightarrow X_Z$  and the Leray spectral sequence, we get

$$0 \rightarrow H^2(X_Z, \mathbb{G}_m) \rightarrow H^2(X, \mathbb{G}_m) \rightarrow H^0(X_Z, R^2 f_*(\mathbb{G}_m)) \rightarrow H^3(X_Z, \mathbb{G}_m)$$

(using  $R^1 f_*(\mathbb{G}_m) = 0$ ); if  $X$  is regular this shows (using  $H^2(X_Z, \mathbb{G}_m) = 0$  for  $i \geq 2$ ):

$$H^2(X, \mathbb{G}_m) = H^0(X_Z, \mathcal{B}r_X) \quad (X \text{ regular})$$

where  $\mathcal{B}r_X = R^2 f_*(\mathbb{G}_m)$  is the sheaf on  $X$  whose fibers are the groups

$$H^2(\text{Spec}(\mathcal{O}_{X,x}), \mathbb{G}_m) = \text{Br}(\text{Spec}(\mathcal{O}_{X,x})).$$

The last equality comes from

$$\text{Br}(X) \simeq H^2(X, \mathbb{G}_m) \quad \text{if } X \text{ local,}$$

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<sup>3</sup> This is the topology (etf), “topologie étale finie”; see SGA3 Exposé IV 6.3.

as you noticed, since, by the choice of the topology, we have

$$H^i(X, \mathcal{F}) \simeq H^i(\pi, H^0(\bar{X}, \mathcal{F})) \quad (*)$$

for any sheaf  $\mathcal{F}$ ,  $\bar{X}$  being the universal covering. (NB I do not know if we would obtain the same result taking the étale topology you like best; this amounts to the question whether, for  $X$  local,  $H^2(\bar{X}_{\text{Mike}}, \mathbb{G}_m) = 0$ . Did you check this result, at least for  $X$  regular?). In fact, now that I am writing about it, I get aware that (lacking the foundations on the étale locally finite topology, which I grant is not too nice), I do not even know if (\*) above is true, therefore I do not know even for  $X$  local if  $\text{Br}(X) = H^2(X, \mathcal{O}_X^*)$ . I wonder even if by chance the category of sheaves for the two topologies (the étale and étale locally finite) are not equivalent, at least for  $X$  normal say, so that the cohomological theory is the same for both; remember that the covering families I take are by no means closed under composition, and that by saturating we may well come very close to the good étale topology of yours. Anyhow, all I stated before is good taking any topology between the étale locally finite and the flat quasi-compact one, the latter gives the “largest”  $H^2(X, \mathbb{G}_m)$  (NB they are included ones in the others), and, I hope, all  $H^i(X, \mathbb{G}_m)$  should be the same in all these topologies, at least for the étale and the quasi-finite and flat one. The flat topologies have the advantage that, for any  $n$ , we have the exact sequence

$$e \rightarrow \mu_n \rightarrow SL(n) \rightarrow PGL(n) \rightarrow e;$$

this shows for instance that the part of  $H^2(X, \mathbb{G}_m)$  (and hence of  $\text{Br}(X)$ ) coming from  $H^1(X, PGL(n))$  comes in fact from  $H^2(X, \mu_n)$  and hence is annihilated by  $n$ . If you write this as meaning that  $A \otimes A \otimes \dots \otimes A \simeq \text{End}(\mathcal{E})$ , some  $\mathcal{E}$ , for any Azumaya Algebra  $A$  of rank  $n^2$ , the tensor product being  $n$ -fold, I do not see any direct geometric description of the  $\mathcal{E}$  in terms of  $A$ !

As for your question whether  $\text{Br}(X) = H^2(X, \mathbb{G}_m)$  in general, I very much doubt it is true, even if  $X$  is regular.<sup>4</sup> Granting it is true if  $X$  is local and regular, this would mean that for variable  $U$  on a regular prescheme  $X$ ,  $U \rightsquigarrow \text{Br}(U)$  is a sheaf on  $X$ , or also that for any two open sets  $U, V$  and elements of  $\text{Br}(U), \text{Br}(V)$  that match in the intersection, there is an element of  $\text{Br}(U \cup V)$  inducing them. Granting the standard local results (which are proved in algebraic geometry over a field), this would imply that whenever  $Y$  is a closed subset of codimension at least 2 in  $X$ , and  $u$  an element of  $\text{Br}(X - Y)$ , it comes from an element of  $\text{Br}(X)$ . All I could do along these lines is remark that any element of  $H^2(X, \mathbb{G}_m)$  can be represented by an element of  $\text{Br}(U)$ , where  $U = X - Y$  with  $Y$  of codimension  $\geq 3$  (using the fact that a reflexive module over a regular local ring of dimension 2 is free). Thus the first counterexample should be looked for in dimension 3. Here is a suggestion for a “universal” counterexample over  $\mathbb{C}$ : take the Eilenberg-Mac Lane space  $K(\mathbb{Z}/n\mathbb{Z}, 2)$ , approximate it homotopically up to dimension  $d \geq 2$  by a non singular variety,

<sup>4</sup> See 7) P.S. of [1963February23] for a counter-example.

(this is possible, if I remember well, by a construction of Atiyah), and take the canonical class in  $H^2(X, \mathbb{Z}/n\mathbb{Z})$  and its image in  $H^2(X, \mathbb{G}_m)$ . The idea is that perhaps one can give some non empty necessary topological conditions on an element of  $H^2(X, \mathbb{Z}/n\mathbb{Z})$  to come from a projective (topological) bundle, or rather for an element of  $H^3(X, \mathbb{Z}) \simeq H^2(X_{top}, \mathbb{G}_m)$  to be the Bockstein coboundary of such an element of  $H^2(X, \mathbb{Z}/n\mathbb{Z})$ . To be precise, ask Bott (say) if the Bockstein of the canonical  $H^2(K(\mathbb{Z}/n\mathbb{Z}, 2), \mathbb{Z}/n\mathbb{Z})$  can be defined by a projective bundle on  $K(\mathbb{Z}/n\mathbb{Z}, 2)$ , as an obstruction to lifting to  $GL(n)$ , i.e. as the inverse image of a certain obvious canonical class in  $H^3(B_{PGL(n)}, \mathbb{Z}/n\mathbb{Z})$ . If not, we get the expected counterexample. . . <sup>5</sup>

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<sup>5</sup> What we have of the letter ends here, without a signature. The editors



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**21 February, 1963**

Bures 21.2.1963

My dear Mumford,

I think I can give an affirmative answer to your question. First note that, to give a functor of the kind you say, is equivalent to giving a functor  $F: (\text{Sch}/S)^\circ \rightarrow (\text{Ens})$  endowed with a structure of  $\mathcal{O}$ -module, where  $\mathcal{O}$  is the functor  $T \rightsquigarrow \Gamma(T, \mathcal{O}_T)$ , and such that  $F$  be of “local type”, i.e. for every argument  $T$ ,  $U \rightsquigarrow F(U)$  for  $U$  an open set of  $T$  is a sheaf (of modules) on  $T$ , and that moreover the previous sheaf be coherent. Your problem then is whether  $F$  is representable (in the usual sense) by a vector bundle, in a way to respect the module structures. Of course, the question is local on  $S$ , so we may assume  $S$  affine for simplicity, say  $S = \text{Spec}(A)$ . It turns out that, in practice, a functor  $F$  as above is in fact always defined via a functor  $M \rightsquigarrow G(M)$  from arbitrary  $A$ -modules to abelian groups (or  $A$ -modules, this amounts to the same), by putting  $F(T) = G(B)$  if  $T = \text{Spec}(B)$ , and deducing  $F(T)$  in general by recollement. I guess there should be a simple way of expressing in general equivalence between giving an  $F$  or a  $G$ , via “Nagata’s trick” for instance, using, to define  $G(M)$  in terms of  $F$ , the algebra  $D_A(M) = A \oplus M$  ( $M$  ideal of square zero) . . . but in fact I do not care too much, as in practice one has a direct hold of  $G$ . The question therefore becomes to characterize (given a noetherian ring  $A$ ) the covariant functors  $C_A \rightarrow (\text{Ens})$  ( $C_A =$  category of  $A$ -modules) which are representable by a module of finite type. (To say that this functor comes from a functor with values in the category  $(\text{Ab})$  of abelian groups just means  $G$  is additive, i.e. transforms finite products into products). Here is a set of necessary and sufficient conditions, which give the answer in those cases I have needed so far:

- 1)  $G$  commutes with filtering direct limits.
- 2)  $G$  is left exact (which means also: additive, and left exact in the sense of additive functors of abelian categories). Of course, due to 1), it is enough to check left exactness for arguments of finite type over  $A$ .

- 3) For every noetherian algebra  $B$  over  $A$ , separated and complete for some  $J$ -adic topology, and every module of finite type  $M$  over  $B$ , the map

$$G(M) \rightarrow \varprojlim G(M/J^{n+1}M)$$

is an isomorphism.

- 4) For every noetherian algebra  $B$  over  $A$ , and  $M$  a module of finite type over  $B$ ,  $G(M)$  is a module of finite type over  $B$ . (In fact, it is enough to check it for  $B = A$ ).
- 5) For every ideal  $J$  such that  $A/J \neq 0$ , there exists a non nilpotent element  $f$  in  $A/J$  such that, if we put  $B = (A/J)_f$ , the “induced functor”  $G_B : C_B \rightarrow (\text{Ens})$  be representable by a  $B$ -module of finite type.

Conditions 1), 2), 3) are “exactness conditions”, in 3) it is enough to take the case where  $B$  is either the completion of a local ring of  $A$  for the usual topology, or the completion of  $A$  itself for some ideal  $J$ . 4) is a simple finiteness condition. In practice 3) and 4) are verified by the standard theorems of the type “finiteness” “comparison” “existence” of EGA III, whereas 1) and 2) are about trivial. Condition 5), of “generic representability”, is more delicate to verify in the applications. Restricted to prime ideals  $J$ , this condition is equivalent to

- 5a) The function  $\mathfrak{p} \rightsquigarrow \text{rank}_{k(\mathfrak{p})} G(k(\mathfrak{p}))$  on  $\text{Spec}(A)$  is constructible. However, this does not imply 5) in general. If  $A$  is quotient of a regular ring (harmless condition, by standard reduction steps to algebras of finite type over  $\mathbb{Z}$ !), 5) is equivalent to the following:
- 5b) For every ideal  $J$  in  $A$  such that  $B = A/J \neq 0$ , and every module  $\Omega$  of finite type over  $A/J$ , there exists  $f \in B$ , non nilpotent, with the following property: for every prime  $\mathfrak{p} \not\supset f$  of  $B$  and every regular sequence  $(f_i)$  of parameters of  $B_{\mathfrak{p}}$ , the canonical homomorphism

$$G(\Omega_{\mathfrak{p}}) \otimes_B (B_{\mathfrak{p}} / (\sum_i f_i B_{\mathfrak{p}})) \longrightarrow G(\Omega_{\mathfrak{p}} \otimes_B (B_{\mathfrak{p}} / \sum_i f_i B_{\mathfrak{p}}))$$

is an isomorphism.

(NB This condition is anyhow necessary, even if  $A$  is not a quotient of a regular ring, and in the stronger form where one does not assume the  $f_i$  to be a whole system of parameters).

The main application I had in mind was in the following situation: let  $f: X \rightarrow Y$  be a proper morphism ( $Y$  locally noetherian),  $\mathcal{E}, \mathcal{F}$  two coherent modules on  $X$ , with  $\mathcal{F}$  flat with respect to  $Y$ , and consider the functor

$$\mathcal{M} \rightsquigarrow \text{Hom}_{\mathcal{O}_X}(\mathcal{E}, \mathcal{F} \otimes_Y \mathcal{M}).$$

This functor is representable by a coherent sheaf  $\mathcal{P}$  over  $Y$  ( $\mathcal{M}$  is a variable quasi-coherent sheaf on  $Y$ ). Same is true for  $\text{Ext}_{\mathcal{O}_X}^i(X; \mathcal{E}, \mathcal{F} \otimes \mathcal{M})$ , assuming

for simplicity  $Y$  affine, provided  $\text{Ext}_{\mathcal{O}_X}^{i-1}(X; \mathcal{E}, \mathcal{F} \otimes \mathcal{M})$  is identically zero (to ensure left exactness). In the Harvard seminar I gave another simpler proof, valid only if  $\mathcal{E}$  was a cokernel of a homomorphism of locally free sheaves, for instance for  $f$  projective; I was unable then to deal with the general case.

As a consequence of the previous statement it follows that, for  $f: X \rightarrow Y$  proper and flat and  $g: Z \rightarrow X$  affine, the prescheme  $\prod_{X/Y} Z/X$  exists and is affine over  $Y$ , and of finite type over  $Y$  if  $g$  is of finite type. An important particular case is the one when  $g$  is a closed immersion, i.e.  $Z$  is a closed subscheme of  $X$ , then we get a closed subscheme of  $Y$ , whose points are the points of  $Y$  the fibers of which are majorized by  $Z$ . In the general application I stated the proof of condition 5b) is not quite trivial, and (apart from standard constructibility considerations) uses some local duality theory!

By a very analogous technique, just a little more delicate, I was able also to give a general characterization of those functors  $(\text{Sch}/S)^\circ \rightarrow (\text{Ens})$  which are representable by  $S$ -preschemes  $X$  which are *locally quasi-finite* and *separated* over  $S$ . This criterion becomes especially handy in the important case when we want  $X$  to be not only locally quasi-finite, but locally non ramified (for instance a monomorphism). In all cases which I have looked at, when I expected to find such a representability, I have been able to prove it by this general criterion, of course independently of any projectiveness assumption. For instance for correspondence classes, Néron-Severi schemes when they are likely to exist etc. In particular, for any abelian scheme I can construct the Néron-Severi scheme, for any two abelian schemes also  $\mathcal{H}om_{S\text{-gr}}(A, B)$  and the scheme  $\text{Corr}_S(A, B)$  of correspondence classes, etc. As an application, if  $A$  is any abelian scheme over  $S$  locally noetherian and *geometrically unibranch*, then  $A$  is globally projective over  $S$ . Another application is to the “flattening functor” you discussed once about with Hartshorne, corresponding to a given proper morphism  $f: X \rightarrow Y$  and a coherent sheaf  $\mathcal{F}$  on  $X$ , which we want to “make flat over  $Y$ ”. As a consequence, as I once mentioned to you, we get that, for any proper scheme  $X$  over an integral noetherian  $S$ , there exists a non empty open set  $U$  in  $S$  such that the Picard scheme of  $(X|_U)/U$  exists, and has various good extra properties such as  $\text{Pic}^\tau$  being both open and closed in  $\text{Pic}$ , flat over the base etc.<sup>1</sup> However, this technique does not seem to give the case when  $S$  is not integral, even assuming the Picard scheme over the local ring of the generic point exists. Anyhow I do not intend to investigate this any further, as I have started at last working on Weil cohomology and this keeps me busy enough. I got some satisfactory results, including good behaviour of cohomology under specialisation, and I am quite optimistic about cohomology being ready-to-use within the next one or two years.

I got a few byproducts about birational transformations, and I wonder if these are known. Let  $f: X \rightarrow Y$  be proper birational,  $X$  and  $Y$  regular schemes. Then  $H^2(Y, \mathbb{G}_m) \rightarrow H^2(X, \mathbb{G}_m)$  is bijective (birational invariance of the extended Brauer group)—at least, for the time being, if everything is

<sup>1</sup> Mumford’s comment in the margin: “NOT assuming  $f_*(\mathcal{O}_X) = \mathcal{O}_S$ ??”

of finite type over a field, or simple over the integers (these restrictions are certainly superfluous, and will be eliminated with the solution of some pending local question in the case of a regular scheme and a regular divisor in it...). Of course  $\pi_1(X) \rightarrow \pi_1(Y)$  is bijective by purity, and from this I can deduce that all geometric fibers  $Z$  are simply connected. Hence  $\mathcal{P}ic^r(Z)$  is unipotent, and I believe is zero (this I checked if  $Z$  is of dimension 1). Using resolution of singularities in the very strong form of Hironaka, namely the fact that  $X$  can be dominated by  $X'$  deduced from  $Y$  by nice quadratic transformations, (available for “good” preschemes of characteristic 0), it follows also that  $R^i f_*(A) = 0$  if  $i$  is odd,  $A$  any coefficient group prime to the residue characteristics, hence  $H^i(Z) = 0$  for  $i$  odd for such coefficients. I wonder if you can check (I won’t think, counter-exemplify!) such things in characteristic  $p > 0$ ? Besides,  $H^2(Z, \mathbb{G}_m) = 0$  (perhaps assuming  $Z$  regular, I don’t remember exactly), in any case  $H^2(Z, \mu_n)$  is “algebraic” i.e., equal to the image of  $H^1(Z, \mathbb{G}_m)^2$  (if  $n$  prime to the characteristic of the ground field for  $Z$ ). —I begin to realize it would be extremely handy to have resolution for all “good” rings, as now seems reasonable; there are still various things I cannot prove without. However I got Mike’s “key lemma” about  $A\{t\}$  without assumption on  $A$ , using local Lefschetz theory as expounded in my seminar of last year. In fact, about every technique I worked out so far seems to be needed to get the basic properties of cohomology of schemes in sufficient generality (and apparently, more will be needed still!).

Sincerely yours  
(signed) A Grothendieck

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<sup>2</sup> A typo in the original, “equal to the image of  $n$ ”, is corrected here. —the editors.

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**23 February, 1963**

Bures 23.2.1963

My dear Mike,<sup>1</sup>

I want to ask you a few questions and give some complements to my last letter.

1) What about Néron's Manuscript?

2) What about Lichtenbaum's notes of Grothendieck's and Mumford-Tate's seminars?<sup>2</sup>

3) I feel very silly lately, as I am wondering if the following is not always true: Let  $X$  be a prescheme,  $X_0$  a closed subscheme of  $X$ ,  $\mathcal{F}$  an injective torsion sheaf on  $X$ , then is  $\mathcal{F}_0 = \mathcal{F}|_{X_0}$  injective, or at least is it true that  $H^i(X_0, \mathcal{F}_0) = 0, i > 0$ ? The analogous statement for Zariski topologies is *false* anyhow, but the étale topology may resemble more to paracompact topologies! This would imply that, whenever  $H^0(X, \mathcal{F}) \xrightarrow{\sim} H^0(X_0, \mathcal{F}_0)$  for every torsion-sheaf  $\mathcal{F}$  (which, for  $X$  noetherian, simply means that for every  $X'$  finite over  $X$  and connected, non-empty,  $X'_0$  is connected, non-empty), then  $H^i(X, \mathcal{F}) \xrightarrow{\sim} H^i(X_0, \mathcal{F}_0)$ , every  $i$ ! Thus the 1<sup>o</sup> of my previous letter would become evident (whereas my proof is a simple but nontrivial one, and uses far more than just the "connectedness theorem" as would be the case if the "conjecture" above were true). Moreover, it would give the analogous comparison theorem for the spectrum  $X$  of any noetherian ring  $A$  separated and complete for some  $I$ -adic topology, with  $X_0 = \text{Spec } A/I$ , (which I have not

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<sup>1</sup> Letter to Michael Artin.

<sup>2</sup> This refers to the Mumford-Tate seminar in the spring of 1962. Lichtenbaum's notes on the lectures by Grothendieck, Mumford and Tate have not been published. See also Remark 9.4.18 in S. Kleiman, The Picard scheme, in *Fundamental Algebraic Geometry*, Amer. Math. Soc. 2005, 235–321, where the contents of Mumford's personal folder for this seminar is described.

proved as yet!)

4) I got a result on cohomological dimension of *affine* schemes, in a rather formal way from the statement 1) of my previous letter (in fact I need only that, for  $X = \mathbb{P}_Y^1$ ,  $Y$  strictly local, and any torsion sheaf  $\mathcal{F}$  on  $X$ ,  $H^i(X, \mathcal{F}) = 0$  for  $i \geq 2$ .)

**Theorem** *Let  $\mathcal{F}$  be a torsion sheaf on  $\mathbb{A}_Y^m$  ( $Y$  strictly local, noeth. of dim.  $n$ ), which is “zero in codimension  $< d$ ,” i.e., if  $\bar{x}$  is a geom. point in codim.  $d$  then  $\mathcal{F}_{\bar{x}} = 0$ . Then  $H^i(\mathbb{A}_Y^m, \mathcal{F}) = 0$  if  $i > m + n - d$ , provided at least  $Y$  comes from a scheme of finite type over a noeth. ring of dim.  $\leq 1$  by “strict localization.”*

**Corollary 1** *Let  $X$  be a closed subscheme of  $\mathbb{A}_Y^m$ , of codimension  $\geq d$ . Then  $\text{cd}(X) \leq n + m - d$ .*

**Corollary 2** *Let  $X$  be any affine scheme of finite type over  $Y$ , let  $a$  be the closed point of  $Y$ , and  $Y' = Y - \{a\}$ ,  $X' = X|_{Y'}$ ,  $X_0 = X_a = X - X'$ , and  $\nu = \sup(\dim X_0, \dim X' + 1)$ . Then*

$$\text{cd}(X) \leq \nu.$$

**Corollary 3** *Let  $X$  be an affine scheme of finite type over a field  $k$ ,  $k$  sep. closed. Then*

$$\text{cd}(X) \leq \dim X.$$

(of course, in fact equality holds).

**Corollary 4** *Let  $Y$  be as in the theorem, and  $U$  an affine open subset of  $Y$  (for instance  $U = Y_f$ , some  $f$ ), then  $\text{cd}(U) \leq n$ . (Take  $m = 0$  in the theorem).*

From Corollary 3 follows the *Lefschetz Theorem*. Let  $X$  be projective over  $k$  sep. closed,  $Y$  a hyperplane section,  $Y$  and  $X$  regular. Then the natural map

$$H^{i-2}(Y, \mathcal{F}_Y \otimes \check{\mathcal{T}}) \longrightarrow H^i(X, \mathcal{F})$$

is surjective if  $i \geq n + 1$ , bijective if  $i \geq n + 2$ , where  $n = \dim X$ . Here  $\mathcal{F}$  is a locally free torsion sheaf of order prime to the char, and  $\mathcal{T}$  the “Tate sheaf” prime to char.  $k$ .

This gives the result (which I understand from Tate you know already)

$$H^n(X, \mu_N^{\otimes n}) \simeq \mathbb{Z}/N\mathbb{Z}$$

( $X$  projective, simple over  $k$ , connected,  $N$  prime to the char,  $n = \dim X$ ), and once we have duality, by transposition, the usual statement

$$H^j(X, \mathcal{F}) \longrightarrow H^j(Y, \mathcal{F}_Y)$$

is a monomorphism if  $j \leq n - 1$ , bijective if  $j \leq n - 2$ .

N.B. I doubt not that the theorem is true for any  $Y$ , at least if  $Y$  is “good,” for instance complete. I checked cor. 4 for  $Y_f$  if  $\dim Y \leq 2$ , any  $Y$ . Whenever cor. 4 is true, it implies the following for the field of fractions  $K(Y)$  of  $Y$ , when  $Y$  is integral:

$$\text{cd } K(Y) \leq n.$$

I wonder if this result can be proved directly in all cases.

5) The suggestion in the previous letter for a counterexample concerning the Brauer group is somewhat inaccurate, in various ways. Anyhow, Serre checked there is no hope to get a counterexample through topological obstructions, namely, for any finite complex  $X$  and any torsion element in  $H^3(X, \mathbb{Z})$ , there exists a projective bundle on  $X$  (some  $n$ ), whose obstruction is  $\xi$ . Thus  $H^3(X, \mathbb{Z}) = H^2(X, \mathbb{C}^*)$  is really the “topological Brauer group” of  $X$ !

Besides, did you notice that the extended Brauer group  $H^2(X, \mathbb{G}_m)$ , for regular  $X$ , is invariant under proper birational morphisms, at least whenever the standard local theorem  $\mathcal{H}_Y^i(\mathbb{G}_m) = \dots$  is true (for instance  $X$  of finite type over a field  $\dots$ ). Thus, at least for surfaces over a perfect field (when resolution is available),  $\text{Br}(X)$  is an invariant for the function field  $K$  ( $X$  a complete regular model).

6) I tried again to prove that, for a “good” strictly local ring  $A$ , the fibers of  $\text{Spec } \hat{A} \rightarrow \text{Spec } A$  are acyclic (for coefficients prime to the residue char of  $A$ ), and simply connected (with same restriction on Galois groups). For the statement “simply connected” I can reduce, by local Lefschetz theory, to the case  $\dim A = 2$ ,  $A$  normal, and to prove that thus any Galois covering of  $\hat{A}$ , unramified outside the origin, comes from a Galois covering of  $A$ . Thus we would be through if one could resolve singularities in dim. 2! I have the feeling that the dim. 2 case is really irreducible in a way, and demands some other methods than those I know. . . I begin to respect dim. 2!

Sincerely yours,  
 (signed) A Grothendieck  
 ie Schurik

7) P.S. It is not always true that  $\text{Br}(X) = H^2(X, \mathbb{G}_m)$ , even if  $X$  is local and normal of dim. 2. In fact, if  $X$  is not regular,  $H^2(X, \mathbb{G}_m)$  is not necessarily a torsion group. To see this, look at the resolution

$$0 \rightarrow \mathbb{G}_{mX} \rightarrow \mathcal{R}_X^* \rightarrow \mathcal{D}_X \rightarrow \mathcal{P}_X \rightarrow 0$$

with  $\mathcal{R}_X^*$  the sheaf of rational invertible functions,  $\mathcal{D}_X$  the sheaf of Weil divisors,  $\mathcal{P}_X$  the cokernel of  $\mathcal{R}_X^* \rightarrow \mathcal{D}_X$ , which can be called the “sheaf of (strictly) local divisor class groups.” The cohomology of  $X$  in  $\dim \neq 0$  with coefficients in  $\mathcal{R}_X^*$ ,  $\mathcal{D}_X$  is torsion, hence *mod torsion* we have

$$\begin{aligned}
H^i(X, \mathbb{G}_{mX}) &\equiv H^{i-2}(X, \mathcal{P}_X), i \geq 3 \\
H^2(X, \mathbb{G}_{mX}) &\equiv H^0(X, \mathcal{P}_X) / \text{Im } H^0(X, \mathcal{D}_X)
\end{aligned}$$

Assume, for instance,  $X$  has an isolated singularity  $x$ , let  $\mathcal{O}_x$  ( $\mathcal{O}_{\bar{x}}$ ) be the local ring (resp. its strict henselization), thus

$$H^2(X, \mathbb{G}_m) = \text{Cl}(\mathcal{O}_{\bar{x}}) / \text{Im } \text{Cl}(\mathcal{O}_x)$$

when  $\text{Cl}$  is the divisor class group. Now there are I believe examples of Mumford's where  $\text{Cl}(\mathcal{O}_x) = 0$ , whereas  $\text{Cl}(\mathcal{O}_{\bar{x}}) \neq 0$ , and even  $\text{Cl}(\mathcal{O}_{\bar{x}})$  a nontorsion group (you should check this point).<sup>3</sup> Hence the counterexample.

One last remark: for a complete, nonsingular surface  $X$  over an alg. closed field, we get an interpretation of  $b_2 - \rho$  as the rank of the module of points of order  $n$  of  $\text{Br}(X)$ , which is a free module over  $\mathbb{Z}/n\mathbb{Z}$ , where  $n$  is prime to the characteristic and to the torsion of the Néron-Severi group.

Best regards to Jean and the kids,

(signed) Your Schurik

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<sup>3</sup> See [61a].

11 June, 1963

Bures, June 11, 1963

Dear Mumford,

I understand you published something (e.g. so-called “pathology”<sup>1</sup>), please send me a reprint; I hope you will put me on your general mailing list, if you have one.

Matsumura lately told me he proved representability of  $\mathcal{A}ut_k(X)$ ,  $X$  proper over a field  $k$ , using Murre’s method.<sup>2</sup> I then tested my criterion for representability of a functor  $F/S$  by a scheme separated and unramified over  $S$ , I told you about, and got more general results. For instance if  $X$  and  $Y$  are locally of finite type over a field  $k$ ,  $X$  proper over  $k$ , then there exists a finite separable extension  $k'$  of  $k$ , depending only on  $X$ , such that  $\mathcal{H}om_{k'}(X_{k'}, Y_{k'})$  is representable; thus, if  $k$  is separably closed,  $\mathcal{H}om_k(X, Y)$  is representable. The analogous result holds if  $X, Y$  are locally of finite type over  $S$  noetherian and integral,  $X$  being proper and flat over  $S$ , provided we restrict to some open non empty subset of  $S$ . Besides, without assuming  $S$  integral, but say  $X$  with integral geometric fibers and admitting a section along which  $X$  is simple over  $S$ ,  $\mathcal{H}om_S(X, Y)$  is representable. The general result from which these can be easily deduced is as follows: Let  $X, Y, Z$  be locally of finite type over  $S$ ,  $X$  and  $Y$  proper and flat, let  $\phi : Z \rightarrow X$  be given (for instance,  $Z$  is a flat finite multisection of  $X/S$ ), hence a homomorphism of functors

$$F = \mathcal{H}om_S(X, Y) \rightarrow G = \mathcal{H}om_S(Z, Y);$$

consider the subfunctor  $U = F_\phi$  of  $F$  where  $F \rightarrow G$  is unramified, more precisely, its points with values in  $S'/S$  consist of those  $u' : X' \rightarrow Y'$  such that, for every  $s' \in S'$ , the following map be injective:

$$\mathrm{Hom}_{\mathcal{O}_{X'_{s'}}}(u'^*_{s'}(\Omega^1_{Y'_{s'}}), \mathcal{O}_{X'_{s'}}) \rightarrow \mathrm{Hom}_{\mathcal{O}_{Z'_{s'}}}(v'^*_{s'}(\Omega^1_{Y'_{s'}}), \mathcal{O}_{Z'_{s'}})$$

<sup>1</sup> [61b] and [62a]

<sup>2</sup> This result was published in H. Matsumura and F. Oort, *Representability of group functors, and automorphisms of algebraic schemes*, Invent. Math. 4 (1967) 1–25.

where  $v' = u'\phi'$ . This functor  $U$  is an *open* subfunctor of  $F$  (and the idea is to exhaust  $F$  by such open subfunctors  $F_\phi$ , with suitably large  $Z$ 's). Look at the induced homomorphism

$$U = F_\phi \rightarrow G;$$

the result is that *this homomorphism is representable by unramified separated morphisms*. As a consequence, if  $G = \mathcal{H}om_S(Z, Y)$  is representable, so is  $F_\phi$ .

It is possible that, for any  $X, Y$  over  $S$  as above,  $\mathcal{H}om_S(X, Y)$  is representable *locally* for the flat quasi-finite topology; this can be checked (even for the étale topology) when  $X$  is simple over  $S$ , or only with separable fibers. A question which is not solved by the method is (in the case of a ground field) whether the connected components of  $\mathcal{H}om_k(X, Y)$  are of finite type over  $k$ ; I suspect not.

Murre is in Bures for one month now. He is trying to prove his general criterion of representability for group functors, dropping the commutativity condition, and the last two conditions (Rosenlicht's condition, and the separation axiom), by relying still more on the techniques of formal moduli and descent. The idea is to construct first the local ring of the generic point of the connected component of  $e$ , by the smallest ring of definition for the canonical point of the functor  $G$  with values in the function field of the formal group prorepresenting  $G$  at  $e$ , and then use an easy generalisation of Weil's theorem on group varieties to construct the whole connected component. But there are considerable technical difficulties involved, such as effectivity criteria for "birational" equivalence relations or "birational" descent data (everything in non flat, non finite, cases). It would be quite a progress for the non projective construction techniques if Murre could overcome these difficulties, even if the final theorem on representation of group functors should not be of frequent use. Perhaps Murre will run a seminar on formal moduli in 64/65.

Anything new going on there? Here Bass proved a beautiful theorem using his  $K^1$  functor of rings, namely for  $SL(n, \mathbb{Z})$ ,  $n \geq 3$ , the topology of subgroups of finite index equals the topology of congruence subgroups.<sup>3</sup>

Sincerely yours  
(signed) A Grothendieck

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<sup>3</sup> This result is published in H. Bass, M. Lazard & J.-P. Serre, Sous-groupes d'indice fini dans  $SL(n, \mathbb{Z})$ , Bull. Amer. Math. Soc. **70**, 1964, 385–392.

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**5 August, 1963**

5.8.1963

My dear Mumford,

Thank you for your letter. I am glad you had such an interesting time in Japan. However, for the time being I do not think of traveling myself, and probably my first long trip abroad will be to Harvard, in perhaps two years or three. I am sorry to hear Hironaka accepted a position at Columbia and will not be around in Cambridge any longer. Couldn't he get a comparable salary at Brandeis?

I knew in effect that it was possible to check Serre's conjecture on the tangent space of  $\text{Pic}^{\text{red}}$  by duality, when I wrote the last formula of SGA1 XI; but I never actually did it.

Let  $X/k$  be proper, and for every  $x_i \in \text{Ass } \mathcal{O}_X$ , let  $y_i \in \bar{x}_i$ , and let  $k_i$  be the separable algebraic closure of  $k$  in  $k(y_i)$ . Let  $k'$  a field extension of  $k$  that splits all of the extensions  $k_i/k$ . Let  $Y$  be any locally algebraic scheme over  $k$ , then  $\mathcal{H}om_{k'}(X_{k'}, Y_{k'})$  exists. As you see, there is no need for the geometric irreducible components of  $X/k$  to be defined over  $k'$ . The proof is easy by the general result I stated in my last letter. Besides, in all this, a more general and more convenient point of view is to abide with the functor  $\prod_{X/k} Y/X$  when  $X$  proper over  $k$ , and  $Y/X$  given; everything I did applies to this situation. Besides, instead of assuming that  $k'/k$  splits the  $k_i/k$ , it would be enough to assume that  $\prod_{k'_i/k'} (Y_{y_i})_{k'}$  are representable (where  $k'_i = k_i \otimes_k k'$ ). For instance if the fibers  $Y_{y_i}$  are quasi-projective, then  $\prod_{X/k} Y/X$  exists, and is in fact an increasing union of a sequence of quasi-projective open subsets.

I have been pretty busy for the last two weeks writing an outline for Hartshorne's seminar on residues and duality. It takes me longer than I thought it would to put things in a decent order, but I think I can begin typing in a few days and he will have most of it by the end of August.<sup>1</sup>

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<sup>1</sup> This appeared in R. Hartshorne, *Residues and duality*, LNM 20, Springer-Verlag 1966. A compendium, in which some of the tricky points are worked out in

Verdier has promised me to write an outline of those results on the foundations of homological algebra I told him I would need. I think, in order to get started, Hartshorne should take these foundations as granted, at least as far as proofs are concerned. Anyhow, as you know, it is planned that Verdier comes to Harvard in 64/65, and he will give probably some course or seminar on homological algebra and duality for topological spaces (the results being formally exactly parallel to duality for coherent modules on schemes).

As for me, I am running two joint-seminars next year, one with Demazure on group schemes, continuing the one of last year (the writing down of my own talks is far from finished and takes a long time!) one with Mike on étale cohomology. I hope we will have time to include duality and the application to  $L$  functions over finite fields.

I will be interested to know what is going on in Harvard, and especially what you are doing yourself. Is there no resolution of singularities for good schemes in view?

Yours  
(signed) A Grothendieck

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greater detail, was published in B. Conrad, *Grothendieck duality and base change*, LNM 1750, Springer-Verlag 2000. An extensive account of the developments in the theory since Grothendieck's manuscript, written by J. Lipman, was recently submitted to LNM.

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 16 September, 1963

Bures, Sept. 16, 1963

Dear Mumford,

Artin transmitted your question concerning passage to quotient in analytic spaces, to construct a Picard modular space as stated in my talk in Cartan's Seminar.<sup>1</sup> Looking back at it, I see I used without proof the following fact, (which I hope is not false!): if  $X/S$  is a projective flat scheme such that  $\mathcal{O}_S \xrightarrow{\sim} f_*(\mathcal{O}_X)$  universally, looking at the modular space  $\mathcal{F}$  for immersions  $X \rightarrow \mathbb{P}_S^n$  of the special type considered in loc. cit., is it true that  $PGL(n) \times \mathcal{F} \rightarrow \mathcal{F} \times \mathcal{F}$  is an *immersion*? (Or at least, when  $S$  is of finite type over  $\mathbb{C}$ , a homeomorphism into for the *usual* topologies). To prove it is an immersion is equivalent to the following: Assume  $S = \text{Spec } V$ ,  $V$  discrete valuation ring, and assume  $i_1, i_2 : X \rightrightarrows \mathbb{P}_S^n$  are given, such that on the *two fibers*,  $i_{1s}$  and  $i_{2s} : X_s \rightarrow \mathbb{P}_s^n$  ( $s \in S$ ) are conjugate, to prove  $i_1$  and  $i_2$  are conjugate under an element of  $PGL(n)(V)$ . Can you say something about this problem?<sup>2</sup>

Yours

(signed) A Grothendieck

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<sup>1</sup> See Séminaire Henri Cartan 13, 1960/62, Exposé 16, Thm. 3.1 and its proof.

<sup>2</sup> Mumford's comment at the bottom margin:

 Have  $\lambda: \mathbb{G}_m \rightarrow PGL_n$ ,  $Z_i \subset \mathbb{P}^n$ ,  $Z_i \rightarrow Z$ .

 Assume  $\alpha_i \in \mathbb{G}_m$  s.t.  $Z_i \hookrightarrow \mathbb{P}^n \xrightarrow{\lambda(\alpha_i)} \mathbb{P}^n$  approaches  $Z \hookrightarrow \mathbb{P}^n$ .

 Say  $H^0(\mathcal{O}_Z) = k$ ,  $H^1(\mathcal{O}_Z(1)) = (0)$ ,  $H^0(\mathcal{O}_{\mathbb{P}^n}(1)) \xrightarrow{\sim} H^0(\mathcal{O}_Z(1))$ .

 $\xrightarrow{?} \alpha_i$  have a limit



## 1963/4 undated

Dear Mumford,<sup>1</sup>

Thanks for your letter, and also for your manuscript on “Geometric Invariant Theory”, which I have not quite read through yet. I noticed in Chap I some inaccuracies (for instance concerning questions of openness of morphisms), a detailed list would perhaps be tedious. I hope in the final draft the cross-references will be easier to find than in the one I read, where I did not always succeed to get the right reference. One remark on terminology: your use of the word “reductive” seems to me misleading, as it conflicts with the terminology generally adopted (which Demazure and I follow also in our seminar), couldn’t you invent some other word? Apart from terminology, you seem always implicitly to assume your groups (at least the reductive ones) smooth over the ground field, without ever stating this. Strictly speaking, your definition of “reductive groups”, in char  $p > 0$ , yields exactly the “multiplicative type groups” i.e. the duals of usual discrete comm. groups (if  $k$  alg. closed), including such groups as  $\mu_p$ . Besides, I give a rather detailed study of these groups (from a point of view of course very different from yours, and over arbitrary ground schemes) in talks VIII to X of SGA3 (and you should get pretty soon talks VIII to XIV, which are being bound).

I was surprised to find the corollary you missed in EGA III 7 was not there. In a way we should have repeated as corollaries, in sections 7.7 and 7.8., whatever we did in the previous sections! But I agree the one you state is particularly useful, and should not have been forgotten.

The references you ask:<sup>2</sup>

- a)  $f : X \rightarrow Y$  open finite type,  $X, Y$  irred.,  $Y$  noeth.  $\implies f$  equidimensional, IV 14.2.2.; converse if  $Y$  is normal (or geom. unibranch) IV 14.4.4. (Chevalley)

<sup>1</sup> We are placing this letter according to its position in Mumford’s file. *Geometric Invariant Theory* was written during the academic year 1962/63 at the IAS. —the editors

<sup>2</sup> In this paragraph IV = EGA IV and III = EGA III.

- b)  $f : X \rightarrow Y$  finite type, then  $x \rightsquigarrow \dim_x f^{-1}f(x)$  is upper semicontinuous  
IV 13.1.3. (Chevalley)
- c)  $f : X \rightarrow Y$  finite type,  $X$  Cohen-Macaulay,  $Y$  regular, then  $f$  open  $\iff$   
 $f$  flat IV 15.4.2.
- d) 
$$\begin{array}{ccc} X & \xrightarrow{f} & Y \\ & \searrow g & \nearrow h \\ & S & \end{array}$$
 Everything of finite pres.,  $g, h$  flat,  $f_s$  flat  $\implies f$  flat on  $X_s$

IV 17... (I will have it more precise when Dieudonné is back from Japan with the manuscript!)

- e)  $X$  reduced of finite type over  $\mathbb{Z} \implies \tilde{X}$  finite over  $X$  IV 7.7. (same remark as above). (Nagata)
- f)  $PGL(m+1)_S$  represents  $\mathcal{A}ut_S(\mathbb{P}_S^m)$ , will be in one of the later paragraphs of III which have not been written up. A sketch of the proof, using available references of Chap III, should take no more than half a page. The reason why I did not include it in III 4 was that I have to use the fact that  $\text{Pic}(\mathbb{P}_k^m)$  is the group generated by  $\mathcal{O}(1)$ , for which one needs that the local rings are UFD... , which we had not available there.

Besides, part 1 (out of 4) of Chap IV has just appeared, and you will have a reprint pretty soon. It contains only IV and IV 1, part 2 (already at the printer) contains paragraphs 2 – 7 (including the theory of “excellent local rings”), most of the references you need are in part 3, (paragraphs 8 to 15), which will be given to the printer within a month or two. Part one contains also the list of all paragraphs 1 to 21 (of which only the two last are still to be written in a publishable shape).

About your functor  $M_g(S)$ , I wonder what you mean by “ordinary *double point*”, namely how are you to prevent (if you want the valuative criterion of properness) two double points to collapse to a triple point? It seemed to me one should allow multiple points of any order, but of “loose” type (as coordinate axes in  $n$ -space) so that  $\text{Pic}(C)$  does not acquire a unipotent component. But I confess I did not think this over seriously. By the way, Igusa seems to have a really beautiful *non-singular projective* model in char 0 for compactifying the usual modular varieties with levels, which he has completely worked out for  $g = 2$ , but which according to him should generalize to all  $g$ , for principally polarized abelian varieties.<sup>3</sup>

I am sorry not to have heard anything before on Schlessinger’s thesis,<sup>4</sup> which sounds interesting; but what you say about it is somewhat short for me to understand, especially what you mumble about the case  $H^0(\mathfrak{g}) \neq 0$  and

<sup>3</sup> The generalization of Igusa’s result mentioned here is known as the *toroidal compactification*; see A. Ash, D. Mumford, M. Rapoport & Y.-S. Tai) *Smooth Compactification of Locally Symmetric Varieties*. Math. Sci. Press, Brookline, Mass., 1975.

<sup>4</sup> Part of M. Schlessinger’s 1964 Harvard Ph.D. thesis is published as M. Schlessinger, Functors of Artin rings, Trans. Amer. Math. Soc. **130**, 1968, 208–222.

the smooth topology. If some day there is anything mimeographed or printed available I would appreciate getting a copy!

I did not prove anything noticeable in the last year, although I lately spent one month or two trying to prove Weil's conjectures. I have found lots of conjectures on algebraic cycles, which I expect will keep me (and others perhaps) busy for quite a while. Mike will tell you about it next month I guess. I'll try again during the vacation to prove something along these lines, it seems time at last to know something at least on algebraic cycles which are not divisors.

Yours sincerely  
A Grothendieck



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31 August, 1964

Bures, 31.8.64

Dear Mumford,

I am just through reading Dieudonné's final version of EGA IV paragraphs 11 to 15. You may be interested in our final version of Chevalley's openness criterion, which reads as follows

**Theorem 14.4.1** *Let  $f: X \rightarrow Y$  be locally of finite presentation,  $y \in Y$ ,  $x$  maximal in  $X_y = f^{-1}(y)$ , assume  $y$  geometrically unibranch on  $Y$ . The following conditions are equivalent:*

- (a)  *$f$  is universally open at  $x$  (or, what trivially amounts to the same, at every point in the closure of  $x$  in  $X_y$ ).*
- (b) *If  $z$  is the maximal generisation of  $y$ , i.e. the generic point of the unique irreducible component  $Y_0$  of  $Y$  through  $y$ , there exists an irreducible component  $Z$  of  $X$ , containing  $x$  and equidimensional over  $Y_0$  at  $x$ , i.e. such that  $\dim_x Z_y = \dim Z_z$ .*
- (b') *For every open neighbourhood  $U$  of  $x$  in  $X$ , and every generisation  $y'$  of  $y$ ,  $\dim U_{y'} \geq \dim_x U_y$  holds.*  
*If  $Y$  is locally noetherian, these conditions are also equivalent to the following:*
- (c)  *$f$  is open at  $x$ .*

NB The equivalence of (b) and (b') is about trivial, the essential part of the theorem being (b)  $\Rightarrow$  (a). This theorem gives as a corollary 14.4.2, equivalent conditions for  $f$  to be universally open at all points of  $X_y$ , namely through conditions (b) (b') or (c) at the maximal points of  $X_y$ ; however, it is easily seen that we may state these conditions as well at *all* points of  $X_y$ , and also state equivalently (b''):  $\dim U_z \geq \dim U_y$  for every open subset  $U$  of  $X$ .

As another consequence 14.4.8, we get a necessary and sufficient condition for a morphism locally of finite presentation  $f: X \rightarrow Y$  to be universally open. (In fact, the criterion obtained is really a pointwise criterion on maximal points of fibers): if  $Y'$  is the normalisation of  $Y_{\text{red}}$ , it is necessary and sufficient that

$X' = X \times_Y Y' \rightarrow Y'$  be open (universally so if  $Y$  is not supposed locally noetherian) which is equivalent also to either of the conditions in terms of dimensions seen above. It is very likely besides that everything holds without any reference to a noetherian condition, but we could not settle this point (and I guess you do not care anyhow).

I included also a proposition of yours as follows:

**Proposition 14.5.10** *Let  $Y$  be noetherian,  $f: X \rightarrow Y$  locally of finite type, surjective and universally open. Then there exists a finite surjective morphism  $Y' \rightarrow Y$  such that  $X' = X \times_Y Y'$  admits sections locally over  $Y'$ .*

As a corollary 14.5.11, we state the conclusion you had in mind, namely that if a morphism  $Y_1 \rightarrow Y$ , locally of finite type, becomes affine after the base change  $X \rightarrow Y$ , then it was affine before. We also give the analogous descent statement 14.5.12 for ampleness of an invertible sheaf on  $Y_1$ , relative to  $Y$ .

Thank you very much for your notes on surfaces,<sup>1</sup> which I looked through with pleasure. Here are a few comments and questions.

1<sup>o</sup> Page 8.1. you state the problem of the existence of a flattening stratification for a proper morphism  $X \rightarrow S$  and a coherent sheaf  $\mathcal{F}$  on  $X$ ,  $S$  locally noetherian. Now this problem is practically solved, namely as I indicated to you in an old letter of mine, the flattening functor in this case is indeed representable by a prescheme  $S'$  of finite type over  $S$ , and a monomorphism  $S' \rightarrow S$ . The only question which remains (and I would rather guess the answer to be negative) is whether  $S' \rightarrow S$  is a stratification; but this is, I believe, rather inessential for all applications. (NB as  $S' \rightarrow S$  is a monomorphism it is automatically quasi-affine, a fortiori quasi-projective).

2<sup>o</sup> I appreciated very much your Chapter 14, particularly the theorem on page 14.4. I did not check through the details of your proof, but I guess your result holds true if, instead of taking sheaves of ideals, you take subsheaves, or equivalently and preferably for my taste, quotient sheaves, of a fixed coherent sheaf? In this form, your theorem is a significant amelioration of a finiteness theorem in my Bourbaki talk on Hilbert schemes, namely loc. cit 2.1. (where instead of “il faut” one must of course read “il faut et suffit”). Now it would seem very likely that an analogous quantitative version should equally exist for loc. cit 2.2. (where instead of  $\leq s - 1$  one should read  $\geq s$ , and instead of  $s - 2$  one should read  $s - 1$ ; in the reformulation 2.3 read  $s$  instead of  $s - 1$ ).<sup>2</sup> Namely that the limitedness of quotients  $\mathcal{F}/\mathcal{H}_i = \mathcal{G}_i$ , as expressed, say, by the twisting  $n_0$  needed so that for  $n \geq n_0$ , both Serre’s statements hold for  $\mathcal{H}_i(n)$ , can be estimated by a polynomial with respect to the coefficients of degree  $\geq s$ , if we restrict to quotients  $\mathcal{G}_i$  such that the associated cycles are all of dimension  $\geq s$  (the polynomial depending only on  $X$ ,  $\mathcal{F}$ ,  $\mathcal{O}_X(1)$  and

<sup>1</sup> Published in D. Mumford, *Lectures on Curves on an Algebraic Surface*, Annals of Math. Studies 59, Princeton U. Press 1966.

<sup>2</sup> DM wrote a question mark (?) at the margin.

s). I wonder if you checked this variant of your theorem, which I would like to consider as a starting point for a systematic “quantitative” version of the standard finiteness theorems (as once Mike told me about, à propos making quantitative Noether’s theorem of finiteness of integral closure).

3<sup>o</sup> I liked also Bergman’s Chap 26–27,<sup>3</sup> and especially his universal Witt scheme, realized as a formal power series functor. This meets with some old ponderings of mine on power series beginning with 1, on which I make some comments in my little paper on Chern classes (the appendix to the Serre-Borel paper). As I point out there this is not only a ring, but a  $\lambda$ -ring (and even a “special”  $\lambda$ -ring), on the other hand since Gabriel’s seminar on formal groups I had the feeling that the Witt rings must also have a  $\lambda$ -structure (or something very close to it). Namely, according to Dieudonné-Cartier-Gabriel, certain algebras over  $W_\infty(k)$  (the Witt vector ring over the perfect field  $k$ ) allow to classify, either commutative formal groups without toroidal part (one might call them ind-unipotent), or ordinary unipotent algebraic groups (the two classifications being in fact dual), in terms of modules over these algebras. Now, in the categories in question, one has not only a structure of abelian category, but also the notion of tensor product and consequently of exterior power. Now this extra structure should be reflected in some extra structure of the mentioned classifying algebra, and presumably, en dernière analyse, by  $W$  itself. This question should certainly be investigated some day, and perhaps Bergman has a good starting point. I guess, besides, you noticed that, analogously, the classifying space of the infinite unitary group of  $K$ -theory is not only a group in the hot-category, but actually a  $\lambda$ -ring, and the same remark applies to the orthogonal case, these facts reflecting simply the  $\lambda$ -ring structure of the  $K$ -functors.

4<sup>o</sup> The proof of the fundamental theorem, Chap 23, via Kodaira Spencer, is not really different from the proof in Chap 25. This is still more striking if one has in mind Cartier’s own proof of his theorem on smoothness of algebraic groups in char 0, or rather formal groups, which is precisely using the exponential, so I suspect that what Kodaira-Spencer do is just giving Cartier’s proof. Besides, the proof you give of Cartier’s theorem is also the one I intend to include in EGA; it goes further for it proves also that if  $G$  is a group scheme locally of finite type over a noetherian ground scheme  $S$  say, such that  $S$  is of char 0 i.e. lies over  $\text{Spec}(\mathbb{Q})$ , and that the sheaf  $\omega_{G/S} = \mathcal{J}/\mathcal{J}^2$  ( $\mathcal{J}$  is the augmentation ideal on  $G$  coming from the unit section) is locally free on  $S$ , then  $G$  is smooth over  $S$  along the unit section (and therefore on the connected components of the identity of the fibers). This can be applied for instance to Picard schemes, where we have a direct construction of  $\omega_{G/S}$  and where the assumption of local freeness of the latter just means that  $X/S$  is “cohomologically flat in dimension 1” i.e., what amounts here to the same, satisfies the base change property for  $R^1 f_*(\mathcal{O}_X)$  ( $f$  is assumed flat, proper, with  $f_*(\mathcal{O}_X) = \mathcal{O}_Y$  universally). One remark which could have been made already in Chap 23 is

<sup>3</sup> “Bergman’s Chap 26–27” appeared as Chap 26 in the published version.

that Kodaira-Spencer's theorem is valid as stated there, simply replacing the char 0 assumption by the assumption that  $\mathcal{P}ic_{X/k}$  is smooth. It is not clear why in Chap 25 you feel obliged to give a weaker statement of the theorem, assuming  $H^1(X, \mathcal{L}) = 0$ .

The result and proof in Chap 28<sup>4</sup> is really very nice and elegant. Suggestion for a thesis: give a version of this theorem as a criterion for smoothness along the unit section of the Picard scheme (or Picard proscheme) over an arbitrary base (or an artin base, which amounts to the same). The right cohomological operations, replacing Serre's Bockstein operations when the base is not of a given char  $p > 0$ , will clearly be the ones arising from the formal powers series scheme, and they would seem to deserve more study.

5<sup>o</sup> I was interested by your numerical result on page 17.7. Do you have an analogous result for higher dimensional varieties? This reminds me also of some positivity questions I once discussed with Mike, which he promised he would tell you about, (but I am not sure he kept his promise!). First take a surface (say projective non singular), and the vector space over  $\mathbb{Q}$  defined through numerical equivalence of divisors, in this space we have the closed cone  $Q$  defined by the quadratic form, and, restricting to the part of degree  $\geq 0$ , the closed cone  $P \subset Q$  generated by ample sheaves, whose interior consist exactly of elements having a positive integral multiple defined by an ample sheaf, and the closed cone  $\bar{R}$  defined by positive divisors, genererating the cone  $R$ . By the prop. on page 18.1  $Q^\circ \subset R$ , hence  $Q \subset \bar{R}$ , moreover  $P \subset Q$ . Using this and Nakai's criterion for ampleness, one finds that  $P = R^\circ$ , the polar of  $R$  or  $\bar{R}$ , hence  $\bar{R} = P^\circ$ . This however gives not Nakai's result, but it suggests the following: if a divisor  $D$  is such that, for every divisor  $C > 0$ , one has  $C.D > 0$ , is it true that  $D$  is ample? A priori, we know only that it follows  $D^2 \geq 0$ , and by Nakai this is almost what is needed to imply ampleness, namely we need  $D^2 > 0$ . Do you know the answer?<sup>5</sup> More generally, if  $X$  is projective smooth of any dimension,  $D$  a divisor, and if  $D.C > 0$  for any curve on  $X$ , is it true that  $D$  is ample? Is it true at least that  $D$  is in the closure  $P$  of the cone defined by ample divisors, which would then imply  $D^p Z_p \geq 0$ , if  $Z$  is a subvariety of  $X$  of dimension  $p$ . This weaker statement is certainly true on a surface, what about a threefold?<sup>6</sup>

I would like even a lot more to be true, namely the existence of a numerical theory of ampleness for cycles of any dimension. Assume for simplicity  $X$  projective non singular connected of dim.  $n$ , let  $A^i(X)$  be the vector space over  $\mathbb{Q}$  deduced from numerical equivalence for cycles of codimension  $i$  (presumably this is of finite dimension over  $\mathbb{Q}$ ), and  $A_i(X) = A^{n-i}(X)$  defined by cycles of dimension  $i$ , presumably  $A_i$  and  $A^i$  are dual to each other. Let  $A_i^+$  be the closed cone generated by positive cycles, and let  $P^i \subset A^i$  be the polar cone. The elements of  $P^i$  might be called pseudo-ample, those in the interior of  $P^i$

<sup>4</sup> Chap 27 in the published version.

<sup>5</sup> DM wrote a "NO" on the left margin.

<sup>6</sup> DM wrote a question mark (?) on the margin.

ample (which for  $i = 1$  would check with the notion of ample divisor, if for instance the strengthening of Mumford-Nakai's conjecture considered above is valid). The strongest in this direction I would like to conjecture is that the intersection of pseudo-ample (resp. ample) cycles is again pseudo-ample (ample), thus the intersection defines

$$P^i \times P^j \rightarrow P^{i+j}.$$

If  $i$  and  $j$  are complementary,  $i + j = n$ , this also means that the natural map  $u_i : A^i \rightarrow A_{n-i}$  maps  $P^i$  into  $A_{n-i}^+$  (and one certainly expects an ample cycle to be at least equivalent to a positive one!). For  $i$  and  $j$  arbitrary, the above inclusion can also be interpreted as meaning that the intersection of an ample cycle with a positive cycle is again (equivalent to) a positive cycle. Of course, one would expect an ample positive cycle to move a lot within its equivalence class, allowing to consider proper intersections with another given positive cycle. I wonder if you have any material against, or in favor of, these conjectures?

I am busy right now, granting Weil's conjectures (via the Lefschetz and Hodge type statements for algebraic cycles) plus Tate's, to get the right feel for what should replace the *rational* cohomology of schemes (there is certainly also something like an integral cohomology, but this is too sharp for the time being), namely to define the right category of "sheaves" and their basic properties. One striking fact, which is certainly true, is that for a scheme  $X$  of finite type over the integers, taking  $l$ -adic sheaves over  $X$  ( $l$  prime to the residue char.) arising through any simple "geometric" construction (as higher direct images of  $\mathbb{Q}_l$  etc), say any of the Tate sheaves  $\mathbb{Q}_l(n)$ , the cohomology modules  $H^i(X, \mathbb{Q}_l(n))$  for variable  $l$  are canonically isomorphic to some  $H^i(X, \mathbb{Q}(n)) \otimes_{\mathbb{Q}} \mathbb{Q}_l$ , where  $H^i(X, \mathbb{Q}(n))$  is a certain vector space of finite dimension over  $\mathbb{Q}$ . Vaguely speaking, this is the (common) subspace of the elements of  $H^i(X, \mathbb{Q}_l(n))$  which can be constructed "in terms of algebraic cycles" . . . The philosophy is here that in a way, for a scheme of finite type over  $\text{Spec } \mathbb{Z}$ , the whole of its cohomology is "algebraic" i.e. has direct arithmetic significance. For the time being unfortunately, nothing new concerning the proofs of the basic conjectures. All I did was to construct "intermediate jacobians" in terms of cycles algebraically equivalent to zero, the necessary majorization for the construction coming from the  $l$ -adic Betti-numbers. But except for the definition, Ind duality for complementary (to  $\dim X - 1$ ) dimensions, which is practically part of the definition, I have no result concerning these abelian varieties. (NB in the classical case, they correspond just to a small piece of Weil's intermediate jacobians).

Sincerely yours  
(signed) A Grothendieck



## 1964 undated

Dear Grothendieck,

Thanks for the long and *very* interesting letter. I've been thinking off and on for the last 2 weeks on some of your questions.

(I) There is a surface  $F$ , with divisor  $D$  such that

$$\begin{aligned} (D^2) &= 0 \\ (D.C) &> 0, \text{ all positive divisors } C. \end{aligned}$$

*Proof.* The idea is to take  $\text{char} = 0$  and  $F$  to be a “generic” ruled surface over a curve  $\Gamma$  of sufficiently high genus  $g$ , of *even* type (i.e.  $F = \mathbb{P}(\mathcal{E})$  where  $\deg c_1(\mathcal{E})$  is even). Then, mod algebraic equivalence,  $\text{Pic}(F)$  is generated by

$$\begin{aligned} f &\text{ — the fibre of the ruling} \\ E &\text{ — any “unisecant”, i.e. cross-section of the ruling.} \end{aligned}$$

Then one may as well replace  $E$  by  $E - kf$  so that  $(E^2) = 0$ . This  $E$  is the example. One has to check  $(E.C) > 0$ , all positive irreducible  $C$ . The idea is this: say  $C$  is algebraically equivalent to  $aE + bf$ , hence  $(E.C) = b$ ; also either  $a = 0 \implies C = f$  or  $a > 0 \implies C$  an  $a$ -fold covering of  $\Gamma$ .

1°  $K$  (the canonical class) is algebraically equivalent to  $-2E + (2g - 2)f$ .  
Then

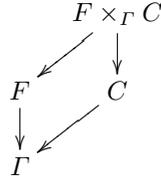
$$2p_a(C) - 2 = (C + K) \cdot C \geq a(2g - 2)$$

since  $C$  is an  $a$ -fold covering of  $\Gamma$ . This implies immediately that  $b \geq 0$  or  $a = 1$ .

2° If  $a = 1, b \leq 0$ , then you have my “un-stable” ruled surfaces which depend on only  $2g - 1$  parameters (as opposed to the  $3g - 3$  moduli for generic ruled surfaces over  $\Gamma$ ).

3° If  $a > 1, b = 0$ , then  $F$  contains a curve  $C$  which is an *unramified*  $a$ -fold covering of  $\Gamma$ .

3°(i)  $a = 2$ . Then we get a diagram



and  $F \times_{\Gamma} C \cong \mathbb{P}(\mathcal{L}_1 \oplus \mathcal{L}_2)$  over  $C$  and  $F = F \times_{\Gamma} C / \mathbb{Z}_2$ . It is easy to check that there are very few of these ruled surfaces.

3°(ii)  $a > 2$ . Let  $\tilde{C}/C$  be a further unramified covering s.t.  $\tilde{C}/\Gamma$  is Galois with group  $\pi$ . Let  $\tilde{F} = F \times_{\Gamma} \tilde{C}$ . Then  $\tilde{F}$  has  $\geq 3$  disjoint sections, so  $\tilde{F} \cong \mathbb{P}^1 \times \tilde{C}$ , so

$$F = \mathbb{P}^1 \times \tilde{C} / \pi.$$

Also the inverse image of  $C \subset F$  in  $\tilde{F}$  is a set of  $\geq 3$  sections  $\{a_i\} \times \tilde{C} \subset \mathbb{P}^1 \times \tilde{C}$  permuted by  $\pi$ . Therefore  $\pi$  acts on  $\mathbb{P}^1 \times \tilde{C}$  by a product of some action on  $\mathbb{P}^1$  with the given action on  $\tilde{C}$ ; there are only a few such, of course. —————

(II) **Re** limited families of sheaves of ideals  $\mathcal{J} \subset \mathcal{O}_{\mathbb{P}^n}$  (the generalization to subsheaves of any  $\mathcal{F}$  is easy by the way). Let  $\mathcal{O}/\mathcal{J} = \mathcal{F}$ . Let

$$P(m) = \chi(\mathcal{F}(m)) = \sum_{i=0}^m a_i \binom{m}{i}.$$

Then we know that determining  $a_0, \dots, a_n$  puts  $\mathcal{J}$  in a limited family. Suppose you go further and assume  $\mathcal{F}$  has no 0-dimensional associated cycles. Fixing  $a_1, \dots, a_n$  does *not* put  $\mathcal{J}$  in a limited family. The example is below. I notice that you don't prove that in Exposé 221 either (so there is no contradiction). (For  $\leq s - 1$  in 2.2, write  $\geq s - 1$  and that seems to be what you proved). *BUT*: If  $\mathcal{F}$  has neither 0 nor 1-dimensional cycles, then we're ok. In fact:

**Theorem.** Look at the set of all coherent sheaves  $\mathcal{F}$  on  $\mathbb{P}^n$  satisfying

- i)  $\chi(\mathcal{F}(m)) - \chi(\mathcal{F}(0)) =$  given  $P(m)$ ,
- ii)  $\mathcal{F}$  has no 0 or 1-dimensional assoc. cycles,
- iii)  $H^i(\mathcal{F}(m)) = (0)$ ,  $i > 0$ ,  $m \geq n_0$ .

Then there is a polynomial  $f_n$  in  $n_0$  and the coefficients  $a_1, \dots, a_n$  of  $P$  such that

$$|\chi(\mathcal{F})| \leq f_n(n_0; a_1, \dots, a_n).$$

**Cor.** If  $\mathcal{F} = \mathcal{O}_{\mathbb{P}^n}/\mathcal{J}$  then, by my arguments in Chap 14, one verifies (iii) for an  $n_0$  depending polynomially on  $a_1, \dots, a_n$  (*n.b.* control of  $H^2$  for  $\mathcal{J}$  is the same as control of  $H^1$  for  $\mathcal{F}$ ). Hence one gets a polynomial  $g_n(|a_1|, \dots, |a_n|)$

s.t.  $m \geq g_n(|a_1|, \dots, |a_n|) \implies \mathcal{J}$  is  $m$ -regular.

*Sketch of Proof:*

**Lemma 1.** Let the coherent sheaf  $\mathcal{F}$  on  $\mathbb{P}^n$  be  $n_0$ -regular, and let

$$\chi(\mathcal{F}(m)) = \sum_{i=0}^n a_i \binom{m}{i}.$$

Then

$$\begin{aligned} \dim H^i(\mathcal{F}(n_0 - \ell)) &\leq \binom{\ell - 1}{i} \cdot \sum_{j=1}^n a_j \binom{n_0 - i - 1}{j - i} \\ &= g_{i,n}(\ell, n_0, a_i, \dots, a_n) \end{aligned}$$

if  $\ell \geq 1, 0 \leq i \leq n$ .

Now, for any  $\mathcal{F}$ , put  $\mathcal{D}^i(\mathcal{F}) = \mathcal{E}xt_{\mathcal{O}_{\mathbb{P}^n}}^i(\mathcal{F}, \Omega_{\mathbb{P}^n}^n)$ . One checks that if  $H$  is a “good” hyperplane, then

$$\mathcal{D}^i(\mathcal{F}) \otimes \mathcal{O}_H = \mathcal{E}xt_{\mathcal{O}_H}^i(\underbrace{\mathcal{F} \otimes \mathcal{O}_H}_{\mathcal{F}_H}, \Omega_H^{n-1})(-1).$$

Call this  $\mathcal{D}^i(\mathcal{F}_H)(-1)$ .

GENERALITIES:

- 1°  $W_i = \text{supp } \mathcal{D}^i(\mathcal{F})$  has codim [at least]  $i$ , dim [at most]  $n - i$
- 2° For  $m \gg 0$ ,  $\dim H^0(\mathcal{D}^i(\mathcal{F})(m)) = \dim H^{n-i}(\mathcal{F}(-m))$
- 3°  $(n - i)$ -dimensional components of  $W_i$  are exactly the  $(n - i)$ -dimensional associated prime cycles of  $\mathcal{F}$ .

(\*) Now say  $\mathcal{D}^i(\mathcal{F}_H)$  is  $n_1$ -regular,  $i = 0, 1, \dots, i_0$ .

Then  $\mathcal{D}^i(\mathcal{F}) \otimes \mathcal{O}_H$  is  $(n_1 + 1)$ -regular,  $0 \leq i \leq i_0$ .  $\therefore$  (as in Chap 14)

$$H^j(\mathcal{D}^i(\mathcal{F})(m)) = (0), \quad j \geq 2, \quad m + j \geq n_1 + 1.$$

Look at the spectral sequence of duality:

$$\begin{array}{cccc}
 & * & * & * & * \cdots \\
 H^0(\mathcal{D}^{i_0+1}) & H^1(\mathcal{D}^{i_0+1}) & H^2(\mathcal{D}^{i_0+1}) & * & \cdots \\
 H^0(\mathcal{D}^{i_0}) & H^1(\mathcal{D}^{i_0}) & 0 & 0 & \cdots \\
 \vdots & \vdots & \vdots & \vdots & \\
 H^0(\mathcal{D}^0) & H^1(\mathcal{D}^0) & 0 & 0 & \cdots
 \end{array}$$

↑  
all transgressions are 0 here  $m \geq n_1 - 1$   
not killed either.

Therefore

$$\begin{aligned}
 \dim H^1(\mathcal{D}^i(\mathcal{F})(m)) &\leq \dim \text{ of } (i+1)^{\text{th}} \text{ term of abutment} \\
 &= \dim H^{n-i-1}(\mathcal{F}(-m)) \\
 &\leq g_{n-i-1,n}(n_0 + m, n_0, a_{n-i-1}, \dots, a_n)
 \end{aligned}$$

by lemma 1. Hence as in Ch. 14, we get estimate:

$$\begin{aligned}
 \mathcal{D}^i(\mathcal{F}) \text{ is } n_2\text{-regular, where} \\
 n_2 = n_1 + 1 + g_{n-i-1,n}(n_0 + n_1, n_0, a_{n-i-1}, \dots, a_n).
 \end{aligned}$$

**Prop.** Using induction, we prove that there are polynomials  $G_{n,i}$  such that:

- $\mathcal{D}^0(\mathcal{F})$  is  $G_{n,0}(n_0, a_{n-1}, a_n)$ -regular,
- $\mathcal{D}^1(\mathcal{F})$  is  $G_{n,1}(n_0, a_{n-2}, a_{n-1}, a_n)$ -regular,
- ...
- $\mathcal{D}^{n-2}(\mathcal{F})$  is  $G_{n,n-2}(n_0, a_1, \dots, a_n)$ -regular,
- $\mathcal{D}^{n-1}(\mathcal{F})$  is  $G_{n,n-1}(n_0, a_0, a_1, \dots, a_n)$ -regular,
- $\mathcal{D}^n(\mathcal{F})$  is  $G_{n,n}(n_0, a_0, a_1, \dots, a_n)$ -regular.

Now apply this to our case: by hypothesis (ii), get

$$\mathcal{D}^n = (0), \text{ and } \mathcal{D}^{n-1} \text{ has 0-dimensional support.}$$

Therefore,  $\exists G(n_0, a_1, \dots, a_n)$  s.t. all  $\mathcal{D}^i$  are  $G(n_0, a_1, \dots, a_n)$ -regular. Hence, if  $m \geq G(n_0, a_1, \dots, a_n)$

$$\dim H^0(\mathcal{F}(-m)) \leq \sum_{i=0}^n H^{n-i}(\mathcal{D}^{n-i}(m)) = 0.$$

$$\therefore |\chi(\mathcal{F}(-m))| \leq \sum_{i=1}^n \dim H^i(\mathcal{F}(-m)) \leq h(m, n_0, a_1, \dots, a_n).$$

**QED**

**Example:**

Family of 1-dimensional subschemes  $X_n \subset \mathbb{P}^3$  s.t.

- (a)  $X_n$  has no 0-dimensional component
- (b)  $\deg(X_n) = 2$ , all  $n$
- (c)  $\{X_n\}$  not limited, esp.  $\dim H^0(\mathcal{O}_{X_n}) \rightarrow +\infty$  in  $n$ .

*Proof* :  $X_n$  is to be double line:  $2\ell$ . Let line  $\ell$  be  $x = y = 0$ , where  $x, y, u, v$  are homogeneous coordinates. Let  $F_n$  be a surface  $xf_n(u, v) + yg_n(u, v) = 0$ , where  $f_n, g_n$  homog. of degree  $n - 1$ , no common linear factor. Then  $F_n$  has degree  $n$ ,  $F_n \supset \ell$ , and  $F_n$  is non-singular along  $\ell$ . Let  $X_n$  be the Cartier-divisor on  $F_n, 2\ell$ . i.e. ideal is  $(x^2, xy, y^2, xf_n + yg_n)$ . Then in fact

$$0 \longrightarrow \mathcal{J} \longrightarrow \mathcal{O}_{X_n} \longrightarrow \mathcal{O}_\ell \longrightarrow 0$$

where  $\mathcal{J}$  is the invertible sheaf  $\mathcal{O}_\ell(n - 2)$ .  $\therefore \dim H^0(\mathcal{O}_{X_n}) = n$ .

(III) Apropos of:<sup>1</sup>

$$\left. \begin{array}{l} V \text{ a n.s. projective 3-fold} \\ D \text{ a divisor on } V \\ (D \cdot \gamma) \geq 0 \text{ for all curve } \gamma \subset V \\ \Downarrow ? \\ (D^3) \geq 0 \end{array} \right\} (1)$$

or of the *stronger* conjecture:

$$\left. \begin{array}{l} V \text{ a n.s. projective 3-fold} \\ D \text{ a divisor on } V \\ \forall \text{ surfaces } F \subset V, \text{ assume that the} \\ \text{divisor class } (D \cdot F) \text{ on } F \text{ is ample.} \\ \Downarrow ? \\ D \text{ ample on } V \end{array} \right\} (2)$$

<sup>1</sup> Mumford wrote the following on the upper right margin above (III):

$$\left. \begin{array}{l} D^3 > 0 \\ (D^2 \cdot H) > 0 \\ (D \cdot H^2) > 0 \end{array} \right\} \stackrel{?}{\implies} |nD| \neq \emptyset \text{ for some } n$$

I don't know anything about their validity. However, they don't look impossibly difficult. Maybe the fellow Kleiman here will have an idea.

I've been working out Néron's latest height paper for applications to Mordell's conjecture. Personally, Néron seems to me to make a real mess of his theory; but I am finally seeing what it amounts to. It looks as if

- a) there is an intersection theory on absolute surfaces (i.e. regular 2-dimensional  $F$ , proper over  $\text{Spec}(\mathbb{Z})$ ), and the index theorem is still valid,
- b) as a consequence, if  $F \rightarrow \text{Spec } R \rightarrow \text{Spec}(\mathbb{Z})$  is Stein factorization,  $g$  (= genus of generic fibre over  $R$ )  $\geq 2$ , and if there is an infinite sequence  $x_1, x_2, \dots$  of rational points (i.e. sections of  $F/\text{Spec}(R)$ ) then  $\exists a > 0, b$  real s.t.

$$\text{ht}(x_i) \geq 2^{ai+b} \quad (\text{additive height})$$

or

$$\text{ht}(x_i) \geq 2^{(2^{ai+b})} \quad (\text{multiplicative height}).$$

Best wishes,  
(signed) David Mumford

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19 December, 1964

Bures 19.12.1964

Dear Mumford,

Can you please tell me if you have a counterexample to the conjecture, say, that in the category of complex analytic spaces (or schemes over  $\mathbb{C}$ ...) the functor corresponding to the classification of projective non singular surfaces and polarization, without automorphisms, is representable. What if we do not put the polarization in the structure, working in  $(\text{Sch})/\mathbb{C}$  say, and still restricting to structures without automorphisms? All I definitely remember is that you made an example showing the modular space is not separated. Did you ever publish examples of that kind?

I take the opportunity to ask you if you know an example of an algebraic surface (proj. non singular) over  $\mathbb{C}$ , whose  $H^2$  is spanned by algebraic cycles, which is not ruled? Or where moreover  $H^1 = 0$ , and which is not rational?<sup>1</sup> In fact, there are analogous question, and s for varieties of arbitrary dimension. . . Sincerely yours, A Grothendieck

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<sup>1</sup> If we interpret  $H^2$  and  $H^1$  as the Betti cohomology with coefficients  $\mathbb{Q}$ , then any Enriques surface  $X$  gives an example Grothendieck asked for. Indeed we have  $h^{2,0}(X) = h^{0,2}(X) = 0$ ,  $h^{1,1}(X) = 10$ , so  $H^2(X(\mathbb{C}), \mathbb{Q})$  is spanned by fundamental classes of divisors. Moreover  $\pi_1(X(\mathbb{C})) \cong \mathbb{Z}/2\mathbb{Z}$ , and  $H^1(X(\mathbb{C}), \mathbb{Z}) = (0)$ .



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 17 January, 1965

17.1.1965

Dear Mumford,

Thank you very much for your letter answering my questions concerning moduli. I am not sure I understand well what you mean though, and would appreciate some more precise information, if possible.

a) Let  $F$  be some open subfunctor of the following one  $G: G(S) =$  set of all classes (up to isomorphism) of  $X$  projective and flat over  $S$ , satisfying  $H^0(X_s, \mathcal{O}_{X_s}) \xrightarrow{\sim} k(s)$ , endowed with polarization (i.e. section of the Pic functor over  $S$ ) which is *very* ample on each fiber and satisfies  $H^1(X_{\bar{s}}, \mathcal{L}_{\bar{s}}) = 0$ ,  $\dim H^0(X_{\bar{s}}, \mathcal{L}_{\bar{s}}) = N$  for every  $s$  (these last restrictions for mere convenience), and every  $X_{\bar{s}}$  “without automorphism respecting the polarization”. By a standard argument using Hilbert schemes, if one assumes that  $F$  corresponds moreover to a fixed Hilbert polynomial,  $F$  is just a quotient  $M/G$ , where  $M$  is a projective scheme over  $\text{Spec}(\mathbb{Z})$  and  $G$  the projective group operating freely on  $M$ . As far as I understand from your letter, you have no example to the effect that  $F$ , i.e.,  $M/G$ , is non representable by a *prescheme* (of finite type over  $\text{Spec}(\mathbb{Z})$  necessarily!); the phenomenon you allude to when speaking of “birationally ruled surfaces” is just the fact that if you do not exclude these, your functor  $F$  will be non separated and more precisely the image of  $G \times M$  in  $M \times M$  will be non closed? On the other hand, when working in the category of analytic spaces rather than preschemes, you state (at least in the case of surfaces) you are pretty sure the quotient is representable—at least when you exclude the above mentioned surfaces. Now I once verified a general theorem of passage to quotient, in the context of analytic spaces, by a flat equivalence relation  $R \hookrightarrow M \times M$ , the conclusion being that the quotient is representable *if and only if*  $R \hookrightarrow M \times M$  is an immersion (or what amounts to the same, iff the topology of  $R$  is induced by the one of  $M \times M$ ); of course, if this condition (trivially necessary) is satisfied, then  $M/R$  is separated if and only if the immersion  $R \rightarrow M \times M$  is *closed*. Now in the case when the analytic situation comes from an algebraic one over  $\mathbb{C}$ , immersion, respectively, closed

immersion in the algebraic or in the analytic sense is the same. On the other hand, in the algebraic case, we have the valuative criterion in order to check immersions, respectively, closed immersions. Thus we are led exactly to the following question: Let  $X/S$  be a polarized scheme as above, corresponding to an element in  $F(S)$ ,  $S$  being the spectrum of a discrete valuation ring, let  $X'/S$  be another one, let  $s_0$  ( $s_1$ ) be the special (general) point of  $S$ , assume that  $X_{s_1}$  and  $X'_{s_1}$  are isomorphic, and  $X_{s_0}$  and  $X'_{s_0}$  isomorphic (by “isomorphism” we mean one respecting polarization; it must be necessarily unique). Is it true that  $X$  and  $X'$  are isomorphic? An affirmative answer is equivalent with the statement that  $R \hookrightarrow M \times M$  is an immersion, and if we restrict to  $S$  lying over  $\text{Spec}(\mathbb{C})$ , or simply “of char 0”, it is equivalent with the possibility of passing to the quotient analytically. Do you have any result concerning this question? Did you actually check that, when restricting to surfaces which are not birationally ruled, you even have the stronger result about  $R \hookrightarrow M \times M$  being a closed immersion, i.e.  $X_{s_1}$  and  $X'_{s_1}$  isomorphic implies  $X$  and  $X'$  are isomorphic? The ideal would be to be able to state some geometric conditions on the fibers of  $X/S$  characterizing those  $F$  for which either of the two valuative conditions are satisfied.

b) I did not understand your motivation for the feeling that, when dealing with variation of structure for non polarized *analytic* varieties without automorphisms, the functors you get will generally not be representable. The case of infinite discrete groups operating badly (as in the case of complex tori) seemed to me to occur precisely because of the existence of automorphisms, which I have excluded. On the other hand, your  $\text{Blow}_F$  functor is not an open subfunctor of the big functor I was considering, and the big one might be representable without the small one being so. In other words, it may happen that you have a family  $X/S$  of surfaces “without automorphisms”, such that the condition making the fibers birationally equivalent to a given fiber (a rather screwy condition by the way, of which I would expect nothing good anyhow) is *not* representable. What would be more convincing would be a case when, on the *local* variety of moduli  $M$  for a given analytic compact non singular variety  $X_0$  without automorphisms, there are points  $s$  arbitrarily near to  $s_0$  where the family  $X/M$  is no longer modular, for instance the Zariski tangent space to  $M$  at  $s$  has not dimension equal to  $\dim H^1(X_s, \mathcal{T}_{X_s})$ . I wonder if such kind of phenomena are actually known to you.

c) Serre told me about your remark on Siegel’s remark, which is extremely nice indeed. Do you think one can recover also the case of arbitrary polarizations (not necessarily separable ones)? I confess I am afraid that the additive type part of the kernel of the polarization might make trouble, as introducing a continuous set of indeterminacies. . . Besides, have you got any results on the actual dimension of the modular variety in char  $p > 0$  for polarized abelian varieties in the case of inseparable polarizations? You remember perhaps that the Zariski tangent space becomes bigger than usual, which implies that the modular variety must either have bigger dimension (which would imply that

there are polarized abelian varieties which do not lift to char 0) or else be non reduced everywhere. Did Serre tell you about his candidate for an abelian variety in char  $p$  which should not lift (an inseparable quotient of the product of two elliptic curves with Hasse invariant 0)?<sup>1</sup>

Sincerely yours  
(signed) A Grothendieck

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<sup>1</sup> It turns out that these two dimensional abelian varieties lift to characteristic zero. Mumford announced in [69a] that every abelian variety in char  $p$  can be lifted to an abelian variety in char 0. This program was completed in P. Norman and F. Oort, *Moduli of abelian varieties*, Ann. Math. 112 (1980) 413–439. It is also shown in loc. cit. that the dimension of the moduli space  $\mathcal{A}_{g,d}$  of  $g$ -dimensional abelian varieties with a polarization of degree  $d$  in characteristic  $p$  is  $g(g+1)/2$ . A theorem of Mumford asserts that the completed local rings for any closed point of  $\mathcal{A}_{g,d}$  is of the form  $k[[t_1, \dots, t_{g^2}]]/I$ , where  $I$  is an ideal generated by  $g(g-1)/2$  elements; see Thm. 2.3.3 on p. 242 of F. Oort, *Finite group scheme, local moduli for abelian varieties, and lifting problems*, p. 223–254 of *Algebraic geometry, Oslo 1970* (Proc. Fifth Nordic Summer-School in Math.), Wolters-Noordhoff, Groningen, 1972. The two results above imply that  $\mathcal{A}_{g,d}$  is a local complete intersection.



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23 January, 1965

I.H.É.S. 23.1.1965

Dear Mumford,

Thanks for your letter, I finally looked at your example with  $\text{Blow}_2$  and got your idea, namely that the valuative criterion I was asking for becomes definitely false if one does not include polarization into the statement—a strange fact in a way! Do you know if for surfaces as the ones of your example, there are *strict* local varieties of moduli, namely that the local variety of moduli of Kuranishi is still modular at points near the center? If so, one gets an “open” subfunctor of the big functor I told you about (except one is forgetting about polarization) which can be written as  $\mathcal{M}/\mathcal{R}$ ,  $\mathcal{M}$  the little Kuranishi modular variety,  $\mathcal{R}$  an analytic equivalence relation which is *étale* (i.e.,  $\mathcal{R} \xrightarrow{\text{pr}_1} \mathcal{M}$  is *étale*) but  $\mathcal{R} \rightarrow \mathcal{M} \times \mathcal{M}$  not being an immersion. It still would remain possible that one can represent the general functor corresponding to varieties of (say) analytic compact spaces without automorphisms, as  $\mathcal{M}/\mathcal{R}$ ,  $\mathcal{M}$  an analytic space and  $\mathcal{R}$  an *étale* equivalence relation—a problem equivalent, I guess, to whether the Kuranishi local modular variety is “strictly” so.

I have no particular use for an answer to the valuative question I asked you about, except that one certainly should know one day what is going on! Besides, I once had to ask you the analogous question for the Picard functor, in order to prove theorem 3.1, page 16-13 of Cartan’s seminar 60/61; I begin to believe that this theorem is probably false, as I do not see any reason why the corresponding valuative criterion should be valid. You would probably be able to get a counterexample out of your shirt’s sleeve, if you tried.

By the way, Raynaud remarked that if  $G \rightarrow S$  is a group prescheme over  $S$ , flat, of finite presentation, with *connected fibers*, then  $G \rightarrow S$  is *necessarily separated*.<sup>1</sup> This yields lots of cases where  $\mathcal{P}ic_{X/S}^0$  and  $\mathcal{P}ic_{X/S}$  are not representable, although the standard conditions (implying formal representability)

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<sup>1</sup> See SGA3 VI<sub>B</sub>, Propriétés générales de schemas en groupes, Cor. 6.5 on page 351 of LNM 151, Springer-Verlag, 1970 for a proof of Raynaud’s remark.

are satisfied. Take for instance  $X \rightarrow S$  with  $S$  the spectrum of a discrete valuation ring,  $f$  proper,  $X$  regular,  $[\dim(X) = 2]$  generic fiber  $X_1$  smooth and geom. connected, special fiber  $X_0 = \sum \nu_i C_i$  ( $C_i$  prime divisors,  $\nu_i > 0$ ), let  $d = \gcd((\nu_i))$ . Then if  $d > 1$ ,  $\mathcal{P}ic_{X/S}^0$  is non separated and hence *non representable*, therefore  $\mathcal{P}ic_{X/S}$  is not reps. either. Besides, if  $d = 1$  one gets a canonical morphism of functors

$$\varphi : \mathcal{J}^0 \rightarrow \mathcal{P}ic_{X/S}^0$$

where  $\mathcal{J}$  is the Néron model of the Jacobian of  $X_1$ , and  $\mathcal{P}ic_{X/S}^0$  representable  $\iff \varphi$  is an isomorphism  $\iff \varphi_0$  is an isomorphism ( $\varphi_0 = \varphi \otimes k(S)$ ). I do not know if these conditions are always satisfied when  $d = 1$ .<sup>2</sup>

Sincerely yours,  
(signed) A Grothendieck

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<sup>2</sup> The condition “ $\dim(X) = 2$ ” is inserted by the editors. Proof of these assertions about  $\mathcal{P}ic_{X/S}^0$  and further information can be found in M. Raynaud, Spécialisation de foncteur de Picard, Publ. Math. I.H.E.S. n<sup>o</sup> 38, 1970, 27–76. The assertion that  $\mathcal{P}ic_{X/S}^0$  is not representable if  $d > 1$  is part of Thm. 2.1 on p. 66. The statement about  $\varphi : \mathcal{J}^0 \rightarrow \mathcal{P}ic_{X/S}^0$  is proved in Thm. 8.2.1 on page 66. The answer to the question in the last sentence is *yes* if for instance the residue field of the closed point of  $S$  is perfect; see Thm. 8.1.4 on page 65.

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 16 April 1965

Bures April 16, 1965

Dear Mumford,

I read through your nice notes from Woods Hole: “Further comments on boundary points”.<sup>1</sup> Unfortunately, my copy stops on page 7 with the words: “the curve of genus 2 depicted below:”. Are there pages lacking, or were you making fun?<sup>2</sup> I have just two mathematical comments: Page 4, lines 1 and 2<sup>3</sup> the equality  $\text{Pic}(X) = \text{Pic}(X_0)$  holds only in the stable case, otherwise you have to replace the Pic groups by the Néron-Severi groups to have a correct statement. Page 5, line 10<sup>4</sup>, it does not seem clear to me (but rather unlikely!) that  $\Theta$  is really an isomorphism of  $\mathcal{M}_g$  with a locally closed subvariety of  $V_g$ , say  $N$ , all I know is that the image  $N$  of  $\mathcal{M}_g$  is indeed locally closed, and  $\mathcal{M}_g \rightarrow N$  is finite and radicial, and in fact makes  $\mathcal{M}_g$  a normalization of  $N$  ( $N$  turns out to be geometrically unibranch). Your statement would mean that  $N$  is normal, which I doubt to be true. Besides, line 11<sup>5</sup> reads  $\mathcal{M}_g$  instead of  $\mathcal{M}'_g$ . Also, page 7, I did not quite see what you mean on line 12<sup>6</sup>, do you just mean to say that there is no natural definition, by analogy with the previous discussions, of a notion of semi-stable curves?

One question about your result on page 4, giving as it seems an axiomatic description of the Satake compactification, analogous to the axiomatic description of  $V_g$  itself: a) Is there a hope to get such a description over  $\text{Spec}(\mathbb{Z})$ ? b) What about throwing in “levels”? How should one define a level  $n$  rigidification on your group schemes?

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<sup>1</sup> [u64b]

<sup>2</sup> The missing picture depicting a stable curve of genus 2 is “a dollar sign lying on its side”.

<sup>3</sup> [u64a], §2, Explanation 1°

<sup>4</sup> the beginning of §3 of [u64b]

<sup>5</sup> This misprint in §3 of [u64b] is corrected in the retyped version in this volume.

<sup>6</sup> the last sentence of §3 of [u64b]

106 29 16 April 1965

Sincerely yours  
(signed) A Grothendieck

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9 October, 1965

Bures 9.10.1965

Dear Mumford,

Thank you very much for your notes on theta functions (pages 1 to 77),<sup>1</sup> and your book on geometric invariant theory.<sup>2</sup> I was certainly pleased by the advertising you are doing there for schemes, and flattered by the opinion you express in your introduction on my own work. However, I am sorry to state I have not really read it as yet, although this is in my program and the short size and vivid style will make it not too hard for me to stick to it, I hope! I just had a quick reading of your notes on  $\theta$ -functions, which look very nice indeed; I hope, when you will have written up the whole, you'll send a copy of the remainder too. Maybe you could include too a description of the group of automorphisms of the extension  $\mathfrak{g}(\mathcal{L})$  (is the first letter supposed to be a gothic  $G$ ?), which will eventually act on the modular scheme, and of what happens when you replace  $\mathcal{L}$  by  $\mathcal{L}^{\otimes n}$ , variable  $n$ , as one would like to know how the corresponding modular schemes match together. A paragraph giving the connection with the transcendental construction would be nice too, for ignorant people like myself—or is this what you intend to do in your paragraph 6? How do you intend to publish the theory? Another book would not seem a bad idea! In this case, or for any other book you would care to write (e.g. theory of surfaces), I would like to mention to you that Kuiper and I (and maybe a third man who is still not well determined) are starting to publish a new series of advanced books on pure mathematics, in North Holland Publishing Company, and we would certainly appreciate to have you in the series. The first book in our program will be one of Giraud on non-commutative homological algebra,<sup>3</sup> essentially his thesis in fact. He did quite

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<sup>1</sup> [66a]

<sup>2</sup> Published as D. Mumford, *Geometric Invariant Theory*, *Ergeb. Math. u. Grenzgebiete* 34, Springer-Verlag 1965

<sup>3</sup> Published as J. Giraud, *Cohomologie non abélienne*, *Grundlehren der Math. Wiss.* 179, Springer-Verlag 1971.

a good job, although to a great extent expository, and it will certainly become a standard reference within a few years. For instance, I will have to use his formalism intensively to formulate a Galois theory for motives. Besides, I am more or less decided to write a book myself on the theory of motives, despite the fact that it is likely that the whole theory will remain conjectural for a long time, so it should be called a program of a theory rather than a theory.

By the way, I have the feeling that one main point in realizing something like Kronecker's "Jugendtraum" is a theory of moduli for *motives*, and it is already clear to me how in this optic to generalize the Siegel generalized half-plane and the Siegel modular group acting on it (these corresponding just to polarized motives of weight 1). The deeper point is how to put an algebraic structure on the quotients one obtains and how to interpret geometrically these quotients as modular schemes for polarized motives. Now this question should in essence be identical with the following one, for which your present work perhaps may give you some idea. By a *Hodge structure*, I will mean a free module of finite rank  $M$  over  $\mathbb{Z}$ , together with a bigrading of the complex vector space  $M \otimes_{\mathbb{Z}} \mathbb{C}$ , having positive partial degrees, and total degree  $n$  if we want a Hodge structure of weight  $n$ , and such that there should exist a bilinear form  $\phi : M \times M \rightarrow \mathbb{Z}$ , alternating or symmetric according as  $n$  is odd or even, such that  $\phi_{\mathbb{C}}$  is compatible with the bigradings when  $\mathbb{C}$  is considered of degree  $(n, n)$ , and that  $\phi_{\mathbb{C}}(\eta x, \bar{x}) (-1)^n$  should be a positive definite Hermitian form on  $M_{\mathbb{C}}$ , where  $\eta$  is multiplication by  $(-1)^p$  on the component of first degree  $p$ . Such a form will be called a *polarization of the Hodge lattice*  $M$ . If  $X$  is a projective non singular variety defined over  $\mathbb{C}$ , then by Hodge theory the lattices  $H^n(X, \mathbb{Z})$  mod torsion can be viewed as Hodge lattices, any polarization of  $X$  (in the classical sense) defining a polarization of that Hodge lattice. In this way, one gets a functor from the category of (semi-simple effective) motives defined over  $\mathbb{C}$  (never mind what that means for the moment!) into the category of Hodge lattices. Hodge's conjecture just amounts to saying that this is a fully faithful functor, at least when working modulo isogeny, and I feel the whole story in this respect should be that it is even an *equivalence* of categories. In a down to earth way, this essentially amounts to saying that any Hodge lattice is isomorphic to a sub Hodge lattice (in fact, a direct factor) of Hodge lattice  $H^n(X, \mathbb{Z})$ , always up to isogeny. By general principles one should be able to take  $\dim X = n$ , (although this would not be a good idea when starting with a Hodge structure of weight 1, as then the natural  $X$  to take would be the associated abelian variety, not some generating curve on it), and to restrict to consideration of the primitive part (in the sense of Hodge-Lefschetz theory, namely the part that, restricted to a hyperplane section, vanishes). The problem then is to give, in terms of the "transcendental data" of a polarized Hodge structure  $M$ , an explicit construction of some canonical  $X$ , presumably with a definite projective embedding, realizing that Hodge lattice, for instance as being isomorphic (or isogenous) just to the primitive part of  $H^n(X, \mathbb{Z})$ . To do things quite canonically, probably something like your  $\theta$ -level structure will be required as an extra-structure on  $M$ . More impor-

tant, the construction should be so canonical as to carry over to continuous, and more specifically, to complex-analytic families of Hodge structures (these being defined in a rather evident way, but taking care that an “integrability condition” has to be satisfied in the complex analytic case). Also, in view of Kronecker’s dream, the finite modules  $M/nM$  over  $\mathbb{Z}/n\mathbb{Z}$ , variable  $n$ , should be recoverable in some way on the model  $X$ , say as finite subsets, as in the case  $n = 1$  the points of finite order of an abelian variety. This latter point, I confess, is still very vague in my mind; it will have to be tied up with some extra structures on  $X$ , replacing the additive structure of an abelian variety (maybe a “hypergroup” structure, as inherited for instance by the quotient of a group by any finite group of automorphisms acting)... Of course, this would not be of any arithmetic use unless it also behaves properly with respect to families. Besides, I feel much less positive about the possibility of this latter element of structure, perhaps the analogy with abelian varieties is fallacious in this direction. After all, even without it, it will turn out that the transcendental functions of passage to quotient, from the Siegel-type modular spaces (which can be defined as certain homogeneous spaces of algebraic groups defined over  $\mathbb{Q}$ ) to their algebraic quotient spaces (obtained by passing to the quotient by various discrete subgroups, commensurable with the group of integral points) will allow a description of the various “motivic” classes of infinite Galois extensions, as generated by values of transcendental functions constructed some way or other from the previous ones.

One question about your theory of  $\theta$ -functions. You always make that assumption of separable polarization. But you have certainly noticed that for any abelian scheme  $X$  over a base  $S$ , endowed with a polarization, defining a morphism  $X \rightarrow X'$  with kernel the finite flat group scheme  $K$ , your definition yields a canonical extension

$$1 \rightarrow \mathbb{G}_{mS} \rightarrow E \rightarrow K \rightarrow 1,$$

and the group-scheme  $E$  operates on  $f_*(\mathcal{L})$  whenever your polarization is given by an actual invertible sheaf  $\mathcal{L}$  (not a great restriction as you know). Maybe it’s nicer to view  $K$  as acting on the Brauer-Severi bundle defined by the polarization! How far does your theory extend to this case? For instance do you know, when  $S$  is the spectrum of an algebraically closed field, if this representation is still irreducible? Because of the variety of possibilities of structure of  $K$  (as a non separable group scheme), you cannot reduce all possible extensions  $E$  to a discrete set of standard types, by which you then rigidify. However, nothing would prevent you from starting with one extension  $E_0$ , and looking at those polarized abelian schemes whose extension  $E$  is locally isomorphic (for the fpqc topology, say) to  $E_0$ , and looking at modular schemes for these. Certainly Cartier’s theorem will tell you anyhow that the alternating form  $K \times K \rightarrow \mathbb{G}_m$  is non degenerate (i.e.  $K$  is autodual in the sense of Cartier), thus the main point seems first to pick out a  $K$  with such an autoduality. For instance, you can start with any flat commutative  $K'$ , and take  $K = K' \times K''$ , with  $K'' = D(K')$  (Cartier dual). The case when you

take precisely for  $K'$  an *étale* group, and  $K''$  his dual, is essentially the one you consider in your notes, but one does not see any reason why to restrict to residue characteristics prime to rank  $K'$ ; in a way, you should still get then the more general polarizations in any characteristics (namely those for which you have the maximum number of geometric points in  $K$ )—they deserve not to get lost on your way! Besides, it looks an interesting question to determine, over a perfect field say, all finite commutative group schemes over  $k$  endowed with an alternating autoduality—there should not be any difficulty to get the complete picture and see whether any such  $K$  can be written as  $K' \times D(K')$  and the obvious form on it (one will have to use Dieudonné-Gabriel's structure theory in terms of modules over the Witt vectors, together with the operations  $V, F, \dots$ ). Maybe even you will be able to get in your system *smooth* modular schemes, which might allow you to solve the problem of lifting an abelian variety to char. 0.<sup>4</sup>

Best wishes  
(signed) A Grothendieck

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<sup>4</sup> See the note at the end of [1965January17].

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1 November, 1965

Algiers Nov. 1, 1965

Dear Mumford,

I have become aware that I have been a bit rash with the conjectures I told you about Hodge structures, as in the form I stated them they contradict Tate's conjectures, which implies the following: if  $X$  is a smooth, connected *simply connected* scheme over  $\mathbb{C}$ , and  $\mathcal{V}$  a polarized complex analytic family of Hodge structures parametrized by  $X^{\text{hol}}$ , then  $\mathcal{V}$  is "algebraic" only if it is a *constant* family. Now it is easy to get examples of  $\mathcal{V}$ 's, (with  $X$  say *any* homogeneous space under an affine group over  $\mathbb{C}$ ) which are *non* constant, therefore (one hopes!) not "algebraic" (as one would like Tate's conjectures to hold). However, one should try to test if one really *cannot* get an algebraic  $\mathcal{V}$  that way. To start, I wonder if a single Hodge structure  $\mathcal{V}$  with *even partial degree* (therefore giving rise to a "Hodge-group"  $G$  s.th.  $G_{\mathbb{R}}$  is *compact*) can be algebraic (except when  $G$  commutative). For instance, do you know an algebraic smooth projective surface  $Y$  over  $\mathbb{C}$ , such that  $H^{1,1}(Y, \mathbb{C})$  is spanned by algebraic cycles, and  $\dim H^{0,2}(Y, \mathbb{C}) \geq 2$ ?<sup>1</sup> I would appreciate your comments! Yours,

(signed) A Grothendieck

P.S. What about your  $\theta$ -functions, are you going to write a book?

P.S. Here is what are the modular varieties one gets from considerations of motive-theoretic Galois theory.

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<sup>1</sup> In [1965December3], AG said that DM solved this question affirmatively.

$G =$  reductive connected alg. group defined over  $\mathbb{Q}$

$\mathcal{U}_{\mathbb{R}} =$  dimension one twisted real torus, with  $\mathcal{U}_{\mathbb{R}}(\mathbb{R})$  identified with the group of complex numbers of modulus 1

$i_0: \mathcal{U}_{\mathbb{R}} \rightarrow G_{\mathbb{R}}$  homomorphism of algebraic groups over  $\mathbb{R}$ , s.th.  $i_0(-1)$  is central, and s.th. the centralizer  $K_{\mathbb{R}}$  of  $i_0(\sqrt{-1})$  is such that  $K_{\mathbb{R}}(\mathbb{R})^0$  is *maximal compact* in  $G_{\mathbb{R}}(\mathbb{R})^0$ . Moreover, for every  $\mathbb{R}$ -simple component of  $G_{\mathbb{R}}$ , the corresponding component of  $i_0$  is not trivial.

$C_{\mathbb{R}} = \text{Centr}_{G_{\mathbb{R}}}(i_0) \subset K_{\mathbb{R}}$

$Q = G_{\mathbb{R}}/C_{\mathbb{R}}$

“Siegel-space” =  $\mathfrak{S} = \frac{G(\mathbb{R})^0}{G(\mathbb{R})^0 \cap C_{\mathbb{R}}(\mathbb{R})} \xrightarrow{\text{(a connected component)}} Q(\mathbb{R})$

N.B.  $\mathfrak{S}$  has a natural complex structure (inherited from the one on  $Q(\mathbb{R})$  in fact) invariant by the operation of  $G(\mathbb{R})$ . Every linear representation (over  $\mathbb{Q}$ ) of  $G$  in a vector space  $V$  (over  $\mathbb{Q}$ ), together with a lattice  $V_0$  of  $V$ , defines a *complex analytic* family  $\mathcal{V}$  of Hodge structures (necessarily globally polarizable—to get a polarization of the family, one picks a “form of polarization”  $\varphi$  on  $V$  invariant by  $G$ ) on  $\mathfrak{S}$ , at least when one fixes a “total degree”  $n$  and assumes that  $i_0(-1)$  operates on  $V$  as the homothety  $(-1)^n$ . (It is not hard to describe  $\mathfrak{S}$  by a universal property as representing some functor  $F(X)$ ,  $X$  complex analytic, explaining that every  $V$  as above should define, in an additive and multiplicative way for varying  $V$ , a Hodge-structure  $\mathcal{V}$  over  $X$ .) If  $\Gamma \subset G(\mathbb{R})$  is the group leaving  $V_0$  fixed, then  $\Gamma$  operates on  $(\mathcal{V}, \mathfrak{S})$ .

Note that in general  $\mathfrak{S}$  is *not* Riemannian symm., but is a fiber space over a R.S. space, with fibers which are projective complex homogeneous spaces under  $K_{\mathbb{C}}$ . This implies at least that  $\mathfrak{S}$  is *simply connected*, hence is the univ. covering of  $\mathcal{M} = \mathfrak{S}/\Gamma$  if  $\Gamma$  operates freely.<sup>2</sup>

The modular varieties I expect to carry algebraic structure *over*  $\mathbb{Q}$  (and morally,  $\mathbb{Z}$ ) are the varieties  $\boxed{\mathfrak{S}/\Gamma}$ , where  $\Gamma \subset G(\mathbb{Q}) \cap G(\mathbb{R})^0$  is commensurable with  $G(\mathbb{Z})$  (for some matrix representation of  $G$ , giving a meaning to  $G(\mathbb{Z})$ ). However, it is possible that one will have to impose on the data some extra conditions, either arithmetic (including existence of “Frobenius elements” in  $I(\mathbb{Q})$ ,  $I = G/\text{flat.aut.}$ <sup>3</sup>), or geometric on  $G_{\mathbb{R}}$  (such as:  $G_{\mathbb{R}}$  has *no compact factor*). A typical case which fits in the general description, but for which I have no evidence so far if  $\mathfrak{S}/\Gamma$  has an algebro-geometric interpretation in terms of moduli for motives, is the following: start with a vector space of finite dimension  $V$  over  $\mathbb{Q}$ , with a fundamental bilinear form  $\varphi$  given, symmetric or alternating, take  $G = \text{SO}(\varphi)$ , take any  $i_0: \mathcal{U}_{\mathbb{R}} \rightarrow G_{\mathbb{R}}$  s.th.  $i_0(-1) = \text{id}$  (if  $\varphi$  symmetric),  $i_0(-1) = -\text{id}$  (if  $\varphi$  is skew-symmetric), and a positivity con-

<sup>2</sup> This paragraph was written vertically in the left-hand margin.

<sup>3</sup> i.e.  $I$  is an inner form of  $G$ .

dition of  $\varphi$  relative to the bigrading of  $V_{\mathbb{C}}$  defined by  $i_0$ , which I will not write down. Then  $\mathfrak{S}$  is a direct generalization of Siegel's half-space, and classifies all Hodge-structures with underlying  $\mathbb{Q}$ -vector space  $V$ , i.e. all bigradings of  $V_{\mathbb{C}}$ , satisfying the usual symmetry condition for complex conjugation, which are compatible with  $\varphi$  (i.e.  $\varphi$  is a polarization), and for which the  $\dim V_{\mathbb{C}}^{p,q}$  have given values (depending on the weights of  $\mathcal{U}_{\mathbb{C}}$  operating on  $V_{\mathbb{C}}$ ...). The thought is that, as these special types of  $\mathfrak{S}$ ,  $\mathfrak{S}/\Gamma$  are in a way "universal" for all others (namely the  $\mathcal{V}$ 's considered above are induced from ones on such special type of  $\mathfrak{S}$ ), if the "modular family"  $\mathcal{V}$  of Hodge-structures on such a  $\mathfrak{S}/\Gamma$  was "algebraic" i.e. came from a relative motive over the complex analytic space  $\mathfrak{S}/\Gamma$ , (or even on some underlying structure of alg. var. over  $\mathbb{C}$ ), the same would be true for *every* modular family on some  $\mathfrak{S}/\Gamma$ . Taking the case  $G(\mathbb{R})$  compact, and thus  $\mathfrak{S}/\Gamma = \mathfrak{S} =$  complex projective homogeneous space  $X$  under  $G_{\mathbb{C}}$  by Borel, we would get a polarized motive over the analytic space  $\mathfrak{S}$  corresponding to  $X$ . If we admit the GAGA yoga (coming from polarized abelian varieties) that such a family must also be a motive over  $X$ , (or the above strengthening of the original assumption), we get a contradiction with Tate's conjectures (because  $X$  is simply connected, as well known). So I really do not know what to believe!<sup>4</sup>

To come back to the general case, maybe I should add that a *necessary* condition for  $\mathfrak{S}/\Gamma$  to be algebraic and the Hodge-structure  $\mathcal{V}/\Gamma$  over  $\mathfrak{S}/\Gamma$  algebraic too, is (*granting* Tate's and Hodge's conjectures): for every  $g \in G(\mathbb{R})^0$ , and every  $\Gamma_0$  of finite index in  $G(\mathbb{Z})$ , the smallest alg. subgroup  $H$  of  $G$  such that  $H_{\mathbb{R}}$  contains  $g[i_0(\mathcal{U}_{\mathbb{R}})]g^{-1}$  and  $\Gamma_0$  is  $G$  itself.<sup>5</sup> Question: If  $G_{\mathbb{R}}$  is semi-simple without compact factor, is  $G(\mathbb{Z})$  always Zariski-dense in  $G$ ?<sup>6</sup>

By the way: did you make out if, for the modular varieties  $\mathfrak{S}/\Gamma$  of your Boulder talk (which I have just read), the image of  $\mathfrak{S}/\Gamma$  in the usual Siegel modular variety is an algebraic variety, or at least constructible?<sup>7</sup>

<sup>4</sup> Mumford's answer "*PROBABLE ANS.*  $\sim \exists$  alg. families of  $\mathfrak{S}/\Gamma$  (since  $\mathfrak{S}/\Gamma$  not alg. itself)" is written vertically in the left-hand margin.

<sup>5</sup> The following sentence is crossed out in the hand-written letter. "I did not try to test directly this condition, even in the case of the *usual* Siegel modular space, if it should be false in that case, as we know that  $\mathfrak{S}/\Gamma$  is algebraic and that so also the  $\mathcal{V}/\Gamma$  over it, it would follow that either Hodge's or Tate's conjectures is wrong."

<sup>6</sup> An arrow is drawn from this question to Mumford's answer "Yes??" in the left-hand margin.

<sup>7</sup> Mumford's answer "I believe so, but haven't written it down." is in the left-hand margin.



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3 December, 1965

Bures Dec. 3, 1965

Dear Mumford,

Thanks for your letter. I am sorry my own from Algiers has not reached you. Before writing it all over again I'll wait to see if by chance it has not gone by sea mail. Please tell me if it should still arrive. Indeed a few days after my over-optimistic letter of October I got more realistic, on the matter of deducing more or less arbitrary families of Hodge structures from motives, as this was going to conflict with Tate's conjectures. In my Sahara letter I was expounding to you in some detail my perplexities, and gave also a detailed description of the complex analytic modular varieties I had been interested in, generalizing the Siegel-Griffiths ones.<sup>1</sup> I well know they are not bounded domains in general, but fiber spaces over such bounded domains, with fibers homogeneous spaces under complex linear groups. . . This in itself would not bother me. Nor do I understand what are the results of Griffiths, and if they are really conclusive to the effect that there are no algebraic structures where I first expected some. With the notations of your letter, did you mean to say that for *every*  $\Gamma$ -linearized invertible bundle on  $D$ ,  $H^0(D/\Gamma, \mathcal{L}^{\otimes n})$  is zero (and this, still when replacing  $\Gamma$  by any subgroup of finite index, so as to achieve for instance that  $\Gamma$  should operate freely)? Besides, I never got any reprint from Griffiths. If his result concerns only the "canonical" bundle of highest differential forms, this alone does not look so convincing. From my point of view the main trouble is that it is very easy, among the modular varieties I alluded to for varying Hodge structures, to get any type of compact algebraic homogeneous space under a complex algebraic linear group, for instance  $X = \mathbb{P}^1$ , but such families can not stem from a motive (say over the field of functions of  $X$ , or its algebraic closure. . .), without

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<sup>1</sup> The "Siegel-Griffiths ones" and "results of Griffiths" below refer to the period spaces announced by P. Griffiths in Proc. Nat. Acad. Sci. USA **55** (1966) 1303–1309 and 1392–1395, **56** (1966) 413–416; later published in Amer. J. Math. 90 (1968) 568–626.

contradicting Tate's conjectures, which imply the following: let  $k$  be a field, (here  $k = \mathbb{C}$ ),  $K$  a "regular" extension of finite type,  $M$  a motive over  $K$  such that the operations of  $\text{Gal}(\overline{K}/K)$  on  $T_l(M) = l$ -adic cohomology realization of  $M$  (some  $l$  prime to the char) is trivial, then  $M$  comes from a motive over  $k$  (and in the case  $k = \mathbb{C}$ ,  $K$  the function field of some  $X$  over  $\mathbb{C}$ , this implies that the corresponding family of Hodge structures on  $X$ , or some Zariski-open subset of  $X$  to be more accurate, is constant). Now  $\mathbb{P}^1$  is simply connected, and any family of Hodge structures parametrized by  $\mathbb{P}^1$  must give rise to a trivial  $T_l$ ! Notice that I do not demand that the "family" of smooth projective algebraic varieties, whose Hodge cohomology should contain the given Hodge structure over  $X$  as part of it, needs to be defined on the whole of  $X$ ; it is enough that it be defined on a Zariski-open subset  $\neq \emptyset$  (or only on some  $X' \neq \emptyset$  étale over  $X$ ), in order to contradict Tate's conjectures. As you surmised, what matters is not that the given family of projective varieties should be non constant, but that its Hodge cohomology (or rather, the piece of it we are looking at...) should be so. The question I was asking, about the existence of individual surfaces with  $\rho = h^{1,1}$  and  $h^{2,0} > 2$ , and which you solved affirmatively,<sup>2</sup> arises when one wants to get such a compact simply connected modular variety for Hodge structures of degree 2, by looking at the "Hodge-group" for it and asking that it be compact. By the way, do you know any actual example of a Hodge structure which cannot be embedded in the Hodge cohomology of an algebraic variety?

Sincerely yours  
(signed) A Grothendieck

P.S.: Did you discuss a bit with Artin on the notion of motive?

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<sup>2</sup> The question in [1965November1] asks for surfaces with  $\rho = h^{1,1}$  and  $h^{2,0} \geq 2$ .

9 December, 1965

Dec 9.12.1965

Dear David,

I am glad to know you got my Algiers letter after all. Maybe you can say me a few words more about Griffiths, as I asked already in my previous letter, which I wrote Monday.

The fact that motives over *any* field forms an abelian category follows from conjectures A and B of the letter to Serre of which I sent you a copy.<sup>1</sup> These, I feel, are considerably less remote than Tate's and Hodge's conjectures, at least I hope so. By the way, why don't you discuss a bit with Mike on motives—I spent about one day telling him about the yoga. Also, in a letter from Algiers to him, I raised a few questions in connection with “*p*-adic” cohomology in char. *p* and Hodge cohomology, with the hope that, if some answers are negative, he or you would know and tell me right away.

Motchane tells me you are planning to come to IHÉS in 1967/68. That would be great—but please tell me if it is not just an extrapolation by M. of what you really stated to him!

Yours,

(signed) A Grothendieck

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<sup>1</sup> This 27 August 1965 letter to Serre was published in *Correspondance Grothendieck–Serre*, Soc. Math. France, 2001, 232–235.



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9 May, 1966

Pisa 9.5.1966

Dear Mumford,

Thanks a lot for your preprint “Abelian quotients of the Teichmüller modular group”<sup>1</sup>. I have a few questions and comments.

- P.1 I think the formulation you give of Riemann’s existence theorem is due not to Artin but to Serre, who gave a Bourbaki talk on this about ten years ago.<sup>2</sup>
- P.2 it seems to me that you prove th.2 only in a weaker form, replacing “rational maps” by “morphisms”. If th.2 is true as stated, maybe you should show why.
- P.6 the lemma is due, I believe, to Matsumura (in his thesis). By the way, your proof of b) using the assumption of blowing down, whereas the statement works on any normal base, is somewhat misleading.
- P.7 cor.1, do you know if “non-singular” can be replaced by “normal”?
- P.10 when you pretend you can compactify the modular variety for curves of genus  $g$ , adding only pieces of codimension at least 2, should you not assume  $g \geq 3$ ? It seems to me Igusa has proved that for  $g = 2$ , the modular variety is affine. In any case, I think it would be useful for the reader that you state somewhat more explicitly what your and Mayer’s result says; (or is it really in your Woods Hole talk<sup>3</sup>?).
- P.15 the statement of Dehn’s main result, by a simple reference to the figure 2, is not very clear. Maybe you could say what the generating curves are? One thing I found very misleading when reading your drawings was the double sense of the word “holes”, which you use in a certain sense p.14, whereas on figure 3 it seems to mean a “handle”.

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<sup>1</sup> Published in [67d].

<sup>2</sup> See Thm. 1 of J.-P. Serre, *Revêtement ramifié du plan projectif*, Séminaire Bourbaki 1959–60, n° 204.

<sup>3</sup> Referring to [u64b].

P.21 can you send me a reprint of [6]?

As a general impression, I found it kind of astonishing that you should be obliged to dive so deep and so far in order to prove a theorem whose statement looks so simple-minded. For instance, using linear pencils of plane curves, could one not prove that any two sufficiently general curves of genus  $g$  can be connected by a linear family of curves?

I recently got a general result on modular varieties for rigidified abelian varieties, which I believe should be shared by all or at least many of the non singular algebraic varieties you get from arithmetic type discrete groups operating on suitable homogeneous spaces—namely the following: if  $X$  is connected, reduced and locally of finite type over the ground field  $k$ , with given geometric point  $x$ , then a morphism  $X \rightarrow M$  is known when you know the geometric point image of  $x$ , and the action on the fundamental groups  $\pi_1(X, x) \rightarrow \pi_1(M, f(x))$ . This is a corollary of the following: if  $A, B$  are two abelian schemes over  $X$ ,  $l$  a prime number,  $u_l: T_l(A) \rightarrow T_l(B)$  a homomorphism, and if the restriction of  $u_l$  to  $x$  comes from a homomorphism  $A_x \rightarrow B_x$ , then  $u_l$  comes from a homomorphism  $u: A \rightarrow B$  (of course unique).<sup>4</sup> These results I can prove only if  $k$  is of char 0, and the proof uses quite sophisticated means, such as some very recent result of Tate's on his " $p$ -divisible groups" on local fields with unequal characteristics, and Serre and Tate's lifting theory for abelian varieties in char  $p > 0$ . I wonder if you could think of any purely transcendental proof of the same result? I am finishing writing up the story and will send you a preprint of the proof pretty soon.

Best wishes

(signed) A Grothendieck

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<sup>4</sup> This result is published in A. Grothendieck, Un théorème sur les homomorphismes des schémas abéliens, *Invent. Math.* **2**, 1966, 59–78.

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18 May, 1966

Manua di Pisa 18.5.66

Dear David,

I wonder if I did not misunderstand the last question of your letter (under the name, devil knows why, of “a Mordell-Weil theorem”), as the affirmative answer seems so trivial. Namely let  $A, B$  be abelian preschemes over any connected  $S$  of char. 0,  $s \in S$ ,  $u_s: A_s \rightarrow B_s$  a homomorphism, claim: there exists a largest abelian subscheme  $Z$  of  $A$ , such that  $u_s|_{Z_s}$  lifts to  $Z \rightarrow B$ , namely: if  $Z, Z'$  are such, they are majorized by a third one  $Z''$  having the same property. To see this let  $v: Z \times_S Z' \rightarrow A$  be the natural morphism,  $N$  its kernel.  $N$  is smooth over  $S$  (true for the kernel of any homomorphism of proper and smooth group preschemes over an  $S$  of char 0, as you will easily check), and of course proper over  $S$ . Take  $Z'' = (Z \times_S Z')/N$ . (NB This exists even without projectivity assumption on  $Z \times_S Z'$  over  $S$ , by the way, using a general theorem of passage to quotient which, I believe, is stated in Murre’s talk on unramified functors).<sup>1</sup> This  $Z''$  can be identified with an abelian subgroup-prescheme of  $A$ . Of course if  $u_s$  is a monomorphism then, for every  $Z$  as above  $Z \rightarrow B$  is a monomorphism, as the kernel is smooth and proper over the connected  $S$ , and the fiber of said kernel at  $s$  is zero, hence also the whole kernel.

In char  $p > 0$  or unequal characteristics these results are no longer true, as follows from Koizumi’s example. It will be true however if  $S$  is regular of dimension 1 if, instead of insisting on sub-group-schemes  $Z$  of  $A$ , you look at morphisms  $Z \rightarrow A$  whose kernel at every fiber is radicial, and on the maximal fibers reduced to 0.

Why do you conjecture the Albanese varieties of the level modular varieties are zero? It’s false for genus 1! What about genus two? What about the subgroups of finite index, made abelian, of the intergral symplectic group

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<sup>1</sup> Séminaire Bourbaki 1964/65, n° 294.

$\mathrm{Sp}(2g, \mathbb{Z})$ , do you know if they are finite?<sup>2</sup> It seems clear anyhow that they cannot contain a factor  $\mathbb{Z}$ , because then the  $p$ -adic analytic group  $\mathrm{Sp}(2g, \mathbb{Z}_p)$  would have an open subgroup having a quotient isomorphic to  $\mathbb{Z}_p$  (if  $g \geq 2$ , and using the non trivial theorem of Bass-Lazard-Serre<sup>3</sup>), which it cannot. But on the other hand, is it not known by Borel-Harish-Chandra that the subgroups of finite index of  $\mathrm{Sp}(2g, \mathbb{Z})$  are finitely generated? If so, this would prove that made abelian they become finite. By the way, thinking of the geometric interpretation of those subgroups as fundamental groups, it occurs to me we know beforehand they are finitely generated, without using Borel-HC, so it seems I myself answered the question I asked you. This solves also your  $g = 2$  case, which you thought you had to solve separately as for  $g = 2$ , the two types of modular varieties, for curves or abelian varieties, are essentially birationally equivalent; or does that silly Galois-group  $\mathbb{Z}/2\mathbb{Z}$  cause serious trouble? Of course it might... By the way do not the Teichmüller groups, just as the  $\mathrm{Spn}(2g, \mathbb{Z})$ , correspond to some algebraic groups, which could allow to apply the  $p$ -adic argument above? I confess I do not have any feeling so far for these Teichmüller groups. It would be nice to have them fit in the general yoga of arithmetic type discrete groups!

I am sorry for this somewhat chaotic letter. I came just back from Pisa where I spent two hours trying without success to overshout the tremendous noise coming from the street, while giving some introductory talk on  $l$ -adic cohomology. The noise here is just killing, otherwise everything is quite nice.

Yours

(signed) A Grothendieck

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<sup>2</sup> The answer is “yes”: the abelianization of any subgroup of finite index in  $\mathrm{Sp}(2g, \mathbb{Z})$  is finite if  $g \geq 2$ , for instance because  $\mathrm{Sp}(2g, \mathbb{R})$  and  $\mathrm{Sp}(2g, \mathbb{Z})$  both satisfy Kazhdan’s property  $(T)$ . For a more general statement on the finiteness of the maximal abelian quotient of an arithmetic subgroup, see Chap. VIII, Corollary 2.8 on page 266 of G. A. Margulis, *Discrete Subgroups of Semisimple Lie Groups*, Springer-Verlag 1991.

<sup>3</sup> Referring to H. Bass, M. Lazard and J.-P. Serre, *Sous-groupes d’indice fini dans  $\mathrm{SL}(n, \mathbb{Z})$* , Bull. Amer. Math. Soc. 70 (1964) 385–392. The congruence subgroup problem for the group  $\mathrm{Sp}(2g, \mathbb{Z})$  here is solved in H. Bass, J. Milnor and J.-P. Serre, *Solution of the congruence subgroup problem for  $\mathrm{SL}_n$  ( $n \geq 3$ ) and  $\mathrm{Sp}_{2n}$  ( $n \geq 2$ )*, Publ. Math. IHÉS 33 (1967) 59–137.

4 November, 1966

Massy 4.11.1966

Dear Mumford,

I do not see why I should take offense when you tell me your opinions on some mathematical matters, all the less when I begged you to do so! As for your fears concerning the eventual inclusion of the SGA seminars into EGA go, I can assure you that in every special instance, we (essentially Deligne and I) will decide to do this work only if the duplication in substance is not excessive, if the treatment of the known material can be made considerably simpler and more satisfactorily than in the SGA texts. This question will not arise before chapter IX, anyhow, as nearly all the material contemplated for the Chapters V to VIII (with the exception of part of what might be included in VII) is not available in the literature even in imperfect form. (Thus nearly everybody—and maybe you are the only exception—considers that my Bourbaki talks on construction techniques are too condensed to be of another use than one of preliminary information on what may be done, and a few indications of proofs). If we go on with a comparable speed as in the past, these chapters alone will keep us busy for another eight years or so, and by then we will have a clearer picture of what would be most useful to do next—and maybe to decide whether we should push the treatise any further at all. Of course in Chap VIII we will include about all we will know by then on Picard schemes, including the existence questions you allude to, about which I do not think I really know much more than you; Raynaud is working on the question for pencils of curves though, and there are a few precise results available in this case, including when the Picard functor is *not* representable. I guess that within the next few years the things one would really like to know along these lines will be clarified, and EGA VIII will be the equivalent of a book, giving an account of a well understood subject!

As for the very spirit of a treatise like EGA, similar to Bourbaki, I have experienced so far that to write a really systematic treatment, even when including topics which are considered well known, is in the long run the more

economical thing to do (on the one hand), and, by forcing you to more care and overthought on even familiar matters, an incentive to new progress as well. Of course it does not serve exactly the same purpose, and should not be read the same way, as a paper or a moderately sized book on a more limited topic. With this restriction in mind I still believe (why should I not defend myself a bit) that it is quite helpful to those who want to work in the field, by relieving them many times from tedious tasks by the possibility of reference to ready-to-use statements (not always is the situation as the one you complained of for Künneth-type relations in EGA III!), and providing some ready-to-use techniques.

I told Mlle Rolland to send you SGA3 3 and 4, and I hope you will finally get it!

Yours

(signed) A Grothendieck

P.S. I am sending you back in this same cover the paycheck from Harvard, as I do not consider to have done any work there, and hence do not think it proper to take any pay for it. I feel bad enough that I was obliged so abruptly and disappoint a few nice people, including myself!

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7 March, 1967

7.3.1967.

Dear David,

You once stated that you may be able to come to the IHÉS for something like two months in spring 1968. I wonder if your plans have grown more definite now and if you could tell us about it. There would be of course many people around here very interested if you could come for some time.

I would like to ask you a mathematical question, related with some results of Bott. One states that if there is, on a non singular analytic compact variety  $V$ , a holomorphic vector field with only isolated zeros, then the Chern numbers can be computed in terms of the behaviour of the vector field at these zeros, and hence vanish if the vector field does not vanish—a somewhat surprising result I found. The other states that if a finite group  $G$  acts holomorphically on  $V$ , so that for  $g \neq e$ ,  $g$  acts with only isolated fixed points, then the Chern numbers mod  $N = \text{card } G$  can be computed in terms of the local action of the isotropy groups at the various points of  $V$ , and hence if  $G$  operates freely, then the Chern numbers are congruent to 0 mod  $N$  (the latter result being rather trivial directly, by the way). Using a somewhat different proof from Bott's, by using Segal's techniques, Illusie can extend the case of a finite group acting to the case of a smooth proper algebraic scheme over any field, it seems.<sup>1</sup> It is plausible too that by using the Lefschetz-Verdier formula for coherent sheaves, one should be able to work out an analogue of Bott's result for a vector field in the abstract case, however it seems that the method would yield information only for the Chern numbers mod  $p$ . Now, the yoga is that in char.  $p > 0$ , to give a vector field is not really better than to give an action of a  $p$ -group (for instance, if  $A^p = 0$  resp.  $A^p = A$ , giving  $A$  amounts to giving an action of

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<sup>1</sup> Comments by L. Illusie: "The result which Grothendieck alludes to is in my paper *Nombres de Chern et groupes finis*, *Topology* 7 (1968), 255-269. However, I worked in the context of almost complex varieties, with Atiyah-Segal K-theory. I didn't discuss the case of smooth proper schemes over an arbitrary field, and I haven't written any other paper on this topic."

$\alpha_p$  resp.  $\mu_p$ ), and therefore I would not really expect anything better than a result mod  $p$  to hold. But on the other hand, of course, the last Chern class on  $V$  can be computed as the number of zeros (with multiplicities) of the vector field  $A$  (as an integer, not only mod.  $p$ ). So an example should decide what to expect. Thus, I would be pleased if you could find a surface in char.  $p > 0$  with a vector field that vanishes nowhere, and still such that  $c_1^2 = K^2$  ( $K$  the canonical bundle) be  $\neq 0$ . Or let's say it the following way: assume a surface (projective, non singular)  $V$  be given in char. 0, with the group  $\mathbb{Z}/p\mathbb{Z}$  operating freely on it, and assume that we can find a non degenerate reduction of  $V$  into char.  $p$ , in such a way that the given action extends to a free action of  $V$  into char.  $p$ , say; now why should this imply that  $K^2 = 0$  (instead of  $K^2 \equiv 0 \pmod{p}$  which we know before-hand)? Of course, we must have  $c_2(V) = 0$ , because the  $c_2$  is invariant under specialization, and the specialized  $V$  will have a nowhere vanishing vector-field; so there does in fact exist a non trivial cohomological necessary condition for being able to reduce as stated the situation into characteristic  $p$ .

By the way, I would appreciate having your comments on my comments to your proof of the Tate-Serre conjecture on the Néron-model; do you agree with my criticism? Did you think again about it? I did not, and I doubt I will have time to include it into this year's seminar.

Did I tell you that Raynaud and I decided to write a book on Picard together? Also, Raynaud is willing to join us to finish writing up EGA (Chapters VI to VIII). So maybe Picard will be part of EGA after all, but the point is not too important of course. Raynaud is developing extremely nice representability theorems concerning Pic in his seminar.

Yours

(signed) Schurik

## 1967 undated

Dear Schurik,<sup>1</sup>

I thought about your question of finding a surface with  $\mu_p$  or  $\alpha_p$  acting freely, yet  $(K^2) \neq 0$ , but I couldn't find one. There may be a better chance with higher dimensional varieties, since, using Kodaira's classification and some old Italian theorems (that I don't trust), there would appear to be very few surfaces in char. 0 for which  $c_2 = 0$ ,  $(K^2) \neq 0$ .

About the Néron model: yes, your comments were quite correct and the "proof" that you indicated in your letters to Serre does indeed use essentially local uniformization and my "proof" is quite false. However, I have applied my theta functions to the problem, and, if  $K$  is a complete discrete valuation field, residue char.  $\neq 2$ , I think I can now prove both (a) the result on the monodromy and (b) the result on the "stable" Néron model. I say "think" because I haven't written down the details systematically. In fact, one should get a rather complete "structure theorem" for these abelian varieties (I hope).

$$\left\{ \begin{array}{l} K = \text{complete discrete valued field, alg. cl. residue field } k, \text{ char}(k) \neq 2 \\ C = \widehat{K} = \text{completion of alg. cl. of } K \\ \text{Let } X/K \text{ be an abelian variety.} \end{array} \right.$$

Then, after replacing  $K$  by a finite algebraic extension, one constructs

a) an algebraic group  $Y/K$  of "toroidal" type, i.e.

$$0 \longrightarrow \mathbb{G}_m^r \longrightarrow Y \longrightarrow Y^* \longrightarrow 0$$

$Y^*$  an abelian variety

b)  $f : Y_C \rightarrow X_C$  a rigid analytic homomorphism defined over  $K$

<sup>1</sup> This letter was written in the summer of 1967 in Main, before DM went to India.

such that:

- (I)  $Y^*$  has non-degenerate reduction, hence there exists a scheme  $\mathcal{Y}/\text{Spec}(R)$  (smooth, connected fibres) whose generic fibre is  $Y$ , whose special fibre  $\bar{Y}$  is again an algebraic group of toroidal type.
- (II)  $f$  induces an algebraic isomorphism

$$\bar{f} : \bar{Y} \longrightarrow [\text{connected component of special fibre of Néron model of } X]$$

where the latter group is stable under finite extensions of  $K$ .

(III) If

$$\begin{aligned} Y'_C &= \{x \in Y_C \mid \text{the set of powers } \{x^n\} \text{ is bounded}\} & 2 \\ &= \{x \in Y_C \mid \text{closure of } x \text{ in } \mathcal{Y} \text{ meets } \bar{Y}\} \end{aligned}$$

then  $Y_C$  splits canonically:

$$0 \longrightarrow Y'_C \longrightarrow Y_C \longrightarrow \Gamma^r \longrightarrow 0$$

( $\Gamma$  = value group of  $C$ ) [i.e. there is a canonical reduction of the structure group  $(C^*)^r$  of  $Y_C/Y_C^*$  to (integers of  $C$ ) $^{*r}$ .] Then

$$\text{Ker}(f) \text{ is } \begin{pmatrix} \text{torsion-free, finitely generated} \\ \text{all its elements are rational } /K \\ \text{Ker}(f) \cap Y'_C = \{0\} \end{pmatrix} .$$

(IV) For all finite algebraic extensions  $L/K$ ,  $f(Y_L) = X_L$ .

About visiting Paris in 1968: yes, I would like to do this and I had meant to write about this for some time. I would like to come for the month of May if this is alright. I will be coming (from India) with wife, 2 kids, and a Norwegian girl who helps with the kids. Perhaps you could ask your secretary to write me about the kind of accommodation that is available? I understand the Institute maintains some apartments? Thanks for your help in this.

Best Wishes,

David

<sup>2</sup> The part “ $\mathcal{Y}$  meets  $\bar{Y}$ ” in the second description of  $Y'_C$  was inserted by the editors. The original words near the margin were cut off during the photocopying process.

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**2 May 1967**

Massy 2.5.1967

Dear David,

Thanks for your interesting letter on connections and stratification. I had already wondered if by chance it is not always true for, say, a quotient of a power series ring over a field of char. 0, that the formal De Rham complex is a resolution of  $k$ , and if the De Rham complex for an algebraic variety i.e. a scheme of finite type over  $k$  does not always yield the correct cohomology (this would be a consequence, when  $X$  is complete, of a complex analytic variant of Poincaré's lemma for  $X$ , using Serre's GAGA). I had noticed with some surprise that it works for quite a few singularities of curves (like ordinary double points and cusps), and also a few Artin rings, and did not succeed to construct an example of an Artin ring giving the wrong  $H^0$  of De Rham. So I am happy you got that silly question out of the way. I wonder though if by chance it does not work right for sufficiently "simple" singularities? I will also appreciate very much getting details on your ideas on compactification of the modular varieties and the connectedness theorem, as soon as you have some notes available. Today I sent you by air mail a photocopy of the notes on De Rham cohomology and crystals; they are still extremely sketchy, despite the floods of sweat they took to the redactors, who I am afraid did not understand too well so far what they were writing. I hope though that you will find the definition of the connection on De Rham cohomology explicit enough; I did not check though that the curvature tensor was zero, but do not doubt this is so. Also you will find, when  $X/S$  is smooth and *moreover*  $S$  of char. zero, a definition of the absolute stratification of  $\mathbb{R}f_*(\Omega_{X/S}^*)$ . As in the case of the connection, this does not a priori imply a corresponding stratification on the cohomology sheaves  $R^if_*(\Omega_{X/S}^*)$ , except in the case when we know that their formation commutes with arbitrary base change. Now when  $f$  is proper, this condition (by the standard Künneth type arguments) also just means that the  $R^if_*(\Omega_{X/S}^*)$  are locally free, and this condition turns out to be a formal consequence of the fact that these sheaves are coherent (assuming  $S$  noetherian)

and that the complex  $K^\bullet = \mathbb{R}f_*(\Omega_{X/S}^*)$  from which they stem is bounded from above and endowed with an absolute connection, i.e. here a connection over a ground field  $k$  (here  $k = \mathbb{Q}$ ). To see this, we are reduced to the case when  $S$  is of finite type over  $k$  by a standard limit argument. On the other hand, we now get the result by *descending induction on  $i$* , using the fact that when the  $\mathcal{H}^i(\mathcal{K}^\bullet)$  are locally free for  $i > i_0$ , then formation of  $\mathcal{H}^{i_0}(\mathcal{K}^\bullet)$  commutes with base change, hence  $\mathcal{H}^{i_0}(\mathcal{K}^\bullet)$  has a stratification, hence is locally free. One can prove the same result by a transcendental argument, not using the canonical stratification, by reducing to the case when  $k$  is the field  $\mathbb{C}$  of complex numbers and  $S$  is reduced to a point (hence artinian), using GAGA to reduce to the corresponding complex analytic statement, and using Poincaré's lemma for the (analytic) De Rham complex of  $X/S$ . I hope the same transcendental argument, using a suitable "relative" Hodge theory (over an Artin ring over  $\mathbb{C}$ ) should prove when  $f$  is projective that the  $R^q f_*(\Omega_{X/S}^p)$  are equally locally free, which will imply that the spectral sequence beginning with these as  $E_1^{p,q}$  and ending up with  $R^n f_*(\Omega_{X/S}^*)$  degenerates, i.e. that the relative Hodge cohomology is just  $\text{Gr}(R^n f_*(\Omega_{X/S}^*))$ , the graded sheaf associated to relative De Rham cohomology. The argument applies in any case (as was pointed out to me long ago by Hironaka) when  $S$  is reduced. I could not get it out when  $S$  is just artinian, by a purely algebraic proof, using the corresponding result over the residue field and the fact that the De Rham cohomology is free, by some general argument of spectral sequences; maybe I just did not try hard enough, as the information available on the spectral sequence seems already rather strong. Notice also that the fact that  $R^1 f_*(\mathcal{O}_X)$  is locally free is proved in my second talk on Picard schemes (corollaire 3.6); when  $f$  is of relative dimension 2, what about the prospective dual  $R^1 f_*(\Omega_{X/S}^2)$ ? (In fact, the former is a priori the dual of the latter, by the global duality theorem. . .)

Of course, the theory of crystals gives considerably more on the  $R^i f_*(\Omega_{X/S}^*)$  than just a stratification, namely it endows them with a canonical structure of an absolute crystal; i.e. these sheaves extend automatically to sheaves over any infinitesimal neighbourhood of  $S$ . Although this point of view is not worked out in the notes, it must come out rather formally, I am convinced, by interpreting these sheaves as coming from  $R^i f_{\text{cris}}(\mathcal{O}_{X_{\text{cris}}})$ , where  $f_{\text{cris}} : X_{\text{cris}} \rightarrow S_{\text{cris}}$  is the morphism of the *absolute* crystalline topoi associated to the morphism of schemes  $f : X \rightarrow S$ . The same remark should hold in arbitrary characteristics too, using now "crystal" in the sense of the IHÉS notes, namely involving divided powers; this is something pretty more precise than just a connection on the sheaves  $R^i f_*(\mathcal{O}_{X_{\text{cris}}})$ . . . For the applications of this to varieties or formal groups in char.  $p > 0$ , in particular to the interpretation of Dieudonné module and infinitesimal variations for  $p$ -divisible groups, as suggested in my Pisa letter to Tate, I want just to point out that if  $A$  is a ring, and  $p$  a prime number which is nilpotent in  $A$ , (for instance  $A = W_n(k)$ ,  $k$  a perfect field of char.  $p > 0$ ) then the ideal  $pA$  admits a canonical structure of divided powers. Thus I think that my interpretation of infinitesimal variations of an

abelian variety or a  $p$ -divisible group works correctly when one takes, as infinitesimal parameter varieties, varieties endowed with an extra-structure of divided powers in the augmentation ideal. This (I think) is enough for various applications, such as a nice description of the Dieudonné module (using the previous remark on the  $W_n(k)$ ), and at the same time rules out the unpleasant counter-example in my Pisa letter, which was concerned precisely with “vertical” (relative to  $\text{Spec}(\mathbb{Z})$  or  $\text{Spec}(W(k))$ ) variations of structure, namely those remaining in char.  $p > 0$ —the explanation now being that in an ideal  $\mathfrak{m}$  of a ring of char.  $p > 0$  there can be no divided power structure unless  $\mathfrak{m}^p = 0$ ! Of course, in char. 0, the divided power structure always exists and is unique, so does not add anything, which explains why things worked so smoothly in char. 0.

Yours

(signed) Schurik



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1 August, 1967

Letter from Groth. 8/1/67<sup>1</sup>

Coates and Jussila are very busy writing down the notes of my talks on De Rham cohomology,<sup>2</sup> which should be ready within a few weeks. I will send you however in this letter a copy of the two pages containing a direct construction of the Gauss-Manin connections. By the way, I had some extra thought about the definition of  $p$ -adic cohomology in char.  $p$ , and believe I have the right definition at last, using still another site, the so-called “De Rham site” of a relative scheme  $X/S$ , whose objects are (Zariski) open subsets  $U$  of  $X$ , together with a “thickening”  $U'$  of  $U$  i.e. a nilpotent immersion  $U \rightarrow U'$  over  $S$ , and moreover a “divided powers structure” on the augmentation ideal for  $\mathcal{O}_{U'} \rightarrow \mathcal{O}_U$ . When working in char. 0, this extra structure is uniquely determined and we get the usual “site cristalline”, whose cohomology with coefficients in the structure sheaf of local rings is just (when  $X$  smooth over  $S$ ) the relative De Rham cohomology  $\mathbb{H}^*(X_{\text{zar}}, \Omega_{X/S}^*)$ . It now seems to me that essentially the same proof will show the same result in arbitrary characteristics, when working with the De Rham site, involving divided powers. These divided powers seem in a most subtle way to rule out the troubles I had come upon in my italian letter to Tate on crystals<sup>3</sup> (in connection with elliptic curves of Hasse invariant zero). It seems to me that, at least for those smooth proper schemes in char.  $p > 0$  which lift to char. 0, all the usual properties for the  $p$ -adic cohomology will then follow readily from the known results in char. 0, the ground ring for the cohomology theory being actually the *ring* of Witt vectors  $W(k)$  (not its field of fractions, as in Washnitzer-Monsky’s theory). This should then rule out the possible presence of denominators  $p^r$  in Weil’s conjectures, which I alluded to in my letter to Serre on the standard conjectures for algebraic cycles.

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<sup>1</sup> handwriting of DM

<sup>2</sup> Published as: Crystals and the De Rham cohomology of schemes, in *Dix Exposés sur la Cohomologie des Schémas*, North-Holland 1968, 306–358.

<sup>3</sup> A scan of this letter is available from

<http://www.math.jussieu.fr/~leila/grothendieckcircle/mathtexts.php>

I also got finally quite an interesting letter from Griffiths. As far as I could understand, though, he does not know about any new “finite” relations for the “geometric” Hodge structures (those embeddable in the Hodge structure associated to a projective smooth variety over  $\mathbb{C}$ ), but *infinitesimal* relations concerning variation of Hodge structure coming from a variation of an algebraic variety. The result he states is enough in any case to take care about my worries concerning Tate’s conjectures, as you pointed out to me yourself. Maybe after all there are no such finite relations as we contemplated, but only infinitesimal ones. It would be of course highly interesting if the necessary conditions he obtains for an infinitesimal variation of Hodge structure to be “geometric” are also sufficient, and to get a corresponding result for finite variations. His results anyhow show the a priori possibility of a completely different picture for moduli of motives from the one I originally had in mind, with an essential part being played by differential equations. This checks very well with my De Rham yoga, where differential operators of arbitrarily high orders are involved in quite an essential fashion.

Yours

(signed) Schurik

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18 March, 1968

Massy, March 18, 1968

Dear David,

Thank you very much for your letter, which has crossed with Deligne's telling me very much the same thing as you told me in yours! I believe your proof is conceptually less sophisticated and therefore simpler than Deligne's (although in essence the same)<sup>1</sup>, but that Deligne's systematic use of "étale topoi" as generalized varieties will eventually provide the better insight into the geometry of these questions. I am convinced this is really an important generalization of the notion of scheme, and we will have to deal with it systematically, alongside with Mike's intermediate generalization<sup>2</sup>, starting with EGA VI.

The proof in my letter to Serre about semi-stable reduction of abelian schemes (via a monodromy theorem  $(1 - g^N)^2 = 0$  for  $l$ -adic  $H^1$ ) works all right in all cases, and yields even in arbitrary dimension (when written out with care)  $(1 - g^N)^{i+1} = 0$  in  $H^i$ , as was known to Griffiths by transcendental methods in the complex case. Thus I have two essentially different proofs of the monodromy theorem, one arithmetic (which works over any discrete valuation ring  $R$  which has residue field of finite type, or which is localized from an algebra of finite type over a field), the other geometric (which works without restriction on  $R$ , provided  $i = 1$ ).<sup>3</sup> Both work in characteristic zero

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<sup>1</sup> Referring to the proof of the irreducibility of  $M_g$ , paper [69e] in this volume.

<sup>2</sup> the notion of *algebraic spaces*

<sup>3</sup> Grothendieck's arithmetic proof of the monodromy theorem was published in the appendix to J.-P. Serre and J. Tate, *Good reductions of abelian varieties*, Ann. Math. 88 (1968) 492–517, and also in the appendix of SGA7, Exposé I, LNM 288, Springer-Verlag 1972. The geometric proof was published in §3 of SGA7, Exposé I. See SGA7, Exposé IX, *Modèles de Néron et monodromie*, for further discussion of the monodromy theorem. For a more elementary proof of the stable reduction theorem for curves see M. Artin and G. Winters, *Degenerate fibres and stable reduction of curves*, Topology 10 (1971) 373–383.

(all  $i$ , any  $R$ ); both use resolution some way, and both would work for any  $R$ , any  $i$  if resolution was known for schemes of finite type over  $\mathbb{Z}$ , except that the geometric proof uses moreover purity. En revanche, it gives a little more precise information in the smooth case (namely the exact exponent  $i + 1$ ); for  $i = 1$ , we need resolution and purity only in a range of dimensions where both are known (by Abhyankar, and Zariski-Nagata's purity theorem). The arithmetic proof on the other hand applies also to non-constant coefficients—all that is needed is that the  $l$ -adic sheaf whose cohomology we are taking comes from a situation of finite type over the integers; morally, this means that it comes from a motive over your scheme  $X$ . I did not try to make the geometric proof yield the same kind of generalization; it may turn out notably more difficult. All these things will be explained at length, including applications to Néron models and the like, in my own exposés in SGA7 “Groupes de Monodromie Locale”, which is a joint seminar by Deligne and myself, and has started this month. Later Deligne will give an algebraic proof of Lefschetz's theorem about the Pic of the “general” surface of degree  $\geq 4$  in  $\mathbb{P}^3$ , which works in any characteristics, after some general facts about vanishing cycle theory. By the way, as you will have gathered, Deligne is extremely bright; I believe, brighter than anybody else I know in mathematics.

Now to your comments about the use of the word “variety”. I was a bit surprised to see that this question nearly upsets you, and still more by what you say on algebraic geometry becoming “a still more unpleasant subject” with its “rival schools”... I never realized algebraic geometry was an unpleasant subject, nor that there was any rivalry among algebraic geometers; I have lived so far in the belief that all of us, although tastes and yogas may largely differ from one to another, are working towards a common goal of better insight into geometry, and that every one among us is glad about any good result any of his colleagues would get, and eager to make use of it, whatever the methods and the spirit in which this result may have been obtained or exposed. Does your own experience really tell you anything to the contrary? Also, I wish to assure you that I would never think of suspecting you or anybody else of being “personal”, as you feared apparently I would, when discussing about any mathematical question, including questions of terminology. I would like you, on the other hand, to be as sure that I don't take it personally either, and that, when working out a terminology, my aim is not to tease, annoy or hurt anybody, and you less than anybody else. In fact, I learned with Bourbaki (and through my own experience) how much a good terminology is important for an easier understanding of mathematics and smoother working, and to be very painstaking in these matters, more than the average mathematician I would think, and to spend a non negligible amount of time on it. It is after serious consideration that I decided myself with Dieudonné, for instance, to change “simple” into “smooth” after SGA3, “non ramifié” into “net” more recently, “locally free sheaf of rank one” into “invertible sheaf” according to Tate's suggestion, and “prescheme” into “scheme” resp. “scheme” into “separated scheme”. I do not pretend the result to be perfect, but I guess it is coherent

and reasonably suggestive, and does not seem to offer difficulties to young people who have no “mental blocks” by too strong a habit of one of the other existing terminologies. But even if you believe the result to be bad, discussion will be easier if you don’t assume it is so by purpose and in order to annoy anybody. Now let me come to the points you make about the word “variety”, and to my own points.

- 1) You contend the word has already a precise and generally accepted meaning, I accept that it has for various mathematicians, but that the meaning is now pretty much different from one to another. You wrote me which is yours: an integral separated algebraic scheme. Mike on the other hand uses the word to mean just any algebraic scheme, and, according to context, he will understand implicitly that the scheme is separated, or that it is only locally of finite type (instead of finite type as an algebraic scheme should be); by the way, he uses much the same way analytic variety to mean any analytic space (in the sense of my exposés in Cartan’s seminar). Weil’s use of “variety” is closer to yours, but still different, as he assumes it to be geometrically integral. I am not too sure what is Zariski’s terminology, but I guess it will be still different, something like a subset of projective space defined by a set of equations; I would have asked him if he were around, and maybe he would have been a bit embarrassed really to tell me what he means by “variety”!
- 2) I do not think there is a strong tendency in French to imply non singularity by the use of the word “variété”. In algebraic geometry at least such an implication has never existed. In topology, most times nowadays when topologists speak about “varieties”, they admit varieties with boundaries, and as soon as one starts taking products, even the boundaries acquire singularities. In the Cartan seminars, “variété analytique” does imply non singularity, but this terminology is by no means universally accepted. See Mike’s use above. Also, if people like Thom or Whitney speak about “analytic varieties”, they are mostly interested in their singularities, which they want to stratify in various ways!
- 3) Quite generally, I think that the natural trend now in the use of the word “variety” is to make its meaning ever wider—so much so as to include even functional spaces, when allowing the varieties to be infinite dimensional! This is in a way not better nor worse than viewing an arbitrary scheme (not only noetherian ones, or those of finite type over a field) as being “varieties”. I believe that the only ‘a priori’ natural limitation to the use of this word should be that it should extend as far as does the specific geometric intuition, and some of the main technical features, of the objects which were initially considered. Objects which have local rings, tangent spaces and higher order differential invariants, for which all or most of the most important geometric constructions (projective bundles and other fibrations, Picard varieties and the like, normalisation etc) can be performed, seem to me to be eligible for the term “variety”. I think however,

like Deligne, that objects like the “étale topoi” present some essentially new features, which demands for a more sophisticated kind of geometric intuition than the usual one, mainly through the fact that morphisms of such objects (and in particular, their geometric points) may have non trivial automorphisms—and that for this reason it would be unwise to subsume them also under the name of “variety”.

- 4) As you admit yourself in your letter, it seems rather likely that the most natural “compact” moduli objects, for curves or abelian varieties, are not schemes, but just what we would like to call “varieties”. Now these modular objects, you will agree, are among the most basic and important ones geometers would like to study, and I am convinced that their importance will still increase both for geometry and number theory in the next fifty years or more. So, just because of a taboo coming from some particular training of yours, you would forbid yourself forever to call these remarkable beings “modular varieties”, as everybody has done so far since Riemann, I believe? I think you have just missed that point, that these “varieties” are precisely the good kind of objects, just varieties! Not really any different from what one has considered so far, and providing just a closer and better link with the usual analytic varieties (or “analytic spaces”)—as various operations which could so far be performed only in the complex analytic context acquire a meaning also in algebraic geometry.
- 5) Quite generally, it is becoming rather clear now that the new “varieties” are the more “natural” objects when compared say with schemes, because the category of these varieties seems to have a remarkable stability with respect to those geometric constructions which seem the most important, and which sometimes get us out of the category of classical minded varieties, or schemes: contractions and other types of passage to quotient, representation of functors of Hilbert and Picard type, modular spaces of all kinds. . . . Therefore these objects do deserve a simple name, and possibly one which has already a rich intuitive content through the use which has been made of it before? I believe this is by no means an insult to the classical people, but will *eventually* turn out to be an homage to them—as *at present* it is intended to stress for the “usager” the geometric significance of this comparatively new notion.

I discussed the matter with Deligne, who essentially shares my opinion on this matter. However, he told me that he did not wish to use this word consistently at the cost of upsetting you as it seems it does. We have little hope to convince you that the usage we want to make of the word “variety” is at present the best, but we do hope at least that you will let yourself be convinced that there is nothing offending to anybody in this use, that each of us came to the conclusion that this is best by objective motives, and not personal ones wishing to hurt anybody. After all, it is really not catastrophic if you go on using the word variety according to your own taste, and certainly quite a few others will do the same. At present, the only motive which keeps us from

using the terminology which objectively seems to us the best, and to go on working with our minds in peace, is a personal one, as neither of us has a wish to hurt your feelings! Therefore, please consider the matter again and write us if your feelings need really be hurt, or if you believe that it is reasonable that *we* should adopt a terminology which, after careful consideration, seems best to both of us, and for which neither of us is able to find a satisfactory substitute.

With my best wishes to you and your family

(signed) Schurik

Please give my best regards to the Seshadris, to Ramanujam and to Ramanathan.



2 August, 1968

Massy 2.8.1968

Dear David,

I looked through Cartier's notes on formal groups, as I am interested in a description of  $p$ -divisible groups over a scheme of char.  $p > 0$ , or more generally over a scheme all of whose residue characteristics are  $p$  or zero. His description does not look too handy directly, especially the filtration he has to use is rather annoying. One would like something which directly generalizes Dieudonné's description over a perfect field: free modules of finite type over the ring  $W$  of Witt vectors, together with  $F$  and  $V$  satisfying the three known relations (in fact,  $V$  following from  $F \dots$ ). Did you work out any such description using Cartier's work? If so, I would be very grateful to you to write me what you know. I have been trying a bit to make more precise what I mumbled to you about crystals and  $p$ -divisible groups; that is why I need Cartier's stuff. By the way, I convinced myself that the description I suggested for  $p$ -divisible groups over unequal characteristic discrete valuation rings works only if the maximal ideal has topologically nilpotent divided powers structure. But this restriction should be unnecessary when dealing with  $p$ -divisible groups *up to isogeny* over  $V$ : such a structure should correspond exactly to a Dieudonné space  $M$  over the field of fractions  $K$  of  $W$ , and a filtration of  $M \otimes_K L$  ( $L = K \otimes_W V$ ) subject to the only condition that the dimensions of the two occurring factor spaces should be the correct ones (namely the dimensions of the group and its dual in char.  $p$ ) I more or less checked this when the group in char  $p$  is "ordinary" i.e. extension of any ind-étale by a multiplicative type  $p$ -divisible group.

Best regards

(signed) Schurik



9 August, 1968

Massy, August 9, 1968

Dear David,

Thanks a lot for your letter of July 24, which I got yesterday. I am sorry you cannot come for a whole year in 70/71, but am glad that you think you can come for about two months. Thanks for suggesting that I should come to Warwick for a while that same year. I guess it could be done, if you do not expect me to be there for longer than a week or ten days. I am ready to tell other people about that symposium to suggest participation, but maybe it would be useful if you could tell me a few words more what such a “low-pressure symposium” will be supposed to look like. Also thanks for your invitation to join the panel of invitations for the next international Congress; as I am not too convinced of the usefulness of such Congresses, I believe however you better leave me out!

I got Griffiths’ preprints and “disclaimer”<sup>1</sup> at the same time as your letters. It looks quite startling indeed, but I had no time to look at it seriously as yet. And I managed to lose the preprints, and had to ask Gr. for another copy! By the way, his results (about which he is himself dubious) do not affect what I really call the standard conjectures, on which the theory of motives relies; these do not assert anything about  $\tau$ -equivalence. But in order to come to a coherent picture concerning intermediate jacobians, and the tie they provide between Hodge’s index theorem and the Néron-Tate form (by interpreting the intersection form on primitive cycles as a Néron-Tate form on a suitable intermediate jacobian), it has been extremely tempting to surmise that  $\tau$ -equivalence equals numerical equivalence. I will have to reconsider the matter anew if really this assumption should turn out to be false. These questions were on my holidays’ program, but I did not start so far, as I was still busy

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<sup>1</sup> The papers are published in *On the periods of certain rational integrals*, I, II, Ann. Math. **90** (1969), 460–495 and 496–541. However the published version contains no “disclaimer”.

trying to come to some understanding on crystals and their relations to  $p$ -divisible groups (which should really be called Barsotti-Tate groups<sup>2</sup>, as  $p$ -divisible should just mean that multiplication by  $p$  is an epimorphism, and not more). I hope you got my last letter asking you questions in this connection on Cartier's theory, and that you will be able to give me some information I need.

Best wishes

(signed) Schurik

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<sup>2</sup> Tell me if you agree, please. (AG's footnote)

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 4 September, 1968

Sept. 4, 1968

Dear David,

Thanks for your letter. If you think I can give you advice on selecting speakers for the next congress, I will certainly not refuse giving it to you; but I guess for this there is no need for me to join your panel. A rather evident thing would be to ask that Griffiths should give a one hour talk. I am particularly impressed by theorem (\*\*\*) stated in his “disclaimer”, opening completely new perspectives. But have you been able to discover where, in Gr.’s paper, this theorem is proved, or even to convince yourself that the proof is OK? In any case, his theorem E (5.6) completely convinced me that my feelings on the relations between Hodge’s index theorem and the Néron-Tate form were erroneous, so that I have no reluctance any longer to admit that  $\tau$ -equivalence is indeed distinct from homological equivalence.<sup>1</sup> As for the explanations you

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<sup>1</sup> Comments by Phillip Griffiths, concerning [1968Aug9] and [1968Sep4]: “Thank you for sending me the email with Grothendieck’s letters to David. I believe what they are referring to (the part about my stuff) is

homological  $\neq$  algebraic

equivalence for higher codimensional cycles, even modulo torsion. Later, Clemens showed the quotient

LHS / RHS

is a countably but not finitely generated abelian group. My methods (and intuition) were classical analytic/geometric and it seems that David—to none of our surprise—was able to understand the argument and convince Grothendieck. Together with David’s example of  $\dim CH^2(X) = \infty$  on a surface  $X$  with  $p_g(X) \neq 0$  this opened up an era (maybe “can of worms”) of stuff regarding cycles with, at least to me, the main real progress since being the conjectures of Bloch-Beilinson which at least bring some order into  $CH^2(X)$  and explain why  $\dim CH^2(X) = \infty$  occurs (cf. the paper by Mark Green and myself in IMRN, 2003 that treats this).

give me in your letter, they seem to me to concern rather Gr.'s theorem E, whose proof I understand (as I knew Lefschetz's proof of Noether's theorem); but I do not see why this should directly give an example, say, of a curve on a three-fold, homologous to zero and not  $\tau$ -equivalent to zero. With your notations ( $V$  a general hypersurface section of high degree of  $W$ ), the question remains why a primitive cycle of middle dimension on  $X$ , whose restriction to  $W$  gives the zero element in Weil's intermediate jacobian  $J(V)$ , should be itself homologous to zero? Gr. himself refers for this to the rather technical sections 11 to 15!

I made some progress with the relations between Tate-Barsotti groups and crystals since I last wrote you, but was waiting to get your answer to my questions before starting some final checking for the crystal interpretation of the Dieudonné module, and also in order to check that if  $S$  is any (?) scheme of char.  $p > 0$ , then a Tate-Barsotti group on  $S$  is "the same thing" as a crystal  $M$  of locally free modules over  $S$  (crystal in the absolute sense, i.e. over  $\text{Spec } \mathbb{Z}$ , or  $\text{Spec } \mathbb{Z}_p$ , and in a sense slightly more sophisticated than in my notes, by asking that the divided power structures in the definition of the crystalline site should be compatible with the one we have on the maximal ideal of  $\mathbb{Z}_p$ ), *plus* the maps  $F$  and  $V$  between  $M$  and  $M^{(p)}$  satisfying  $FV = p \cdot \text{id}$ ,  $VF = p \cdot \text{id}$ . (In case  $p = 2$  one will have to be more careful, but I believe that an analogous statement will still make good sense). Then the analogue of Tate's theorem for the equicharacteristic case should follow from the general crystal-theoretic fact (which I did not try to check either so far) that if  $M, M'$  are crystals of locally free modules over the noetherian normal connected scheme  $S$  (no  $F$  and  $V$  here, and indeed I like to think about crystals as coming from cohomology groups of higher dimension as well), then any morphism between the generic fibers of these crystals is induced from a morphism  $M \rightarrow M'$ . I also see along which lines to look for a generalization of Tate's theorem to crystals in the unequal characteristic case, via the definition of a functor from filtered crystals with "Frobenius"  $F$  to Galois modules over the general point; the description of this functor remains however the most mysterious point, which I will have to elucidate first in the case of Tate-Barsotti groups, with the help of Cartier's theory and Tate's ideas. The theorem should be that this functor is full, faithful working mod isogeny. Granting this functor, I see also what should replace Serre-Tate's theorem (cor.1 to th.A in my *Inventiones* paper) in higher dimensions, so as to get a principle of proof of conjecture 1.4 of that paper in arbitrary dimensions: namely for a projective smooth  $X$ , over the unequal characteristic discrete complete valuation ring  $V$ , a De Rham cohomology class should be algebraic if it is algebraic when interpreted as a

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Of course the central question—the Hodge conjecture together with its generalizations by Grothendieck—has seen no real progress in 50+ years (except that it has so far been consistent with other known / conjectural things).

What comes through also in Grothendieck's letters, and this was my personal experience as well, is how direct and to the point he is. Oh that he had written EGA."

crystalline cohomology class of *the special fiber*  $X_0$ , and if moreover it has the correct filtration. In other words, the functor from semi-simple motives over  $V$ , to pairs of a motive  $M_0$  over the residue field  $k$ , together with a filtration of its crystalline realization  $T_{\text{cr}}(M)$  (a finite dimension vector space over the quotient field  $K$  of  $V$ ), is fully faithful. (Analogue of Hodge conjecture!) Maybe these statements will even turn out to be provable!

In connection with these questions, I wonder if for a projective smooth variety  $X_0$  over  $k$ , one can foresee the value of the  $h^{p,q}$  of any lifted variety from the structure of the crystalline  $H^n(X_0)$  ( $n = p + q$ ) together with the  $F$ -structure on it, namely the semi-linear map  $H^n \rightarrow H^n$  stemming from the Frobenius map  $X_0 \rightarrow X_0^{(p)}$ . For instance, are the  $h^{p,q}$  independent of the lifting? I would appreciate to know if you have any idea on this.

Best wishes

(signed) Schurik

My personal address:

2 Av. de Verrières, Massy (Essonne) France

NB. I take my mail at the IHÉS only once a week.

Re P.S. I am puzzled about Gr.'s 10.12, which looks false: take a family of subvarieties  $W$  of  $\mathbb{P}^r$  with variable periods, and blow them up! Therefore I am dubious about the proof of 10.13 as well.



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 October 10, 1968

Massy 10.10.1968

Dear David,

Thanks for your letter. In the meantime I have thought some more about Griffiths' result, and come to exactly the kind of proof you outline. It seems to me (although I did not check carefully enough) that the same argument carries over to char.  $p > 0$ , whenever we know that Lefschetz's hard theorem holds true (no trouble for complete intersections for instance!) and provided we know moreover that, for a *general* hypersurface section of  $X^{2m}$  of some high enough degree, the vanishing cohomology of  $Y^{2m-1}$  is not of level 1; and for this, if we take the ground field to be finite, it is enough that we can find *some* hypersurface section  $Y$  of that degree, non singular, defined over a finite field with  $q$  elements, such that the proper values of Frobenius acting on  $E(Y^{2m-1})$  (the vanishing part of the cohomology of  $Y$ ), divided by  $q^{m-1}$  are not all algebraic integers; or what amounts to the same, that the coefficients  $c_i$  of the corresponding polynomial  $f(t) = \prod(1 + \alpha_i t)$  are not divisible by  $(q^{m-1})^i$ . Of course, the transcendental situation suggests that we should even get maximum level  $2m - 1$ , i.e. we should not be able to divide by  $q^i$  even. Now Katz told me that this can be effectively checked for various *complete intersections* in  $\mathbb{P}^r$ : indeed, it is enough that  $Y$  has no point rational over its field of definition  $k$ , because the number of such points (if  $Y$  is of dimension  $n$ , even or odd, it does not matter of course) is

$$1 + q + \cdots + q^n + (-1)^n \sum \alpha_i$$

which implies that not all algebraic integers are divisible by  $q$  if this sum is to be zero! On the other hand, you can find a hypersurface of given degree, multiple of  $q - 1$ , rational over the field with  $q$  elements and which has no point over that field, by taking  $\sum a_i X_i^{d(q-1)} = 0$ , the  $a_i \in F_q$  being such that no partial non empty sum of them is equal to zero; this works at least if  $q$  is  $>$  number of variables, by taking all  $a_i$  equal to 1. If the intersection of

that hypersurface with  $X$  is non singular, we win. This works for instance for Griffith's quadric in  $\mathbb{P}^5$  (if char.  $\neq 2$ , at least).

A weird fact is that Griffith's construction gives examples only over function fields, no field algebraic over the prime field. It seems quite hard to deduce an example over a number field, say, although there should certainly be such an example! I feel less secure over a finite field, and would not be surprised if it turned out that in the case where the ground field is the algebraic closure of a finite field, then numerical equivalence implies  $\tau$ -equivalence. One heuristic reason is that the points of abelian varieties over such a field are of finite order. Another key invariant we can associate to a cycle  $Z$  on  $X/k$  which is cohomologically trivial on  $X_{\bar{k}}$ , namely the element of  $H^1(k, R^{2i-1}f_*(\mathbb{Q}_l(i)))$  stemming from the Leray spectral sequence, should vanish (by virtue of the Weil conjectures) when  $k$  is a finite field. I would suspect that for  $k$  of finite type in the absolute sense, the vanishing of that class implies  $\tau$ -equivalence to zero, and more precisely should be characteristic of some more refined equivalence, something like Picard-equivalence up to torsion, which in the transcendental case would be expressible by the fact that the image of the class in Griffiths' torus is a torsion element.

I did not prove what I surmised about the relations of Barsotti-Tate groups in char.  $p$  to Dieudonné crystals, and have not been thinking about these things for some time. Cartier says he checked the statement I proposed about classification of B-T groups over unequal characteristic discrete valuation rings with divided powers in the maximum ideal, in terms of a filtration of the extended Dieudonné module. I do not think he looked at the corresponding statement (without divided powers) for classification up to isogeny. Yours

(signed) Schurik

20 November, 1968

20.11.1968

Dear Mumford,

Thanks for your letter and your very nice paper on rational equivalence, which I just read.<sup>1</sup> Some trivial comments: in par. 1 the non singularity of  $X$  is not used. On page 4, line -6, the relation  $S = \tilde{S}/G$  would need a word of explanation, as it uses normality of  $S$  and char 0. Page 5, instead of “any” you mean “commutative” I guess; it took me a while to understand what you meant to say before lemma 1, till I realized that  $f : S \rightarrow Y$  and  $\eta_f$  were as before, but that you just allowed  $\tilde{S}$  and  $p$  and  $\tilde{f}$  to change. (NB I often have trouble when reading you with such trivial matters, whereas otherwise your informal style makes understanding rather easier.) Page 6, line 10 the meaning of “too” is mysterious. Page 9, line -2, I guess  $m$  depends on  $i$  too. I think you may add as a corollary to your result that Samuel’s conjecture (or question) that  $((x) - (y)) \times ((x') - (y'))$  on a product variety is rationally equivalent to zero is false already for the product of two elliptic curves (as this would imply in this case that rational equivalence of 0-cycles is the same as Albanese equivalence).

Your objection to carrying over Griffiths’ construction to char.  $p$  is met with by observing that the monodromy representation of  $\pi_1(\mathbb{P}^1 - \{\text{critical points}\})$  is irreducible on the vanishing cycles space, just as in the transcendental setup; a fortiori, the corresponding “motive” cannot split. This is proved by Lefschetz’s argument using the Picard-Lefschetz formula and the fact that the vanishing cycles corresponding to various critical points are still conjugate to each other. The first fact will be proved by Deligne in our seminar this year, by an argument of reduction from char. 0 to char.  $p$ , which requires care but is of rather standard nature; in char zero the transcendental theory can be used. (I do not know if a purely algebraic proof can be found; I suppose yes for a pencil of curves, and you should know best...) The conjugacy state-

<sup>1</sup> [69d] in Volume 1

ment should come out just the same way as in the classical case, using the irreducibility of the variety of critical hyperplanes, and will certainly be done too by Deligne in this seminar. He needs it for proving Noether-Lefschetz's theorem in char.  $p$ . I confess I never wrote out full proofs myself, but am quite confident it will come out all right. I would expect Deligne to talk on this in January or February, and if you are interested he may send you a Xerox of his notes then (or even now if they exist already in readable shape).

Katz made an interesting suggestion towards proving the conjecture suggested by what happens in char zero, namely that the level of the vanishing cycles space of the general hypersurface section in a pencil, of a given high degree, is maximal, by proving the conjecture that the  $p$ th power map

$$H^{n-1}(Y_t, \mathcal{O}_{Y_t})^{(p)} \longrightarrow H^{n-1}(Y_t, \mathcal{O}_{Y_t})$$

(or rather on the "vanishing part") is not nilpotent (possibly even semi-simple). The same would hold then for any sufficiently general  $t$ , and, if we are working over a *finite* ground field, this would imply that, for most specialisations of  $Y_t$  to a finite ground field, the Frobenius acting on the vanishing part of  $H^{n-1}(Y_s, \mathcal{O}_{Y_s})$  has (some) non vanishing proper values. **If** the proper values of Frobenius acting on Hodge cohomology were just the reductions mod  $p$  of the proper values of Frobenius acting on  $l$ -adic (or crystalline) cohomology, we would be through, and we would have the extremely precise statement: the number of proper values of Frobenius (acting on some  $H^i$  or a piece thereof) which are units is exactly equal to the semi-simple rank of Frobenius acting on the corresponding  $H^i(X, \mathcal{O}_X)$  resp. a piece thereof (the contribution of the other pieces of the Hodge cohomology does not count for obvious reasons). Now things in general won't be that simple, because of  $p$ -torsion phenomena for  $Y$ , which will make the Hodge cohomology a little too big as far as rank i.e. number of proper values, is concerned. But Lefschetz's theorems as stated in his Borel tract suggest that the torsion of  $Y$  should be just the torsion of  $X$ , i.e. independent of the degree of the hypersurface section, so I guess asymptotically (for high degrees) it should not count—provided we prove that the semi-simple rank of Frobenius acting on  $H^{n-1}(Y_t, \mathcal{O}_{Y_t})$  becomes large. So this is really what should be proved. The trouble is that, although this is again a purely geometric question, it does not seem at all a trivial one, even restricting to the case  $X$  a surface (say even  $X = \mathbb{P}^2$ !) and taking a general hypersurface section (not restricted to belong to a pencil). The question then is whether the general curve thus obtained has étale coverings of order  $p$  not coming from coverings of  $X$ . Do you have any feeling about this question?<sup>2</sup>

Do you have any idea how to get Griffiths' example over a number field? And no illuminating examples concerning the Hodge conjecture?

<sup>2</sup> False for  $\mathbb{P}^2$ ;  $\mathbb{P}^2$  is simply connected, but the general curve on it has cyclic covers of order  $p$ .

I wonder if you, Mike and Hironaka would object in principle to including a paper by Atiyah in Zariski's blue volume<sup>3</sup>, in case a paper should be ready before the fixed deadline. He promised us a paper already a while ago from his index series and wants to keep his promise now, but would not like to have too long publication delays. He says he would be quite willing to dedicate something to Zariski, if he got a suitable paper ready in time.

Yours

(signed) Schurik

PS Maybe it will interest you that I worked out something like a formal substitute, for a smooth morphism  $f: X \rightarrow S$  with  $S$  of char 0, and a cycle on  $X$  which is cohomologically equivalent to zero on the fibers with respect to De Rham cohomology, of the corresponding section of the system of Weil-Griffiths jacobians of the fibers (which make sense only transcendently). Namely, the tangent space  $\mathfrak{t}$  along the zero section of this system makes sense purely algebraically in terms of relative De Rham cohomology, and heuristically Griffiths' section, when expressed locally as an exponential, defines a jet of infinite order of that vector bundle over  $S$ , at least up to translation by the image of a horizontal section of the De Rham cohomology sheaf  $\omega$  on  $S$ . Now this jet (more precisely a certain section of  $\mathcal{P}_{S/\mathbb{Q}}^\infty(\mathfrak{t})$  modulo the image of  $\omega$ ) can be given a purely algebraic definition, and even a pretty simple one. As a consequence, the images of the Griffiths section, à la Manin, corresponding to "Picard-Fuchs equations", can be given also a purely algebraic description. I think analogous constructions can be made in unequal characteristics, but I did not clear up my mind on this as yet.

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<sup>3</sup> I.H.E.S. Publ. Math. **36**, Volume dédié au Professeur Oscar Zariski à l'occasion de son 70<sup>e</sup> anniversaire.



8 April, 1969

Massy 8.4.1969

Dear David,

I am establishing a bibliography of your papers and would like you to help me, as there are a few of your papers which I do not have (including some of which you sent me preprints or reprints, which I gave away without keeping track to whom). Could you either give me precise bibliographical indication or send me a reprint of the following of your works:<sup>1</sup>

- 1° Your paper on Teichmüller groups (don't have it any longer).
- 2° Your book on abelian varieties, after your Tata course.
- 3° Your theorem about liftings of abelian varieties to char. 0,<sup>2</sup> which you explained at Tata. Reference to that Tata talk would do (title?).
- 4° Extension to char.  $p > 0$  of the Italian theorems characterizing rational and ruled surfaces.<sup>3</sup> (Never got a preprint.)
- 5° Your recent counterexample to Severi's "theorem" on 0-cycles on surfaces (don't have any preprint left).<sup>4</sup>
- 6° Is there any better reference for your work on abstract  $\theta$ -functions, compactifications of Néron models etc, than the Bowdoin notes by H. Pittie<sup>5</sup> (which I found pretty poor)?

Sorry to write you such an uninteresting letter! Nothing very interesting to report upon. I guess Katz sent you notes of his nice theorem about  $L$ -functions

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<sup>1</sup> In the original there are short horizontal lines, drawn with a ballpoint pen, through 1° and 2°; with the same pen there are also ticks in the margin next to 3°, 4° and 5°. Most of the letter was typewritten, except for some corrections (by Grothendieck) made with a fountain pen. The editors are not sure who made the marks with the ballpoint pen, or why.

<sup>2</sup> See [69d] in this volume.

<sup>3</sup> See [69a] in volume I.

<sup>4</sup> See [69d] in this volume.

<sup>5</sup> See [u67a] in this volume.

mod.  $p$ , which will be used in our seminar to do Griffiths' example for complete intersections. Berthelot found a better definition for the crystalline site, dropping the nilpotency condition for the divided powers and replacing it by the condition that  $p$  is nilpotent on the objects of the site. The trouble with char.  $p = 2$  disappears, and the construction of Dieudonné modules via crystals (for Barsotti-Tate groups) comes out beautifully for all  $p$ . Exponentials no longer exist but logarithms do, and this is OK for defining Chern classes for instance. No doubt left that the definition of Berthelot is good.<sup>6</sup> But there is an immense amount of work to be done on crystalline cohomology! Also, to tie it up with Deligne's beautiful generalized Hodge theory for arbitrary algebraic varieties over  $\mathbb{C}$ ...<sup>7</sup> I guess I will spend the next time trying to understand a little better crystalline cohomology in char.  $p > 0$ , and just leave Griffiths and Deligne to find out how things look like over  $\mathbb{C}$ —and Deligne to explain everything to us in the coming year's seminars!

I hope Mike, Hei and you were not too annoyed at the blue journal not coming out when expected, and that by now Oscar got at least the title pages with the dedications.

Best regards

(signed) Schurik

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<sup>6</sup> Berthelot's thesis, containing the work Grothendieck mentions, was published in *Cohomologie Cristalline des Schémas de Caractéristique  $p > 0$* , LNM 407, Springer-Verlag 1974

<sup>7</sup> P. Deligne, Théorie de Hodge I, Actes du Congrès International des Mathématiciens (Nice, 1970), Tome 1, Gauthier-Villars, Paris, 1971, 425–430; Théorie de Hodge II, I.H.E.S. Publ. Math. **40**, 1971, 5–57; Théorie de Hodge III, I.H.E.S. Publ. Math. **44**, 1974, 5–77.

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14 April, 1969

14.4.1969

Dear David,

Thanks a lot for your letter. I appreciated very much your proof of the fundamental intersection formula, and (with your permission) would like to include your proof in SGA5 VII, as an appendix to the exposé (to be ready soon) of Jouanolou, where he proves the same formula, but in the relative case over any base  $(X, Y$  smooth over  $S)$ , no quasi-projectivity assumption, and working in the  $l$ -adic cohomology ring.<sup>1</sup> Indeed, Deligne remarked about one year ago that  $f^*f_*(y)$  is multiplicative in  $y$  i.e.  $= yf^*f_*(1)$ , in the cohomological context, which comes out rather trivially (such types of results will be in his exposé SGA4 XVIII of Poincaré duality for étale cohomology, which he is supposed to finish writing this summer), and using this Jouanolou proves formula (4.6) of SGA6 XIV<sup>2</sup> by reducing to the case of codimension 1, using the blowing up, in a rather simple way (less sophisticated than yours). I find it amazing how nicely things come out finally, just introducing these blown up schemes which at first may seem extraneous to the situation! By the way, did you try also to get a proof of the formula (4.8) of SGA6 XIV (p.11)<sup>3</sup> by analogous arguments?<sup>4</sup> If so I would appreciate knowing your proof, and reproducing it alongside with your other proof. In the case of  $l$ -adic cohomology, Jouanolou again did the work (not neglecting torsion), and in SGA5 VII this will figure, together with the corresponding structure theorem for the cohomology of the blown up scheme (which previously I could handle only up to torsion, and only using Deligne's preliminary result!) By the way, in

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<sup>1</sup> Mumford's proof of the "self intersection formula" appeared in SGA5 VII, Thm. 9.2 on p. 337 of LNM 589, Springer-Verlag 1977. The proof of the "key formula" appeared in SGA VII, Prop. 9.6 on p. 343 of LNM 589.

<sup>2</sup> On p. 676 of LNM 225, Springer-Verlag 1971.

<sup>3</sup> On p. 677 of LNM 225

<sup>4</sup> Another question is written on the left margin: "And, do you have a proof of RR without denominators for an immersion, in the *Chow ring*?"

this connection I would like to point out to you a nice foundational paper of Manin on motives (not using any conjectures) whose main result is the computation of the *motive* of a blown-up variety. It is “Correspondences, motives and monoidal transforms”, Mat. Sbornik, T.77 (119), n° 4, p.475-507. I hope it will be translated into English.

I did not understand your allusion to Schottky-Wirtinger, as I am ashamed to confess that I never heard about them. I will ask Serre about it, and would of course be interested to get any notes of yours.

Incidentally, I have already your  $\theta$ -functions I to III<sup>5</sup> in *Inventiones*, and am at present only out of part I, which I lent to Raynaud. If you have parts I left, please send one to Raynaud (you sent him only II and III) and one more to me, to make a complete set I to III which I can give away to some chap here; otherwise I send you back II and III.

I spent a few days in Romania. There are two or three bright chaps there, and some more pretty good young people, and I enjoyed discussing with them, including on non mathematical topics; but life as a whole looks pretty grim there, it gives the impression of a devastated country from the very start. People hate the Russians a great deal, and their own police still more, but they will say the first aloud (although never in print), but the second they won't. There is still a seminar going on applications of materialisme dialectique to mathematics, an inheritance from times which remain pretty little removed and quite fresh in everybody's memory—but I am pretty sure there is not a single person in Romania who really gives a damn for communism, at least statistically speaking (because nuts you will find anywhere if you look out for them), and excepting the police of course.

Yours

(signed) Schurik

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<sup>5</sup> [66a], [67a] and [67b] in volume I.

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8 August, 1969

Massy, 8.8.1969

Dear David,

Thanks for your letter. I have no comment to make to your tentative list of invited speakers—except that Lubkin [...] <sup>1</sup> As for the generalization you suggest for EGA IV 17.5.5 (and a variant of 17.15.15) I agree I should have included it, and will do so in the next edition, if Dieudonné lives old enough. (By the way, there is quite a bit already I would like to change in EGA IV!). However I believe you forget one assumption on  $f: X \rightarrow Y$  (besides  $f$  loc. of finite type,  $Y$  integral, all components of  $X$  dominate  $Y$  and have generic fiber of  $\dim \geq n$ ,  $\Omega_{X/Y}^1$  is locally free of rank  $\leq n$ ), namely:  $Y$  geometrically unibranch. (Otherwise take  $Y$  to be a curve with an ordinary double point, and  $X$  the normalisation.) It is enough then that, instead of assuming  $\Omega_{X/Y}^1$  to be locally free of rank  $\leq n$ , we assume it generated locally by  $n$  elements. This implies that we have locally a factorization of  $f$  as  $M \rightarrow X' = Y[t_1, \dots, t_n] \rightarrow Y$ , with  $X \rightarrow X'$  neat (= unramified); but  $X'$  is integral and geom. unibranch since  $Y$  is, and it is easily seen that every component of  $X$  dominates  $X'$ , hence by EGA IV 18.10.2,  $X \rightarrow X'$  is étale, hence  $X \rightarrow Y$  is smooth.

Best regards

(signed) Schurik

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<sup>1</sup> One phrase each in this and two other letters, [1986Jan9] and [1987Feb11], were deleted per instructions from DM. Grothendieck and Mumford wrote their convictions—they did not believe in everything Lubkin claimed.



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**5 January, 1970**

Massy, 5.1.1970

Dear David,

A month ago I got an official invitation to give a talk at the Congress of Nice, to my surprise, as I knew the panel on alg. geom. had not proposed me as a speaker. After asking Serre about it, he explained that the organizing committee had proposed me directly, and that by giving a 50 minutes talk I would not prevent any other geometer who may have more interesting things to say, i.e. by refusing no one else would be invited instead. So I accepted, with the idea of giving an outline of my ideas (or what will have become of them by September) on relations between Barsotti-Tate groups and crystals—as it is my intention to devote most of my research time during the next months to these questions; I hope it is OK with you.

Jouanolou has finally worked out in all details your nice proof of the self-intersection formula in the Chow ring (without neglecting torsion), which will be part of SGA5 VII.9. Using still your idea, he was able to prove also the “key-formula” for a blown-up scheme in the Chow ring, still without neglecting torsion (same reference), and the Riemann-Roch formula without denominators for an immersion of quasi-projective smooth schemes over a field (also in the Chow ring). He also is able to prove the correct formula for  $\lambda^n(i_*(x))$  as an element of  $K(X)$ , as given in my 1957 RR report in the case of char. zero.<sup>1</sup> I think I already wrote you that he proved some time ago the correct  $l$ -adic formula for the cohomology of a blown up variety, again without neglecting torsion, and even as a formula in a derived category. . . Thus a number of questions raised in the last exposé<sup>2</sup> of the Riemann-Roch Seminar SGA6 are settled.

During the month I was in Italy I worked out various foundational questions on Barsotti-Tate groups, over a more or less arbitrary base, assuming

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<sup>1</sup> The results mentioned here appeared in J. P. Jouanolou, *Riemann-Roch sans dénominateurs*, Invent. Math. 11 (1970) 15–26.

<sup>2</sup> exposé XIV

only (most times)  $p$  to be nilpotent. The fact that the base is no longer assumed to be artinian demands considerable extra care, and practically everything one wants to prove for the BT group  $G = \varinjlim^n G(n)$  has to be refined

to statements about truncated BT groups  $G(n)$ . Moreover, for this, a general deformation theory of flat group schemes is needed, for which I have quite clear-cut *statements* ready, but which has still to be proved as part of Illusie's thesis. Granting this, one of the striking byproducts is the following, which for simplicity I will state over a field  $k$  of char.  $p$ : consider the formal variety of moduli  $M$  of the BT group  $G_0$  over  $k$ , hence a universal deformation  $G_M$  of  $G_0$  over  $M$ . Consider  $G_M(1) = \text{Ker}(p \cdot \text{id}_{G_M})$ , which is a flat deformation of  $G_0(1)$  over  $M$ . Then  $M$  is *versal* for  $G_0(1)$  (viewed as a group *killed by*  $p$ ) in the sense of Schlessinger, i.e. " $G_0$  and  $G_0(1)$  have the same variety of (formal) moduli".

In this connection, I wonder if the following might be true: assume  $k$  alg. closed, let  $G$  and  $H$  be BT groups, and assume that  $G(1)$  and  $H(1)$  are isomorphic. Are  $G$  and  $H$  isomorphic? This is true, according to Lazard, if  $G$  is a formal group of dimension 1. Another question is: what are the finite groups  $\Gamma$  which are isomorphic to a  $G(1)$ , for  $G$  a BT group? A necessary condition is that  $\Gamma$  be killed by  $p$  and that the sequence

$$G \xrightarrow{F} G^{(p)} \xrightarrow{V} G$$

be exact. Is this condition sufficient?

Thanks for your notes on "varieties defined by quadratic equations"<sup>3</sup>, which I had no time to look through as yet. Since I set back to do some research, I considerably had to cut down reading! Still, I keep on my table whatever I think I should read sooner or later, so please do not stop sending me reprints!

Best wishes to your and your family for the new year! Yours

(signed) Schurik

P.S. Please send your mail to my *personal address*, as I stopped working at IHÉS (as Deligne probably told you)

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<sup>3</sup> [70] in this volume



**2.** In general, if  $G_1, G_2$  are two BT-groups,

$$G_1(1) \cong G_2(1) \not\Rightarrow G_1 \cong G_2$$

*Pf.* Just look at Manin's long paper<sup>2</sup>, classifying all BT-gps over  $k$  & this is pretty clear. For instance, take the case of 2-dimensional  $G$ 's (i.e. the associated formal gp is 2-dimensional). The  $G(1)$ 's, as described in **1**, depend on at most 4 parameters, while Manin's types depends on arbitrarily many. This is not precise but it "clearly" could be made so.<sup>3</sup>

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<sup>2</sup> Y. Manin, *The theory of commutative formal groups over fields of finite characteristic*. Usp. Math. 18 (1963) 3-90; Russ. Math. Surveys 18 (1963) 1-80.

<sup>3</sup> However  $G_1(1) \cong G_2(1) \implies G_1 \cong G_2$  if one of the two BT-groups  $G_1, G_2$  is *minimal*; see Thm. 1.2 of F. Oort, *Minimal  $p$ -divisible groups*, Ann. of Math. 161 (2005) 1021-1036. See also 4.1 and 4.2 in *loc. cit.* for examples of the negated implication in **2**.

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15 January, 1970

15.1.1970

Dear David,

I agree that the answer to the question of characterizing groups  $G(1)$  for  $G$  a BT group, over an algebraically closed (or more generally, perfect) field is rather trivial, using Dieudonné's theory. By the way, a similar argument shows that if  $\Gamma$  is a finite group over  $k$  which is a flat module over  $\mathbb{Z}/p^n\mathbb{Z}$  (or, what amounts to the same, killed by  $p^n$  and such that  $\text{Ker}(p \cdot \text{id}_\Gamma) = \text{Im}(p^{n-1} \cdot \text{id}_\Gamma)$ ), then, if  $n \neq 1$ , there exists a BT group  $G$  over  $k$  (perfect field) such that  $\Gamma \simeq G(n)$  if  $k$  algebraically closed or  $\Gamma$  is radicial unipotent. On the other hand, using the (not as yet proved) deformation theory of Illusie for flat groups I alluded to in my last letter<sup>1</sup>, one can prove that if  $S$  is a local complete noetherian scheme with residue field  $k$  of char.  $p$ , and  $\Gamma$  a finite flat group scheme over  $S$  which is killed by  $p^n$ , then, if  $\Gamma_0$  is isomorphic to a group  $G_0(n)$ ,  $\Gamma$  is isomorphic to a group  $G(n)$  ( $G_0, G$  being BT groups over  $k, S$ ). Thus, if  $k$  is perfect, we get a nice characterisation of the groups  $G(n)$ , which presumably should hold also without any restriction on  $k$ .

On the other hand, I could not make any sense out of the indications you gave me for constructing an example (over alg. closed  $k$ ) where  $G(1) \simeq G'(1)$  but  $G \not\simeq G'$ . You say that for two-dimensional formal  $p$ -divisible groups  $G$ , the moduli for  $G(1)$  form a variety of dimension at most four; but this seems nonsense, because if you fix not only the dimension  $d$  of the Zariski tangent space to  $G(1)$ , but also the corresponding number  $d^*$  for the Cartier dual  $G(1)^*$  (so that  $d + d^*$  is the "height"), then the moduli space for  $G(1)$  is of dimension  $dd^*$ , which for variable  $d^*$  gets arbitrarily high! Maybe there has been some misunderstanding on my part.

Thanks for your comments concerning my troubles with IHÉS. Fortunately things got arranged, as I was backed by my colleagues from IHÉS for demanding that no military funds should be used for the budget. Finally Motchane

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<sup>1</sup> Published as L. Illusie, *Complexe Cotangent et Déformations I & II*, LNM 239 (1971) and LNM 283 (1972).

166 52 15 January, 1970

told us that no such funds were being used in 1970, and that he gave us “une assurance morale” (not being qualified to give us a formal commitment in this respect) that no such funds were to be used in the future. Thus I have taken up my job at IHÉS again, which of course is also the best solution in personal respect, as the position at IHÉS is quite satisfactory in various respects. Maybe you can inform Deligne about this outcome, as I will probably not write him before a week or so.

Best wishes to you and your family

(signed) Schurik

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 24 May, 1984

Professor Alexander Grothendieck  
 Mathematiques  
 University of Montpellier  
 2, Place Eugène Bataillon  
 34060 Montpellier  
 France

May 24, 1984

Dear Grothendieck,

Thank you very much for sending me your “Esquisse” which I have shared with half a dozen others here. I felt thrilled to hear what you are thinking about and very excited by your ideas.

You asked me in the margin about whether  $T_{g,\nu}$  was known to be the same as  $\pi_1(“M_{g,\nu}$  at  $\infty”)$ . I had wondered about this, too — a long time ago I had asked Tits about it. It seems to me that it follows now from the Thurston-Hatcher paper in *Topology*<sup>1</sup>. But it also follows from the much more powerful and amazing result of Harer<sup>2</sup> (conjectured by Mosher). He constructs a complex  $C_g$  of dimension  $6g - 4$  on which  $T_{g,1}$  operates (the idea will apply directly if  $\nu = 1$  and adapts, I guess, to other  $\nu$ ), plus a subcomplex  $D_g$  containing the  $(2g - 1)$ -skeleton of  $C_g$ , plus a  $T_{g,1}$ -equivariant homeomorphism

$$(C_g - D_g) \xrightarrow{\sim} \text{Teich space of type } (g, 1).$$

In fact,  $C_g$  is a union of  $k$ -simplices, one for each  $(k + 1)$ -tuple  $\sigma_0, \dots, \sigma_k$  of disjoint arcs in a reference surface  $S_g$  with base point  $P_g$ , such that all  $\sigma_i$  begin and end at  $P_g$ ,  $\sigma_i$  are not homotopically trivial and no two are isotopic,

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<sup>1</sup> Referring to A. Hatcher & W. Thurston, A presentation for the mapping class group of a closed orientable surface, *Topology* **19**, 1980, no. 3, 221–237.

<sup>2</sup> This result is published in J. Harer, The virtual cohomological dimension of the mapping class group of an orientable surface, *Invent. Math.* **84** (1986), no. 1, 157–176.

all mod isotopy of  $(S_g, P_g)$ . And  $C_g - D_g$  is the set of simplices for which  $S_g - \bigcup \sigma_i$  is union of cells. The homeomorphism can be constructed elegantly using the theory of Strebel differentials, or the theory of measured foliations. In any case, I think it proves what you want — and more! Enclosed is a xerox of a letter<sup>3</sup> of mine with more details.

I would like to ask you, on another level, would you consider coming here for some period — it could be a short period, or it could be much longer — to pursue your research? We can offer you travel and support at \$850 per week; or a full salary if you can come for a longer period. I realize you are deeply attached to your retreat in the south of France, but perhaps you'd like to think over the possibility of coming here. We can offer you time to do research and contact with lots of people with common interests. It would be wonderful to welcome you here again.

On a more personal note, let me tell you that I have been working in artificial intelligence recently — specifically computer vision. There are some fascinating ideas and problems here. I'd love to discuss these with you if we ever get a chance.

I hope to hear from you soon,

Warmly,

(signed) David Mumford

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<sup>3</sup> DM drew an arrow to the following notes on the margin. "I can't find this right now. I'll send it later."

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**29 June, 1984**

Les Aumettes 29.6.84

Dear David,

I was very pleased to get your warm and enthusiastic letter in response to my “Esquisse”. This is the first time since 1978 (the first glimpse upon the anabelian iceberg!) that one of my former friends in the mathematical world shows a sign of interest in these things, which my own instinct very evidently tells me are basic and exciting indeed. It is very strange—and I do feel like a stranger today among those people I used to like a lot. I should add that it seems kind of natural to me that you should be the one exception, which is in accordance with some lively former impression I’ve got about you.

I must apologize for being late in replying. One reason is that I’ve been sick for a few weeks now, from overwork I’m sorry to say—something really stupid! During the last four months I’ve been “just about to finish” some work, which had started as the “introduction” to Pursuing Stacks, and which has grown into a 500 page retrospective of my life as a mathematician, and of the predicaments which struck some of my work after I left the mathematical milieu. It was a very interesting and fruitful reflection, which is going to be the main part of vol. 1 of Reflexions Mathématiques. But with this feeling, of just having to finish up the stuff and be through, I overstrained, and it turns out I’ll have to take a complete rest for a few weeks, maybe months. Still, I think I’ll send you a copy of that retrospective (in French) sometime in September, and hope it will interest you. I’ve learned a lot writing it. . .

For the reason just said I’ll not, at present, dive into mathematical matters in connection with your letter, except to tell you I’m glad the property that I needed for  $\pi_1(\mathcal{M}_{g,n})$  is ok.<sup>1</sup>

Thanks also a lot for your suggestion to spend some time at Harvard. If any place should be congenial to me, apart from my home, for discussing

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<sup>1</sup> In this case  $\mathcal{M}_{g,n}$  means the moduli *stack* of curves of genus  $g$  with  $n$  marked points, and the  $\pi_1$  is a mapping class group. In the letter Grothendieck refers to the stacks  $\mathcal{M}_{g,n}$  as “Teichmüller multiplicities”.

some of the things in math which have been interesting me in the last years, it is the Harvard area indeed. It is not clear though that I'll feel the desire or need within the next years to leave my home for the sake of doing maths more efficiently. If it should happen I'll contact you again. In the meanwhile, if you've a chance to drop by at my place, you'll be very welcome (I am able to accommodate you, if you feel like staying a few days). In any case I'll write you if I've mathematical questions which I feel you may know about (or simply be interested in). But for the time being I've got to rest! Affectionately, (signed) Alexander.

PS Rather write to my *personal* address: Les Aumettes, 84570 Mormoiron, France.

26 December, 1985

Dec. 26, 1985

Dear Grothendieck,

I've spent quite a bit of time in the last week trying to read your long testimony—something I was very interested in as you have always been a very important and vivid figure in my life. I have been impeded by my inadequate knowledge of French—it is very easy for me to read French mathematics, but not at all easy to read more elaborate things. But I have been very moved by many of the things you say and very upset by others. I hesitated for some days trying to imagine how I might reply. I want to do so but I don't know what I can say which is helpful.

One thing I want to write you about is a rather specific suggestion. For at least 10 years I have had the hope that at some point the right occasion would arise to propose the publication, in a suitably edited form, of a large number of your mathematical *letters* to your friends. For me, the letters that you wrote me are by far the most important things which explained your ideas and insights. The letters are vivid and clear and unencumbered by the customary style of formal French publications. I assume that the letters you wrote to others are similar. They express succinctly the essential ideas and motivations and often give quite complete ideas about how to overcome the main technical problems. I have been very conscious of the difficulties that the younger generation has in getting a clear idea of your theories. This may be blunt and insensitive, but I should say that I find the style of the finished works, esp. EGA, to be difficult and sometimes unreadable because of its attempt to reach a superhuman level of completeness. But, for myself, I never liked Bourbaki either! This is a personal thing, but the point is that your letters would offer a clear alternative for students who wished to gain access rapidly to the core of your ideas. My proposal would be to approach someone with a broad knowledge of your theories, such as Artin or Mazur, and give

them permission to approach the others to whom you wrote at length to send them copies of your letters (with personal details removed). They could then examine the whole corpus and glue and paste and provide orienting remarks, producing a first draft of a publication for you to review. I feel sure that such a collection would be extremely useful to the younger generation, many of whom don't have a good appreciation of your ideas at all. I'm not just thinking of *motives* or *D-modules*  $\sim$  *crystals* of which you write at length. I find equally distressing the lack of understanding of *duality* (no-one reads Hartshorne's book on duality because they find it too long), of *topoi/stacks* (or the related general existence theorem of Artin's), or (earliest of all) the comparison theorem for  $\pi_1$  (never published outside SGA1 I believe). There are hordes of smaller but crucial insights such as your letter to me about the yoga of Koszul ( $\phi \circ \psi = (-1)^{ab} \psi \circ \phi, \dots$ ) which showed me what was what with determinant bundles.<sup>1</sup> What do you think? The collection of letters would also serve as a key to the other published things—SGA, EGA.

On another level, I wanted to make some other remarks vis-a-vis intellectual "burial" and the influence of other people. It is very impressive that very few truly innovative ideas ever become current through the straightforward direct route of simply being published, read, understood and used by one's contemporaries. Quite often others "rediscover" them—which is a euphemism for the idea coming to them directly or indirectly at a time when they are not prepared to understand it—and then, when they are prepared, they think it is their idea. Other times, the focus of the international community shifts and a beautiful insight is forgotten for 30-40-50 years, until a new eddy carries people back to renew the old. Then again, people's idiosyncrasies sometimes prevent them from publishing their ideas in an accessible way: I think of Thurston especially who has published almost nothing of his basic theory of 3-manifolds and the associated developments in the theory of surfaces (e.g. geodesic laminations, "train-tracks", etc.). I think that it is rare that a beautiful idea is really forgotten, but it is probably fairly common that ideas get mis-attributed. I was quite disturbed by the merely passing recognitions that is given the work of Shafarevich, Arakelov and Parshin, when Faltings provided the last push that achieved the Mordell conjecture. (I don't mean that he is not a very strong and deep thinker too, whose faith and belief in the method was very very crucial—but just that the Russian school had provided both the basic tools and the motivating ideas of the proof.) Anyway, I do *not* think your ideas are buried, at least not buried too deeply! I believe there is a world-wide reaction today, a trend, towards mathematics that is more concrete and even computational, as opposed to extremely abstract ideas epitomized by *categories/functors*/*Tohoku*". This is not universal. Mathematics (as opposed to, e.g. particle physics) has the luxury of meandering and dividing

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<sup>1</sup> The letter from Grothendieck containing the yoga of Koszul alluded to here is not among the letters in Mumford's file. —the editors.

like a river in a delta (Shafarevich's metaphor was the amorphous growth of an amoeba). But my perspective is that the intellectual center of gravity is shifting *right now* back to rather concrete problems. This makes it very hard for people to read SGA and EGA. There is clearly a dialectic in mathematics between the concrete and the abstract, and nothing short of the death of mathematics could prevent the center of gravity from shifting back, however, at some point. My feeling therefore is that certain themes in your work, esp. the definition itself of a scheme and the theory of étale and crystalline cohomology, will continue to play a central role, while some of the perspectives will be less appreciated for a while. But this is all facile generalization: the reality is always more complicated.

On a more personal note, in a minor way I can share some of your feelings. What has happened in my life is that I came to a point where I felt that my life was passing quickly and that there were other ideas, other questions that I had once wanted to think about but had totally forgotten while I was immersed in my career as a pure mathematician. These questions were those about the nature of intelligence, about how one thinks and what "thought" consists of. I felt as a student, and I feel now, that the *computational* perspective offers one a fantastic tool to help disentangle these questions and wanted to pursue these ideas before it was too late. Anyway, for three years now I have done essentially no algebraic geometry. What is most disconcerting for me is the feeling of being, on a professional level, in a limbo: before, I had a clear acknowledged position in a clear limited field. When I dropped that I was a complete unknown, sometimes a mistrusted outsider, often a confused student faced with a diverse array of unfamiliar specialties. Anyway, it has been a bit difficult.

Thank you for sending me your testimony, and let me wish you, in the conventional phrase, a "very happy new year". I do indeed wish you a fruitful and rewarding New Year.

Sincerely, with Best Wishes,

David Mumford



9 January, 1986

Les Aumettes Jan. 9, 1986

Dear David,

Thank you very much for your letter, and for your sympathy and concern. It is all the more precious and welcome to me as, among the host of my former friends and students from before my departure, there have been extremely few who so much as took the trouble to reply to ReS., and (believe it or not) altogether only three including you, who would express the feeling that there was maybe something wrong somewhere. The other two are Samuel and (remarkably enough) Illusie, one of the three main artisans of the “Burial”. Beside these three, I got a number of letters expressing interest, sympathy and concern from more recent friends from the mathematical milieu, and still more from people wholly outside and (presumably for this very reason) less reluctant to accept a certain disturbing picture of this milieu, which progressively comes into the fore as the reflection in my “testimony” proceeds.

I greatly appreciate too the effort you made to dig through the French, which certainly isn’t easy, as I have been using often rather colloquial expressions which you wouldn’t find in any dictionary. I was contacted by a New York publisher who wants to have the book translated and distributed in the US. I hope the project is realized, and will be glad to have you get a copy as soon as it is available.

Your letter strikes me as friendly and thoughtful, eager to be “helpful” one way or another—which in itself is comforting, and I am grateful for your concern. Let me be outspoken though, David, and tell you that I feel the *emphasis* in your letter and in your concern is misplaced. Namely, you need not worry (any more than I did and do) about *me*, or about my work and the recognition given to it. This is not the problem. As far as my person goes, my life is a very happy and fulfilled one, and the episode of writing “Reaping and Sowing”, and discovering the “Burial” in its various aspects and impressive proportions, has been part of it (including the moments which have been kind of hard, and which all the less I would wish not to have gone though. . .). If

I don't get a renewal of nomination in CNRS (which has become still more hazardous through the publication of ReS), I'm entitled anyhow to retirement after two more years, which will then allow me to devote myself entirely to those things which fascinate me most, foremost among which, meditation. But even before this, I am to a large extent independent of the good or bad will of my colleagues in the mathematical milieu. As for my work, including the part of it which is still buried or dismantled or made fun of, it is quite clear to me that, if mathematics (and mankind) goes on for a while still (which I don't really feel any more sure about than fifteen years ago...), people couldn't possibly prevent themselves from exhuming my ideas and a certain overall vision, or else rediscovering it. And as far as paternities go, I am not sure really many people seriously believed (even if they pretended to) that étale cohomology, motives and motive-theoretic Galois groups are due to Deligne, étale duality and derived categories to Verdier, the key ideas and the very notion of crystal to Berthelot—and for those who decided to forget what the score was, I guess that (whether they like it or not) the publication of ReS is going to call it back to their minds. And this will be all the more so if (as I now plan) I do spend a few years still, giving a sketch of the vision which was buried, and developing a little, in an informal way, one or the other of its tenets: six operations, crystals, motives, “stacks” and a certain approach to homotopy theory. (And presumably, leaving out the big program of Teichmüller-Galois theory and of anabelian algebraic geometry.)

If something deserves thought and concern it isn't me and my work, but the air in the mathematical milieu you are part of<sup>1</sup>, as I once was part of it. You are explaining to me, but perhaps rather explaining to yourself as a way to reassure yourself, about the pendulum of fashion or moods swinging back and forth between the “concrete” and the “abstract” (which is surely familiar to me, be it only from my own work...), and about people rediscovering things which had become forgotten (which is just as familiar to me). However, all this has nothing to do with the Burial and the spirit of the Burial. You know it yourself, and I need not explain it to you. What I do not as yet understand at all is what my particular person, and my particular impact on the mathematics of my time, or on the friends and students sharing with me the same milieu (and the same passion for mathematics), has to do with the present deep degradation of the professional ethics and of the quality of relations between mathematicians; and notably, the relation between those in position of prestige and power, and the others. What is clear to me though, from various echoes I

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<sup>1</sup> Grothendieck's footnote: (Jan. 10) The expression “air” (in the mathematical community) strikes me as inadequate. My perception of reality would be better expressed by saying that I see this community as a gangrened body, of someone who doesn't care to take notice. In such a case, whatever you may say to him about what you are seeing is lost—the words, however plain, have just lost their meaning. This impression has been very strong lately, with the response to ReS from within the community... And I wonder: what is the sense of doing mathematics, with ears and eyes shut, within such a context?

got from here and there, is that this degradation is by no means restricted to the super-fraud and the derision around my work (and the work of Mebkhout) and the derision around my person. More still than by your suggestions of how to “remedy” the oblivion of, or the difficulties in approaching my work, I would be interested in your personal testimony, echoing mine, about your own contacts with the Burial during the last sixteen years, and beyond this, possibly, about occasional glimpses of the general degradation I have been alluding to; and be it only to tell me that you haven’t noticed anything at any time, if this should be so. This degradation need not express itself, necessarily, by outright dishonesty and cynicism, it may just as well show through the gradual thickening or fading-out of liveliness, mutual concern and delicacy in relations between people. To speak of just two close colleagues of yours with whom I felt ties of close friendship and sympathy; my last letters to Barry Mazur and to John Tate go back to 1976 and to 1981 and never got a reply, and none of the two (nor Raoul Bott, who I was quite fond of too) took the trouble to reply to *Récoltes et Semailles*, which I sent with a personal dedication to each. I mention them because they happen to be at Harvard—but similar cases have been countless for the last ten years, and still more so with the sending out of personal copies of *Récoltes et Semailles*.

But let me get back to cases of outright fraud and ruthless cynicism, as exemplified by the remarkable volume LN 900 on motives (one of the most cited books in the literature), or by the very *name* “SGA4 $\frac{1}{2}$ ” (same remark), or, more shameless than all, the *Colloque Pervers* (same remark again for the Proceedings of the Colloquium, in two volumes, published in Astérisque). The fraud in these cases is evident and glaringly clear to all those who are in touch with the topics dealt with—in the third case, it should add up to at least fifty world-wide known specialists, including such “stars” as Deligne, MacPherson, Beilinson, Malgrange, Verdier, and many others. Here it is not some well-known “ancestor”, who used to be in a position of power himself but who isn’t around any more, who is being plundered—or, if he is indeed, this isn’t really the crux of the matter. The whole Colloquium (exhibiting for the first time a substantial portion of the panoply of the unnamed ancestor...) took place through the solitary and obstinate work of an unknown pioneer, who (drawing inspiration from the ancestor) succeeded to do the work that Deligne had been unable to conceive of and to do, ten years before. Through the connivance of the participants in this Colloquium this “unknown soldier”, who did the work which none of these brilliant people had ever dreamed of, isn’t named at all in the first volume of the Proceedings, and in the second only quite incidentally and never with reference to the main result which was the very spring of the Colloquium. I said enough about this affair in ReS, so that I need not dwell upon any more details. In connection with your letter, the association came to me with the Lubkin affair—I believe you never really looked too closely at what happened, and (as far as I remember) I never took

the trouble to discuss the matter with you, which appeared to me as [...] <sup>2</sup>—the only case I was confronted with in the period till 1970 (when I left). I was surprised that it should go through, unnoticed, but I was too busy then doing mathematics to worry too much. Maybe, in those clement times, what protected Lubkin was that his status was a modest one, so people would feel it wasn't nice in such a case to be too fussy. And I am not sure there is a direct link between this particular isolated case of fraud, and the “new times” which set in about ten years or so later, when fraud gradually has become the “new look” in mathematics. What strikes me is that the situation is now exactly reversed—nowadays it is a whole bunch of some among the most prestigious mathematicians, who will (by common agreement) shamelessly rob an obscure “assistant” in a provincial university, that nobody (except the bunch in the know) has ever heard of. . .

The point I want to make is this one. This kind of “new look” is alien to your own ways, sure enough, and getting to come into touch with it some way or other is embarrassing and painful, so it is understandable you prefer to turn away your eyes and forget about it. And I am afraid that most of the mathematicians and maybe all, who have not been won over to the “new look” (and I doubt not there are many still), just react that very same way. Maybe they'll say “poor Grothendieck” or (à la rigueur) “poor Mebkhout” (as far as the Burial goes) or “poor such and such”—and then turn to more pleasant thoughts. It may be more beneficial though to have a closer look to what is going on (however painful), and, when this is done, not to hesitate to call a fraud a fraud, and a crook a crook, even if this should be disagreeable to those who like to carry on their swindle, and to trample on the defenseless. Just turning your head away will indeed benefit the new style, and contribute your own share (in the passive mode) to the proliferation of such things as the Colloque Pervers, which already now is considered as something perfectly normal and honorable by the entire mathematical community. At any rate I got precious few responses, from within the math community, who would clearly imply they do *not* consider it normal nor honorable. Your response at any rate, however thoughtful, friendly and sympathetic, does not.

Just one more word. The situation of Mebkhout, since ReS was sent out, has become more difficult than ever—he has to face the hostility of nearly all colleagues, who will let off their own embarrassment (at the inequity of what was done to him through some of the most brilliant members) by holding him responsible for ReS, as the one holding the strings etc. He is at present at IAS, for three months, and I suppose he must be very isolated and ill at ease at this place, with people like Deligne, Langlands, Faltings (all very much “partie prenante” of the Burial). His friends (none of whom has the courage or weight to speak out publicly) foretell him that he may well be fired from CNRS (where he finally got admitted a few years ago), and that his future will no doubt be a dark one. And this prediction cannot but turn

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<sup>2</sup> short phrase deleted—the editors.

true, if the climate of cynicism, and of indifference to it I have been trying to describe should prevail. I am not sure that any single action by anyone, however prominent, can really remedy this state of affairs. But I do know that (on a wholly different level) just *one* act of decency and respect, of just anyone, however humble his status, means a lot...

This letter has become prohibitively long, David, and still I am far from having responded to all you touched upon in your long and thoughtful letter. Maybe some other time—this time, I responded first to what was strongest on my mind, I hope you won't mind (indeed, I'm sure you will not!). I look forward to hearing from you again. Please give my regards to our common friends, and above all to Oscar and Yole, if you have a chance. (I heard from Yole, and later from Mike Artin,<sup>3</sup> that Oscar isn't at all in good shape.)

Affectionately

(signed) Alexander

All the best for the New Year too!

Personal address (much speedin')  
 Les Aumettes  
 84570 Mormoiron  
 France

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<sup>3</sup> Grothendieck's footnote: Mike had the courtesy to answer and acknowledge receipt of ReS, in a few embarrassed lines before passing to news about Oscar, and then to mathematical matters which to me have just no meaning any more...



11 February, 1986

Alexander Grothendieck  
Les Aumettes  
84570 Mormoiron  
France  
Dear Alexander,

February 11, 1986

Thank you for your long and moving letter which I have thought about quite a bit. I also discussed the questions you raise with various friends to see if this would give me a different point of view.

But after all this, I really must say I don't agree with you that there has been a general degradation in the manners and customs of the mathematical community. By moving into different fields, what I have found is that on the contrary pure mathematics has better manners and is much more gentle than any of the other fields I have touched. So many other fields have a standard custom of not acknowledging your rivals' work if you can avoid it (and, in fact, everyone has "rivals" to begin with) and being brutal in your criticism of others whenever you have an opportunity. I feel that there are lapses in the mathematical world, but they are rare, the people involved are usually guilty more due to oversight than to intent, and almost everyone tries to rectify the errors. On the other hand there is more of a general tendency in mathematics to forget whatever the previous generation did!

I can't comment on the specific cases of fraud you talk about because I haven't been involved and I don't want to second guess who did what without asking them. I know that others besides yourself have been very upset by particular incidents (Siggy Helgason or Gabriel Stolzenberg for instance). As for Lubkin, as I recall it, he was indeed [...] <sup>1</sup>, but I also believe he had some good ideas of his own (his version of étale Čech cohomology for instance) and he got his sad reward by being wholly ignored.

So that's my perception of this isolated corner of the world: still rather blissfully lucky. I'm sorry that this perception is so different from yours. I feel

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<sup>1</sup> short phrase deleted

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this letter is a very inadequate way of communicating. It would be very nice if we could meet in person again some time. I hope you know how vivid and influential a figure you were in my life and my development at one time. Let me extend my very best wishes to you now.

Sincerely,

(signed) David Mumford