1976a Duality in the flat cohomology of surfaces

(Ann. Sci. Ecole Norm. Sup. 9 (1976), 171-202).

By the early 1960s, the fundamental importance of the cohomology groups

$$H^i(X, (\mathbb{Z}/\ell^r\mathbb{Z})(m)) \stackrel{\text{def}}{=} H^i(X_{\text{et}}, \mu_{\ell^r}^{\otimes m})$$

for the study of the arithmetic of algebraic varieties was already clear. However, there was a gap in the theory: the groups are only defined when $\ell \neq p$ (the characteristic of the ground field k). What should the groups $H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(m))$ be? Crystalline cohomology doesn't provide the answer, because it is the analogue of de Rham cohomology, and is not (directly) useful, for example, in studying the Brauer group of a variety.

For $m = 0$ there is no problem: one can take

$$H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(0)) \stackrel{\text{def}}{=} H^i(X_{\text{et}}, \mathbb{Z}/p^r\mathbb{Z}).$$

For $m = 1$, the sheaf μ_{p^r} is zero on X_{et}, but its flat cohomology has the correct properties: one can take

$$H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(1)) \stackrel{\text{def}}{=} H^i(X_{\text{fl}}, \mu_{p^r}).$$

This suggested trying

$$H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(m)) \stackrel{???}{=} H^i(X_{\text{fl}}, \mu_{p^r}^{\otimes m})$$

where $\mu_{p^r}^{\otimes m}$ is the sheaf $\mu_{p^r} \otimes \cdots \otimes \mu_{p^r}$ on X_{fl}, but this is not promising because the sheaf $\mu_{p^r} \otimes \cdots \otimes \mu_{p^r}$ is a big mess (in contrast, $\mu_{\ell^r} \otimes \cdots \otimes \mu_{\ell^r}$ is just a twist of $\mathbb{Z}/\ell^r\mathbb{Z}$).

In my thesis, I studied the flat cohomology of μ_{p^r} in the following way. Let $f\colon X_{\text{fl}} \to X_{\text{et}}$ be the "continuous" map defined by the identity map. The exact sequence of sheaves on X_{fl}

$$0 \to \mu_{p^r} \to \mathbb{G}_m \xrightarrow{p^r} \mathbb{G}_m \to 0$$

provides an exact sequence of sheaves on X_{et}

$$0 \to f_*\mathbb{G}_m \xrightarrow{p^r} f_*\mathbb{G}_m \to R^1 f_*\mu_{p^r} \to 0 \to 0 \to \cdots.$$

Therefore $R^j f_*\mu_{p^r} = 0$ for $j \neq 1$, and so

$$H^i(X_{\text{fl}}, \mu_{p^r}) = H^{i-1}(X_{\text{et}}, \nu_r)$$

where ν_r is the étale sheaf $R^1 f_*\mu_{p^r} \simeq \mathbb{G}_m/p^r\mathbb{G}_m$. In my thesis, I studied ν_1 using the exact sequence

$$0 \to \nu_1 \to \Omega^1_{X,\text{cl}} \xrightarrow{1-C} \Omega^1_X \to 0 \quad (*)$$

where C is the Cartier operator. Eventually, this suggested the following to me:

(a) Instead of looking for the mythical flat sheaves "$\mu_{p^r}^{\otimes m}$", one should posit that $R^j f_*$"$\mu_{p^r}^{\otimes m}$"$=$ 0 for $j \neq m$ (since this is true $m = 0, 1$), and instead look for the étale sheaf $\nu_r(m) \stackrel{\text{def}}{=}$"$R^m f_*\mu_{p^r}^{\otimes m}$". Thus, conjecturally,

$$H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(m)) \stackrel{\text{def}}{=} H^{i-m}(X_{\text{et}}, \nu_r(m))).$$

(b) To study $\nu_1(m)$, one should look for a sequence like (*).

In 1969, when Tate visited London (from Paris) to give a lecture, I told him that Ω^2 should play the role of the mythical cohomology group $H^2(X_{\text{fl}}, \mu_p \otimes \mu_p)$. A few days later, he sent me a letter (see below) saying that my idea seemed to be correct, because he had been able to define a new symbol (now called the Tate symbol) with values in Ω^2 analogous to the Galois symbol which takes values in $H^2(k, \mu_\ell^{\otimes 2})$.

In the early 1970s, Artin conjectured a flat duality theorem for the cohomology of μ_{p^r} on a smooth projective surface. I succeeded in proving the conjecture for $r = 1$ by using the sequence (*). In fact, for an arbitrary smooth projective variety X, I defined the étale sheaves $\nu_1(m)$ by the sequence

$$0 \to \nu_1(m) \to \Omega^m_{X,\text{cl}} \xrightarrow{1-C} \Omega^m_X \to 0. \quad (**)$$

and proved a duality theorem for the groups $H^i(X, (\mathbb{Z}/p\mathbb{Z})(m)) \stackrel{\text{def}}{=} H^{i-m}(X_{\text{et}}, \nu_1(m)))$. Since $\nu_1(1) \simeq R^1 f_* \mu_p$, this gave Artin's conjecture for a surface, but only for sheaves killed by p.

Spencer Bloch spent 1974-1976 at the University of Michigan. In 1974, when I told him that I had a good theory for the groups $H^i(X, (\mathbb{Z}/p^r\mathbb{Z})(m))$ for $r = 1$ using the sheaves of differentials Ω^j, but that I didn't know how to extend it for $r > 1$ because the sheaves Ω^i are killed by p, he was able to tell me that he had defined sheaves of differentials that are killed only by p^r. This was his work on what became known as the de Rham-Witt complex. Using Bloch's work, I was able to complete the proof the duality theorem for surfaces (Artin's conjecture). Moreover, for varieties of arbitrary dimension, I showed that the five-lemma would (trivially) extend the proof of the duality theorem from $r = 1$ to all r once one had exact sequences

$$0 \to \nu_1 \to \nu_r \xrightarrow{p} \nu_{r-1} \to 0 \quad (***)$$

(cf. Remark 3.14 of the paper; also 1.7 and 1.11 of my 1986 AJM paper). Bloch's definition of the de Rham-Witt complex was difficult to work with (typical curves on K-groups). Deligne suggested a much simpler construction, and in working out the details of Deligne's idea, Illusie was able to prove (***).[1] See also Milne1987, notes.

Both Bloch and I spoke on our work at the AMS Summer Institute on Algebraic Geometry, Arcata 1974, but neither of us was invited to contribute to the published proceedings[2] of the Institute.

The sheaves $\nu_r(m)$ have proved to be very successful in playing the role of "$R^m f_* \mu_{p^r}^{\otimes m}$". See, for example, the notes for my papers 1986a and 1988b and my joint papers with Niranjan Ramachandran.

Francophiles may prefer the exposition of the proof of the duality theorem (case of surfaces only) in Berthelot 1981.[3]

[1] Illusie, of course, is aware of all this, but nevertheless credits the duality theorem to one of his students.

[2] Algebraic Geometry – Arcata 1974 (Proc. Sympos. Pure Math., Vol. 29), Amer. Math. Soc., Providence, R.I., 1975.

[3] Berthelot, P. Le théorème de dualité plate pour les surfaces (d'après J. S. Milne). Algebraic surfaces (Orsay, 1976–78), pp. 203–237, Lecture Notes in Math., 868, Springer, Berlin-New York, 1981.

Erratum

Conditions on p. I only needed the assumptions on p (e.g., p186, $p > 2, m$) because I had to rely on Bloch's paper for the de Rham-Witt complex and he makes those assumptions (I should have made that clear). Once Illusie's paper became available they could be dropped, as Berthelot made clear when he rewrote my paper in the case of surfaces (see footnote above).

Remark 1.2, assumes that is if S is perfect, then $\Omega^i_{X/S} = \Omega^i_{X/\mathbb{F}_p}$. This is incorrect, as illustrated by $X = S = \operatorname{Spec}\mathbb{F}_p(t)^{\mathrm{al}}$. The remark isn't used anywhere.

Timo Keller point out that there is no (1.16) (cited on p176). Further:

- ◇ In the statement of Lemma 1.7 it should read $d\Omega^{m-r-1}_{X/S}$. (add -1);
- ◇ also on p. 177 in the middle diagram, and in the diagram before Corollary 1.10: +1 should be -1.
- ◇ In the diagram in the proof of Lemma 1.7, it should read $F_*(\Omega^r_{X/S})$ (move the) up)

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Dear Milne,

Hope all went well with your plans for next year. I'm leaving tomorrow for two weeks in Moscow. Back in Paris in June and probably most of July. Then in late August back to Cambridge, Mass.

Did you mention to me something about Ω^2 playing the role of $R^2(\mu_p \otimes \mu_p)$ in characteristic p? This seems the right philosophy for $K_2 F$, F a field: I recall that a homomorphism $K_2 F \to A$ (A an abelian group written multiplicatively) is the same thing as a "symbol on F with values in A", ie a map $(\,,\,) : F^* \times F^* \longrightarrow A$ such that 1) $(xy, z) = (x,z)(y,z)$ and $(x, yz) = (x,y)(x,z)$

2) $(x, 1-x) = 1$ for $x \neq 0, 1$, $x \in F$.

Let p be a prime. 1 If $p \neq \mathrm{char}\, F$, let F_s be ~~the~~ a separable algebraic closure of F and $G = Gal(F_s/F)$ and $\mu_p = \mu_p(F_s)$. The exact sequence of G-modules

$$0 \to \mu_p \to F_s^* \xrightarrow{p} F_s^* \to 0$$

gives $F_s^*/(F_s^*)^p \overset{\delta}{\rightarrow} H^1(G, \mu_p)$, and the cup product $H^1(G,\mu_p) \otimes H^1(G,\mu_p) \longrightarrow H^2(G, \mu_p \otimes \mu_p)$ gives then a map $(\,,\,)$ $F^* \times F^* \longrightarrow H^2(G, \mu_p \otimes \mu_p)$, via $(x,y) = \delta x \cup \delta y$ which is easily seen to be a symbol.

2 If $p = \mathrm{char}\, F$, then $(x,y) = \frac{dx}{x} \wedge \frac{dy}{y} \in \Omega^p_{F/\mathbb{F}_p}$ is obviously a symbol.

Furthermore, in both cases one has, if $\{\,,\,\} : F^* \times F^* \to K_2 F$ denotes the universal symbol:

$\{x, y\} \in p K_2 F \iff y$ is norm from the algebra $F[T]/(T^p - x) \iff (x,y) = 0$, so the two cases are really quite analogous. Best regards, J. Tate

Mathematics Department,
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21st July, 1987.

Dear Mr Suwa,

The statement in your letter that "the duality for the logarithmic Hodge-Witt complex was a folklore" has prompted me to review its origins.

(a) In my thesis (1967) I showed the duality between the cohomology of $\mathbf{Z}/p^n\mathbf{Z}$ and μ_{p^n} over a curve. I also showed at the time (to the surprise of Artin) that the cohomology of α_p was not self-dual (in the naive sense) when the ground field was infinite. This suggested that the flat cohomology of μ_p on a surface over an algebraically closed field is not self-dual in any naive sense.

(b) By 1969 I had the idea that Ω^2 should somehow play the role of $R^2f_*\mu_p\otimes\mu_p$, where $f\colon X_{fl} \to X_{et}$. I don't know if this is what gave Tate the idea for his symbol, but shortly after I told him the idea, he sent me a letter (14/5/69) saying that it seemed to be "the right philosophy" and defining the Tate symbol $K_2F \to \Omega^2$.

(c) Sometime in the early 70's, I found the duality theorem for $\nu_1(r)$ on a variety over a finite field. Bloch was spending a year at Michigan and noted that he was able to define (using K-theory) sheaves of "differentials" killed by p^n (his IHES paper). Also Artin sent me a preprint of the paper (Ann ENS 1974) in which he conjectured the flat duality theorem for a surface over an algebraically closed field. In 1975 I wrote my Ann ENS paper proving my theorem. The purpose of the paper was to prove what was needed for Artin's paper and my 1975 Annals paper.

Of course, at the time I wrote the paper I regarded the definition of the $\nu_n(r)$ as being tentative when n and $r \geq 2$: in (3.14) of the paper I noted that they should be defined so that there are exact sequences

$$0 \to \nu_{n'}(r) \to \nu_{n+n'}(r) \to \nu_n(r) \to 0,$$

and predicted that the logarithmic differentials should have this property (at the time, no one was clear on the difference between the Milnor and Quillen

K-groups). Of course the "de Rham-Witt complex" didn't exist, except in Bloch's version, in 1975. When Illusie wrote his paper (1979) he effectively verified my prediction (without saying so). Then, of course, a trivial induction argument allows one to pass from $\nu_1(r)$ to $\nu_n(r)$ in the general case.

(d) Throughout the 1970's, I promoted the philosophy that that $\nu_n(r)$ should be thought of $R^r f_* \mu_{p^n}^{\otimes r}$. This has many implications, most notably:

(i) it suggests the notation $H^i(X, \mathbf{Z}_p(r)) = \varprojlim H^{i-r}(X, \nu_n(r))$;

(ii) it suggests a purity statement, which should lead to a cycle map into $H^{2r}(X, \mathbf{Z}_p(r))$, and therefore into crystalline cohomology;

(iii) it suggests that there should be an analogue

$$0 \to \nu_n(r) \to \oplus\ \iota_* \nu_n(r)_{k(x)} \to \cdots .$$

of the Bloch-Ogus sequence.

I verified parts of these, for example (ii) for $\nu_1(r)$, and informed the Paris mathematicians (Gabber, Illusie,...) about them, but published nothing except for my 1982 Compositio paper, which is the substance of my talk at the 1978 Rennes conference (except that I didn't know 4.1 at the time).

Since 1977 my research has concentrated on Shimura varieties, but in 1982 I was inspired by Lichtenbaum's talk at the Durham conference (see his 1983 paper) to generalize my 1975 Annals paper to varieties of dimension > 2 (and values other than 1). This entailed completing and writing up some of my earlier work, done in the mid 70's, and constitutes my AJM paper.

Illusie had always seemed sceptical of the significance of the cohomology of the $\nu_n(r)$'s. Consequently I was surprised to receive Gros's thesis in 1983 which carries out part of the philosophy in order to define also a cycle map. Moreover, I was angered to find that the Introduction of the first version of the thesis didn't mention my work (except, perhaps, anonymously in the second last sentence), and gave the impression (second paragraph) that the whole subject of the "cohomology plus fins" $H^i(X, \nu_n(r))$ began with Illusie's 1979 paper. This was corrected in the published version.

In conclusion, while the philosophy and its implications may seem obvious now (even folklore), I think it is well to remember that this was not always the case.

Concerning the other points in your letter: Good exposition requires good notation, and $W_n\Omega^i_{X,\log}$ is very clumsy notation; I chose $\nu_n(i)$ because of its simplicity and it similarity with $\mu_{p^n}(i)$; I see no need to change it. I think I have explained above the conjectural origins of the complex you prove in your paper.

I should say that I was happy to see the excellent new results that you and Gros obtained in your paper. There is obviously still a great deal to be done concerning logarithmic cohomology; perhaps the most immediate problem is that of extending the duality theorem to noncomplete varieties. It always seemed to me that it should be possible to do this using [Hartshorne, Math. Ann 1972] or Deligne's Appendix to SLN 20, but I never carried it out.

Yours sincerely,

J S Milne

J. S. Milne