Mathematics Department,
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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 $f\colon X_{fl} \longrightarrow X_{et}$. 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 $K_2F \longrightarrow \Omega^2$.

(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

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

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

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