

# COURRIER DES LECTEURS

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## Comments on non-references in Weil's works

S. Lang (Yale University, New Haven)

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In 1999, the *AMS Notices* published several articles on André Weil's works (April, June-July, September). These were complemented in the April 1999 *Notices* with an editorial on Weil by the *Notices* editor in chief Anthony Knapp. Concerning a comment at some Weil talk that proper credit was not given by Weil for some theorem, Knapp quoted Weil's answer. "I am not interested in priorities", and added his own comment : "This was the quintessential Weil. Mathematics to him was a collective enterprise." I object. In the sense that mathematics progresses by using results of others, Knapp's assertion is tautologically true, and mathematics is a collective enterprise not only to Weil but to every mathematician.

However, there is also another sense. Mathematics is often a lonely business. Public recognition of the better mathematicians is a fact. A competitive spirit, present in Weil as in other mathematicians, sometimes degenerates into dishonesty. Mathematicians are made aware early in their career of the need to attribute results properly. Weil transgressed certain standards of attribution several times throughout his life in significant ways. I documented at least one of these ways in my *Notices* Forum piece on the Shimura-Taniyama conjecture [La 95b]. In this piece, I reproduced

a letter from Weil to me (3 December 1986), ending with Weil's own peremptory conclusion : "Concerning the controversy which you have found fit to raise, Shimura's letters seem to me to put an end to it, once and for all." Now a year after Knapp's editorial, Rosen returns to the Shimura-Taniyama conjecture with some comments [Ro 00] p. 476, where he does not accept Weil's own conclusion.

The Knapp editorial and Rosen's comments prompt me to complement my *Notices* article by further historical remarks showing how Weil several times throughout his life did not properly refer to his predecessors, but was "interested in priorities". These constitute significant examples, when Weil does not regard mathematics as a "collective enterprise" in the sense that he hides the extent to which he uses previous work, and sets up or pokes fun at some of his predecessors, as we shall now document.

### *On Hasse and Deuring's work concerning correspondences*

It was Hasse who uncovered the source of proof for the Riemann hypothesis in function fields (Artin's conjecture from his thesis). Weil's books on curves and abelian varieties [We 48a], [We 48b] published in the late forties do not mention Hasse's or Deuring's contributions. In particular, Weil does not mention that Hasse not only proved the theorem for

curves of genus 1, but that he uncovered the relation between characteristic 0 and characteristic  $p$ , and that Hasse followed by Deuring pointed to the theory of correspondences as the key to the solution in general. Furthermore, Weil was indeed interested in priorities, as when he wrote tentatively that some results of Severi were “rediscovered by Deuring”, thereby minimizing his predecessors’ discoveries, and misrepresenting the context in which they were made. For example, in Weil’s 1940 letter to Simone Weil (*Collected Papers* Vol. I, p. 253) (which he calls an “*esquisse d’histoire de la théorie des nombres*” in the appended comments), Weil writes (my translation)<sup>1</sup> :

“... it is incredible the extent to which people as distinguished as Hasse and his students, who gave their most serious thoughts to this subject for years, have not only neglected, but deliberately disdained the riemannian direction : it’s to the point where they can’t read works written in Riemannian (Siegel once poked fun at Hasse who had told him about not being able to read my paper in the *Liouville journal*), and that they rediscovered sometimes with considerable pain, in their dialect, important results which were already known, such as those of Severi on the ring of correspondences, rediscovered by Deuring.”

This quote might be “quintessential Weil”, but it shows something other than “mathematics to him was a collective enterprise.” It is actually a very tendentious presentation masquerading as history. Artin, Davenport, Hasse, Mordell, Siegel, Weil, had limitations, like all of us, including me. The phrase “not only neglected but deliberately disdained” (“non seulement négligé, mais dédaigné de parti pris”) is an example of Weil’s tendentious attributions. One of Hasse’s limitations was that he was not able to read the classical transcendental versions of the theory of abelian functions, as in Poincaré, Castelnuovo, or Weil’s paper [We 38], and was not able to read the Italian geometers as well as Weil, but it was not a question of “disdain” or “neglect”. Hasse and Deuring did not merely “rediscover ... in their dialect” results already known to Severi. Notably Hasse, who had just written major papers on complex multiplication (1927-1931), saw first the connection with the Riemann hypothesis in function fields of genus 1 [Ha 34], and he pointed to the theory of correspondences and endomorphisms as holding the key to the problem of the Riemann hypothesis in function fields generally. Thus Hasse made a fantastic step forward in connecting the complex theory with the purely algebraic theory in characteristic  $p$ , and showing how reduction

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<sup>1</sup> « ... il est incroyable à quel point des gens aussi distingués que Hasse et ses élèves, et qui ont fait de ce sujet la matière de leurs plus sérieuses réflexions pendant des années, ont, non seulement négligé, mais dédaigné de parti pris la voie riemannienne : c’est au point qu’ils ne savent plus lire les travaux rédigés en riemannien (Siegel se moquait un jour de Hasse qui lui avait déclaré être incapable de lire mon mémoire de Liouville), et qu’ils ont retrouvé quelques fois avec beaucoup de peine, en leur dialecte, des résultats importants déjà connus, comme ceux de Severi sur l’anneau des correspondances, retrouvés par Deuring. »

In [We 60], Weil wrote another similar put down of his predecessors, without citing them by name, stating that « les meilleurs spécialistes des théories arithmétiques et ‘galoisiennes’ ne savaient plus lire le riemannien, ni à plus forte raison l’Italien... » *Collected Works* Vol. II, p. 412, [My translation : “the best specialists of arithmetic and ‘galois’ theories didnt know any more how to read ‘Riemannian, let alone italian...”]

mod  $p$  mixes with complex multiplication in the theory of endomorphisms.<sup>2</sup>

Readers cannot get an inkling of the origins of such fundamental insights either from Weil's own works or from the accounts of Weil's works in the *Notices* (1999). For instance, Raynaud's account [Ra 99] refers to Hasse in just one sentence : "[The Riemann hypothesis in the case of curves over finite fields] was first proved by Hasse [4] in the case of elliptic curves ( $g = 1$ )."

After breaking open the whole question as above, Hasse [Ha 36] in three *Crelle* papers developed the theory purely algebraically on elliptic curves in characteristic  $> 0$ , independently of reduction mod  $p$ . Later Deuring started dealing with the theory of correspondences algebraically in characteristic  $> 0$  for higher genus [De 37], [De 41a], [De 41b]. Among other things, he started the representation of the endomorphisms on the points of finite order of the jacobian ( $l$ -adic representations). Certain previous results of algebraic geometry, some coming from the more algebraic methods of Severi and others from more transcendental methods of Castelnuovo, needed to be algebraicized completely because they were needed in this generality for the applications to the Riemann Hypothesis on higher genus curves in characteristic  $p$ .

I don't know how justified Weil is in attributing to Siegel the reaction toward Hasse as Weil's describes it. But Siegel had no reason to ridicule or poke fun at ("se moquait de") Hasse for his limitation in not understanding Weil's transcendental approach to abelian functions. Although Siegel

himself understood and handled this type of analysis, Siegel's limitations were evidenced later by his inability to understand much of the mathematics and especially algebraic geometry developed in the fifties and sixties, as partly described in my article concerning Siegel's letter to Mordell [La 95a].

I myself have had my own limitation in that I was not (and still am not) able to read the papers of the Italian geometers. I needed the algebraic translations by van der Waerden, Chevalley, Zariski and Weil himself to get into the subject. It was not at all the case that I "not only neglected but deliberately disdained" those works.

### *On Castelnuovo's work*

Weil also did not regard mathematics as a collective enterprise with Castelnuovo, by leaving out of his references throughout his life the extent to which he used Castelnuovo's ideas concerning the equivalence defect and the jacobian of a curve.

In my book on abelian varieties, I systematically gave Weil credit for his ability to make the contributions of Severi and Castelnuovo available to the postwar period of algebraic geometry, and to go beyond. In fact, in historical comments concerning Castelnuovo's equivalence defect, I stated that Weil "was the first to recognize that Castelnuovo's theorem on the equivalence defect of correspondences on a curve could be expressed as a theorem on abelian varieties." It turns out that I was wrong. I was taken to task for this attribution by Kani [Ka 84], see especially p. 27, footnote 12. Indeed,

<sup>2</sup> Essentially, in [Ha 34], Hasse gives a one-line proof for the Riemann hypothesis on elliptic curves, assuming appropriate foundations. Indeed, he argues as follows. Lift the curve from characteristic  $p$  to characteristic 0, and also lift the Frobenius endomorphism to a complex endomorphism  $\mu$ . The degree of Frobenius is  $q$ . Hence  $\mu\bar{\mu} = q$ , so  $|\mu| = q^{1/2}$ , which is one formulation of what one is after.

Weil makes only one reference to Castelnuovo, in his book on abelian varieties, for some of the basic theorems on abelian varieties by stating (my translation)<sup>3</sup> : "... already Castelnuovo had recognized how to use the latter, although it is not easy to find in his works a formulation or even less a precise justification ... The proof of Poincaré's theorem from the above principle, which one will find in No. 51 of the present work, is for instance substantially the same as the proof given by Castelnuovo in the classical case, in No. 9 of his memoir."

However, Weil does not refer to any other paper by Castelnuovo, and he omitted a far more important reference to another of Castelnuovo's papers *Sulle funzione abeliane* [Ca 21]. This paper is also reproduced in Castelnuovo's collected works. I learned of this paper and of Castelnuovo's fundamental contributions from Kani [Ka 84]. In the complex case, the relation between Castelnuovo's equivalence defect and an intersection number on the Jacobian is clearly established in [Ca 21]. Furthermore, Castelnuovo defines the characteristic polynomial of an endomorphism of the jacobian (determinant of the pfaffian of the complex representation). He shows that the equivalence defect occurs as the penultimate coefficient of the characteristic polynomial, as on pp. 536, 538 and 541, and that all these coefficients can be expressed as intersection numbers. Castelnuovo also gives the intersection formulas of the sum of the curve with itself  $r$  times and the theta divisor,

as well as powers of the theta divisor. See pp. 547-548. In the fifties, I learned such results from Weil's book and lectures on abelian varieties. Weil in his book used essentially the same notation as Castelnuovo for the varieties equal to the sum of the curve with itself  $r$  times on the Jacobian,  $r = 1, \dots, p-1$ , and for the theta divisor. (Weil uses  $W_r$  and Castelnuovo uses  $V_r$ , the theta divisor being equal to  $W_{p-1}$  ( $p = \text{genus}$ .) But there are no references to Castelnuovo on these matters in Weil's works, nor were there in his courses, nor are there in the AMS *Notices* article on the above subject matter [Ra 99]. What Weil did in the forties was to algebraicize Castelnuovo's theory, and extend it, following Hasse's fundamental discoveries and Deuring's subsequent work on the subject, as described above. Of course, to carry out this plan was a first rate mathematical achievement. For two decades, Weil was the only one in the world capable of pulling it off, in large part because he knew how to read Castelnuovo and Severi. I have the highest regard for his mathematics. But being a great mathematician is not a license for obscuring and misrepresenting the works and original ideas of others who opened up the field, and for poking fun at them.

### *On Mordell's conjecture*

Weil correctly referred to Mordell's conjecture in his thesis [We 28],

<sup>3</sup> « ... déjà Castelnuovo avait reconnu le parti qu'on peut tirer de ce dernier<sup>(3)</sup>, sans qu'il soit pourtant facile d'en trouver chez lui une formulation ni encore moins une justification précise. » Weil's footnote 3 refers to Castelnuovo's « beau mémoire *Sugli integrali semplici appartenenti ad una superficie irregolare* (Rend. Acc. Linc. (v) xiv, 1905), reproduit (N° . xxvi) dans G. Castelnuovo, *Memorie Scelte*, Bologna 1937 la démonstration du théorème de Poincaré à partir du principe en question, qu'on trouvera au n° 51 du présent travail, est par exemple substantiellement identique à celle qu'en donne Castelnuovo, pour le cas classique, au n° 9 de ce mémoire. »

when he stated that (my translation)<sup>4</sup> “...this conjecture, already stated by Mordell (loc. cit. note 4) seems confirmed to some extent by an important result recently proved...”, and then cites Siegel’s theorem on the finiteness of integral points on curves of genus at least 1. Weil made a similar evaluation in *Arithmetic on algebraic varieties* [We 36], but without reference to Mordell, namely : “On the other hand, Siegel’s theorem, for curves of genus  $> 1$ , is only the first step in the direction of the following statement : On every curve of genus  $> 1$ , there are only finitely many rational points.”

Subsequently, Weil explicitly denigrated Mordell’s contribution. In his *Two lectures on number theory, past and present* [We 74a], he wrote : “For instance, the so-called Mordell conjecture on Diophantine equations says that a curve of genus at least two with rational coefficients has at most finitely many rational points.” Why “so-called”? Weil goes on : “It would be nice if this were so, and I would rather bet for it than against. But it is no more than wishful thinking because there is not a shred of evidence for it, and also none against.” In his *Collected Papers Vol III*, p. 454, he goes one better (my translation)<sup>5</sup> : “We are less advanced with respect to ‘Mordell’s conjecture’. This is a question which an arithmetician can hardly fail to raise ; in any case, one sees no serious reason to bet for or against it.”

I have several objections to Weil’s tendentious evaluation (“quintessential Weil”).

First, Weil puts Mordell’s conjecture in quotes, as if there was some question about Mordell’s famous insight. Second, concerning a “question which an arithmetician can hardly fail to raise”, I would ask when ? It is quite a different matter to raise the question in 1921, as did Mordell, or decades later, especially following Mordell’s insight. Furthermore, Weil here goes against the evaluations which he himself made in the two papers mentioned above, dating back to 1928 and 1936. Weil at the end of his 1928 thesis even proposed a generalization of Mordell’s conjecture as follows (my translation)<sup>6</sup> : “The most important problem of the theory is no doubt precisely to know if, among all virtual systems of degree  $\leq p - 1$  arising from a finite set of generators, there are infinitely many effective ones ; if this question has a negative answer, it would follow in particular that on a curve of genus  $p > 1$  there is only a finite number of rational points, whatever be the domain of rationality (for example, Fermat’s equation  $x^n + y^n = z^n$ , would have only a finite number of solutions for each value of  $n > 2$ .)” However, when I learned abelian varieties (from Weil’s books and his course in Chicago in 1954), I observed that Weil’s proposed generalization for effective  $(p - 1)$ -cycles on curves was false because the

<sup>4</sup> « cette conjecture, déjà énoncée par Mordell (loc. cit. note 4) semble confirmée en quelque mesure par un important résultat démontré récemment... »

<sup>5</sup> « Nous sommes moins avancés à l’égard de la conjecture de Mordell. Il s’agit là d’une question qu’un arithméticien ne peut guère manquer de se poser ; on n’aperçoit d’ailleurs aucun motif sérieux de parier pour ou contre. »

<sup>6</sup> « le problème le plus important de la théorie est sans doute précisément de savoir si, parmi tous les systèmes virtuels de degré  $\leq p - 1$  qui se déduisent d’une base finie, il peut s’en trouver une infinité d’effectifs ; si la question devait être résolue par la négative, il s’ensuivrait en particulier que sur une courbe de genre  $p > 1$  il n’y a qu’un nombre fini de points rationnels quel que soit le domaine de rationalité (par exemple l’équation de Fermat,  $x^n + y^n = z^n$ , n’aurait qu’un nombre fini de solutions pour chaque valeur de  $n > 2$ ). »

theta divisor could contain an abelian subvariety of dimension  $\geq 1$ . I then made my general conjecture that a subvariety of an abelian variety is Mordellian if (and only if) it does not contain the translation of a non-trivial abelian subvariety. My conjecture was proved by Faltings three decades later.<sup>7</sup>

Third, concerning Weil's statements in 1974 and 1979 that there is no "Shred of evidence" or "motif sérieux" [serious reason] for Mordell's conjecture, they not only went against his own evaluations in earlier decades, and similar evaluations by others since<sup>8</sup>, but they were made after Manin proved the function field analogue in 1963; after Grauert gave his other proof in 1965; after Parshin gave his other proof in 1968, while indicating that Mordell's conjecture follows from Shafarevich's conjecture (which Shafarevich himself had proved for curves of genus 1); at the same time that Arakelov theory was being developed and that Zarhin was working actively on the net of conjectures in those directions (Shafarevich conjecture, Tate conjecture, isogeny conjecture, etc.); and within four years of Faltings' proof.

***On the Shimura-Taniyama conjecture***

I gave a systematic account of this item in my *Notices Forum* article [La 95b], which I now urge readers to look at again in the present broader context. Weil's first reaction when Shimura told him the conjecture was

to make the comment : "I don't see any reason against it, since one and the other of these sets are denumerable, but I don't see any reason either for this hypothesis." [We 79], III, p. 450 When others brought out the role of Shimura and Taniyama, Weil started inveighing against conjectures, and kept it up for the next decade. In my article, I quote from a letter where Shimura writes : "For this reason, I think, he [Weil] avoided to say in a straightforward way that I started the conjecture[...] Of course Weil made a contribution to this subject on his own, but he is not responsible for the result on the zeta functions of modular elliptic curves, nor for the basic idea that such curves will exhaust all elliptic curves over  $\mathcal{Q}$ ." If Weil had started his 1967 paper with a couple of sentences stating that Shimura told him this basic idea, and that the paper was the result of his thinking about the idea, then there would be evidence in this instance for Knapp's purported description of Weil's motivation. As it is, Weil's suppression of Shimura's role in making the conjecture was evidence of something opposite to viewing mathematics as a "collective enterprise". It is unfortunate that the accumulated evidence was not taken into account by some people to follow Weil's own conclusion in his letter to me, already quoted in the introduction : "Concerning the controversy which you have found fit to raise, Shimura's letters seem to me to put an end to it, once and for all."

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**Références**

<sup>7</sup> In his article [Fa 91] p. 549, Faltings states that the conjecture was made "by A. Weil and also by S. Lang"; later in [Fa 94] p. 175, it's "by A. Weil (as well as apparently independently by S. Lang)." I objected to Faltings about the attribution to Weil, which is incorrect. Cf. the quotes from Weil I give in the above text.

<sup>8</sup> For instance, Parshin in 1968 [Pa 68] wrote : "Finally when  $g > 1$ , numerous examples provide a basis for Mordell's conjecture that in this case  $X(\mathcal{Q})$  is always finite. The one general result in line with this conjecture is the proof by Siegel that the number of integral points (*i.e.* points whose affine coordinates belong to the ring  $\mathcal{Z}$  of integers) is finite."

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## Quelques remarques sur l'interview de Jean-Yves MÉRINDOL

Jean-Michel Lemaire

(ancien Directeur scientifique adjoint au département SPM du CNRS)

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Permettez-moi d'abord de préciser que je ne m'exprime qu'à titre personnel, sur la base évidemment de mon expérience passée, mais en aucune façon au nom du CNRS ou de son département des Sciences Physiques et Mathématiques. Dans sa conclusion, Jean-Yves MÉRINDOL estime que les mathématiciens ne manifestent pas de style particulier dans l'exercice des fonctions de Président d'Université. Malgré la haute opinion que je m'étais formée de Jean-Yves au cours des contacts que nous avons eu dans l'exercice de nos fonctions respectives, je ne puis que déplorer qu'il n'applique pas à ses déclarations les exigences de rigueur qu'on pourrait attendre d'un mathématicien, et d'ailleurs de n'importe quel scientifique. Notons en passant que le fait que certains géophysiciens aient manifesté cette absence de rigueur à une toute autre échelle ne permet pas pour autant d'inférer quoique ce soit de général sur le comportement de nos collègues de cette discipline placés en situation de responsabilité : il existe d'ailleurs des contre-exemples.

Venons-en aux faits :

1) "le CNRS n'est ni le seul ni le plus riche organisme de recherche. Il y en a d'autres très puissants (...) qui développent des compétences importantes, y compris en mathématiques."

Que veut dire "riche" dans ce contexte ? S'agissant des mathématiques, le CNRS est riche de ses 350 chercheurs et 160 ingénieurs et techniciens, tous affectés dans des laboratoires universitaires. Même avec une définition très large de

la notion de mathématicien(ne), on n'arrive pas à ce chiffre dans tous les autres organismes réunis<sup>1</sup>, et surtout, à l'exception de quelques chercheurs de l'INRIA, ces personnes exercent leurs fonctions dans des laboratoires propres à leurs organismes. Quel est donc le sens de cette remarque par rapport à la question posée ? MÉRINDOL suggérerait-il que le président de la SMF devrait s'adresser à ces organismes pour soutenir le CIRM par exemple ? Si estimables qu'ils soient, je doute fort qu'ils soient enclins à mettre à sa disposition un bâtiment de 30 chambres et un quart de million d'euros de subvention annuelle.

2) Le CNET : que la qualité de la recherche qui s'y faisait fût excellente, et qu'une partie de cette recherche ait mis en œuvre des mathématiques sophistiquées, nul n'en doute. Le CNRS a d'ailleurs su mobiliser plusieurs dizaines de postes - à un moment de forte pression budgétaire - pour accueillir des chercheurs du CNET lors de son démantèlement. Mais sur quelle base objective peut-on affirmer que la qualité de la recherche du CNET était bien supérieure à celle du CNRS ? L'affirmation est gratuite, le dénigrement ne l'est sans doute pas.

3) Jean-Yves MÉRINDOL expose une vision partielle - et partielle - du rôle historique du CNRS vis-à-vis des mathématiques. Pendant la récession des années 75-85, c'est le recrutement au CNRS qui a permis à l'école française de mathématiques de se maintenir au niveau qui est le sien. Quelques années après, la décision d'affecter les deux tiers des recrutements

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<sup>1</sup> L'annuaire SMF-SMAI 2000 recense une quarantaine d'adhérents à l'INRIA et autant au CEA, une douzaine à l'ONERA, 4 à l'INRA (unité de biométrie) et au CNES, aucun à l'INSERM, mais 33 à la DER/EDF !

en province a été une option stratégique forte de l'ensemble de l'organisme, qui a été un facteur important du développement de pôles d'excellence hors Île-de-France. Et en ce qui concerne les mathématiques, l'effort d'association d'équipes nouvelles n'a jamais cessé jusqu'à aujourd'hui : ainsi, pour se limiter à la "petite couronne" parisienne, il y a à présent des UMR à Marne-la-Vallée, Cergy, Versailles et Evry, alors qu'il n'était nullement évident pour ces centres de définir et de mettre en œuvre un projet scientifique original, si près des grands centres de Jussieu et d'Orsay qui en avaient formé la plupart des acteurs, et où ces derniers étaient fortement tentés de conserver leur activité de recherche...et leur bureau ! En ce qui concerne les chercheurs du CNRS, la direction du SPM n'a pas ménagé ses efforts pour inciter les chercheurs parisiens à aller animer ces équipes nouvelles. L'expérience m'a cependant montré que les pressions amicales sont plus efficaces que les mesures administratives...

4) En matière de prospective, Jean-Yves Mérimondol a raison d'affirmer que le problème démographique "doit être abordé globalement par les universités et les organismes de recherche". Ce devrait être le "noyau dur" des contrats entre le CNRS et les Universités, et chacun sait qu'on en est encore très loin. Le CNRS y a sa part de responsabilité, certes, mais ses partenaires (Strasbourg est-elle vraiment une exception ?) n'offrent guère de répondant à ma connaissance : "les commissions de spécialistes sont souveraines...", dit-on, et on en reste là. On aurait aimé en savoir un peu plus sur ses idées pour "aborder la

relation avec le CNRS sur un mode différent" en mathématiques. S'agissant de la mobilité CNRS-Université, rappelons tout de même que pour la seule année 2000, 14 CR1 sont devenus professeurs...

5) Enfin, Jean-Yves Mérimondol soulève la question de la politique du CNRS en région et de son expression. Il a raison d'en souligner la faiblesse, et l'ambiguïté du rôle du délégué régional. Il s'agit d'un problème de gouvernance générale de l'organisme, mais dont la solution ne passe certainement pas par l'attribution d'un pouvoir d'arbitrage *scientifique* au délégué régional : s'il est parfois vrai, comme me l'a souvent dit Jean-Yves, que nos partenaires ont en face d'eux sept (et maintenant huit !) CNRS et pas un seul, il ne faut pas pour autant risquer d'en avoir vingt. J'espère vivement que la réflexion menée par la Présidence du CNRS débouchera sur une capacité accrue de l'organisme à exprimer une politique lisible dans toutes ses dimensions, interdisciplinaire, régionale, mais aussi européenne. Cela dit, pour affirmer que les universités "sont dans une situation plus simple et peuvent afficher des priorités scientifiques", il faut - malheureusement aussi - une bonne dose...disons d'angélisme.

En conclusion, j'aurais préféré lire, sous la plume d'un président de la valeur de Jean-Yves, qu'il est grand temps que les Universités et le CNRS se regardent enfin comme des partenaires, dont chacun cherche à tirer parti des forces de l'autre et non à en dénigrer les faiblesses. Dans la compétition internationale, nous gagnerons ou nous perdrons ensemble.