"LEIBNIZ'S LAWS OF CONTINUITY AND HOMOGENEITY"
(manuscript reference: MONTHLY-D-11-00666).

The issue of inconsistency of the Leibnizian calculus (and the possible means of remedy it) was the subject of countless works in history and epistemology, with four conclusions almost unanimous among commentators. I summarize schematically. On the one
hand this calculation, although it is very effective and fruitful, is by no means consistent and
contains insoluble paradoxes within a mathematical framework, in particular regarding the
very definition of infinitesimals by Leibniz ; as far as I know, this essential conclusion was
highlighted by all mathematicians since the time of Leibniz. Second, this calculation was from the seventeenth century the source of controversy of great interest. During the eighteenth
century, for example, the most important mathematicians (e.g. Lagrange) have continued to
point out inconsistency and attempted to remedy. Third, the works of Cauchy and Weierstrass
in the nineteenth century provided an analytical framework mathematically correct to the differential calculus (that one used by today mathematicians) without using infinitesimals,
however.
Finally in the twentieth century, Abraham Robinson elaborated an outstanding theory, namely NonStandard Analysis (NSA), in order to provide a solution to the inconsistency of
the Leibnizian constructions by giving a modern new definition of infinitesimals[1]. NSA was
followed and in some sense extended by Internal Set Theory (IST), a creation of Edward

Nelson[2]. NSA was created in the 1960s within the frame of model theory, that is to say, in essence, as a derived product, on the one hand of the Lowenheim-Skolem's theorem, and on the other hand, of the methods of ultra-products, following the work of Lös. The aim of Robinson was to build by "enlargement" an extended version of IR (denoted *IR), and therefore of the real Analysis[3]. Such an extension, since it proposed legitimate infinitesimals, took into account (in its own way) the hierarchical vision of the world of Leibniz, with new methods, however, inconceivable by Leibniz and the mathematicians of the XVIIth century.

These two theories (NSA and IST) have resulted in an abundant mathematical literature.

I repeat : since its inception in the 1675s, no serious mathematician has argued that the calculation of Leibniz was consistent. However, here is a paper submitted in 2011 to the *Monthly* which affirms that the Leibnizian calculus is indeed consistent. The argumentation of the authors is in my opinion incorrect, as I explain below ; it is likely to mislead nonspecialist readers of this part of the history of mathematics. I can therefore but disagree with the conclusions of the submitted paper.

XVIIth century

The list of references of the submitted article is disproportionately long (one page, and 23 references for an article of four pages of text!). Despite this, the works referenced hardly correspond to the data of the problem, either at the time of Leibniz, or that of Abraham Robinson, as we shall see below.

These bibliographical references seem fully inappropriate as to the seventeenth century and the reception of Leibniz's work. First, the authors work hard (page 1) so as to

[1] See A. Robinson's fundamental treatise: *Non Standard Analysis*, Princeton University Press. 1966.

[2] Nelson's seminal paper, "Internal set theory : a new approach to Non Standard Analysis" was published in *Bulletin of the AMS* 83, Number 6, November 1977. It has just been reprinted in its entirety in a very recent issue of the same *Bulletin*, Volume 48, October 2011.

[3] Note that Robinson did not intend, however, to develop an extension of the entire ZFC, as Nelson did later.

'criticize the critics' of Berkeley on the calculus, which he developed in his book *The Analyst* in 1734. It is well known today that Berkeley was a somewhat strange philosopher (and not a mathematician), who had gone to war against all infinitary calculations, Leibniz's differential calculus as well as Newton's calculus of fluxions, on behalf of theological prejudice against freethinking. *The Analyst* is just like a work of propaganda, in which the author addresses an "infidel mathematician". D.M Jesseph's interesting paper ([4]) is very clear on this point. He analyzes in this way "the purpose of *The Analyst*" (§2 (p; 123)

«Theologically, *The Analyst* was part of Berkeley's battle against free thinking, and his principal argument intends to show that freethinkers who deride revealed religion for its mysteries cannot consistently accept the calculus, since it contains suppositions at least as extravagant and incomprehensible as anything revealed in religion (...) the full title of The Analyst, which characterizes the work as *a Discourse addressed to an infidel mathematician ; wherein it is examined whether the object, principles, and inferences of the modern analysis are more distinctly conceived, or more evidently deduced, than religious mysteries and points of faith.*»

Berkeley's work has been the subject of numerous studies[5]. They usually describe a biased construction. It would have been, in my opinion, much more interesting for the readers

of the *Monthly* that the authors detail the controversy with mathematicians of the time about the foundation of calculus. There was so much controversy, between Leibniz and genuine mathematicians ; they were often friends or followers of Leibniz, as John Bernoulli[6], Varignon, or the Marquis de l'Hospital. But one has to highlight the critics, more bitter but still relevant, of Niewientijt, or of Fatio de Dhuillier, because their objections were also undeniably consistent in the context of the time. The arguments in response from Leibniz to Niewientijt (GM V, 320-328) and Fatio de Dhuillier (GM V, 340-349), mainly based on analogies, seem largely inadequate on a theoretical level within the contemporary context. A statement of Leibniz such as : "A quantity which is infinitely small with respect to another quantity can be neglected if compared with that quantity" (cf. page 3 of the submitted article), which is at the basis of the law of homogeneity could only take its meaning within the framework of a new system which should be mathematically consistent. Yet, there was no ontological frame in the seventeenth century (no more than in the eighteenth) in the name of which it could be considered that with $dx \neq 0$, one could write $x = x + dx$ (this was one of the Niewientijt's objections). This was only possible in the twentieth century with ANS, but the justification for it, this time consistent, is not included in the Leibniz's system.
One has also to point out that Leibniz himself has changed his position on the definition of dx. He hesitates constantly between two definitions, depending on the

requirements of the moment. This swing is quite understandable, since Leibniz could not
provide a real basis of his theory. Cajori notes :
«In the interpretation of *dx* and *dy*, Leibniz vacillated. At one time they appear in his writings as finite lines ; then they are called infinitely small quantities, and again, *quantitates inassignabiles*, which spring from *quantitates assignabiles* by the law of continuity[7].»

Wurtz[8] distinguishes two periods for the vacillation, namely 1695 and 1698:

[4] D. M Jesseph in: "George Berkeley, The Analyst (1734)", in : *Landmark Writings in Western Mathematics 1640-1940* (I. Grattan-Guinness ed.). Elsevier. Amsterdam, Boston, etc. 2005, pp. 121- 130

[5] See for instance F. Cajori, *A History of Mathematics*, pp. 219. See other reponses to Berkeley in the XVIIIth century (Jurin, Robins, MacLaurin, Paman) in : Jesseph, op. cit., pp. 129-130.

[6] There are numerous texts on the controversy about infinitesimals. Cf. for instance the correspondence between Leibniz and John Bernoulli in GM III/2 , p; 524, 529, 536.

[7] F. Cajori, op. cit., pp. 218

[8] J. P. Wurtz : "La naissance du calcul différentiel et le problème du statut des infiniment petits", in: *La mathématique non-standard*, Paris 1989, pp. 14- 41.

«On voit donc que Leibniz a eu clairement conscience à la fois de ce qui permettait et justifiait son calcul et, sinon de la fragilité, du moins de l'absence de rigueur absolue à son fondement à tel point que Fontenelle (...) Nous constatons, quant à nous, que, dans sa quête d'un statut précis des infiniment petits, Leibniz a tour à tour, dans ses deux positions extrêmes de 1695 et de 1698, fait un premier pas dans les deux voies sur lesquelles le XIX° siècle d'abord, le XX° ensuite se sont engagés pour fonder son nouveau calcul en toute rigueur (page 40).»

Note also that Leibniz's arguments on the calculus may be significantly different, depending on whether it is for a mathematician (e.g Varignon) or for a philosopher (e.g Wolff).

XVIIIth century

Some of the greatest mathematicians of the eighteenth century, D'Alembert and

Lagrange in the first, never ceased to worry about the inconsistency and contradictions of Leibniz's system, and try to repair it. The Academy of Sciences in Berlin went so far as to offer a prize for a mathematician who published a rigorous theory of infinitesimals[9]. Lagrange decided to compete. In the words of Lagrange[10] :

«On sait que la Haute Géométrie fait un usage continuel des infiniment grands et des infiniment petits. Cependant les géomètres et même les analystes anciens, ont évité soigneusement tout ce qui approche de l'infini et de grands analystes modernes avouent que les termes sont contradictoires. L'Académie souhaite donc qu'on explique comment on a déduit tant de théorèmes vrais d'une supposition contradictoire, et qu'on indique un principe sûr, clair, en un mot vraiment mathématique, propre à être substitué à l'infini.»

XXth century

Thus there were three main periods, namely: the time of the reception of the work of Leibniz in the seventeenth century, the time of Cauchy and Weierstrass (of which the authors do not say a word), and finally the period of the twentieth century with ***attempts to make rigorous a calculation, which was not previously***. About the inconsistency, again, all comments coincide, whether they come from mathematicians (e.g. A. Robinson[11], N. Bourbaki[12]) or from historians of mathematics (e.g. M. Kline[13], F. Cajori[14], I. Lakatos[15], J. Dauben (op. cit.)). I cannot list all the authors. Lakatos asks for instance: "How to evaluate inconsistent theories, such as the calculus of Leibniz and Cauchy (...) the Continuum?" (op. cit., pp. 59). A. Robinson[16] states :

[9] In this section, I often relied on J.W Dauben's article : "Abraham Robinson : les infinitésimaux, l'Analyse Non

Standard et les fondements des mathématiques", in: *La mathématique non-standard*, op. cit., pp; 157-184. Here,
pp. 162.

[10] J. L Lagrange : "Prix proposés par l'Académie Royale des Sciences et Belles Lettres pour l'année 1786",
Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres, pp. 12-14.

[11] *Non-standard Analysis*, op. cit., pp. 265-266.

[12] Bourkaki is admiring quite rightly the mathematical work of Leibniz. With regards to its consistency, he notes
however: "En particulier, il faut bien reconnaître que la notion leibnizienne de différentielle n'a à vrai dire aucun
sens", N. Bourbaki in: *Éléments d'histoire des mathématiques*. Paris. 1969, pp. 237.

[13] Morris Kline in : *Mathematical Thought from Ancient to Modern Times*. Oxford University Press. New York.
1972, pp. 384-385.

[14] *A History of Mathematics,* op. cit.

[15] I. lakatos, in: "The Significance of Nonstandard Analysis for the History and Philosophy of Mathematics", in:
Mathematics, Science and Epistemology, Philosophical papers (Worral and Currie eds.), Vol. 2, Cambridge
University Press, 1978, pp. 59

[16] "On the theory of Normal Families", = *Works*, vol. 2, pp. 62

«For about one hundred and fifty years after its inception in
the seventeenth century, mathematical Analysis developed vigorously on
inadequate foundations. »

Also Cajori[17] :

The explanations of the fundamental principles of the calculus, as given by Newton and Leibniz, lacked clearness and rigor. For that reason it met oppositions from several quarters. In 1694 *Bernhard Nieuwentijt* (1654-1678) of Holland denied the existence of differentials of higher orders and objected to the practice of neglecting infinitely small quantities. These objections Leibniz was not able to meet satisfactorily.

In the words of Morris Kline, in his *Mathematical Thought from Ancient to Modern Times* (section "The Soundness of the Calculus") (op. cit., pp. 384-385):

«Leibniz was not too concerned about the lack of rigor in the calculus. In response to criticism of his ideas, Leibniz made various, unsatisfactory replies (...) As to the ultimate meanings of *dy*, *dx*, and *dy/dx*, Leibniz remained vague (...) A flurry of attacks and rebuttals was initiated in the books of 1694 and 1695 by the Dutch physician and geometer Bernhard Nieuwentijt (...) Leibniz, in a draft of a reply to Nieuwentijt (...) gives various answers (...)
Leibniz's argument thus far was that his calculus used only ordinary mathematical concepts. But since he could not satisfy his critics, he enunciated a philosophical principle known as the law of continuity (...)»

In the twentieth century, there were therefore methods to try to resume the status of infinitesimals, foremost among which NonStandard Analysis of Abraham Robinson, then IST of Edward Nelson. The authors of the submitted article make a very brief allusion to Robinson (page 1), but do not try any mathematical analysis nor explanation of his work. One wonders why they mention only one text (from Edwin Hewitt), certainly interesting, but much earlier (1947) and which, above all, do not deal with the status of infinitesimals in Leibniz, unlike Robinson. The reader could therefore ignore all of the very important work of Robinson and his school (in the U.S. and abroad) to ground the infinitesimals within a new frame. He could also ignore that Robinson was also very interested in questions of history and philosophy of mathematics, on issues that are raised specifically by the infinitely small elements, from Leibniz to NSA. See for instance his papers, *The metaphysics of the Calculus* (*Collected Works*, vol. II, pp. 537-555)[18], *Concerning Progress in the Philosophy of Mathematics* (*idem*, pp. 556-567) *Some Thoughts on the History of Mathematics* (*idem*, pp. 568-573). On the other hand, however, it must be pointed out that Robinson's method, although it is perfectly consistent, requires a significant background on ultra-products, and is, for example, not so easy to explain to post graduate students.

Anachronisms.

I now turn to the anachronism which regrettably runs throughout the submitted article.

I resume the phrase (page 4) :

«While consistent, Leibniz' system unquestionably relied on heuristic principles such as the laws of continuity and homogeneity, and thus fell short of a standard of rigor if measured by today's criteria» (page 4),

which contains a contradiction in my opinion. How the system could be both "consistent" and "short of a standard of rigor if measured by today's criteria" ? The situation is indeed this:

17 Op. cit., pp. 218.

18 This paper is followed by the text of a discussion between A. Robinson, P. Geach, H. Freudenthal, A. Heyting, Y. Bar-Hillel, and M. Bunge.

admittedly, what Leibniz called his law of continuity and his law of homogeneity, are heuristic principles which may be later very fruitful, but they will remain metaphysical and not mathematical ; in other words, the Leibnizian principle of continuity, for example, although indisputably at the origin of the modern concept of continuity, is stated by Leibniz in metaphysical terms[19] ; moreover it is not available everywhere and in all situations, contrary to what Leibniz wished and claimed. Since, for him, his principle of continuity was the very foundation of his Calculus... [20].

The real situation is rather this: although it was entirely inconsistent, the calculus of Leibniz found, in the seventeenth century, an extraordinary number of verifications in practice through new, full of interest, and diverse, applications (e.g. calculations of areas, rectifications, osculating circles ...)[21]. Leibniz, however, always appeared unable to rigorously justify the foundations. But he was a rigorous and honest man, who could not be satisfied with a simplistic argument favoring utility against ontology. He therefore turned to the metaphysics and then devoted a part of his philosophy to develop a system of the world (i.e. a philosophical structure) that can account for this discrepancy. The principle of continuity, in particular, is undoubtedly a genuine philosophical principle[22].

On the other hand, modern concepts, such as ultra-products, of which IST and ANS make use, are in no way contained in the work of Leibniz, even if they were raised by it (e.g., issues of objectification of the infinitely small elements, and of non-Archimedean structures).

The authors are constantly engaged in an anachronistic confusion between Leibniz methods *stricto sensu*, and modern methods. I repeat : these methods (IST and ANS) were developed precisely because of the inconsistency of the Leibnizian system; however, they are not directly connected, as methods, with those from Leibniz (no more than from Newton). It's an anachronism to judge and interpret the processes of the seventeenth century through the eyes of modern procedures. I agree on this point with Henk Bos who writes[23] :

«It is indeed an interesting feature that, contrary to what was thought for a very long time, the Leibnizian use of infinitesimals can be incorporated (after some reinterpretations and readjustments) in a theory which is acceptable by present-day standards of mathematical rigor.

(...) I do not think that the appraisal of a mathematical theory, such as Leibniz's calculus, should be influenced by the fact that two or three quarter centuries later the theory is "vindicated" in the sense that it is shown that the theory can be incorporated in a theory which is acceptable by present-day mathematical standards.»

Conclusions : The submitted paper lacks first of a serious historical account: the controversy at the time of Leibniz (Niewientijt, for example, is not cited), then the position of the mathematicians of the eighteenth century, and the Cauchy-Weierstrass' solution in the

nineteenth century. Moreover, it does not describe the goals, nor the means of the ANS theory
in the twentieth century, which are a fundamental base to understanding the problem of the
Leibnizian infinitely small elements by twentieth century mathematicians. The paper is also

19 On Leibniz's conception of his principle of continuity, and the historical modern developments of the concept
of continuity, see M. Serfati, 'The principle of continuity and the 'paradox' of Leibnizian mathematics', in *The*
Practice of Reason: Leibniz and his Controversies. (M. Dascal (ed.)) Amsterdam: Benjamins (Controversies,
volume 7), 2010. 1-32. For instance, «The principle of continuity and some of its consequences in current mathematics : The schema of 'proof by continuity'», pp. 23-25.

20 Cf. "Variations about the principle", in M. Serfati: op. cit., pp. 17-20.

21 See for instance "Calcul infinitésimal" in Bourbaki : op. cit. pp. 207.

22 See the final lines of the above quotation of M. Kline. Also M. Serfati, "The principle of continuity as a solution to the paradox", in: op. cit., pp. 10-12. Also G. Granger in: "Philosophie et mathématiques leibniziennes", in : *Revue de Métaphysique et de morale* **1** (1981), 1-37.

23 H.J.M. Bos :"Differentials, higher-order differentials and the derivative in the Leibnizian calculus", *Archive*
for history of exact sciences, Vol. 14, 1974, pp. 82. This article by Bos is quoted by the authors (N°1), although
its conclusions are in my opinion contrary to that of the submitted article.

anachronistic (see previous section). If it tries an explanation of some principles, called "heuristics" (continuity and homogeneity), it is unfortunately fully wrong when it claims that
these principles are a consistent basis for the work of Leibniz. This can lead to misconceptions among non-specialist readers. I repeat: there is a large literature on the subject. I resume here a conclusion of Dauben (op. cit., pp. 166):

«Leibniz, par exemple, n'a jamais établi des fondements consistants pour
son calcul infinitésimal, ce qui a exposé son oeuvre aux critiques mordantes de
Niewientijt, par exemple. Au cours du XVIIIème siècle, les fondements
contestables (ou plutôt, le manque de fondements) du calcul infinitésimal ne

cessait de tracasser les mathématiciens, jusqu'à ce que la méthode epsilon-delta
de Cauchy et la rigueur "arithmétique" de Weierstrass aient reconstruit
l'analyse en termes finis acceptables.»
Let me repeat as a final conclusion : as far as I know, since the XVIIth century, no serious mathematician has claimed that the system of Leibniz was consistent. And here's a
text in 2011 which claims it's true ... Will there soon be an article claiming the possibility of
squaring the circle?
The conclusions of the submitted article are clear but, in my opinion, completely false, as I have shown. I do not thus see, regrettably, a means which would allow to improve this
text. I regret I must disadvise the publication of the submitted article in the *Monthly*.