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The Metaphysics of the Calculus: A Foundational Debate in the Paris Academy of Sciences, 1700–1706

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The differential calculus faced a strong opposition within the Academy of Sciences of Paris at the very beginning of the 18th century. The opposition came from a group of mathematicians who criticized the new analysis both for what they considered to be its lack of rigor and for the results that it produced. A bitter debate raged for about 6 years until the proponents of the new calculus prevailed. © 1989 Academic Press, Inc.

Zu Anfang des 18. Jahrhunderts war die Differentialrechnung innerhalb der Akademie der Wissenschaften zu Paris scharfer Kritik ausgesetzt. Eine Gruppe von Mathematikern kritisierte die neue Rechenart sowohl wegen ihres angeblichen Mangels an Präzision als auch wegen der Resultate, die sie hervorbrachte. Eine hitzige Debatte zog sich rund sechs Jahre hin, bis die Fürsprecher der Differentialrechnung die Oberhand gewannen. © 1989 Academic Press, Inc.

Au début du XVIII$^{\text{ième}}$ siècle, une forte opposition au calcul différentiel s'est manifestée à l'intérieure même de l'Académie Royale des Sciences de Paris. Un groupe de mathématiciens s'opposait à la fois au manque de rigueur et aux résultats de la nouvelle analyse. Un débat passionné déchira l'Académie pendant environ six ans pour se conclure en faveur des partisans du nouveau calcul. © 1989 Academic Press, Inc.

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1. INTRODUCTION

Abraham Robinson, the famous inventor of nonstandard analysis, once asserted that "from the XVII to the XIX century the history of the Philosophy of Mathematics is largely identical with the history of the foundations of the calculus" [Robinson 1966, 280].

Unfortunately, the subject of the foundation of the calculus—especially for the beginning of the 18th century—has been neglected. For example, there is no complete history of the long debate among Leibniz, Hermann, and Nieuwentijt [1]. Even less attention has been given, especially in English-speaking countries, to the debate that occupied the Parisian Academy of Sciences from 1700 to 1706. Previous treatments [Costabel 1965; Fleckenstein 1948; Montucla 1802; Sergescu 1938, 1942; Blay 1986] have in general emphasized specific aspects of the debate and, with the exception of [Blay 1986], have ignored the foundational aspects of the debate [2].

The history of the last-mentioned debate is the subject of my paper. This de-

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bate—which involved savants like Leibniz, Fontenelle, Malebranche, Varignon, Johann (I) Bernoulli, Rolle, l'Hôpital, and a number of other less-known academicians—is important for several reasons. I mention two of these reasons below:

(a) the end of the debate among the academicians decreed the complete victory of the infinitesimal calculus in France;

(b) the attacks by Rolle against the "metaphysics of the calculus" had both philosophical and mathematical significance.

2. THE SPREAD OF THE DIFFERENTIAL CALCULUS IN FRANCE

In 1696 the Marquis de l'Hôpital published the first textbook on the differential calculus on the continent: *Analyse des infiniment petits pour l'intelligence des lignes courbes*. In the eulogy of l'Hôpital read in 1704 Fontenelle asserted that until the publication of the *Analyse des infiniment petits* "la Géométrie des Infiniment petits n'étoit encore qu'une espece de Mystere, &, pour ainsi-dire, une Science Cabalistique renfermée entre cinq, ou six personnes" [Fontenelle 1704, 131] [3]. Fontenelle was certainly thinking of Leibniz, Newton, the Bernoulli brothers, Varignon, and l'Hôpital.

In 1684 Leibniz published his first memoir on the new calculus, the "Nova methodus . . ." in the *Acta Eruditorum*. By 1687 the Bernoulli brothers were already in full command of the differential calculus and major parts of the integral calculus. (The geometric version of the fluxional calculus, i.e., the method of prime and last ratios, made its appearance also in 1687 in Newton's *Principia*.) Through the teaching of Johann (I) Bernoulli a group of French mathematicians, centered around the charismatic figure Malebranche, came in contact with the new calculus around 1690. This group included l'Hôpital, Varignon, Montmort, Carré, Reyneau, and other less famous mathematicians. In the winter of 1691–1692 Johann (I) Bernoulli initiated the Marquis de l'Hôpital in the most remote secrets of the differential and integral calculus. Bernoulli's lectures were instrumental in later enabling l'Hôpital to write the *Analyse des infiniment petits*. This textbook had a remarkable success and for quite a long time represented the only accessible road to the differential calculus.

Various French scholars, Robinet [1960] and Costabel (in [Malebranche 1958–1968 XVII-2]), have documented the intense activity and collective effort of the group led by Malebranche to come to a full understanding of the new infinitesimal techniques. Malebranche himself had studied the calculus deeply and was, in effect, the main patron of the "infinitesimalist revolution" in the Academy of Sciences. When the academy was renewed in 1699 a number of new places were opened and Malebranche was elected an honorary member. In the following few years the academy came to include a compact group of "infinitesimalists," among them Carré, Saurin, Guisnée, and Montmort. This group was under the technical guidance of l'Hôpital and Varignon, who were older academicians [4].

The presence within the academy of a group of mathematicians (including Rolle, Ph. de la Hire, and Galloys), who were decidedly adverse to the new calculus, created an explosive situation [5]. From 1700 to 1706 the academy was divided over the admissibility of the new techniques: on one side stood the infinitesimalist group characterized by its total adherence to the new Leibnizian calculus in the version codified by l'Hôpital and in general by a commitment to the existence of infinitesimal quantities; on the other side was the finitist faction characterized by a refusal to give a rigorous status to infinitesimal considerations and by a general adherence to classical techniques.

Since the debate centered on the logical admissibility of the differential calculus given in l'Hopital's textbook, it is important to sketch the structure of the book.

3. THE STRUCTURE OF THE *ANALYSE DES INFINIMENT PETITS*

In the first two definitions l'Hôpital characterized the basic primitives of the theory:

> DÉFINITION I. On appelle quantités *variables* celles qui augmentent ou diminuent continuellement; & au contraire quantités *constantes* celles qui demeurent les mêmes pendant que les autres changent.
>
> DÉFINITION II. La portion infiniment petite dont une quantité variable augmente ou diminue continuellement, en est appellée la Différence. [l'Hôpital 1696, 1–2] [6]

An an example of the first case consider $y = ax^2$. The parameter a is a constant but the coordinates x and y of the parabola are variable quantities. As an illustration of the second definition l'Hôpital gave the diagram in Fig. 1 (Figs. 1–3 are taken from [l'Hôpital 1696, 24]).

Thus, for example, Pp is the differential of AP and Rm the differential of PM and so on. Furthermore the notational convention d is introduced to denote differentials. For example, if $AP = x$ then $Pp = dx$.

Before I introduce the postulates, I think two remarks are appropriate. Note that Definition I presupposes as a primitive of the theory the notion of a continually increasing or decreasing quantity. A constant quantity is merely a specific

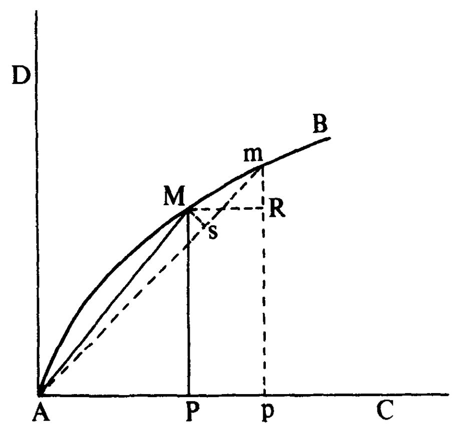

FIGURE 1

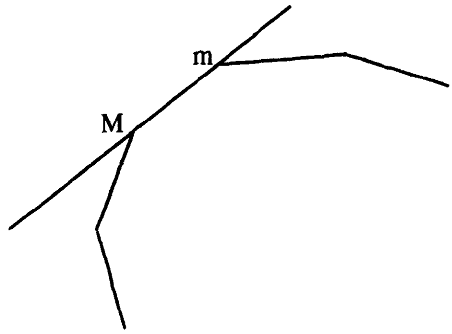

FIGURE 2

case of the latter; it is in fact a variable for which the differential is zero. Moreover, Definition II postulates dx as an infinitely small quantity. But note that l'Hôpital gave a diagram in which dx must always be represented as a finite increment. The pictorial representation left many, as we shall see, in doubt.

The postulates of the work were:

> I. DEMANDE OU SUPPOSITION. On demande qu'on puisse prendre indifféremment l'une pour l'autre deux quantités qui ne différent entr'elles que d'une quantité infiniment petite: ou (ce qui est la même chose) qu'une quantité qui n'est augmentée ou diminuée que d'une autre quantité infiniment moindre qu'elle, puisse être considérée comme demeurant la même. [l'Hôpital 1696, 2–3] [7]

Thus, Postulate I says that $x + dx = x$.

> II. DEMANDE OU SUPPOSITION. On demande qu'une ligne courbe puisse être considérée comme l'assemblage d'une infinité de lignes droites, chacune infiniment petite: ou (ce qui est la même chose) comme un poligône d'un nombre infini de côtés, chacun infiniment petit, lesquels déterminent par les angles qu'ils font entr'eux, la courbure de la ligne. [See Fig. 2.] [l'Hôpital 1696, 3] [8]

With respect to the Cartesian tradition, the two postulates of l'Hôpital represented a concept-stretching: he stretched both the notion of equality, considered now as a relation between two quantities that differ by an infinitely small quantity, and the notion of polygon, extended now to encompass curves.

The examples that follow give an idea of the formal manipulations that l'Hôpital's postulates allowed. To compute the differential of a product xy l'Hôpital wrote

$$\begin{aligned} d(xy) &= (x + dx)(y + dy) - xy \\ &= ydx + xdy + dxdy \\ &= ydx + xdy, \end{aligned}$$

since by Postulate I it follows that $dxdy$ is of a lower order of magnitude than ydx and xdy.

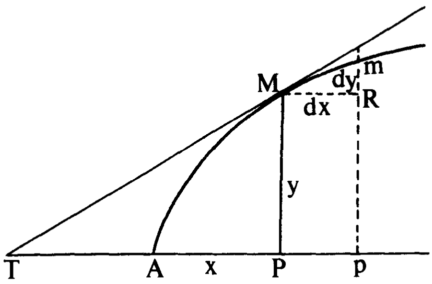

FIGURE 3

Let us see now how Postulate II is used to compute the length of the subtangent for the parabola. Let $ax = y^2$ be the equation of the parabola (Fig. 3).

The problem of constructing the tangent is equivalent to the problem of finding the subtangent TP. Given Postulate II, we can set the following proportion: $dy : dx = MP : PT$. Hence $dy : dx = y : PT$. It follows that $PT = ydx/dy$. We now use the differential equation for the parabola whose derivation makes use of Axiom 1: $adx = 2ydy$. This yields $dx = 2ydy/a$. Using the last equality and substituting in the equation for PT, we get $PT = 2y^2/a$, and since $y^2 = ax$ it follows that $PT = 2x$, which is the length of the subtangent.

It is important to note here the relationship between l'Hôpital's treatment of tangents and the approaches of Newton and Leibniz. One can think of tangents either dynamically or statically. In the first approach, stressed by Newton, the intuition is to consider the tangent at a point p as a limit of the secants $S(p, q)$ for q approaching p. In the second conception, emphasized by Leibniz, a tangent is not a limiting process but a state, i.e., a position. The tangent to a point, in this second approach, is a straight line that cuts the curve in two points infinitely near or on coincident points. It is clear that the formal means needed to express these intuitions differ. In the first case we deal with a limit of finite ratios. In the second case we need to introduce, as Leibniz did, the notion of differential increment. As the previous example makes clear, l'Hôpital's textbook presented the static conception of tangents.

4. THE BEGINNING OF THE DEBATE: ROLLE–VARIGNON, 1700–1701

The concept-stretching proposed in l'Hôpital's book was heuristically very powerful but was open to attack from the point of view of rigor. L'Hôpital stated boldly at the end of the preface to *Analyse des infiniment petits:*

> D'ailleurs les deux demandes ou suppositions que j'ai faites au commencement de ce Traité, & sur lesquelles seules il est appuyé, me paroissent si évidentes, que je ne crois pas qu'elles puissent laisser aucun doute dans l'esprit des Lecteurs attentifs. Je les aurois même pû démontrer facilement à la maniére des Anciens, si je ne me fusse proposé d'être court sur les choses qui sont déja connues, & de m'attacher principalement à celles qui sont nouvelles. [l'Hôpital 1696, xv]

Leibniz and Newton had been much more circumspect in their infinitesimal considerations. Also, clearly for strategic purposes, they avoided explicit reference to infinitesimals in their first public expositions of the subject. Leibniz's "Nova methodus . . ." contained no reference to infinitesimals. The existence of a draft of the "Nova methodus . . ." (never published by Leibniz), in which the calculus is justified by means of infinitesimal considerations, is witness to Leibniz's doubts about the rigor of the infinitesimal approach (see [Horvath 1982, 1986]). Newton, too, had been extremely careful and, in fact, in the *Principia* he used the method of prime and last ratios in which no explicit infinitesimal considerations were made. (See [Kitcher 1973] for an analysis of the strategies of justification of the calculus in Newton.)

There is, however, no doubt that infinitesimal considerations were heuristically the cutting edge of the new tool. With l'Hôpital we observe a phenomenon not uncommon in the history of mathematics. L'Hôpital tried to stretch the admissible methods, lifting the heuristics of the infinitesimal calculus to a full rigorous system involving infinitesimals. By this I mean that l'Hôpital's textbook with its axiomatic structure was trying to formalize the intuitive concept of infinitesimal quantities and the operations regulating their use. This was a doubtful move for those who were unwilling to accept infinitesimal considerations as rigorous. The most outspoken adversary of the recognition of the infinitesimal calculus as a subject in rigorous mathematics was the algebraist Michel Rolle (1652–1719), who opened his memoir "Du Nouveau Systême de l'Infini":

> On avoit toujours regardé la Géométrie comme une Science exacte, & même comme la source de l'exactitude qui est répandue dans toutes les autres parties des Mathématiques. On ne voyoit parmi ses principes que de véritables axiomes: tous les théorêmes & tous les problêmes qu'on y proposoit étoient ou solidement démontrés, ou capables d'une solide démonstration; & s'il s'y glissoit quelques propositions ou fausses ou peu certaines, aussi-tôt on les bannissoit de cette science.
>
> Mais il semble que ce caractere d'exactitude ne regne plus dans la Géométrie depuis que l'on y a mêlé le nouveau Systême des Infiniment petits. Pour moi, je ne vois pas qu'il ait rien produit pour la vérité, & il me paroît qu'il couvre souvent l'erreur. [Rolle 1703a, 312]

Although published in the proceedings of the Parisian Academy of Sciences for 1703, the memoir represented material used in the early stage of the debate, that is, during 1700–1701.

We see that Rolle formulated three distinct attacks: the calculus is not rigorous, it leads to mistakes, and it has not produced any new truth. The first two lines of attack were used by Rolle in the first part of the debate that began in July 1700 and lasted until the end of 1701. The last claim was made in a much stronger way in the second part of the debate (1702–1705). The first part of the debate consisted of a fight within the academy between Pierre Varignon and Rolle. Varignon (1654–1722), who had been working on applications of the calculus to mechanics, took the task of defending the new calculus. Although several memoirs were produced, the only published outcome of this part of the debate was the later "Du Nouveau Systême . . ." The other sources available to us (see note 2) are the correspondence between Leibniz, Johann (I) Bernoulli, and Varignon (see [Leibniz 1843–1863 III; Bernoulli 1988]), the Registres des Procès verbaux des séances de l'Académie Royale des Sciences, and a manuscript entitled "Extrait des Réponses faites par M^r. Varignon, en 1700 et 1701 aux objections que M^r. Rolle avait faites contre le calcul différentiel," which has been ascribed by Costabel [1965] to the mathematician Charles Reyneau (1656–1728). From Reyneau's summary it is clear that this first part of the debate can be further divided into two phases. The first phase was primarily of a foundational nature (i.e., concerned with the logical and metaphysical admissibility of the new calculus). The second and later phase was of a more technical nature.

The first half of Reyneau's manuscript provides valuable insight into the foundational part of the debate and gives a careful abstract of Varignon's answers to Rolle. However, as Rolle's position is only summarized in short statements, I will also use "Du Nouveau Systême . . ." as a source for Rolle's arguments.

Rolle articulated his foundational attack in three main objections [9]:

(a) the differential calculus postulates a hierarchy of arbitrarily large and arbitrarily small orders of infinities (the reader may, carefully, think of them in terms of higher-order differentials);

(b) a quantity + or − its differential is made equal to the very same quantity, which is the same as saying that the part is equal to the whole;

(c) sometimes the differentials are used as nonzero quantities and sometimes as absolute zeros.

Note that objections (a) and (b) were grounded in the denial of the existence of quantities not satisfying the Archimedean axiom and in the refusal to accept a negation of common notion 5 in Euclid and that in (c) Rolle attacked the manipulations of the infinitesimal calculus because the denotation of the differential was shifted at will during the computation. For each of the previous points I will expose Rolle's objections and Varignon's answers.

In the arguments for the first objection Rolle made two related claims. The first claim was that (despite l'Hôpital's claims) the infinitesimalists had given no proof of the existence of these various orders of infinities. What bothered Rolle here was l'Hôpital's claim, made in the preface of *Analyse des infiniment petits,* to be able to give a proof of the existence of infinitesimal quantities by the way of the ancients (i.e., the method of exhaustion). In the paradigm of the period, had the above been carried through, this would have meant a truth-status for statements about infinitesimal quantities and not just the status of an arbitrary mathematical hypothesis.

In his second claim Rolle asserted, apparently without an argument, that talking about differentials was nonsense, because it could be proved that differentials were absolute zeros. In [Rolle 1703a, 318] he provided an argument by using the equation $y^2 = ax$ as an example. Using l'Hôpital's rules he obtained the differential equality $adx = 2ydy$. Finally, under the assumption that the point $(x + dx, y + dy)$, lies on the parabola, using the definition of the parabola he obtained $ax + adx$

$= y^2 + 2ydy + dy^2$. Putting the three equations together and solving the system, using the ordinary algebraic law that subtracting equals from equals yields equals (whose validity Rolle took for granted), he arrived at $dy^2 = 0$ and hence $dy = 0$. Finally substitution of $dy = 0$ in the equation $adx = 2ydy$ yielded $dx = 0$. Therefore, Rolle concluded, infinitesimals could not be real quantities. They were in fact absolute zeros. (The example works only if we systematically anchor our interpretation to a domain that has only zero and finite quantities.)

Rolle's involved argument can be made clear as follows: under the assumption that the same algebraic manipulations rule finite quantities and infinitesimals, from the equation $x + dx = x$, one can infer $dx = 0$. Rolle concluded:

> D'abord on y voit que tous ces Infinis du premier genre tels que *dx* ou *dy*, n'ayant aucune étendue réelle, tous les Infinis des autres genres ne seroient aussi que des zeros absolus dans le calcul. Toutes ces suites infinies d'Infinis, que fournit le Systême, ne seroient que des riens qu'on suppose être infiniment compris dans d'autres riens. [Rolle 1703a, 324]

Trying to respond to Rolle's first objection, Varignon provided "proofs" of the existence of infinitesimally small quantities. A representative sample is the following "proof" reported by Reyneau. We can divide an interval of time indefinitely, and so this interval of time can be divided into parts infinitely small, which are called moments. Consider now a body A that moves with constant speed for a time T. The spaces traversed by this body are proportional to the times so that the space described in each moment is to the totality of the space S as an instant t is to T. Therefore the space described (on the line) in each instant is a differential.

Rolle's second objection expressed his refusal to identify the whole with the part as in letting $x + dx = x$. Once again Varignon's answer was an attempt to clarify the nature of infinitesimal quantities. It is interesting that Varignon appealed to Newton's *Principia* as the source for a rigorous foundation of the calculus. Throughout his answer to Rolle, Varignon quoted verbatim Newton's scholium to Lemma XI in Book I of the *Principia*. Rolle became confused, said Varignon, because he had not mastered the nature of differentials, which consisted in being variable and not fixed quantities and in decreasing continually until they reached zero, "in fluxu continuo." These quantities were considered only in the moment of their evanescence. This was after all, he continued, the same notion as Newton's "fluxiones," i.e., "incrementa vel decrementa momentanea." Being considered in the moment of their evanescence they were therefore neither something nor absolute zeros. Reyneau summarized the point in the following way:

> M[r]. V.[arignon] explique que quêtant *evanescentia divisibilia,* elles [les differentielles] sont toujours reelles et subdivisibles à l'infini jusqu'à ce qu'enfin elles ayent tout a fait cessé d'être; et c'est là le seul point où elles se changent en absolument rien. [Reyneau, 147; Bernoulli 1988, 356]

Varignon did not deny that a differential was considered nothing with respect to its integral and he offered a proof of the statement by using the techniques of the ancients (i.e., exhaustion):

> Puisque la nature des differentielles [. . .] consiste a etre infinim' petites et infinim' changeantes jusqu'à zero, à n'etre que *quantitates evanescentes, evanescentia divisibilia,* elles seront toujours plus petites que quelque grandeur donnée que ce soit. En effet quelque difference qu'on puisse assigner entre deux grandeurs qui ne diffèrent que d'une différentielle, la variabilité continuelle et indefinie de cette differentielle infinim' petite, et comme à la veille d'estre zero, permettra toujours d'y en trouver une moindre que la difference proposée. Ce qui à la maniere des Anciens prouve que non obstant leur differentielle ces deux grandeurs peuvent être prises pour égales entr'elles. [Reyneau, 147; Bernoulli 1988, 357] [10]

This justified, concluded Varignon, the manipulations used in the calculus. Insofar as they were manipulated during the computations the differentials were something on the verge of being zero and only at the end did they become zero, in the sense that they were considered in the moments of their evanescence, i.e., "non antequam evanescunt, non postea sed cum evanescunt." This also provided an answer to Rolle's third objection.

Still, it is clear that Varignon's answers were highly unsatisfactory. Thirty years later Bishop Berkeley, with much more wit than Rolle, made fun of the "ghosts of departed quantities."

It is important to note how Varignon kept quoting Newton as the source for a rigorous presentation of the calculus. He took for granted that the Leibnizian calculus and the Newtonian calculus were equivalent and that Newton's version of the calculus was rigorous. This kind of reasoning can be found later in the century, for example, in [Montucla 1802 III, 110–119]. However, none of these assumptions could be easily justified.

Rolle, on the other hand, was defending an approach in which there were no infinitesimals, and he proposed the methods of Fermat and Hudde as everything one needed to solve tangent problems and maxima and minima problems.

Rolle and Varignon were unable to find a common ground on which to resolve their difficulties. Despite the claims of his ability to prove the existence of infinitesimally small quantities by the way of the ancients (the paradigm of rigor), Varignon managed only to give us his inner perception of mathematical reality: a universe made up of variable quantities, dynamic in its essence, where fixed quantities were merely a special case of the former. To the finitist Rolle this was pure nonsense. Only by reducing differentials to zeros could he make sense of Varignon's claims. Rolle's universe was made up of finite quantities and zero; there was no place in it for amphibians.

The stress on the methods of Fermat and Hudde led Rolle to challenge his adversary on very specific mathematical examples. I will sketch the nature of Rolle's claims. Rolle claimed that the differential calculus led to mistakes. His general approach to the problem was to concoct examples of specific curves in which the individuation of maxima and minima carried through with the differential calculus was at odds with the results given by Hudde's rule [11]. Varignon painstakingly interpreted all of Rolle's alleged counterexamples and managed to show that Rolle had made several mistakes as to the nature of Hudde's rule and the applications of the differential algorithm. This explains the reason for Rolle's

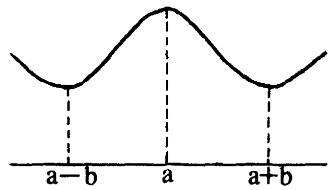

FIGURE 4

mistakes in sketching the curves he proposed. Two examples, both of which Reyneau reported, will suffice.

On March 12, 1701, Rolle proposed the curve $a^{1/3}(y - b) = (x^2 - 2ax + a^2 - b^2)^{2/3}$. He claimed that the infinitesimal method did not give all the maxima and minima provided by Hudde's rule, and sketched the curve as in Fig. 4. Hudde's rule in fact gave three ordinates that corresponded to the abscissae a, $a - b$, $a + b$, respectively. Rolle had applied the differential calculus by putting $dy = 0$, and that gave him a maximum at a, but he had not put $dx = 0$, which is also required for a complete application of the algorithm. Varignon showed that the application of the differential calculus had not been correct and then gave a correct treatment of the curve (whose graph is shown in Fig. 5).

On July 2, 1701, Rolle proposed the curve $y = 2 + \sqrt{4}x + \sqrt{4 + 2x}$. He claimed that by using the differential calculus, one got an imaginary maximum for $x = -4$, whereas by rationalizing the equation and applying Hudde's rule, one got the maximum at $x = 2$. The quartic obtained through the elimination of radicals is $y^4 - 8y^3 + 16y^2 - 12xy^2 + 48yx - 64x + 4x^2 = 0$ (Fig. 6). The sketch given by Rolle is shown in Fig. 7. Varignon showed that the point D was only the intersection of two branches of the curve and that the correct application of Hudde's rule would yield both the real value and the imaginary one [12]. Rolle had believed that Hudde's rule provided only maxima and minima without realizing that the rule also gave any point in which the curve has double roots and hence all points of intersections.

These unfortunate examples constructed by Rolle were one of the reasons for Montucla's aversion toward Rolle. It should be noted, though, that Rolle's attacks

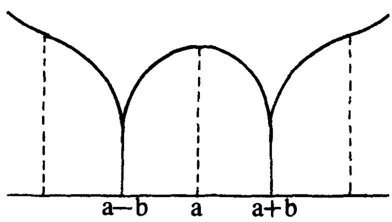

FIGURE 5

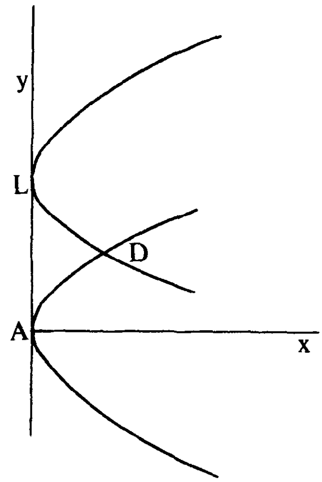

FIGURE 6

had the merit of raising the question of the criteria for individuating maxima and minima as opposed to simple points of intersection. It was only in 1706 that Guisnée proposed a criterion for distinguishing intersection points from maxima and minima [13]. Furthermore, Rolle's objections stimulated reflection on the nature of Hudde's rule, and its relationship to the methods given in the *Analyse des infiniment petits*, as is witnessed by the several letters that Leibniz and Johann (I) Bernoulli [Leibniz 1843–1863 III, 660–672] exchanged on the matter and by Guisnée's work.

Let us summarize now how the problem of the foundations was seen by Varignon and Rolle. Varignon tried to show that infinitesimals existed. This belief in the existence of infinitesimals was common to all the French infinitesimalists and they shared it with (and probably got it from) Johann (I) Bernoulli. Their position can be analyzed as an attempt to provide a semantic referent to the formal notion of differential. From this point of view Rolle and Varignon were closer than we may think. They both shared the assumption that the foundational problem consisted in making sense of a "realistic ontology." Moreover, we see that both

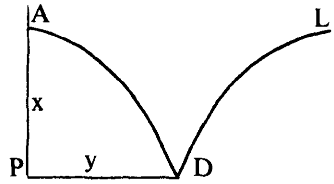

FIGURE 7

opponents agreed on the paradigm of rigor: Varignon tried in fact to "prove" his claims by using the technique of exhaustion.

Trying to provide a semantic referent for the Leibnizian *dx*, Varignon made use of Newton and Leibniz at the same time. Although Varignon espoused the Leibnizian formalism he interpreted the differential *dx* as a process, i.e., the process by which a quantity *x* became zero (*dx* represented the instant in which *x* became zero). But this only shifted the problem one step further. In fact, *dx* functioned as a numerical constant, and, interpreting it as a process, Varignon's approach created an asymmetry, an incongruity, between the formalism and its referents. (See [Petitot 1977, 450] for further considerations on this issue.)

5. THE PUBLIC DEBATE: EXPLAINING AND INTERVENING

Until this point the debate had raged only within the academy. The academy explicitly forbade its members to make public statements concerning this debate despite Varignon's request "d'avoir aussi le public pour Juge" [Bernoulli 1988, 254]. The unwillingness of some members to take a public stand, and perhaps a real concern with the public image of the newly reorganized academy, contributed to this decision. Nonetheless, Varignon had sent the memoirs concerning the debate to Johann (I) Bernoulli and to Leibniz, asking them not to make any public mention of the fight. At the end of 1701 the academy silenced Rolle and Varignon, and the Abbé Bignon, president in 1701, nominated an adjudicatory commission composed of Gouye, Cassini, and Ph. de la Hire to judge the claims made by the contenders. This was the usual procedure in the academy. This commission was very favorable to Rolle (Gouye and Ph. de la Hire were in fact on his side) but it never gave a judgment. Among the reasons for not giving a judgment was that the situation was becoming extremely fluid. People were becoming less hostile to the infinitesimalist position and were slowly changing their attitudes. This was the case, for example, with the Abbé Gouye, who had anonymously attacked the new calculus in an issue of the *Journal de Trévoux* in May 1701. Reviewing an article by Johann (I) Bernoulli, Gouye attacked the analysis of the various orders of infinity and ended his attack by saying: "Il ne suffit pas en Géométrie de conclure vray, il faut voir evidemment qu'on le conclut bien" [Gouye 1701, 234]. There was no mention in Gouye's article of the ongoing debate within the academy. As an academician, Gouye was compelled to stay silent on that point. Leibniz answered Gouye in the famous letter to M. Pinson written on August 29, 1701, parts of which were published in December by the *Journal de Trévoux*. Replying to the attacks of Gouye, Leibniz stated:

> on n'a pas besoin de prendre l'infini icy à la rigueur, mais seulement comme lors qu'on dit dans l'Optique que les rayons du soleil viennent d'un point infiniment éloigné, & ainsi sont estimez paralleles. Et quand il y a plusieurs dégrez d'infini; ou infiniment petit, C'est comme le Globe de la terre est estimé un point à l'égard de la distance des fixes, & une boule que nous manions est encore un point en comparaison du semidiametre du Globe de la terre. Desorte que la distance des fixes est un infiniment infini ou infini de l'infini par rapport au diametre de la boule. Car au lieu de l'infini ou de l'infiniment petit, On prend des quantitez aussi grandes & aussi petites qu'il faut pour que l'erreur soit moindre que l'erreur donnée: de sorte qu'on ne

differe du style d'Archimede que dans les expressions qui sont plus directes dans nôtre methode & plus conformes à l'art d'inventer. [Leibniz 1701, 270–271]

The claim of being able to recast any proof involving infinitesimals into a proof in the style of Archimedes, i.e., a proof using the method of exhaustion, was extremely suggestive but it was never developed in a completely convincing way. In any case, this last part of Leibniz's letter was ignored by the anti-infinitesimalists who emphasized a literal reading of the first part of the letter.

This declaration by Leibniz did not help the infinitesimalists fighting within the academy at all. If in fact, as Leibniz stated, a differential was to its variable as a pebble of sand to the earth, then it was clear that the differential was still a finite quantity and therefore the calculus could be granted only the status of an approximation method and not that of a rigorous science. That this conclusion was drawn by others is confirmed by the first letter of Varignon to Leibniz (November 28, 1701). In this letter Varignon, having identified the Abbé Galloys as the sponsor of the anti-infinitesimalist position, asked Leibniz to make a precise statement on what should be understood by "infinitesimal quantity." This was absolutely necessary since:

Les ennemis de vôtre calcul ne laissent pourtant pas d'en triompher, et de répandre cela [the letter to Pinson] comme une déclaration nette et précise de vôtre sentiment sur cette matière. Je vous supplie donc, Monsieur, de vouloir bien nous envoyer au plustost cette déclaration nette et précise de vôtre sentiment sur cela. [Leibniz 1843–1863 IV, 90]

The disciples were asking the master to lead them through the conceptual maze in which they were caught. Leibniz answered Varignon's letter on February 2, 1702. Parts of this letter were published by the *Journal des Sçavans* the same year. The position held by Leibniz in the letter may be summarized in three points:

(a) There is no need to base mathematical analysis on metaphysical assumptions.

(b) We can nonetheless admit infinitesimal quantities, if not as real, as well-founded fictitious entities, as one does in algebra with square roots of negative numbers. Arguments for this position depended on a form of the metaphysical principle of continuity.

(c) Or, one could organize the proofs so that the error will be always less than any assigned error.

Leibniz ended by pointing out the positive nature of debates in helping sciences acquire better foundations. This had been the case for algebra and geometry, both of which had survived the attacks of their opponents. "J'espere que nostre Science des infinis," Leibniz added, "ne laissera pas de subsister aussi" [Leibniz 1843–1863 IV, 94].

It is important to pause a second to reflect on Leibniz's claims. Leibniz was trying to shift the foundational issue onto other grounds. Leibniz did not think that the calculus was to be justified by its "metaphysics." Consequently, for Leibniz, the problem is not *"Do infinitely small quantities exist?"* but *"Is the use of infinitely small quantities in the calculus reliable?"* [Bos 1980, 87].

By this time, the use of square roots of negative numbers in algebra was a well-established and accepted practice, although the question of the soundness of its foundations was still largely unresolved. Yet Leibniz in (b) appealed to this accepted practice as a justification for his own. Finally, I should mention the existence of two different foundational approaches that merged in Leibniz's letter. The first was related to the classical methods of proof by exhaustion; the second was based on a metaphysical principle of continuity. (For a detailed analysis of these two foundational efforts see [Bos 1974, 1980; Horvath 1982, 1986].)

We are interested here in the consequences of this intervention by Leibniz. If we are to trust Varignon's comments in a letter to Johann (I) Bernoulli (see [Leibniz 1843–1863 IV, 97]), Leibniz's letter had the welcome effect of answering the Abbé Gouye's doubts. These were important moves within the academy. The debate had not been settled yet and the infinitesimalists needed to modify an atmosphere that was not in their favor. Even if the letter did have this positive outcome, the infinitesimalists were quite unsatisfied with it. Leibniz had not expressed any commitment to infinitesimal quantities and l'Hôpital got to the point of asking Leibniz not to write anything more on the matter. This is how Leibniz, in a letter of 1716, recalled the events:

> Quand ils [nos amis] se disputèrent en France avec l'Abbé Gallois, le Père Gouge et d'autres, je leur témoignai que je ne croyois point qu'il y eût des grandeurs véritablement infinies ni véritablement infinitésimales [. . .]. Mais comme M. le Marquis *de l'Hospital* croyoit que par là je trahissois la cause, ils me prièrent de n'en rien dire. [Leibniz 1716, 500]

We can conclude, therefore, that the infinitesimalists were deeply dissatisfied with the master. They had looked for a light to follow and they found that Leibniz had no definitive truth to give them concerning infinitesimals. Signs of this deep disappointment were clearly expressed by Fontenelle in his eulogy of Leibniz read in 1716:

> Il ne faut pas dissimuler ici une chose assez singuliere. Si M. Leibnitz n'est pas de son côté aussi-bien que M. Neuton l'Inventeur du Sistême des Infiniment petits, il s'en faut infiniment peu. Il a connu cette infinité d'ordres d'Infiniment petits toujours infiniment plus petits les uns que les autres, & cela dans la rigueur géométrique, & les plus grands Géometres ont adopté cette idée dans toute cette rigueur. Il semble cependant qu'il en ait ensuite été effrayé lui-même, & qu'il ait crû que ces différents ordres d'Infiniment petits n'étoient que des grandeurs *incomparables*, à cause de leur extrême inégalité, comme le seroient un grain de sable & le Globe de la Terre, la Terre & la Sphere qui comprend les Planetes, &c. Or ce ne seroit-là qu'une grande inégalité, mais non pas infinie, telle qu'on l'établit dans ce Sistême. Aussi ceux même qui l'ont pris de lui n'en ont-ils pas pris cet adoucissement, qui gâteroit tout. Un Architecte a fait un Bâtiment si hardi qu'il n'ose lui-même y loger, & il se trouve des gens qui se fient plus que lui à sa solidité, qui y logent sans crainte, &, qui plus est, sans accident. Mais peut-être l'adoucissement n'étoit-il qu'une condescendance pour ceux dont l'imagination se seroit révoltée. S'il faut tempérer la vérité en Géométrie, que sera-ce en d'autres matieres? [Fontenelle 1716, 114–115]

The attacks by Rolle had split the infinitesimalists on the problem of the foundations.

Although it is not my intention to discuss the complex issues related to Leibniz's philosophy of the calculus, I want to consider for a moment the deep difference between Leibniz's position (as perceived by the French) and the French infinitesimalists' position on the problem of foundations. We have seen that the French took very seriously the notion of "different orders of infinity." In their view this was the foundation of the building. They read Leibniz as insisting on the notion of incomparability. For Leibniz, they thought, it was enough to claim that a quantity and its differential were incomparable. The French considered this a fatal mistake. If in fact, they argued, two quantities were only incomparable, then their difference was a finite quantity and therefore a finite mistake was introduced in the calculus. This was not the case if dx was an infinitely small quantity. In this case, in fact, the mistake would be less than any finite quantity.

This was a very narrow way to read Leibniz's claims. Leibniz was in fact attempting to define a more subtle position by considering the infinitesimals as well-founded fictions. By reading Leibniz literally rather than metaphorically on the sand and globe metaphor, the French mathematicians were unable to understand Leibniz's more complex position. In effect, Leibniz was proposing a sophisticated "formalistic" foundation for his algorithm. However, by considering the infinitesimals as well-founded fictions, he was introducing a gap between the formal apparatus and the referents. We can say that Leibniz's system was based on a "subversion" of the semantics in favor of a consistent formalism. This could somehow justify his claims that, linguistically, the opposition finite/infinite could be easily relativized.

> & il se trouve que les Regles du fini réussissent dans l'infini, comme s'il y avoit des Atomes (c'est-à-dire, des élemens assignables de la matiere,) quoy qu'il n'y en ait point, la matiere étant actuellement sous-divisible sans fin; & que *vice versa* les Regles de l'infini réussissent dans le fini, comme s'il y avoit des infiniment petits Metaphysiques, quoy qu'on n'en ait point besoin, & que la division de la matiere ne parvienne jamais à des parcelles infiniment petites. C'est parce que tout ce gouverne par raison, & qu'autrement il n'y auroit point de science ny de Regle; ce qui ne seroit point conforme avec la nature du souverain Principe. [Leibniz 1702, 186]

The Parisian mathematicians tried to provide a concrete reference to Leibniz's formalism. However, at this state of the art, the attempt was hopeless; since finite quantities and infinitesimals were assumed to be ruled by the same algebraic laws, nothing could prevent the inference from $x + dx = x$ to $dx = 0$. Jakob Bernoulli had earlier warned against the use of the usual algebraic laws, such as "if equals are subtracted from equals, the results are equal," in computations involving infinitesimal quantities. By not addressing the problem explicitly, l'Hôpital gave his opponents the opportunity for a strong criticism of the *Analyse des infiniment petits*.

6. ROLLE–SAURIN, 1702–1703: THE SECOND PART OF THE DEBATE

This second part of the debate was fought publicly in the *Journal des Sçavans*, which had always been very open to the academicians. On April 3, 1702, the *Journal des Sçavans* published Rolle's article, "Regles et remarques, pour le problème general des Tangentes." In this article Rolle proposed some new rules for solving tangent problems. The methods given so far, in Rolle's opinion, were insufficient to discover all the tangents to geometric curves. Rolle emphasized that the rules he was proposing had their origin in ordinary analysis (as opposed to the new analysis). The article ended with a challenge clearly addressed to the infinitesimalists. Rolle used various examples showing, he claimed, that when we have more than one tangent at a given point on a curve (corresponding, for example, to a point of self-intersection of the curve), the "most-used" methods were no longer sufficient. One of Rolle's examples was again the curve

$$\text{(A)}\quad y^4 - 8y^3 - 12xy^2 + 48xy + 4x^2 - 64x + 16y^2 = 0.$$

Although Rolle never mentioned Varignon or *Analyse des infiniment petits*, the article was a clear challenge to the infinitesimalists. The reply to Rolle was written by a protégé of l'Hôpital, Joseph Saurin (1659–1737), who was not yet an academician. He interpreted Rolle's article as a direct attack against the infinitesimal calculus. Rolle had claimed (we are already familiar with his strategy) that in the case of multiple points the new analysis would not give the classical results. It was true that, for example, in (A) dy/dx becomes indeterminate for $x = 2$. Saurin, using l'Hôpital's rule, was able to show for some of the cases how the methods given by l'Hopital's book were perfectly fine. He then accused Rolle of plagiarizing l'Hôpital's methods by using notational variants of them and attacked Rolle with a purely ad hominem argument: "En lisant cet Article, on sent un Auteur, qui chagrin de ne pouvoir se passer du Calcul differentiel qu'il n'aime pas, tâche de profiter ce qu'il peut y avoir de commun entre ce Calcul & la methode de M. de Fermat pour le confondre entierement avec cette methode" [Saurin 1702, 531]. Finally, Saurin challenged Rolle to apply his methods to mechanical curves. Once again the successful applications of the infinitesimal calculus were playing a major role in its acceptance as a rigorous method.

From this point, the debate became more personal and political. Each faction used any means at its disposal to create the conditions for its victory. For example, the debate went on in the *Journal des Sçavans,* which Gouye and Bignon directed in 1702. Whereas Rolle's article was published without cuts, Bignon had cut Saurin's answer. Here is the bitter comment of Varignon taken from a letter to Johann (I) Bernoulli written in the summer of 1702:

> Quant à ce que vous luy [l'Hopital] aviez envoyé pour être publié dans le Journal de Sçavans, je vous diray qu'on n'y met plus du tout de mathematiques depuis la lettre de M. Leibnitz que j'y fis inserer il y a 5 ou 6 mois, le party étant pris de n'y en plus mettre à moins que ce ne soit dans des Journaux extraordinaires, pour lesquels obtenir il faut avoir de quoy les remplir, outre qu'on ne les accorde encore qu'avec peine à cause du peu de gens qui en achetent. C'est pour cela que M. le Marquis de L'hopital avec tout son credit a eu touttes les peines du monde à en obtenir un pour publier la Reponse qu'il a fait faire à M. Rolle par un nommé Mr. Saurin; encore M. l'Abbé Bignon (qui a aussi la direction de ce Journal comme Neveu de M. le Chancelier, et qui n'avoit (dit-il) reçu l'Ecrit de M. Rolle que parce qu'il n'y paroissoit aucune contestation) a-t-il voulu qu'on en retranchast tout ce qu'il y avoit de personnel; ce qui a tout à fait défiguré cette Reponse. [Bernoulli 1988, 324]

We can see therefore that the editorial policy of the directors of the *Journal* had favored Rolle over Saurin.

Rolle attacked again in 1703 and in 1704 (see [Rolle 1703b, 1704]) with another memoir on the inverse of tangents (i.e., the integral calculus). Saurin did not immediately answer these attacks but another devoted infinitesimalist decided to join the battle: he was Fontenelle, the perpetual secretary of the academy.

7. FONTENELLE AND THE EULOGY OF L'HÔPITAL

Fontenelle had been elected perpetual secretary of the academy in 1697. Among his duties was the yearly compilation of the *Histoire et Mémoires de l'Académie Royale des Sciences*. He also delivered public speeches representing the academy, including the eulogies of the deceased academicians. Since 1694, he had been very close to the group led by Malebranche.

Until this point Fontenelle, although on the side of the infinitesimalists, had publicly spoken of the debate only in a small note published in the *Histoire* of the academy for the year 1701. One may question the editorial policy of Fontenelle on the subject: his short note did not do justice to a debate that occupied the academy for 2 years—the Registres des Procès Verbaux of the academy for 1700 and 1701 are filled almost entirely with these debates (see [Blay 1986] for extensive quotations from the Registres). Still, the note at least gave a hint of the existence of a true problem concerning the foundations of the infinitesimal calculus: "Il saura bien, si la nouvelle Géometrie n'est pas solide, se retracter de la grande vogue qu'il commence à lui donner, & y démêler, avec le tems les erreurs qu'il n'y a pas encore apperçûes" [Fontenelle 1701, 89]. But on the whole the note was very flattering to the new system proposed by l'Hôpital. In particular, l'Hôpital's silence during the debate was carefully explained to avoid the impression that l'Hôpital had any fears concerning his calculus. Fontenelle himself had not yet taken an official stand.

In 1704 the debate was at its peak. On February 2 l'Hôpital died, and on April 2 Fontenelle read the "Eloge de M. le Marquis de l'Hôpital." In this eulogy, the differential calculus was described as the "sublime géométrie." L'Hôpital was emphatically described as possessing a map to the "Pays de l'Infini" and as knowing its most remote paths:

> M. de l'Hôpital résolut de communiquer sans réserve les trésors cachés de la nouvelle Géométrie, & il le fit dans le fameux Livre de l'*Analyse des Infiniment petits*, qu'il publia en 1696. Là, furent dévoilés tous les secrets de l'Infini Géométrique, & de l'Infini de l'Infini; en un mot, de tous ces différens ordres d'Infinis, qui s'élevent les uns au-dessus des autres, & forment l'Edifice le plus étonnant & le plus hardi que l'Esprit humain ait jamais osé imaginer. [Fontenelle 1704a, 131]

Given these words it is difficult to imagine that the very foundations of this building were still under violent attack within the academy. But Fontenelle, addressing the opposition, went even further:

> Aussi cet Ouvrage a-t-il été reçu avec un applaudissement universel: car l'applaudissement est universel, quand on peut très-facilment compter dans toute l'Europe les suffrages qui manquent, & il doit toujours en manquer quelques-uns aux choses nouvelles & originales, sur-tout quand elles demandent à être bien entendues. Ceux qui remarquent les évenemens de l'Histoire des Sciences, savent avec quelle avidité l'Analyse des Infiniment petits a été saisie

> par tous les Géometres naissans, à qui l'ancienne & la nouvelle méthode sont indifférentes, & qui n'ont d'autre intérêt que celui d'être instruits. Comme le dessein de l'Auter avoit été principalement de faire des Mathématiciens, & de jetter dans les esprits les semences de la haute Géométrie, il a eu le plaisir de voir qu'elles y fructifoient tous les jours, & que des Problemes réservés autrefois à ceux qui avoient vieilli dans les épines des Mathématiques, devenoient des coups d'essai de jeunes gens. Apparemment la révolution deviendra encore plus grande, & il se seroit trouvé avec le temps autant de Disciples qu'il y eût eu de Mathématiciens. [Fontenelle 1704a, 133]

The opposition's voice was silenced by the universal recognition given to l'Hôpital, as if truth were just a matter of universal agreement. The message was all too clear: those who criticized the differential calculus did not understand it. The reference to Rolle and Galloys was very explicit in the comparison made between those who were devoted to the ancients and those who were devoted to learning, regardless of modern or ancient methods. We see how easily Fontenelle skipped over the fundamental issue of the foundations of the new algorithm. This problem had not been settled and Fontenelle knew this all too well. He himself was working on a book that would have provided "la vrai metaphysique" of the infinitesimal calculus.

Nevertheless, the need to destroy the anti-infinitesimalist opposition was too important. No mention was made here of any foundational problem. Reducing the critiques of the noninfinitesimalists to pure ignorance, Fontenelle was taking a very definite stand on the ongoing debate within the academy. To avoid the suspicion that my reading is a superimposition on the text, I quote another source to show that Fontenelle's words had a much clearer and stronger meaning in the context in which they were originally uttered:

> Mr. l'Abbé Bignon en donnant à Mr. de Fontenelle les loüanges qu'il meritoit pour les deux beaux discours qu'il venoit de prononcer, luy dit qu'il avoit fait si hautement l'éloge de la Geometrie des infiniments petits, qu'après cela on ne pouvoit douter qu'il n'en fût le partisan declaré. Que cependant ceux qui n'étoient point initiez dans les mysteres de cette nouvelle Geometrie étoient effrayez d'entendre qu'il y eût des infinis, des infinis d'infinis & des infinis plus grands ou plus petits que d'autres infinis; parce qu'ils ne voyent que le comble de l'édifice sans sçavoir sur quel fondement il étoit appuyé. Il exhorta donc Mr. de Fontenelle qui travaille à des Elemens du calcul differentiel de les donner au plûtôt au public, afin de convaincre tout le monde de la solidité de cette sublime Geometrie à qui il venoit de donner tant d'éloges. [*Journal de Trévoux* 1704, 1016–1017]

It is clear that the eulogy of l'Hôpital was perceived as an open declaration of partisanship on Fontenelle's part. Not only did he use his lofty position to make public statements concerning the truth or falsity of the anti-infinitesimalists' claims—a practice that one may clearly question—he went further. The first page of the *Histoire et Mémoires de l'Académie des Sciences* for 1704 had the following "Avertissement":

> On a imprimé dans les Mémoires de 1703, page 312, un Ecrit de M. Rolle, intitulé, Du nouveau Systeme de l'Infini. Les Réflexions que diverses personnes ont faites sur cet Ecrit, sur les principes qui y sont avancés, & sur le conséquences qu'on en pourroit tirer, obligent à déclarer que quoqu'il se trouve parmi les autres Ouvrages destinés à l'impression par l'Académie, son intention n'a jamais été d'adopter rien de ce qui s'y peut trouver. [Fontenelle 1704b]

This official condemnation of Rolle's memoir, a flagrant contradiction of the spirit of the academy, raised several doubts concerning the alleged impartiality of this institution. For us the condemnation is important because it showed that Fontenelle (and Bignon) had already made a decision on the debate. This sheds light on the composition of the two groups and, as we shall see, on the composition of the adjudicatory commission nominated in 1705.

8. ROLLE–SAURIN, 1705: REPRISE AND THE PEACE OF THE INFINITELY SMALL

Saurin had not responded to the attacks made by Rolle in 1703 and 1704. Rolle's flush of papers and his boasts could have given the impression that he had silenced his adversaries. However, on April 23, 1705, Saurin attacked Rolle again. By then the debate had completely degenerated into mere invective. Some short quotations will give an idea of the level of these last articles. In the above-mentioned article Saurin wrote: "Qui s'imagineroit, qu'avec cette assurance, il ne va qu'à tâtons, & ne parle qu'au hazard?" [Saurin 1705a, 252]. Replying on June 2, Rolle called Saurin "un pitoyable géomètre" [Rolle 1705a, 318]. The final article in this long debate was written by Rolle and published on July 30, 1705 (see [Rolle 1705b]). Rolle kept accusing l'Hôpital of plagiarizing the classical algebraic methods in his *Analyse des infiniment petits* but on the whole there was little new theoretical or philosophical content to this last part of the debate. Meanwhile, Saurin was repeatedly imploring the academy to give a final judgment. The feeling that the academy would soon heed his calls and nominate a commission for this purpose only added urgency to the exchange. Leibniz was very annoyed about the whole situation. He thought it important to get a favorable judgment from the academy and a public condemnation of Rolle. Writing to Varignon, he said:

> J'ay receu enfin le Journal du 13me d'Avril de cette année, qu'un Suedois m'a apporté, et j'ay vû que je n'avois pas besoin d'autre instruction, ny de beaucoup de discussion, pour examiner ce qui est contesté entre M. Saurin et M. Rolle. C'est pourquoy, pour satisfaire à vostre desir, et au sien, quoyque d'ailleurs je n'aime pas les contestations, je vous envoye le papier cyjoint, esperant qu'il sera conforme à vostre intention. La mienne seroit que sans le publier on le communiquat a M. l'Abbé Bignon [. . .].
>
> Peutestre qu'elle le portera à terminer selon la justice une dispute scandaleuse du costé de celuy qui fait des objections les plus frivoles qui se puissent voir, en l'obligeant de reconnoistre qu'on a satisfait sur cet article. Je pense même à en écrire aussi à M. l'Abbé Gallois et à adresser la lettre pour luy à M. l'Abbé Bignon. Si cela ne servira de rien, il faut abandonner la pensée de faire rendre justice à M. Saurin et à nostre calcul par l'Academie, et nous tacherons de ramasser des jugemens des autres. [Leibniz 1843–1863 IV, 127–128]

At the end of 1705 the Abbé Bignon nominated a commission, including himself, Ph. de la Hire, Galloys, Fontenelle, and Cassini, to provide an official judgment on the whole affair. Varignon's testimony is important in assessing the political composition of the commission:

> Vostre lettre du 26 Julliet dernier me fut rendue sur la fin du même mois. Je fus aussi tost porter à M. l'Abbé Bignon celle que vous m'adressiez pour lui, avec celle que son paquet contenoit aussi pour M. l'Abbé Galloys. M. l'Abbé Bignon lut la sienne sur le champ, et il me dist qu'il ne manqueroit pas de vous faire réponse, et qu'en attendant j'eusse à vous asseurer

qu'il avait desja donné des ordres pour terminer la dispute d'entre M. Saurin et M. Rolle; que pour Juges avec luy, il avait nommé M. Cassini, M. de la Hire, M. l'Abbé Galloys et M. de Fontenelle, qui est le seul de ceux qui sont pour les infiniment petits, qui n'ait pas été récusé. Pour nous, nous n'avons récusé personne, non pas même M. l'Abbé Galloys, tout ennemi déclaré qu'il est de ce calcul, ny M. de la Hire, quelque livré qu'il soit à M. l'Abbé Galloys: M. Saurin a seulement demandé que le jugement de chacun de ces Mrs. fust rendu public, pour retenir les ennemis du calcul par la crainte d'exposer leur réputation. [Leibniz 1843–1863 IV, 131–132]

It is clear that the fight was far from being decided.

The academy made its decision public in January 1706. Rolle was asked to conform better to the regulations of the academy and Saurin was "renvoyé a son bon coeur." Fontenelle referred in 1719 to this decision as the "paix des infiniment petits." Leibniz and Johann (I) Bernoulli were of course dissatisfied; Leibniz considered the judgment "magis morale quam mathematicum" [Leibniz 1843–1863 III, 794]. But it is clear that given the composition of forces within the academy no other verdict would have been possible.

The judgment stopped Rolle's attacks, and the death of Galloys in 1707 put an end to the opposition. Varignon wrote to Johann (I) Bernoulli (November 10, 1706):

J'écris a M. Hermann que M. Rolle est enfin converti: il vous dira comment il m'est l'est venu a marquer et à M. de Fontenelle; il l'a aussi marqué au P. Malebranche, disant qu'on l'avoit poussé a faire ce qu'il a fait contre les infiniment petits, et qu'il en etoit faché. . . . [Malebranche 1958–1968 XIX, 739]

We can speculate about Rolle's change. He had alienated himself from the rest of the mathematical community. He probably thought it best to excuse himself and accept a dignified peace. I cannot help but quote Leibniz's comment regarding Rolle's alleged conversion: "Plus gaudii est in coelo nostro geometrico ex uno peccatore converso, quam ex decem justis" [Leibniz 1843–1863 III, 811]. What is certain, however, is that Rolle never did convince himself of the soundness of the infinitesimal calculus. Writing to Leibniz in 1708, Varignon mentioned that Rolle was still making adverse comments: "J'apprend cependant que M. Rolle ne laisse pas de décrier encore sourdament ce calcul par le monde" [Leibniz 1843–1863 IV, 167].

The death of Galloys and the withdrawal of Rolle marked the final victory of the infinitesimal calculus on the continent. In his preface to the *Élémens de la géométrie de l'infini* Fontenelle could finally boast: "Malgré tout cela l'Infini a triomphé, & s'est emparé de toutes les hautes spéculations des Géometres. Les Infinis ou Infiniment petits de tous les ordres sont aujourd'hui également établis, il n'y a pas plus deux partis dans l'Académie" [Fontenelle 1727, preface]. The battle had not been an easy one.

CONCLUSION

I made two claims in my introduction. It is now time to comment on them.

(a) There is no doubt that the withdrawal of Rolle and the death of Galloys marked the complete victory of the infinitesimal calculus in France, a victory sought with constant appeal to the authority of the most famous geometers and to the increasing success of the differential algorithm in solving problems inaccessible to the previous algebraic techniques. We must be amazed at the effort spent by the infinitesimalists on winning their battle. The foundational issue remained unclear but the analysts pushed ahead, as Kline would say, "with vigor but without rigor."

(b) My second claim concerned the philosophical and mathematical significance of Rolle's objections. As to the mathematical significance, although flawed by several mistakes Rolle's attacks had the merit of pushing research toward areas not yet completely understood: witness the work by Saurin in the next 2 decades on singularities on curves and Guisnée's work on Hudde's and Fermat's rules. As to the philosophical significance, the opposition finite/infinite is one of the long-standing issues in the philosophy of mathematics. Rolle addressed the problems of rigor in mathematics and of the acceptability of infinitary mathematics. This forced the infinitesimalists to address explicitly the foundational problem, and, as we have seen, they were far from having conclusive answers.

Rolle's criticisms also foreshadowed Bishop Berkeley's more famous attacks against the fluxional and the differential calculus. Several questions could be asked about the relationship between the early criticisms of the infinitesimal calculus (of Rolle, Nieuwentijt, Cluver, etc.) and the successive ones. For the moment I will limit myself to some brief remarks about the similarities and dissimilarities between Rolle's criticisms and Berkeley's *Analyst* while referring the reader to [Blay 1986] for a more thorough analysis. It is quite interesting to find that Rolle's three main objections are raised in the *Analyst*. In particular, Berkeley's paragraphs 6 and 7 contain a critique of the existence and conceivability of differentials, and paragraph 18 contains an attack on the use of dx both as a quantity and as an absolute zero. Although the motivations for Rolle's attacks and Berkeley's criticisms differed, the two agreed on a number of points and on an explicit finitism. For Rolle this finitism was embedded in the Cartesian refusal to admit infinitary mathematics as a rigorous discipline; for Berkeley, more explicit epistemological considerations accounted for the finitist commitment.

I must also remark on the different logic in the strategies employed by Rolle and Berkeley. Rolle thought that the wrong principles of the analysis were bound to produce falsities; Berkeley never questioned the results of the calculus and proposed his theory of double mistakes to explain how one could, through several errors, arrive "though not at science yet at truth" [Berkeley 1734, 78].

Finally, it is my opinion that Rolle's position within the academy made his attacks much more dangerous for the French infinitesimalists than Berkeley's attacks were for the British mathematicians. Moreover, Rolle's challenge was extremely radical, as Fontenelle points out: "il y a certainement encore des difficultés à éclaircir dans le Systême de la nouvelle Géometrie; mais on parloit de renverser le Systême total" [Fontenelle 1719, 98].

Rolle provoked inside the academy a foundational "crisis"—according to Herbert Mehrtens's definition, a "phenomenon [whereby] in a given mathematical community—for whatever reasons—the common commitment of the groups are questioned and, consequently, the stability of this social system is at risk" [Mehrtens 1976, 303]. The means to solve this crisis ranged from clarifications to authority, from persuasion to proof, from dubious editorial policies to public condemnations.

Once again we must reach the conclusion that mathematics and its development are due to human efforts and not only to the soundness of the ideas involved.

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NOTES

1. For some recent work in this direction see [Giorello 1985, chap. vii].

2. When this paper was already in its editorial stage I came across the just-published edition of the second volume of the epistolary of Johann (I) Bernoulli [Bernoulli 1988]. This volume contains several letters exchanged between J. (I) Bernoulli and Varignon that are relevant to the debate and were not included in [Fedel 1932]. The same volume (Annexe IV, pp. 351–376) contains an edited text by J. Peiffer of the manuscript [Reyneau]. Although I had used the original manuscript, I have subsequently changed the quotes from [Reyneau] to conform to [Bernoulli 1988]. In addition, the quotes from [Fedel 1932] have been changed to [Bernoulli 1988]. In the introduction to Reyneau's text, Peiffer also refers to the article [Blay 1986], which had escaped my attention, in which the foundational aspect of the Rolle–Varignon debate is emphasized. By comparing Rolle's and Berkeley's critiques, Blay shows that the technical success of the differential calculus in the first 3 decades of the 18th century brought a change in the "style of criticisms" leveled against the differential calculus. Since Blay uses material from the Rolle–Varignon debate, there is some overlap between Section 4 of this paper and the first half of Blay's article. Blay's insightful article must be recommended for the extensive use of the Registres des Procès Verbaux des Séances de l'Académie Royale des Sciences (in particular, vols. 19 and 20), where one can find the second, third, and fifth memoirs by Rolle and the first four replies by Varignon. The archival sources used by Blay enrich but do not alter the general picture of the Rolle–Varignon debate as conveyed by Reyneau. Thus, I have not found it necessary to modify Section 4, although I refer the reader to Blay's paper where appropriate.

3. In this paper all quotations appear in their original form; no attempt has been made to modernize the spelling or to resolve many of the inconsistencies in spelling of the original text.

4. The reader is referred to [Malebranche 1958–1968 XX] for detailed biographies of the "malebranchists."

5. Rolle was an algebraist, Galloys (1632–1707) had done some work on ancient geometry, and Ph. de la Hire (1640–1718) had done important work on conic sections.

6. Since these definitions are central to what follows, I provide the translation given in [Struik 1967, 313]:

> DEFINITION 1. Variable quantities are those that continually increase or decrease; and constant or standing quantities are those that continue the same while others vary.

> DEFINITION 2. The infinitely small part whereby a variable quantity is continually increased or decreased is called the differential of that quantity.

7. "POSTULATE 1. Grant that two quantities, whose differences is an infinitely small quantity, may be taken (or used) indifferently for each other: or (which is the same thing) that a quantity, which is

increased or decreased only by an infinitely small quantity, may be considered as remaining the same" [Struik 1967, 314].

8. "POSTULATE 2. Grant that a curve line may be considered as the assemblage of an infinite number of infinitely small right lines: or (which is the same thing) as a polygon of an infinite number of sides, each of an infinitely small length, which determine the curvature of the line by the angles they make with each other" [Struik 1967, 314].

9. See [Reyneau 144; Bernoulli 1988, 352] and letter 55 from Varignon to J. (I) Bernoulli in [Bernoulli 1988, 255].

10. This interesting passage is clearly related to Leibniz's attempts to give a justification of the calculus in the spirit of the exhaustion method (cf. Section 5).

11. Descartes, in the *Géométrie*, had given a method for finding the normal at a point of an arbitrary curve. This method rested essentially on the determination of a double root of a suitable equation obtained from the data of the problem. The difficulty remained in finding the double root. It was Johann Hudde (in 1659) who published a rule for determining double roots of an arbitrary polynomial. The generalization to an arbitrary algebraic curve $\sum c_{ij}x^iy^j = 0$ was given by Sluse (published in 1672). The letter by Hudde was published in the second edition of the Latin translation of Descartes' *Géométrie*. The rule can be described as follows. Starting from an arbitrary polynomial $P(x) = \sum_{i=0}^{n} a_ix^i$ and an arbitrary arithmetical progression $a, a + k, a + 2k, \ldots, a + nk$, if we multiply a_0x^0 by a, a_1x^1 by $a + k$, and so on, we obtain another polynomal $P^*(x) = \sum_{i=0}^{n} a_i(a + ik)x_i$. Hudde asserted that if c is a double root of $P(x) = 0$ then c is a root of $P^*(x) = 0$. (Note that for $a = 0$ and $k = 1$ we have $P^*(x) = xP'(x)$, where $P'(x)$ is the derivative of $P(x)$.) This rule can be used to determine points of maxima and minima. A maximum or a minimum value M of $P(x)$ occurs, as Fermat had already noted, at a double root of the equation $P(x) - M = 0$, and hence at a root of $(P(x) - M)^* = 0$.

12. [Blay 1986, 237–240] contains an extensive discussion of the first example. For Figs. 4–7 and the discussion of the previous two examples, I followed [Bernoulli 1988, 363, 364, 277; Fleckenstein 1946, 130–131; Costabel 1965, 20–22]. The labeling in Figs. 4 and 5 does not follow the original one.

13. The rule for finding maxima and minima given in the *Analyse des infiniment petits* required that one set $dy = 0$ and $dx = 0$. Guisnée's criterion stated that when from both these suppositions one obtained the same finite values for x and y (i.e., both conditions determined the same point of the curve), then that point was an intersection point and not a true point of maximum or minimum. "Lorsque dans l'une & dans l'autre supposition de $dx = 0$ (qui est la même chose que $dy = \infty$), & de $dx = \infty$ (qui est la même chose que $dy = 0$), l'on trouvera, pour chacune des deux coordonnées x & y, les mêmes valeurs en termes finis ou nuls; on sera assûré que la Courbe, dont la nature est exprimée par l'équation sur laquelle on opere, à un noeud au point où les coordonnées ont les valeurs trouvées" [Guisnée 1706, 34].

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