Was the Early Calculus an Inconsistent Theory?

August, 2007
Word Count: 8,820

ABSTRACT

The ubiquitous assertion that the early calculus of Newton and Leibniz was an inconsistent theory is examined. Two different objects of a possible inconsistency claim are distinguished: (i) the calculus as an algorithm; (ii) proposed explanations of the moves made within the algorithm. In the first case the calculus can be interpreted as a theory in something like the logician's sense, whereas in the second case it acts more like a scientific theory. I find no inconsistency in the first case, and an inconsistency in the second case which can only be imputed to a small minority of the relevant community.

1. Introduction

The existence of inconsistencies in mathematical and scientific theories is the motivation for several claims in contemporary philosophy of science, including the importance of paraconsistent logics to model scientific reasoning. Several familiar examples are usually drawn upon to establish such claims. But the inconsistency of these theories is rarely if ever rigorously demonstrated, and when it is demonstrated it usually follows from a contentious construal of the precise theoretical content. What is required is a consensus as to the content of a particular theory, and such that a contradiction follows – non-controversially – from that content.

The example here will be the early calculus as introduced by both Newton and Leibniz. This has been widely drawn on in the literature to establish that there are many examples of inconsistent theories. Lists of such examples, all of which include the early calculus, are to be found in Lakatos (1966, p.59), Feyerabend (1978, p.158), Shapere (1984, p.235), Priest and Routley (1983, p.188) and da Costa and French (2003, p.84). Both Newton's 'calculus of fluxions' and Leibniz's 'infinitesimal calculus' are referred to. But so entrenched is the understanding that the early calculus was inconsistent that most authors don't even provide a reference to support the claim. Priest and Routley are an exception, citing Boyer's esteemed history of the calculus:

> [circa 1720] mathematicians still felt that the calculus must be interpreted in terms of what is intuitively reasonable, rather than of what is logically consistent. (Boyer, 1949, p.232)

However, Boyer himself doesn't provide an *argument* that the early calculus was inconsistent. One is instead led to the words of Bishop George Berkeley, who criticised the calculus with his famous 1734 publication *The Analyst*.

Is the inconsistency of the early calculus so transparent that Berkeley's words stand as proof of the fact? In this paper I aim to show that one should be very careful in distinguishing several different claims Berkeley made. I begin with Berkeley in §2, presenting the argument just as he did by way of introduction. In §3 I then proceed to analyse the criticisms in more detail, distinguishing two principle objects of a possible inconsistency claim. In §4 the first of these is discussed, and I argue that it is very natural to call this object 'the early calculus'. But it is noted that this *isn't* what most people mean to label 'inconsistent', and no inconsistency is found. In §5 the second possibility is discussed. This takes us on a journey through the commitments of Newton and Leibniz and the mathematical communities in England and France following them. Johann Bernoulli stands out as a proponent of an inconsistent, even contradictory theory, but little significance can be attached to his position. §6 is the conclusion.

2. Berkeley and the early calculus

We might imagine Galileo dropping an apple from the top of the tower of Pisa. He makes measurements to establish that the relationship between the distance travelled s (in metres) and the time taken t (in seconds) is $s=5t^2$. But he finds it much harder to measure speed, so he wants to find the speed out mathematically. What is the apple's speed at the precise moment it hits the ground? Galileo could draw a graph for $s=5t^2$, then he could find the speed of the apple at any time or distance by finding the slope of the tangent to the curve at that time or distance.

It’s then obvious how to achieve an excellent approximation to the tangent, and thus the speed. Consider the following figure:

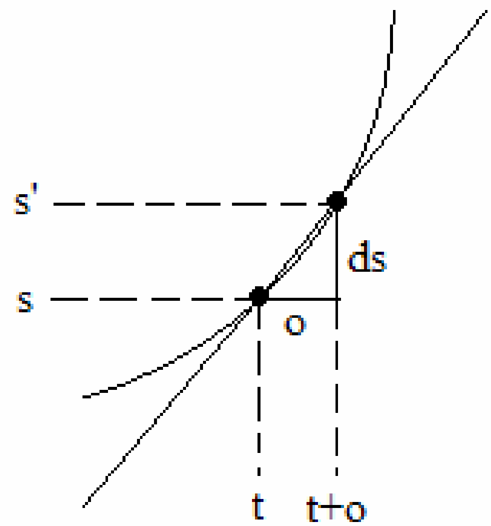

Fig.1

To get an approximation to the tangent at the point (s,t) one can simply take another point on the curve, (s`,t+o), and draw a line through the two points. We can then reduce *o*, taking the second point as close as we want to the first point to get as good an approximation as we want. However, although this is fine for practical purposes, it clearly isn’t much good for achieving an exact result. We need two *different* points to draw a line through, but then we never get the exact tangent at a *single* point. This is the basic problem.

Newton’s method, applied to this specific case, followed the latter illustration rather closely. Taking the two points (s, t) and (s’, t+*o*) we start with the expression,

$$s' - s = 5(t+o)^2 - 5t^2$$

Now we expand the brackets and cancel to give,

$$s' - s = 10to + 5o^2$$

Now the slope of the tangent (and thus the speed) is given on the left hand side by dividing through by *o* (change in distance divided by change in time):

$$\frac{s'-s}{o} = 10t + 5o \qquad \text{(Eq.1)}$$

Now we can cancel any remaining terms on the right hand side which contain *o*, and we achieve,

$$\frac{s'-s}{o} = 10t \qquad \text{(Eq.2)}$$

which is of course the right answer.

Clearly the pressing question is, on what grounds do we 'cancel any remaining terms' in the final step, (Eq.1) to (Eq.2)? The fact that *o* can be made very small doesn't seem to be good enough, since to cancel these terms it would have to be made equal to zero. But it *cannot* be made equal to zero, since then we would no longer be considering two different points on the line. As Berkeley puts it (making adjustments for the given example),[1]

> Hitherto I have supposed that [t] flows, that [t] hath a real increment, that *o* is something. And I have proceeded all along on that supposition, without which I should not have been able to have made so much as one single step. From that supposition it is that I get at the increment of [$5t^2$], that I am able to compare it with the increment of [t], and that I find the proportion between the two increments. I now beg leave to make a new supposition contrary to the first, i.e. I will suppose that there is no increment of [t], or that *o* is nothing; which second supposition destroys my first, and is inconsistent with it, and therefore with every thing that supposeth it. I do nevertheless beg leave to retain [10t], which is an expression obtained in virtue of my first supposition, which necessarily presupposeth such supposition, and which could not be obtained without it: All which seems a most inconsistent way of arguing... (*The Analyst*, §XIV)

He later mimics the mathematician as follows:

> Let me contradict my self: Let me subvert my own Hypothesis: Let me take it for granted that there is no Increment, at the same time that I retain a

[1] Berkeley is actually attacking Newton's 'theory of fluxions' here – thus his talk of 'flowing' quantities – but he attacked Leibniz's 'differentials' in just the same way. The difference between fluxions and infinitesimals/differentials need not concern us for the majority, and will be further explicated in §5, below.

> Quantity, which I could never have got at but by assuming an Increment. (§XXVII)

The charge of inconsistency is then clear enough: *o* is at first necessarily assumed to be *something*, and then later on assumed to be *nothing* in order to cancel terms which include it.

Now from a contradiction anything and everything follows, because from the premises,

(1) A
(2) ~A

we may infer from (1),

(3) A v B

where B is any arbitrary statement. And we may infer from (2) and (3), by disjunctive syllogism,

(4) B

This demonstrates what is called ECQ, *ex contradictione quodlibet*, which can be roughly translated as 'from a contradiction everything follows'. This is where the paraconsistent logicians step in, because the defining characteristic of a paraconsistent logic is that it is *not* the case that 'everything follows' from a contradiction. The rules of inference used above are doctored in one way or another. However, before we resort to meddling with logic we may well ask precisely what counts as the 'A' in the early calculus, such that we have 'A&~A'. Indeed, what *is* 'the calculus' here?

3. Two Units of Analysis

The obvious candidate for 'A' is that '*o* is something', meaning that *o* is a non-zero quantity or number. However, we might ask whether this assumption is really part of the calculus. Certainly at the beginning of the procedure one manipulates the equations *as if o* is something, then at the end one drops certain terms *as if o* is nothing. But as a 'bare procedure' the claim that *o* first *is* something and then later *is* nothing need not accompany the calculus. And surprisingly enough, in the early days the calculus really was widely taken as a ‘bare procedure’, an algorithm, a set of steps to be followed without accompanying justification.

The motive for delineating the calculus in this way comes directly from the primary literature. Newton and Leibniz, acting independently of course, both adopted this instrumentalist attitude in the majority of their publications, with only marginal comments on what might be called interpretation, explanation or justification. As Kitcher writes,[2]

> Newton typically expressed his algorithms in the form of a set of instructions to the reader. (1973, p.37)

The first sign of Newton’s calculus in print was the *Principia* (1687). As Boyer writes:

> His contribution was that of facilitating the operations, rather than of clarifying the conceptions. As Newton himself admitted in this work [*Principia*], his method is “shortly explained, rather than accurately demonstrated.” … [M]athematics was a method rather than an explanation. (Boyer, 1949, p.193)

And his *De Quadratura* (1704) had a similarly instrumental flavour, as Kitcher writes:

> It is unclear whether there is any evidence of Newton asking himself what *o* denotes (ie, what an infinitesimal is). Indeed, in the light of his *De Quadratura* with its instrumentalist attitude toward infinitesimals, the question would seem to be meaningless for him. (1973, p.42f.)

And his *De Analysi* (1711) begins by stating the methodological rules of the calculus, without justification, before going on to present various examples (Edwards, 1979, p.201). Turning to Leibniz I quote Mancosu (1996, p.151), who writes of Leibniz’s first publication of the calculus (1684),

> The paper is remarkable for the paucity of the explanations given by Leibniz… Leibniz does not explain how he arrived at his equations and leaves the reader totally in the dark as to the heuristics and formal proofs of the results therein presented.

[2] See Kitcher’s paper for some primary evidence.

He continues (p.153), "In 1684 Leibniz has presented without justification the … rules for the calculus."

Of course both of the founders did spend some time attempting to justify their procedures, but these 'justifications' both *can* be distinguished from the procedures and *were* so distinguished at the time. And not just by the founders but by the communities following them. As Kitcher writes,

> The mathematical community had appreciated the power of Newtonian and Leibnizian techniques, and had shelved worries about the explanation of their success. (1983, p.256)[3]

When textbooks started to emerge in England, the distinction was clear:

> All these treatises, however advanced they may have been, did not introduce the student to the calculus as a theory […], but rather explained to him how to employ in geometry and mechanics a set of rules. (Guicciardini, p.58f.)

Today we might wonder how the lack of justification could have been acceptable to the community, but it is perhaps difficult to appreciate just how revolutionary the calculus was at that time. Before Newton and Leibniz there had been just a handful of disparate techniques for solving what were really calculus problems. Now, with the new algorithm, an infinity of previously intractable problems were made accessible, and the calculus produced success after success. In this environment, the founders were able to present the calculus without explaining *why* it worked.

This distinction between the calculus as an algorithm and the justification of the calculus is already prevalent in historical and philosophical literature. Back in 1869 Bauman was to write,

> Thus we discard the logical and metaphysical justification which Leibniz gave to the calculus, but we decline to touch the calculus itself. (Cited in Lakatos, 1966, p.58)

Russell wrote in terms of 'interpreting the infinitesimal calculus' in 1948, and compares the distinction to that between elementary arithmetic and the definition of the natural numbers (Ibid., p.58). Lakatos emphasises the distinction,

[3] Cf. Bos, 1980, p.80: "Most mathematicians spend most of their time not in contemplating these concepts and methods, but in using them to solve problems." See also Grabiner, 1983, p.197, for a striking example of this attitude in the work of Fermat.

describing it as 'metaphysical versus technical' and 'instrument versus interpretation'.[4] Edwards is typical in referring to the early calculus as "a powerful algorithmic instrument for systematic calculation." (1979, p.190). Kitcher is yet more explicit in distinguishing "the algorithmic level" from the level of justification (1973, pp.36-37 and §4).[5] And the same distinction is central to Chemla's paper on early Chinese mathematics, as the title suggests: 'The Interplay Between Proof and Algorithm in 3rd Century China: The Operation as Prescription of Computation and the Operation as Argument' (Chemla, 2005).

Berkeley himself nearly always referred to the calculus as the 'method of fluxions', and occasionally as the 'algorithm of fluxions' (Lavine, 1994, p.25). Indeed, he noted the distinction in question here, writing, "It appears that his [Newton's] Followers have shewn themselves more eager in applying his Method, than accurate in examining his Principles." (*The Analyst*, §XVII). But then, taken *purely* as an algorithm, the two contradictory assumptions Berkeley focuses on are external to the calculus. They are (proposed) justifications for moves made within the algorithm, rather than part of the algorithm itself. So we might well conclude that Berkeley *himself* didn't really mean to label the *calculus* inconsistent, but instead the explanation of it.

We will see in §5 that it is not even clear that any justifications actually proposed were inconsistent, and that it is far from clear that there is anything we would want to call an inconsistent *theory*. However, it is first important to argue that the 'calculus itself', as an algorithm, is not an inconsistent object.

4. The Algorithmic Level

Is the 'calculus itself' even the kind of thing which *could* be inconsistent? I think it is. First, we need to get clearer on what is meant by the 'bare algorithm'.[6] It can be represented as a conditional proposition, as follows:

> *If*, for any function $y=f(x)$, we calculate $\dfrac{f(x+o)-f(x)}{o}$, where o acts as a numerical constant, and then take away any remaining terms which are multiples of o, *then* we are left with the derivative.

[4] Lakatos himself rejects the distinction, but if his arguments are successful at all they need only persuade us to accept that the distinction *sometimes* breaks down.
[5] Cf. Grabiner (1983, p.16), Bos (1980, p.60), Guicciardini (1989, p.38).
[6] In what follows I focus purely on the example given in §2, as paradigmatic. Certainly this is the focus of Berkeley's objections.

We see how this is the kind of thing which *could* be inconsistent if we make a comparison with the axiom scheme for induction in Peano arithmetic.[7] In words we have:

> *If*, for any formula A(x), A(0) holds and for all x if A(x) holds then A(x+1) holds, *then* A(x) holds for all x.

Notice how in the latter case, just as the former, there is no accompanying explanation for *why* the consequent holds when the antecedent does. Rather, it stands as a hypothesis, which can be tested by applying different formulas A(x).

Now the axioms of Peano arithmetic would be deemed inconsistent if a contradiction followed from them. Similarly we may ask whether a contradiction follows from the calculus. But in mathematics by 'contradiction' we don't need to find an 'A&~A'; it is acceptable to label '2=1' a contradiction, for example.[8] So in fact we might call the calculus inconsistent if it gives us even a *single* false result. To give an example, consider the following equality:

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \ldots$$

This equation was derived by Leibniz with the use of his calculus. Now if the calculus had led to something slightly different, so that it followed that $\pi>4$, then we would have a contradiction. We get $\sim\pi>4$ 'for free', so to speak, from the meanings of the symbols in play.

But in fact the early calculus was not inconsistent in this sense – it never did give a wrong result. Certainly Berkeley was happy to admit this, writing that "Analysts arrive at truth". His objection was rather that they didn't know *how* they arrived at truth (§XXII). Many no doubt saw this as charitable, since some *did* see utter falsehoods as following from the early calculus. But in fact such falsehoods only follow once we *add* to the 'bare' calculus some justificational assumptions. For example, consider the following calculation:

(i) $x+o=x$
(ii) $x+2o=x+o$

---

[7] See Boolos *et al*, 2003, p.214. As briefly noted in §2, Russell also makes a comparison between modern arithmetic and the early calculus.
[8] The contradiction follows from the meanings of '1', '=' and '2'. I am considering the calculus as an *interpreted* proposition here, so the comparison is really with Peano arithmetic *as interpreted*.

(iii) $2o=o$
(iv) $2=1$

Equation (i) seems to follow from the calculus, since at the final stage of the procedure one drops all terms which are multiples of o (in the example in §2, recall, it appears from the final step that $10t+5o=10t$). (ii), (iii) and (iv) then follow from natural application of the algebraic laws, and one reaches the absurdity (contradiction) that 2=1. Has this then followed *from the calculus*? Not if the calculus is the bare algorithm suggested here. One might well object that the terms multiplied by o can be dropped *only in the context of the procedure*. Berkeley was certainly willing to accept this, since he made no such objection.

Others were not so charitable. Rolle, in the early 1700s in France, claimed to be able to prove that $o=0$, and also that the calculus led to mistakes. However, the calculation leading to $o=0$ stepped outside the accepted algorithm, as (i)-(iv) do, and the alleged mistakes were shown to follow from mistaken application (see §5.4, below). The calculus stood up to the challenge in every case; no mathematical falsehoods followed from the application of the algorithm.

5. The Level of Justification

If the calculus *had* given rise to a false result, then that *might* have been labelled a *logical* falsehood, in comparison with '2=1', and thus a contradiction. But of course this isn't the reason why the calculus has been widely labelled 'inconsistent'. What is really meant, following Berkeley, is that there is an inconsistency at the level of justification. The most obvious explanation of the moves made within the algorithm is inconsistent: o is necessarily taken to be *something*, and then at the final step it is taken to be *nothing*. But to be involved in a calculation at all o must be a quantity or a numerical constant, and therefore must remain the same throughout the calculation. Therefore, o appears to have contradictory properties, regardless of whether we give it a suggestive name like 'infinitesimal'.

This explanation certainly is inconsistent. However, if *nobody* genuinely proposed this explanation, then of what interest is it? What is surely required for an inconsistent theory to be of significance for the history and philosophy of science is a significant commitment to that theory. We can of course invent an inconsistent explanation of any method in science or mathematics, but this won't be of any philosophical interest, since nobody would sign up to it; everyone would sign up to a *consistent* explanation instead. But if there *was* no known consistent explanation, what then? This certainly wouldn't mean that everyone then signed up to the *inconsistent* explanation. It would rather mean that they didn't have *any* explanation, or that they only had an *inadequate* or *incoherent* explanation.

In this section I will be on the lookout for the latter three possibilities. When we look to the relevant individuals and communities with whom we associate the early calculus, did they indeed have inconsistent explanations, or did they rather have (a) no explanation at all, (b) an inadequate explanation, or (c) an incoherent explanation? Just because the most *obvious* explanation of the moves made within the procedure of the calculus is inconsistent should not mean that it counts as a default. For it to count there must be the appropriate *commitment*, just as we would expect for any theory or belief system.

So in sum the job of this section is to ask, did the relevant individual or community sign up to infinitesimals with their contradictory properties or not? Evidence that they did not will take two forms: (1) an explicit rejection of infinitesimals, and (2) attempts at an alternative justification.

5.1 Newton

In §3, above, I was keen to note that Newton presented the calculus as an algorithm, without any concomitant justification. This picture satisfies the majority of Newton's work on the calculus, but there are small but important passages where Newton *does* turn to justification. *Prima facie* it looks like Newton presents three different justifications, one in terms of 'infinitesimals', another in terms of 'fluxions', and a third in terms of 'primary and ultimate ratios' (Boyer, p.219). We might ask whether any of these are candidates for Berkeley's objections.

Of course in the search for inconsistency our attention turns to the infinitesimal justifications. Boyer for one has written that,

> Berkeley's objection to Newton's infinitesimal conceptions as self-contradictory was quite pertinent. (1949, p.226)

However, today there is a consensus in the literature that Newton's explanation of the calculus was much more sophisticated than this would suggest. Here is a taster:

> The common answer of the fluxionists was that Berkeley's logical criticism was applicable only to the differential method, which was employed by

> Newton merely to abbreviate the proofs. Newton's genuine method was the method of limits. (Guicciardini, 1989, p.44)

> [The criticisms were] uncharitable. Berkeley's reading presupposes the unfavourable interpretation of the argument. (Kitcher, 1983, p.239, fn.15)

> [Berkeley's] criticism may be a bit unfair to Newton, who can, as we have seen, be read as having some idea of using something like limits to replace the procedure of setting *o* equal to zero. (Lavine, 1994, p.24)

This consensus arises for various reasons. Certainly important are the fact that in Newton's first two *published* works using the calculus (*Principia*, 1687; *De Quadratura*, 1704) disclaimers tell us not to take talk of infinitesimals too seriously:

> Those ultimate ratios with which quantities vanish are not truly the ratio of ultimate quantities, but limits towards which the ratios of quantities decreasing without limit do always converge; and to which they approach nearer than by any given difference, but never go beyond, nor in effect attain to, till the quantities are diminished *in infinitum*. (Newton, *Principia* – cited in Edwards, 1979, p.225)

Citing another passage from the beginning of the *Principia*, and a passage from the introduction to *De Quadratura*, Edwards writes,

> In other words, Newton says, exposition in terms of indivisibles or infinitesimals is simply a convenient shorthand (but not a substitute) for rigorous mathematical proof in terms of ultimate ratios (limits). (1979, p.226, see also Kitcher, 1973)

Of course, Newton never gave such a rigorous proof, at least by modern standards. But then we should impute to him an *inadequate* explanation. Talk of infinitesimals is then just shorthand for this inadequate explanation.

Berkeley himself noted a passage in Newton's *De Quadratura* which said that, "errors are not to be disregarded in mathematics, no matter how small." However, instead of assuming that the final stage in the procedure was shorthand for something else, Berkeley saw Newton as breaking his own rule here. Newton is to blame to some extent; his *actual* justification in terms of the limits of ratios wasn't quite clear enough, nor was it very clear, perhaps, that this *was* his actual justification. But he was at least clear enough that we cannot impute to him Berkeley's blatantly contradictory justification.

5.2 Leibniz

Leibniz had faced his own Berkeley in the 1690s in the form of Nieuwentijdt. Although this was altogether a quieter affair, Leibniz was indeed moved to clarify his position on infinitesimals. The consensus today is that Leibniz, much like Newton, had a more sophisticated position than his critics recognised.

Right from his first publication on the calculus it is clear that Leibniz was concerned to avoid infinitesimals with their apparently contradictory properties. Whereas an extant draft of his *Nova Methodus* of 1684 speaks of infinitesimals, these have been removed in the published version (Mancosu, 1989, p.229; cf. Bos, 1974, p.62ff.). By 1689 he was corresponding with Johann Bernoulli (who we will meet in §5.4), arguing squarely against the thesis that infinitesimals 'exist'. Indeed it is clear that he, as Newton, has something like limits on his mind:

> For if we suppose that there actually exist the segments on the line that are to be designated by ½, ¼, …, and that all the members of this sequence actually exist, you conclude from this that an infinitely small member must also exist. In my opinion, however, the assumption implies nothing but the existence of any finite fraction of arbitrary smallness. (Cited in Rescher, 1955, p.113)

This attitude continues in 1701, when he wrote,

> [I]nstead of the infinite or the infinitely small, one takes quantities as large, or as small, as necessary in order that the error be smaller than the given error. (Cited in Bos, 1974, p.56)

And looking back in 1716 Leibniz wrote,

> When they [our friends] were disputing in France with Abbé Gallois, Fr. Gouge and others, I assured them that I didn't at all believe that there are quantities which are truly infinitesimal. (Cited in Mancosu, 1989, p.237)

It therefore seems fair to say that Leibniz used infinitesimals as 'meaningless symbols' (Bos, 1980, p.87). His *real* justification for the calculus was something more like Newton. On occasion he appealed to what he called the ‘principle of continuity’:

> In any supposed continuous transition, ending in any terminus, it is permissible to institute a general reasoning, in which the final terminus may also be included. (In Horvath, 1986, p.67)

But for the most part he simply *used* the calculus, discovering impressive new equalities like the one given above for $\pi/4$. What justificatory beliefs he did have were perhaps inadequate or to some extent incoherent, but surely not inconsistent.

We see at this point, then, that we cannot associate an inconsistent theory with either of the two founders of the calculus. However, this need not overly perturb those who believe the early calculus is inconsistent. Their claims will still be highly significant if it can be established that there was a significant *community* which held to an inconsistent theory. It is to the two most relevant mathematical communities we now turn, the English and the French.

5.3 The English

English thought in the early years of the calculus can, broadly speaking, be split into two different periods: before and after Berkeley’s *Analyst* of 1734. Before this time there appears to have been very little if any consensus on the foundations. It just was the case that the calculus could be applied as a method, an instrument to get interesting results and do calculations which had been very difficult or impossible up until that point. Thus the development of the calculus throughout this period was “almost automatic”, as Boyer has put it (1949, p.243). There was much to be done *with* the calculus without worrying about *why* it worked.

When thoughts did occasionally turn to justification and explanation opinions were varied. Here one possible source of confusion was the publication, in 1711, of Newton’s *De Analysi*. No wonder that Newton’s development of thought was lost on many, since this was Newton’s last publication on the calculus (in his lifetime), but displays some of his earliest thoughts, being written as it was around 1669. Here there is less discussion in terms of fluxions or prime and ultimate ratios, and more talk of the “infinitely small” (Boyer, pp.191,193f.; Lavine, p.16f.).

Another factor is Raphson's *The History of Fluxions* (1715), which confused fluxions with moments[9], and clearly considers Newton's fluxion theory to be just the same thing as Leibniz's differential calculus. There is no care given to definitions or foundations (Boyer, p.222). And in Stone's 1730 translation of l'Hôpital's textbook of 1696, fluxions and differentials are taken to be the same thing. According to Guicciardini, "equating fluxions and differentials was a common practice in this early period; and this created a great confusion in the terminology of early fluxionists." (1989, p.41).

Thus Lavine considers Berkeley's 1735 summary of the state of play to have been "pretty fair":

> Some fly to proportions between nothings. Some reject quantities because infinitesimal. Others allow only finite quantities and reject them because inconsiderable. Others place the method of fluxions on a foot with that of *exhaustions*, and admit nothing new therein. Some maintain a clear conception of fluxions. Others hold they can demonstrate about things incomprehensible. Some would prove the algorithm of fluxions by *reduction ad absurdum*; others *a priori*. Some hold the evanescent increments to be real quantities, some to be nothings, some to be limits. As many men, so many minds… Some insist the conclusions are true, and therefore the principles… Lastly several … frankly owned the objections to be unanswerable. (Cited in Lavine, 1994, p.25)

If this is a fair representation, then even if one position or another *was* inconsistent, we would be hard pushed to attach any significance to that position.

After 1734 there was increased debate on the foundations in an attempt to quash Berkeley's attack. A guiding influence here must have been the publication in 1736 of Newton's *Methodus Fluxionum*, espousing the method and terminology of fluxions. This was actually written some years before *De Quadratura*, but the significance of this seems to have been lost on the mathematical community at that time (Newton was no longer available to clarify, having died in 1727). The method and terminology of fluxions gradually took hold, and with it an explanation of the success of the calculus in terms of *kinematics*.

This position is summed up by the following quotation by Newton from 1693:

[9] Newton, 1714: "Fluxions and moments are quantities of a different kind. Fluxions are finite motions, moments are infinitely little parts." (Kitcher, 1973, p.40)

> I don't here consider mathematical quantities as composed of parts extremely small, but as generated by a continual motion.

So when trying to find the tangent at a point on a curved line, one should imagine a particle traversing such a line. The speed of the particle is then separated into its x and y components, such that we have dx/dt and dy/dt. One then considers a 'moment' of time *o* (infinitesimal in length), and the distance travelled by the particle in the x and y directions in that time, which are given by,

$$\frac{dx}{dt}o \quad \text{and} \quad \frac{dy}{dt}o\,.$$

One then reaches the derivative via the following equation:[10]

$$\frac{dy}{dx} = \frac{\dot{y}}{\dot{x}} = \frac{dy}{dt}\frac{dt}{dx}$$

Of course infinitesimals were still used, in a sense, but now they were infinitesimal *moments in time*. The explanation was meant to follow from the fact that the continuous flow of time is, in some sense, intuitive. As Boyer writes,

> This concept [the continuous flow of time] Newton seems to have felt was sufficiently impelling and so clearly known through intuition as to make further definition unnecessary. (1949, p.194)

Significantly, this explanation of the calculus was rife in the 1736 publication, just two years after *The Analyst*, and at a time of some concern vis-à-vis the foundations. It is no coincidence that in 1742 the most involved book on the foundations of the calculus at that time attempted to provide an axiomatic approach to the continuity of time and motion. This was Maclaurin's *Treatise of Fluxions*. Around this time a great number of textbooks were emerging on the calculus, and Maclaurin played a part in all of them. Guicciardini writes,

> In all these textbooks the reader was introduced in a preface or first chapter to the kinematic meaning of the concepts of the calculus; here Maclaurin was followed as the authority on these foundational aspects. (1989, p.59)

---

[10] In fact, although this looks rather different, the method is similar to that given above, in §2. See Kitcher, 1973, p.43f.

Is fluxion theory an inconsistent theory? Certainly ‘indivisible moments of time’ remained a central part of the theory, but the community made it clear that these were meant to be something different to infinitesimals. As Guicciardini writes,

> Since 1742 [Maclaurin] almost all the fluxionists accepted Maclaurin’s rejection of infinitesimals. (p.51)

In making their distinction between infinitesimals and ‘moments’ they could also draw on Newton, who wrote in *De Quadratura*,

> I have sought to demonstrate that in the method of fluxions it is not necessary to introduce into geometry infinitely small figures. (Boyer, p.202)

It’s not quite clear that this distinction solves the problems, though. The ‘moments’ appear to have contradictory properties just as the infinitesimals do. Kitcher has argued that,

> [T]he infinitesimal linelet is just Newton’s “moment” of a fluxion… The methods involving fluxions are supported by infinitesimal justifications. (1973, p.40)

Indeed, Kitcher argues that fluxions do not represent justification at all, but rather remain at ‘the algorithmic level’.

This may have been Newton’s conception of fluxions, but I maintain that there were many who *did* see fluxions as justificatory. Contrary to the ‘static’ infinitesimals, much was made of the fact that in the terminology of fluxions quantities ‘flowed’, and the passage of time was meant to play a central role. Compare James Jurin in his reply to Berkeley:

> Not too fast Good Mr. *Logician*. If I say, the increments now exist, and the increments do not now exist; the latter assertion will be contrary to the former, supposing now to mean the same instant of time in both assertions. But if I say at one time, the increments now exist; and say an hour after, the increments do not now exist; the latter assertion will neither be contrary, nor contradictory to the former, because the first now signifies one time, and the second now signifies another time, so that both assertions may be true. (Guicciardini, 1989, p.173f.)

In this passage it looks like the 'moments' themselves are considered to change, from something to nothing, even as the calculation progresses! That this sort of position was widely credited and taken on board by others is evidenced by the fact that Buffon referred to Jurin as "solid, brilliant, admirable." (Boyer, p.246).

Are we then to label this sort of position inconsistent? What is required, to repeat, is the derivation of a contradiction from what can be idenitifed as a set of widely accepted assumptions. In fact, there is some evidence that even Berkeley did not see an inconsistent position here, but instead a confused one. Several passages in *The Analyst* refer to "confusion", "obscurity" and "incomprehensible metaphysics" (e.g. §XLIX).

If a representative inconsistent theory *can* be reconstructed, little significance can be attached to it. On the main part, even after 1734, there was very little concern with the foundations of the calculus. Many textbooks appealed to Maclaurin, as noted, but it isn't even clear that the writers of these textbooks had *read* his *Treatise of Fluxions*. It has been written that Maclaurin was "praised by all, read by none." (Grabiner, 1997, p.394). Again, the real work was considered to be the *use* of the calculus. Here I repeat the quotation from Guicciardini, 1989:

> All these treatises, however advanced they may have been, did not introduce the student to the calculus as a theory […], but rather explained to him how to employ in geometry and mechanics a set of rules. (p.58f.)

In fact Lavine writes that in 1741 lack of rigor was widely considered to be a virtue (1994, p.25f.). It was written at that time,

> But the tables have turned. All reasoning concerned with what common sense knows in advance, serves only to conceal the truth and to weary the reader and is today disregarded. (*Ibid.*)

With this attitude in place we cannot expect to find inconsistency at the explanatory level in any meaningful sense.

5.4 The French

The most obvious source for an inconsistency claim lies with the French community. Following Leibniz's publication of 1684 a group of mathematicians arose which are sometimes referred to as the 'French infinitesimalists'. They are usually characterised as united in their belief in the *existence* of infinitesimals. As Mancosu writes,

> 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. (1989, p.234)

Here, then, lies a candidate for Berkeley's inconsistent theory of the calculus.

Within this community the following names are usually mentioned: Johann Bernoulli, Jakob Bernoulli, l'Hôpital, Malebranche, Varignon, Fontenelle, Saurin, Carré, Montmort, Guisnée and Grandi. However, we certainly should not equate the French community with the infinitesimalists. As Mancosu (1989) writes, in the first decade of the 18^{th} century "a bitter debate raged for about 6 years" within the Academy of Sciences of Paris. The opposing force was made up of Rolle, Ph. de la Hire, Galloys and Gouye, and to some extent Leibniz himself.[11] We might also note that many of the 'infinitesimalists' wrote little or nothing on the foundations of the calculus, and thus are fairly described as merely paying lip-service to infinitesimals. So already we are reduced to analysing the foundational claims of but a few individuals. And in fact, as we will see, it is far from clear that even these central figures signed up to Berkeley's inconsistent theory.

I take for example Jakob Bernoulli. Edwards writes,

> [W]hereas Leibniz himself was somewhat circumspect regarding the actual existence of infinitesimals, this appropriate caution was generally not shared by his immediate followers (such as the Bernoulli brothers), who uncritically accepted infinitesimals as genuine mathematical entities. (1979, p.265)

However, Jakob should not be bracketed together with his younger brother in this way. At times Jakob certainly *was* critical of infinitesimals – on at least one occasion he rejected infinitesimals outright, stating that a quantity smaller than any given magnitude is zero (Boyer, p.240). At other times he *did* appear to commit to infinitesimals (Boyer, p.240; Mancosu, p.238), but the fact that he vacillated surely reduces his significance within the community.

Secondly I turn to Varignon. He is particularly important, since during the 'bitter debate' at the beginning of the 18^{th} century Varignon stepped up for the

---

[11] Fontenelle wrote of Leibniz in 1716, "An architect constructed a building which was so audacious that even he didn't dare to live in it; and there are people who are more convinced of its solidity than he as, and who live in it without fear and, what is more, without accident." (Cited in Mancosu, p.237).

infinitesimalists, as a representative, to defend their position. Rolle provided the familiar objections:

> A quantity + or – its differential is made equal to the very same quantity, which is the same thing as saying that the part is equal to the whole… Sometimes the differentials are used as nonzero quantities and sometimes as absolute zeros. (Mancosu, p.230)

However, instead of defending the inconsistent properties of infinitesimals, Varignon rejected the suggestion that infinitesimals were constant quantities at all. He wrote to Leibniz in 1701,

> The Abbé Galloys, who is really behind the whole thing, is spreading the report here [in Paris] that you have explained that you mean by the "differential" or the "infinitely small" a very small, but nevertheless constant and definite, quantity… I, on the other hand, have called a thing infinitely small, or the differential of a quantity, if that quantity is inexhaustible in comparison with the thing. (Cited in Boyer, 1968, p.475)

As Boyer writes at this point, "The view that Varignon expressed here is far from clear, but at least he recognized that a differential is a variable rather than a constant." (Ibid.).

The view of differentials as variable quantities bears more than a passing resemblance to Newton's 'flowing moments'. Surprisingly enough, in an effort to prove that infinitesimals exist, Varignon turned directly to Newton's fluxions and an explanation in terms of kinematics (Mancosu, p.231). In fact, he "appealed to Newton's *Principia* as the source for a rigorous foundation of the calculus." (Ibid.). At one stage he even gives an explanation reminiscent of Newton's ultimate ratios, citing infinitesimals as "infinitely changing until zero". He goes on,

> [T]hey (the differentials) are always real and subdivisible to infinity, until in the end they have completely ceased to exist; and that is the only point at which they change into absolutely nothing. (Ibid.)[12]

[12] Cf. Cauchy, 1821: "When the successive numerical values of a variable decrease indefinitely so as to become smaller than any given number, this variable becomes what is called an *infinitesimal*, or infinitely small quantity." (Cited in Lakatos, 1966, p.48)

Within this web of 'incomprehensible metaphysics', as Berkeley might have put it, one thing is clear: Varignon is concerned to show that the inconsistency claim is not relevant to his position. So if Varignon is representative of the infinitesimalists, this community is *also* not guilty of committing to the inconsistent justification put forward by Berkeley. And he *does* seem to be representative – that the majority of the community took differentials to be variable rather than fixed quantities is stressed by Bos (1974, p.17). For example, in 1710 Leibniz wrote, "[T]he quantity *dx* itself is not always constant, but usually increases or decreases continually." (Ibid.).[13]

Finally I turn to Johann Bernoulli, arguably the key proponent of the existence of infinitesimals as genuine entities. We saw in §5.2 that he argued with Leibniz in 1689 that infinitesimals exist, and by 1691 he had written an essay on the 'differential calculus'. The very first line of this essay delivers the following postulate:

> 1. A quantity, which is diminished or increased by an infinitely small quantity, is neither diminished nor increased.[14]

To repeat: A quantity which *is* increased is *not* increased. Certainly here Bernoulli appears to have embraced contradiction. So here, finally, we have a candidate for the inconsistency claim. But does it only pertain to a single individual?

We must ask whether Bernoulli had any followers who explicitly took on board his contradictory justification. But in fact Bernoulli's 'Postulate 1' was known only to one individual, the Marquis de l'Hôpital. By 1691 Bernoulli had entered into a contract with the Marquis whereby it was agreed that, for a handsome sum, Bernoulli would communicate all his ideas directly to l'Hôpital and nobody else. So Bernoulli wasn't *allowed* to publish his Postulate![15] The postulate does come to light in 1696 in l'Hopital's famous textbook of 1696, *Analyse des infiniment petits*, but now in a slightly weaker form. L'Hôpital writes,

[13] Mancosu writes, "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." (1989, p.235). So we might say that the justfication is inconsistent with the application, but it certainly doesn't follow that the justification *itself* is inconsistent.

[14] "1. Eine Größe, die vermindert oder vermehrt wird um eine unendlich kleinere Größe, wird weder vermindert noch vermehrt." (Bernoulli, J. 1924 [1691/2]).

[15] For more on this relationship see Bos, 1980, pp.52 and 73.

> Grant that two quantities, whose difference 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. (Mancosu, p.227)

The emphasis on *use* is certainly intentional; l'Hopital himself had very little concern for foundations. As Boyer writes, "l'Hôpital did not in this book discuss the nature of the basic concepts of the calculus." (p.238). The textbook may have been remarkably popular, being reprinted in 1715, 1720, 1768 and 1781, and translated into English in 1730. But certainly we cannot conclude from this that Bernoulli's *justification* had a following.

What of the rest of the French infinitesimalists? Is it fair to present them as merely paying lip-service to infinitesimals, and as not even *having* a 'theory of the calculus' in any significant sense? What of the "final victory of the infinitesimal calculus" over its critics in 1707 (Mancosu, p.243)? What must be noted here is that by this time the battle had turned from justificatory issues to issues of application. Rolle now claimed that the new calculus led to mistakes and he brought forth example after example in an attempt to show this. Sadly for him, in every case it was shown that the new calculus *did* give the right answer, and that Rolle had made a mistake in applying it (Mancosu, pp.232-240). With this strategy in place there is little wonder that pressure mounted up against Rolle. In the face of the common threat, those who *had* justificatory concerns but no concerns over application (such as Leibniz) naturally merged with those who had no concerns of justification or application. Thus it is a shame that Rolle didn't take Berkeley's attitude, accepting that "Analysts arrive at truth" and focusing on the foundations. In this way the French community might have been pushed to clarify things a little further. As it happened, the 'victory' of 1707 was a victory of application, a victory of terminology and method, but not really a victory of justification. Leibniz for one still had his doubts, but by this stage he had squarely placed himself in the infinitesimal camp, and put foundational questions to one side.

There is clearly more to be said here, but I think I've said enough to severely reduce the significance of the claim that the early calculus was inconsistent. For starters, during the most significant period of foundational disputation the French community was sharply divided. But then even within the infinitesimal camp there was division, between those who apparently took infinitesimals to be constant quantities (Johann Bernoulli) and those who took them to be variables (Varignon). Bos (1974) seems to suggest that the majority sided with Varignon. And it goes without saying that even Johann Bernoulli was much more concerned with application than justification.

6. Conclusion

Johann Bernoulli's theory of the calculus was, perhaps, inconsistent. But can we impute this same theory to the rest of the French infinitesimalists? Varignon provides the prime example of how one can 'believe that infinitesimals exist', and thus be an 'infinitesimalist', without being subject to Berkeley's inconsistency claim. In addition there were several within the French community who rejected infinitesimals as genuine entities, including Leibniz. Taking a still wider perspective, we can say that the inconsistency claim also falls short with the English community and Newton in particular. It should surely be noted, then, that very little significance can be attached to any claim that the early calculus was an inconsistent theory.

In addition one may focus on the word 'theory' here, and question whether Bernoulli's position is really an inconsistent *theory*. At the level of justification 'theory' is being used to mean something like 'explanation'. But it is hard to accept that the blatant contradiction Bernoulli puts forward can even *count* as an explanation. Even those who have defended the possibility of 'Believing the Self-Contradictory' (Williams, 1982) do not mean by 'self-contradictory' *blatant* contradictions of the sort Bernoulli puts forward. I am inclined to agree with Gordon Belot, who in another context has recently written "[It] does not deserve to be called a theory precisely because it is inconsistent." (2006, p.16). If Bernoulli's position is a theory, 'theory' is being used in a rather peculiar sense.

But even if we accept that Bernoulli's justification counts as a theory, it isn't clear that it deserves a place on the lists of 'inconsistent scientific theories' noted in §1. For example, the issue with Bohr's theory of the atom – which also graces many lists of 'inconsistent scientific theories' – is how reasoning can continue, given ECQ (recall §2, above). A typical paraconsistent approach is presented by Bryson Brown (1992). But with the early calculus there is no such issue, since in practice there *is* no reasoning with the inconsistent theory in question. Rather, all the reasoning is done at the algorithmic level. Thus, no paraconsistent account is required to explain 'how reasoning continues'; there is no work here for the paraconsistent logicians to do.[16]

To sum up, the situation is as follows. To identify a theory as inconsistent is all very well, but for that identification to be of any interest it must be clear that

[16] Or very little. Jakob Bernoulli did propose at one point that infinitesimals could be adopted such that x+dx=x if one rejected the principle that 'equals subtracted from equals give equals' (Mancosu, p.238, Boyer, 1949, p.240). But this approach was never seriously pursued.

there is a genuine commitment to that theory by a relevant community. Now the inconsistency claim here is at the level of justification, whereas the majority of the commitment remained at the algorithmic level, so already the significance of the claim is significantly reduced. But then at the level of justification the actual commitments are better described as inadequate (not matching actual practice), or incoherent (invoking nebulous concepts such as 'evanescent increment'). To bring the charge of inconsistency via Berkeley is to read selectively. As the passages already cited indicate, the take-home message of the *The Analyst* is that there is no adequate, coherent explanation of the calculus. Little doubt that even Berkeley would stop short of terming the calculus an 'inconsistent theory'.

References

Belot, G. (2006): 'Is Classical Electrodynamics an Inconsistent Theory?', forthcoming in *Canadian Journal of Philosophy*.

Berkeley, G. (1734): *The Analyst*, online edition.

Bernoulli, J. (1924 [1691/2]): *Die Differentialrechnung / von Johann Bernoulli. nach der in der Basler Universitätsbibliothek befindlichen Handschrift / übersetzt, mit einem Vorwort und Anmerkungen versehen von Paul Schafheitlin*. Leipzig: Akademische Verlagsgesellschaft.

Boolos, G., Burgess, J. and Jeffrey, R. (2003): *Computability and Logic*, 4th edition, Cambridge University Press.

Bos, H. (1974): 'Differentials, Higher-order Differentials and the Derivative in the Leibnizian Calculus', *Archive for History of Exact Sciences* **14**, pp.1-90.

_______ (1980): 'Newton, Leibniz and the Leibnizian Tradition', in I. Grattan-Guiness (ed.) *From the Calculus to Set Theory, 1630-1910*, pp.49-93. Gerald Duckworth & Co., London, 1980.

Boyer, C.B. (1949): *The History of the Calculus and its Conceptual Development*, Dover, New York.

_______ (1968): *A History of Mathematics*, New York, Chichester: John Wiley & Sons.

Brown, B. (1992): 'Old Quantum Theory: A Paraconsistent Approach', *PSA 1992*, Vol.2, 397-411.

Child, J. (1920): *The Early Mathematical Manuscripts of Leibniz*, Chicago-London.

Da Costa, N.C.A. and French, S. (2003): *Science and Partial Truth*, Oxford.

Edwards, C.H. (1979): *The Historical Development of the Calculus*, Springer-Verlag, New York.
Feyerabend, P. (1978): ‘In Defence of Aristotle’, in G. Radnitsky and G. Anderson, *Progress and Rationality in Science*, Reidel, Dordrecht.
Grabiner, J. (1983): ‘The Changing Concept of Change: The Derivative from Fermat to Weierstrass’, *Mathematics Magazine*, vol.56, no.4, pp.195-206.
_______ (1997): ‘Was Newton’s Calculus a Dead End? The Continental Influence of Maclaurin’s Treatise of Fluxions’, *The American Mathematical Monthly,* vol.104, no.5, pp.393-410.
Guicciardini, N. (1989): *The Development of Newtonian Calculus in Britain, 1700-1800*, Cambridge University Press.
Horvath, M. (1986): ‘On the Attempts made by Leibniz to Justify his Calculus’, *Studia Leibnitiana* **18**(1), pp.60-71.
Kitcher, P. (1973): ‘Fluxions, Limits and Infinite Littlenesse. A Study of Newton’s Presentation of the Calculus’, *Isis* **64**, No.1, pp.33-49.
_______ (1983): *The Nature of Mathematical Knowledge*, Oxford University Press.
Lakatos, I. (1966): ‘Cauchy and the Continuum: The Significance of Non-Standard Analysis for the History and Philosophy of Mathematics’, *Mathematical Intelligencer* **1** (1978): pp.151-61.
Lavine, S. (1994): *Understanding the Infinite*, Harvard University Press.
Mancosu, P. (1989): ‘The Metaphysics of the Calculus: A Foundational Debate in the Paris Academy of Sciences, 1700-1706’, *Historia Mathematica* **16**, pp.224-248.
_______ (1996): *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*, OUP.
Priest, G. and Routley, R (1983): *On Paraconsistency*, Research Report 13, Logic Group, Research School of Social Sciences, Australian National University.
Rescher, N. (1955): ‘Leibniz’ Conception of Quantity, Number, and Infinity’, *The Philosophical Review* **64**, no.1, pp.108-114.
Shapere, D. (1984): *Reason and the Search for Knowledge*, Dordrecht: Reidel.
Struik, D. (1967): *A Source Book in Mathematics, 1200-1800*, Cambridge, MA: Harvard University Press.
Williams, J. (1982): ‘Believing the Self-Contradictory’, *American Philosophical Quarterly*, vol.19, no.3.