Ghosts of Departed Errors: The Early Criticisms of the Calculus of Newton and Leibniz

Eugene Boman

Mathematics Seminar, February 15, 2017

Eugene Boman is Associate Professor of Mathematics at Penn State Harrisburg.

Calculus is often taught as if it is a pristine thing, emerging Athena-like, complete and whole, from the head of Zeus. It is not. In the early days the logical foundations of Calculus were highly suspect. It seemed to work well and effortlessly, but why it did so was mysterious to say the least and until this foundational question was answered the entire enterprise was suspect, despite its remarkable successes. It took nearly two hundred years to develop a coherent foundational theory and, when completed, the Calculus that emerged was fundamentally different from the Calculus of the founders.

To illustrate this difference I would like to compare two proofs of an elementary result from Calculus: That the derivative of $\sin(x)$ is $\cos(x)$. The first is a modern proof both in content and style. The second is in the style of the Seventeenth and Eighteenth centuries.

A MODERN PROOF THAT THE DERIVATIVE OF $sin(x)$ IS $cos(x)$

A modern proof that $\frac{d(\sin(x))}{dx} = \cos(x)$ is not a simple thing. It depends on the theory of limits, which we will simply assume, and in particular on the following lemma, which we will prove.

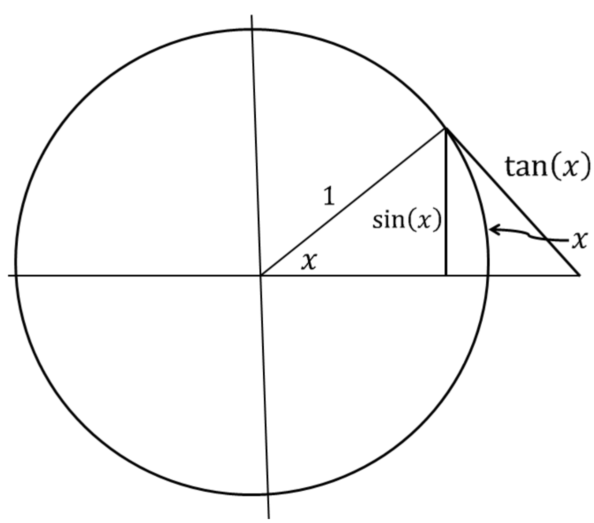


Figure 1

__**Lemma:**__ $\lim_{x\to 0} \frac{\sin(x)}{x} = 1.$

__**Proof of Lemma:**__ Since in Figure 1 the radius of the circle is one we see that radian measure of the angle and the length of the subtended arc are equal. Thus

$$\sin(x) < x < \tan(x),$$

and so

$$1 < \frac{x}{\sin(x)} < \frac{1}{\cos(x)},$$

or

$$1 > \frac{\sin(x)}{x} > \cos(x).$$

From the Squeeze Theorem (also called the Sandwich Theorem) we conclude that

$$\lim_{x\to 0} \frac{\sin(x)}{x} = 1.$$

QED

We note for later use that an immediate corollary of this lemma is:

<u>Corollary:</u> $\lim_{x\to 0} \frac{\cos(x)-1}{x} = \lim_{x\to 0} \frac{\cos^2(x)-1}{x(\cos(x)+1)} = \lim_{x\to 0} \frac{\sin(x)}{x} \frac{\sin(x)}{\cos(x)+1} = 0.$

Now that we have the tools we will need, we can prove our result: $\frac{d(\sin(x))}{dx} = \cos(x).$

By definition

$$\frac{d(\sin(x))}{dx} = \lim_{h\to 0} \frac{\sin(x+h) - \sin(x)}{h},$$

and from the Addition Formula for $\sin(x)$ we have:

$$\frac{d(\sin(x))}{dx} = \lim_{h\to 0} \frac{\sin(x)\cos(h) + \cos(x)\sin(h) - \sin(x)}{h},$$

from which

$$\frac{d(\sin(x))}{dx} = \lim_{h\to 0} \frac{\sin(x)\,(\cos(h)-1)}{h} + \lim_{h\to 0} \frac{\cos(x)\sin(h)}{h}$$

or

$$\frac{d(\sin(x))}{dx} = \sin(x)\left[\underbrace{\lim_{h\to 0} \frac{(\cos(h)-1)}{h}}_{\boxed{=0}}\right] + \cos(x)\left[\underbrace{\lim_{h\to 0} \frac{\sin(h)}{h}}_{\boxed{=1}}\right].$$

So

$$\frac{d(\sin(x))}{dx} = \cos(x).$$

QED

Most modern Calculus textbooks use some variant of this argument to prove that the derivative of the $\sin(x)$ is the $\cos(x)$. There are a couple of aspects of this proof worth noting.

First, this proof is complicated. Even assuming the entirety of the theory of limits (which we did), we still had to prove a non-obvious lemma and its corollary before we could even begin to prove the theorem.

Second, while both valid and rigorous, this proof is not enlightening. None of the steps involved arises naturally and the whole thing feels a bit contrived. Indeed, the only aspect of this proof which recommends it, is that it is, in fact, both valid and rigorous. Otherwise it is actually a rather painful argument to endure. No one in the seventeenth or eighteenth centuries would have done it this way. This proof is quite modern in every way. The theorem would not have been proved in this way until the late-nineteenth century.

SECOND PROOF THAT THE DERIVATIVE OF $sin(x)$ IS $cos(x)$

The following proof is very much in the style of Newton and Leibniz, the original inventors of Calculus. To be clear, this precise proof is not found in the works of either man, but it is similar to passages in the works of both.

Again we start with the unit circle and a right triangle as in Figure 2. This time, however we let the angle x vary by a tiny amount we will call dx.

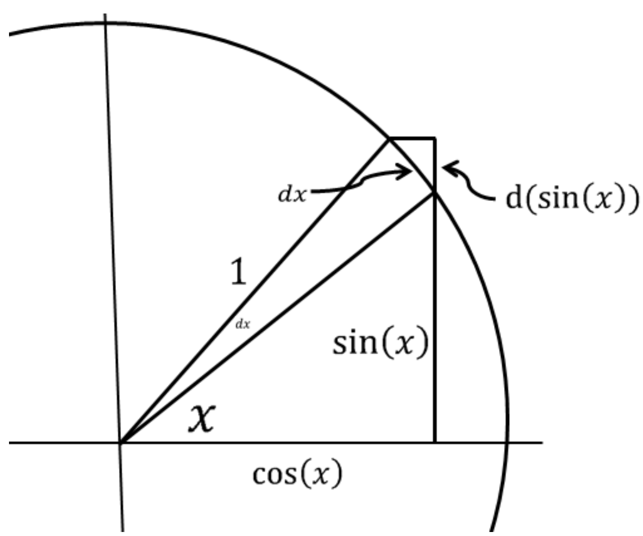


Figure 2

Consider for a moment what the quantity dx means. The originators of Calculus quite explicitly thought of curves as being comprised of infinitely many, infinitely small straight lines called infinitesimals or differentials. Leibniz in particular denoted them with the "d" operator, and we will adopt his notation here. So dx is an *infinitely* small change in the angle x. As before, we see that since the radius of the circle is one, then the radian measure of the subtended arc is also dx. Hence, the smaller triangle in the diagram (1) is *infinitesimally* small, (2) is actually a triangle and, (3) dx is its hypotenuse. Leibniz called this a “differential triangle” since all of its sides are differentials. The vertical leg of the differential triangle is an infinitesimal extension of $\sin(x)$, hence it is denoted $d(\sin(x))$.

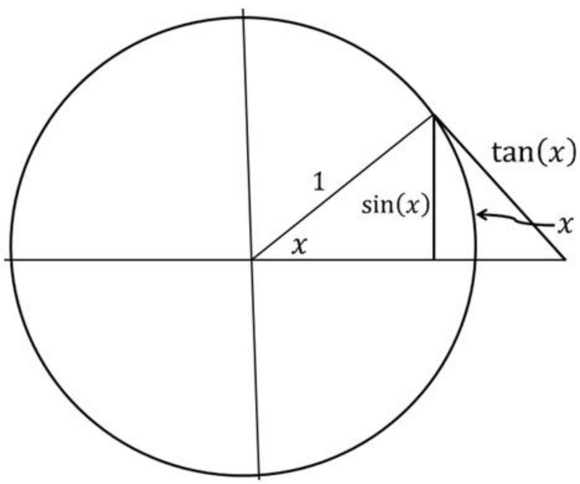
All of which is just to say that the two triangles in the diagram are similar and the lengths of the legs are as given in the diagram. By the properties of similar triangles from plane geometry we have

$$\frac{d(\sin(x))}{dx} = \frac{\cos(x)}{1} = \cos(x)\,.$$

QED

This is the proof I use when I teach calculus. It is quick. It is easy. It is intuitive inasmuch as it does not involve the theory of limits or any advanced trigonometry. All we need is an understanding of similar triangles from elementary geometry and an intuitive notion of differentials. Otherwise, this proof is simplicity itself.

Let us look at both proofs together:

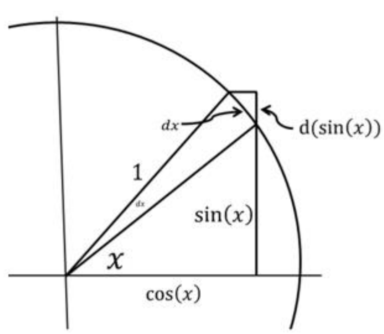


$$\frac{d(\sin(x))}{dx} = \lim_{h\to 0}\frac{\sin(x)\cos(h) + \cos(x)\sin(h) - \sin(x)}{h},$$

$$\frac{d(\sin(x))}{dx} = \lim_{h\to 0}\frac{\sin(x)\,(\cos(h)-1)}{h} + \lim_{h\to 0}\frac{\cos(x)\sin(h)}{h}$$

$$\frac{d(\sin(x))}{dx} = \sin(x)\left[\underbrace{\lim_{h\to 0}\frac{(\cos(h)-1)}{h}}_{\boxed{=0}}\right] + \cos(x)\left[\underbrace{\lim_{h\to 0}\frac{\sin(h)}{h}}_{\boxed{=1}}\right].$$

$$\frac{d(\sin(x))}{dx} = \cos(x).$$



$$\frac{d(\sin(x))}{dx} = \frac{\cos(x)}{1} = \cos(x)$$

Figure 3

Even if you do not understand all of the details, when these proofs are laid out together as

In Figure 3, it is absolutely clear that the older proof is much, much simpler. This raises the question: Why did mathematicians give up the simpler (seventeenth century) method of proof in favor of the more complicated (nineteenth century) method?

An easy (and incorrect) answer goes like this: Mathematicians prefer complexity for its own sake, so naturally we'd prefer the more complex proof. But that is a caricature. Mathematicians actually crave simplicity and elegance. We only accept complexity when it is forced upon us.

IN THE BEGINNING . . .

Calculus was invented in the late-seventeenth century, first by the Englishman Isaac Newton and some years later by the German Gottfried Wilhelm Leibniz. Both of these men are fascinating in their own right, and for far more than the invention of Calculus, but our focus is on their mathematics.

It took a few years for the word to get around, but once the invention of Calculus was generally known, science had a great new toy that did absolutely everything. Calculus gave us:

1. A way to precisely describe the motion of the moon,
2. And the tides.
3. A way to approximate, via infinite series, quantities which had hitherto been incalculable.
4. A way to construct tangent lines to analytic curves.

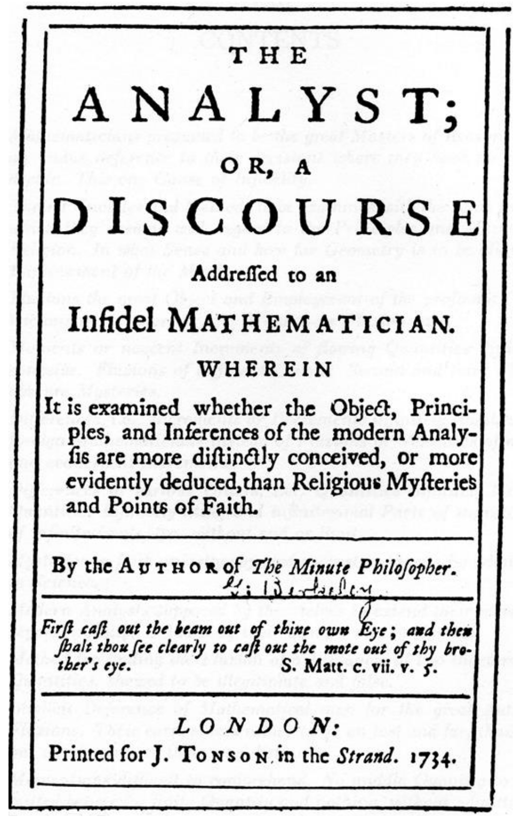
This last item is more important than it may appear. Descartes once said that if he could solve the tangent problem, he could solve all the outstanding scientific problems of the age. This is an overstatement, but not by much. When a method for finding the line tangent to a curve was made available through Calculus, problems that had previously been quite difficult became easy and a host of problems which had previously been wholly intractable became at least manageable. The invention of Calculus was a big deal.

Throughout the eighteenth century, scientists used this marvelous new toy to solve, or at least address in a meaningful way, evermore complex and difficult scientific questions. This was the Age of Reason, the Scientific Revolution, and it was fueled, at least in part, by the invention of Calculus. Moreover, all that was needed to make Calculus work was a working knowledge of plane Geometry and a few simple assumptions about the nature of curves (e.g., that they are composed of infinitesimals).

In particular, in order for Calculus to work, and by extension in order for anything that Calculus could explain to work, there was no need for the controlling influence of a Deity. Because so much could apparently be explained without resorting to a Deity, some people began to ask if there was truly a need for a Deity at all. They were known, somewhat paradoxically, as Deists. In England they were also known as Freethinkers and their stated goal was to explain the world with rational thought.[1]

But rational thought is exactly how Newton and Leibniz had invented Calculus. They did not do experiments. They did not pore over sacred, ancient texts. They looked at the world and they thought about it rationally; they applied their ability to reason. The Calculus they invented was seen as the quintessential product of pure reason. They sat, they thought, and in so doing they invented this marvelous new toy, Calculus, which seemed at the time destined to explain everything.

If they could do that just by sitting and reasoning, what else might we accomplish just by thinking? What if we did this with politics? Wouldn’t it be grand if our politics were rational? Perhaps financial markets could be understood this way? And what about religion? Religion, specifically Christianity, was the foundational institution of the time. Wouldn’t it be great if religion could be understood rationally?


THE

ANALYST;

OR, A

DISCOURSE

Addreſſed to an

Infidel MATHEMATICIAN.

WHEREIN

It is examined whether the Object, Principles, and Inferences of the modern Analyſis are more diſtinctly conceived, or more evidently deduced, than Religious Myſteries and Points of Faith.

By the AUTHOR of *The Minute Philoſopher.*

Firſt caſt out the beam out of thine own Eye; and then ſhalt thou ſee clearly to caſt out the mote out of thy brother's eye. S. Matt. c. vii. v. 5.

LONDON:

Printed for J. TONSON in the *Strand.* 1734.

Figure 4: The title page of *The Analyst*

The Deists argued that Christianity had the following problems, among others, which would have to be addressed before religion could become rational:

1. Mysteries are accepted without examination: e.g., Virgin birth, and the existence of a soul.
2. The pronouncements of established authorities were accepted solely by virtue of that authority: e.g., the Bible, St. Augustine, or St. Thomas Aquinas.
3. Most damning of all, religion was illogical.

They argued further that science did not have these flaws because it is a rational discipline, and they held up “the modern analysis” (Calculus) as emblematic of science as a whole because it had been created from pure reason.


Figure 5: Bishop Berkeley

George Berkeley, the bishop of Cloyne, Ireland, was horrified at the treatment his religion was receiving from the Freethinkers, many of whom were mathematicians. He responded to them in a 1734 pamphlet (Figure 4) with the ponderous title, *The Analyst, or a Discourse Addressed to an Infidel Mathematician, wherein it is examined whether the Object, Principles, and*

Inferences of the modern Analysis are more distinctly conceived, or more evidently deduced, than Religious Mysteries and Points of Faith.[2] Most people refer to it simply as *The Analyst.* Although cumbersome, Berkeley's title is informative. Upset that his religion had been attacked, Berkeley vigorously defended it. He did this by turning the arguments of the Freethinkers back against the Calculus of which they are so proud. In essence he said, let us take a look at this Calculus you have created and see how it fares when held to the same standard you have set up for religion.

Just in case anyone might have missed the point, on the title page he also quotes the book of Matthew, chapter 7, verse 5 from the New Testament: "First cast out the beam out of thine own Eye; and then shalt thou see clearly to cast out the mote out of thy brother's eye."

Very early in *The Analyst* he repeated his goal:

> I shall claim the privilege of a Free-Thinker; and take the Liberty to inquire into the Object, Principles and Method of Demonstration admitted by the Mathematicians of the present Age, with the same freedom that you presume to treat the Principles and Mysteries of Religion; to the end, that all Men may see what right you have to lead, or what Encouragement others have to follow you.

Although Berkeley's intent was simply to defend his church, *The Analyst* was widely seen in England as a critique not only of Newton's Calculus, but of Newton personally. In those days Newton was a "rock star," a "demigod." If you wanted to be taken seriously as a scientist in England, you did not criticize or contradict Newton. He was famous, he was powerful, and he did not take criticism well. He was, quite literally, a bigwig.


Figure 6: Isaac Newton

But this was exactly the problem as far as Berkeley was concerned. Too many people were accepting Newton's word simply because he was Newton, and far too many people were losing their Christian faith as a result. And hadn't the Freethinkers argued that blind deference to authority was a flaw in religion, but not in their science?

Incidentally, Newton was also a deeply religious man. He would have been as appalled as Berkeley at his countrymen turning away from Christianity, especially if they cited his work as the cause, but he had died in 1727 so he was in no position to weigh in on the matter.

A DERIVATION OF THE PRODUCT RULE IN THE STYLE OF NEWTON OR LEIBNIZ:

The following derivation of the Product Rule is very similar to the approach both Newton and Leibniz would have used. Arguments very much like it appear in their works, so it will serve to illuminate their methods, as well as the nature of Berkeley's specific criticisms.

If x and y are variable quantities, then their product at a given time can be visualized as the area $A = xy$ in Figure 7. If we allow time to tick forward a bit, x will be incremented by dx and y by dy. Clearly then dA, the change in the area of A, will be the L-shaped region around the top and right side of A in Figure 7 so that:

$$dA = x\,dy + y\,dx + (dx)(dy)$$

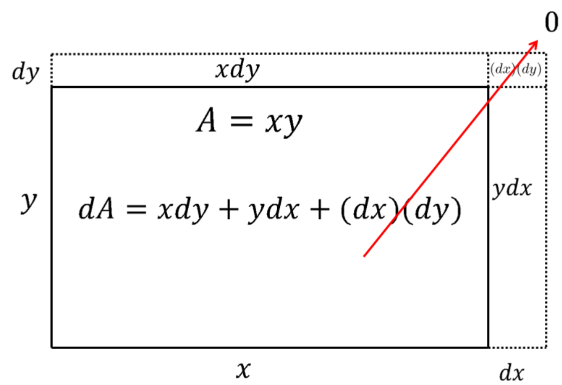


Figure 7

as shown. Take particular notice of the quantity $(dx)(dy)$. Both Newton and Leibniz knew by other means that the correct Product Rule was actually

$$dA = x\,dy + y\,dx$$

so they needed for the troublesome term, $(dx)(dy)$ to go away. To accomplish this they reasoned as follows. The quantity $(dx)(dy)$ is the product of two infinitely small quantities, two "infinitesimals." Every other term in the formula above is either an infinitesimal itself (dA), or the product of an ordinary number and an infinitesimal $(x\,dy)$. Clearly, according to Newton and Leibniz, the product of the two infinitely small

Figure 7

numbers dx and dy is so small compared to the other terms that it is unworthy of consideration and may thus be safely discarded. That is, we have:

$$dA = x\,dy + y\,dx,$$

as desired. Essentially, they simply threw away the troublesome term because it is too small to matter.

Faced with such sloppy reasoning Berkeley was scornful. In the first place, he said, although the quantity $(dx)(dy)$ may be very small it is manifestly not zero, and thus cannot simply be ignored when it is inconvenient. In the second place, he pointed out that Newton himself had said that this is not a valid way to reason by citing the introduction of Newton's book *The Quadrature of Curves*, wherein he wrote, "The very smallest of Errors in mathematical Matters are not to be neglected."[3] Thus, said Berkeley, this was a clear case of illogical reasoning by "the modern analysts," and even they recognize it as such.

Then, in the most famous line from *The Analyst* (the line from which the title of this article is drawn), he questioned the use of infinitely small numbers, what Leibniz called infinitesimals and Newton called evanescent increments, in any context at all. ". . . And what are these same evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them the ghosts of departed quantities?"

Berkeley made his case. Illogical reasoning and the unqualified acceptance of mysteries were the two other criticisms the Deists offered against religion. He made a strong case that the "modern analysts" also accept mysteries in the form of infinitesimals, and illogical reasoning to get the results they want. Therefore, they were in no position to criticize religion on these points. Berkeley went on to say, “Such reasoning as this for Demonstration, nothing but the obscurity of the Subject could have induced the great Author of the Fluxionary Method [Newton] to put upon his followers. And nothing but an implicit deference to authority could move them to admit . . . .”

Berkeley offered several more examples of (1) mysteries, (2) deference to authority, and (3) illogical reasoning found in the Calculus, as it was understood at the time. Some points were stronger than others, but we need not enumerate them here. As a whole, he made a strong case.

However, now Berkeley had a problem. He seems to have made his case: Mysteries, illogical reasoning, and implicit deference to established authority are just as prevalent among “the modern analysts” as among religious folk. As he put it:

> I have no Controversy about your Conclusions. . . . It must be remembered that I am not concerned about the truth of your Theorems, but only about the way of coming at them; whether it be legitimate or illegitimate, clear or obscure, scientific or tentative. To prevent all possibility of your mistaking me, I beg leave to repeat and insist, that I consider the Geometrical Analyst only as a Logician, i.e. so far forth as he reasons and argues; and his Mathematical Conclusions, not in themselves, but in their Premises; not as true or false, useful or insignificant, but as derived from such Principles, and by such Inferences.

Calculus, according to Berkeley, was a house built on sand. A minor shift of the sand would bring the whole structure tumbling down. However, the problem is that it doesn’t. Calculus works! Even Berkeley agreed that Calculus works. To be sure, this is not enough to conclude that the method is valid and he said so quite pointedly. “I say that in every other Science Men prove their Conclusions by their Principles, and not their Principles by the Conclusions.” However, his argument would be stronger if he could show how the flawed principles can lead to correct conclusions. This he did next.

BERKELEY’S COUP DE GRACE: THE COMPENSATING ERRORS ARGUMENT:

Berkeley next argued that “the modern analysts” got correct results from false assumptions because they made two errors that compensated for each other. This was a remarkable claim, made even more so by the fact that he was able to demonstrate exactly what the errors were by borrowing the diagram in Figure 8 from Newton’s *Quadrature of Curves.* The problem was to find the line BT that is tangent to the graph of the equation $y^2 = x$ (in blue) at point

Figure 9

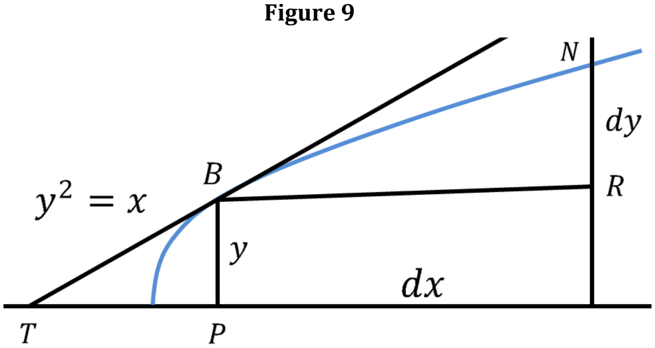


Figure 8

B. In practice, this is equivalent to finding the length of the line segment PT. For, if $P, B,$ and the length of PT are known, then T can be constructed. Once T has been constructed the line segment BT can also be constructed. The same problem would be posed in modern terms as, "Find the slope of the line tangent to $y^2 = x$ at the point B" because the slope of the tangent line is more useful than the line itself.

Newton's construction of PT was straight forward. He assumed that dx is infinitely small, so that the curved line segment BN is also infinitely small. Hence, it is actually a straight line. In that case, triangle TPB and triangle BRN are similar. Therefore $\frac{RN}{RB} = \frac{PB}{PT}$, so that after we make the substitutions $PB = y, RN = dy,$ and $RB = dx,$ as indicated in Figure 8 we have

$$PT = \frac{y\,dx}{dy}.$$

Next, since $y^2 = x,$ Newton's Calculus (that is, modern Calculus) immediately gives

$$dy = \frac{dx}{2y},$$

from which,

$$PT = \frac{y\,dx}{\frac{dx}{2y}} = 2y^2.$$

Again, Berkeley would have nothing to do with this. He pointed out that however small the curved line BN may be, it is still curved and as a result the correct proportion to begin with is not Newton's $\frac{RN}{RB} = \frac{PB}{PT}$, but rather $\frac{RN+z}{RB} = \frac{PB}{PT}$. Making the same substitutions as before and solving for PT gives:

$$PT = \frac{y\,dx}{dy + z}.$$

Newton's argument misses the z term entirely. This is the first error.

Next if we increment x by dx and y by dy then since $y^2 = x$ we have

$$(y + dy)^2 = y^2 + 2y\,dy + dy^2 = x + dx.$$

Solving this for dy gives

$$dy = \frac{dx}{2y} - \frac{(dy)^2}{2y}.$$

Comparing this with Newton's computation of dy, we see that Newton has also missed the term $\frac{(dy)^2}{2y}$. This is the second error.

Berkeley next invokes Proposition 33 from the book *On Conics* by Apollonius, a Greek mathematician of antiquity, to show that $z = \frac{(dy)^2}{2y}$. The details of that calculation need not trouble us. What is important here is when we make these substitutions into Berkeley's computation of PT above we get:

$$PT = \frac{y\,dx}{dy+z} = \frac{y\,dx}{\left[\frac{dx}{2y} - \frac{(dy)^2}{2y}\right] + \frac{(dy)^2}{2y}} = \frac{y\,dx}{\frac{dx}{2y}} = 2y^2,$$

just as Newton did. Notice that Newton's two purported errors are equal and opposite in sign so their sum is zero.

Berkeley made a strong case, but it was not quite as strong as he thought. After all, he only showed that the errors cancel out for this one particular curve. Nevertheless, Ivor Grattan-Guiness showed in 1969 that Berkeley's approach can be used to show that these "compensating errors" occur for any real analytic function.[4]

Berkeley showed that "the modern analysts" were guilty of the same errors that they attributed to religion and he explained that they obtained correct answers by virtue of making complementary errors, not by the correctness of their methods. Therefore, they are not in a position to criticize religion. He then declared victory:

> All these points, I say, are supposed and believed by certain rigorous exactors of evidence in religion, men who pretend to believe no further than they can see . . . you, who are at a loss to conduct your selves, cannot with any decency set up for guides to other Men. . . . .With what appearance of Reason shall any man presume to say that Mysteries may not be Objects of Faithe, at the same time that he himself admits such obscure Mysteries to be the Objects of Science? . . . He who can digest a second or third Fluxion, a second or third Difference, need not, methinks, be squeamish about any point in Divinity.

THE CONTROVERSY: WHO SAID WHAT?

After *The Analyst* was published in 1734, the response was immediate and largely ineffectual. Within months, two men had published purported refutations of Berkeley's points. He replied to both, they each counter-replied, and finally, Berkeley had his last word on the matter in a twenty-four page letter to his good friend Thomas Prior, which was eventually published with the title *Reasons for not Replying to Mr. Walton's Full Answer*.[5] A complete list of the titles of the entire exchange from 1734 to 1735 in the order that they were published follows:

- George Berkeley, *The Analyst*.[6]
- Philalethes Cantabrigiensis [James Jurin], *Geometry No Friend to Infidelity: or a Defence of Sir Isaac Newton and the British Mathematicians*.[7]

- J. Walton, *A Vindication of Sir Isaac Newton's Principles of Fluxions Against the Objections Contained in the Analyst.*[8]
- George Berkeley, *A Defense of Free-Thinking in Mathematics*.[9]
- James Jurin; *The Minute Mathematician or, The Free-Thinker no Just-Thinker*.[10]
- J. Walton; *The Catechism of the Author of the Minute Philosopher Fully Answer'd*.[11]
- George Berkeley; *Reasons for not Replying to Mr. Walton's Full Answer*.[12]

James Jurin, the first respondent, was and remains quite famous, but not as a mathematician. He was a medical doctor. However, he had served as the Secretary of the Royal Society while Newton was the President of that body. As a friend and colleague of the deceased Newton, Jurin was predisposed to take offense to Berkeley's comments on Newton's behalf and he did so, vehemently and dramatically. At one point Jurin accused Berkeley of attempting, by his criticism, to bring the Spanish Inquisition to England. His mathematical arguments were only slightly more thoughtful.

Of J. Walton we have three facts: (1) his first initial was J, (2) his last name was Walton, and (3) he wrote the pamphlets listed above. Otherwise his identity is a mystery. In *In Defense of Free-Thinking in Mathematics*, Berkeley referred to him as "this Dublin professor." However, there is no record of anyone with the surname "Walton" on the faculty of Trinity College, which was the only institution of higher learning in Dublin at the time. In 1994, Ruth Wallis made a fairly convincing circumstantial argument that his first name was Jeremiah and that he was one of the founding members of the Dublin Royal Society.[13] If this is true, then he died a few years after these pamphlets were published.

This exchange is notable primarily for the weakness of the arguments brought against Berkeley's position. Neither Jurin nor Walton was able to mount a convincing refutation of Berkeley's arguments. Newton or Leibniz might have been able, but unfortunately both men were dead, Leibniz in 1707, and Newton in 1727.

Every author of a Calculus text written in the latter half of the eighteenth century paid homage to *The Analyst,* or at least they paid it lip service. At minimum, they would acknowledge that Berkeley's objections were valid and deserved to be addressed, even if a particular author did not actually address them himself.

Figure 10: Colin Maclaurin

Two of the most notable of those who did try to address Berkeley's arguments were the Scottish mathematician Colin Maclaurin (Figure 9), and the Englishman Benjamin Robins.

In 1742 Maclaurin published his *Treatise on Fluxions,* which he intended as a final answer to Berkeley.[14] He succeeded reasonably well from a logical point of view, but unfortunately, his book was nearly impenetrable at the time and is even less readable now. Steeped as it was in a classical and geometric point of view and methodology, it could not serve as a vehicle for teaching Calculus, and thus remains nearly unknown except to historians of mathematics.

In 1741 Benjamin Robins published *A Discourse Concerning the Nature and Certainty of Sir Isaac Newton's Methods of Fluxions and of Prime and Ultimate Ratios*.[15] His approach was more consistent with Newton's conception of the foundations of Calculus, and is far more readable than Maclaurin's text. It was also surprisingly modern in its approach. Unfortunately, as a result of Robins's premature death, his *Discourse* is also almost completely unknown outside of the literature on the history of mathematics.

This brings us back to the original question: Why did mathematicians abandon the much simpler proof technique using differentials and elementary geometry in favor of the much more complicated and hard to understand modern proof based on the theory of limits? The answer, of course, is that it was Berkeley's fault. In *The Analyst*, he pointed out that there were deep and subtle flaws in the differential approach that could not stand as logical constructs. As originally conceived, the differential approach simply does not work, however attractive and intuitive it may be. So a new approach was required, and that new approach was not easy to come by. In fact, it was not until the 1860s, almost 200 years after Calculus was invented, that Berkeley was finally answered and we had a theory—the theory of limits—that could withstand his criticisms.

CODA: LEIBNIZ'S DIFFERENTIALS VS. NEWTON'S LIMITS

I have casually referred to differentials as if they were a mainstay of Calculus for both Newton and Leibniz, but this is not precisely true. Newton probably used the idea of differentials at first, but his approach evolved over the course of his long life. Leibniz used differentials consistently throughout his career. I have blurred the distinction between their methods in order to elucidate my main point as efficiently as possible. However, Newton's approach to the Calculus at the end of his life was very distinct from Leibniz's, and closer to the modern approach using limits. Therefore the late-nineteenth century founding of Calculus on the modern theory of limits is, in some sense, a Newtonian victory.

However, in the middle of the twentieth century Abraham Robinson provided an equally solid and rigorous foundation for the differential approach of Leibniz, which he called *Non-Standard Analysis*.[16] Robinson's intention in this effort was to vindicate Leibniz's ideas and his approach to Calculus. As a result, today Calculus can be placed, rigorously, upon either foundation. However, most textbooks still take the Newtonian approach simply because most textbook authors, having been classically trained, are more familiar with it.

NOTES

1. Anthony Collins, *A Discourse on Free-Thinking*, (London, 1713).
2. Douglas M. Jessup, *George Berkeley, De Motu and The Analyst*, The New Synthese Historical Library, Volume 41, (Dordrecht: Kluwer Academic Publishers, 1992), pp. 156-221.
3. Isaac Newton, *Two Treatises of the Quadrature of Curves*, trans. John Stewart (London: James Bettenham, 1745).
4. Ivor Grattan-Guinness, "Berkeley's Criticism of the Calculus as a Study in the Theory of Limits," *Janus*, 56 (1969): 215-227.
5. George Berkeley, *Reasons For not Replying to Mr. Walton's Full Answer in a Letter to P. T. P*. (Dublin: Printed by M. Rhames, for R. Gunne, 1735).
6. Jessup, *George Berkeley, De Motu and The Analyst.*
7. Philalethes Cantabrigiensis [James Jurin], *Geometry No Friend to Infidelity: or a Defence of Sir Isaac Newton and the British Mathematicians* (London: Printed for T. Cooper at the Globe, 1734).
8. Jacob Walton, *A Vindication of Sir Isaac Newton's Principles of Fluxions, Against the Objections Contained in The Analyst* (Dublin: Printed by S. Powell, for William Smith, 1734).
9. George Berkeley, *In Defense of Free-thinking in Mathematics* (Dublin: Printed by M. Rhames, for R. Gunne, 1735).
10. James Jurin, *The Minute Mathematician: or, The Free-Thinker no Just-Thinker* (London: Printed for T. Cooper at the Globe, 1735).
11. Jacob Walton, *The Catechism of the Author of the Minute Philosopher Fully Answer'd* (Dublin: Printed by S. Powell, for William Smith, 1735).
12. Berkeley, *Reasons For not Replying.*
13. Ruth Wallis, "Who was J. Walton, Adversary of Bishop Berkeley," *Annals of Science*, 51 (1994): 539-540.
14. Colin Maclaurin, *A Treatise on Fluxion* (London: Knight & Compton, 1801).
15. Benjamin Robins, "A Discourse Concerning the Nature and Certainty of Sir Isaac Newton's Methods of Fluxions and of Prime and Ultimate Ratios," in *Mathematical Tracts of the late Benjamin Robins, Esq.* (London: James Wilson, 1741), pp. 1-77.
16. Abraham Robinson, *Non-Standard Analysis* (Amsterdam: North-Holland Pub. Co., 1974).