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ScienceDirect
Historia Mathematica 38 (2011) 219–231
HISTORIA MATHEMATICA
www.elsevier.com/locate/yhmat

One of Berkeley's arguments on compensating errors in the calculus

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Abstract

This paper addresses three questions related to George Berkeley's theory of compensating errors in the calculus published in 1734. The first is how did Berkeley conceive of Leibnizian differentials? The second and most central question concerns Berkeley's procedure which consisted in identifying two quantities as errors and proving that they are equal. The question is how was this possible? The answer is that this was not possible, because in his calculations Berkeley misguided himself by employing a result equivalent to what he wished to prove. In 1797 Lazare Carnot published the expression "a compensation of errors" in an attempt to explain why the calculus functions. The third question is: did Carnot by this expression mean the same as Berkeley?

Riassunto

Questo articolo affronta tre domande concernenti la teoria di Berkeley sulla compensazione degli errori nel calcolo pubblicata nel 1734. La prima è, come concepiva Berkeley i differenziali leibniziani? La seconda domanda, quella più importante, concerne la procedura di Berkeley consistente nell'identificare due quantità come errori per poi provare che esse sono uguali. La domanda è, come è possibile questo? La risposta è che ciò non è possibile dato che nei suoi calcoli Berkeley si ingannò utilizzando un risultato equivalente a quello che voleva provare. Nel 1797 Lazare Carnot pubblicò l'espressione "compensazione degli errori" in un tentativo di spiegare come mai il calcolo funzioni. La terza questione è: con questa esperessione Carnot voleva dire la stessa cosa di Berkeley?

MSC code: 01A50

Keywords: Berkeley; Carnot; Compensating errors

1. Introduction

This paper is written as a note to one particular argument in George Berkeley's essay the *Analyst* (1734). The *Analyst* is well-known in the history and philosophy of mathematics

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0315-0860/$ - see front matter
doi:10.1016/j.hm.2010.07.001

and its contents thoroughly described in the literature. Hence, here I just summarize briefly the setting of the work. Berkeley addressed it to an infidel mathematician. The latter might have been Edmund Halley but could also be any mathematician who had expressed scepticism about God while accepting Newton's method of fluxions and Leibniz's differential calculus, or as Ivor Grattan-Guinness so aptly has suggested "all infidel mathematicians who chided theologians like himself [Berkeley] for confused thinking" [Grattan-Guinness, 1969, 218; see also Breidert, 1989, 100 and Jesseph, 1992, 130–132]. Berkeley wanted to show this addressee that if he could believe in the mysteries of the calculus there was no reason why he should have any doubts about the existence of God.[1] The majority of the *Analyst* is dedicated to showing the illogicality of the Newtonians' and Leibnizians' arguments when deriving their results and in particular to attacking the concept of infinitesimals. A present day mathematician would say that the method of fluxions and the calculus lacked foundation. As pointed out by Niccolò Guiccardini, for Berkeley and his contemporaries this took the form of a worry about, "the ontological status of the objects of the calculus and ... the correctness of the methods of the calculus according to the Aristotelian standard of logic" [Guiccardini, 1989, 38].

Among Berkeley's discussions are a few dealing with "compensating errors". One of his examples has in particular received much attention and is the topic for my paper. I was led to this example long ago when I was supervising a former student, Merete Lemke, who wrote an essay on Berkeley as a philosopher and a mathematician. She found it difficult to grasp the mathematical contents of Berkeley's example; and she was not helped by the standard literature on the theme. Lemke did not go into the mathematical details, but I became so curious that I decided to analyze the mathematical contents of his argument one day — which turned out to be much later than I had first imagined.

Before turning to Berkeley's argument I want to point out that my aim is not to discuss the historically interesting question of how Berkeley's contemporaries reacted to his argument[2] nor to discuss the rest of the contents of the *Analyst*, but to offer a clarifying interpretation of Berkeley's calculations in his most discussed example. To do so I present what Berkeley considered a paradox, discuss Berkeley's interpretation of the Leibnizian differentials, describe Berkeley's presentation of Leibniz's method of determining tangents and the two "errors" he considered this method to contain, paraphrase his calculations that made him conclude that the "errors" compensate each other in the mentioned specific case, and finally discuss Berkeley's argument. Before concluding I touch briefly upon two questions that I find relevant in connection with the story about Berkeley's compensating "errors". The first one is whether the nature of Berkeley's argument was general. The second one is inspired by the fact that at the end of the 18th century Lazare Carnot claimed that the calculus functions because compensating errors are involved. The question I have in this connection is whether Carnot meant the same as Berkeley.

[1] For literature on Berkeley's criticism of the calculus in the *Analyst* from the time of Augustus De Morgan to the mid 20th century see Grattan-Guinness [1969, note 3, 216]. The later literature on the theme includes Grattan-Guinness [1969], Cantor [1984, 668–683], Blay [1986, 243–253], Breidert [1989, 99–109], Guiccardini [1989, 38–41], Jesseph [1992, 132–145; 1993, 178–230; 2005, 124–128; 2008, 249–254], Pycior [1997, 232–241].

[2] This question is dealt with in Grattan-Guinness [1969, 225–226], Guiccardini [1989, 43–51], Breidert [1989, 109–123], Jesseph [1992, 145–148; 1993, 231–295; 2005, 129–130], Pycior [1997, 235–241].

2. The apparent paradox

Berkeley acknowledged that mathematicians who applied Newton's method of fluxions or Leibniz's calculus ended up with valid results. However, as mentioned, he considered their calculations to be based on incorrect assumptions and to violate the rules of logic. Confronted with this situation he wrote:

> And forasmuch as it may perhaps seem an unaccountable Paradox, that Mathematicians should deduce true Propositions from false Principles, be right in the Conclusion and yet err in the Premises; I shall endeavour particularly to explain why this may come to pass, and shew how Error may bring forth Truth, though it cannot bring forth Science. (§20)[3]

Berkeley did not attempt to explain why Leibniz's and Newton's new methods worked in general, but presented some examples in which he claimed to have proved why the Leibnizians and the Newtonians got the correct results. His idea was that they made two mistakes in their reasoning and that these mistakes canceled each other. He gave three examples of this, and it is the first one that in particular has become associated with his assertion that one "error" is compensated by another equal "error" (§23). It deals with applying the Leibnizian method of determining tangents to a parabola and is the main topic of this paper. Berkeley's two other examples are different from the first one in the sense that in his first example he introduced explicit expressions for his "errors" while in the two others examples he pointed generally to "errors" committed by not following the rules of finite quantities (§§24–28).[4] Because Leibniz's differentials are central to the example, I find it appropriate to start by considering Berkeley's treatment of these differentials.

3. Berkeley and the Leibnizian differentials

I would like to begin by commenting upon the term 'differentials' itself, before looking more specifically at how Berkeley conceived of differentials.[5] Today, it is common to use this term for the fundamental elements in Leibniz's calculus. However, Leibniz himself would often refer to 'differences' and so did many of his followers. For instance when Guillaume François Antoine L'Hospital wrote the first textbook on the new calculus, *Analyse des infiniments petits*, he introduced Leibniz's concept dx as a difference of x [L'Hospital, 1696, 2]. Berkeley, who referred to L'Hospital's work (§17), followed this tradition.

A characteristic feature of the Leibnizian differences is that they were treated as infinitely small quantities, also called infinitesimals: They were not zero, but obeyed among other rules

$$\text{the quantity } x + dx \text{ can be replaced by } x, \tag{1}$$

or in other words the dx can be discarded when it occurs together with x. To avoid having to specify whether a particular difference is finite or infinitely small, I reserve the word differences for the finite ones and use the expression differentials for the infinitesimal differences.

Leibniz considered a differential such as dx as a quantity to which a differential could be assigned; the latter became known as a second-order differential, while dx was called a

[3] All references marked only by a § sign are to Berkeley's *Analyst* — that is Berkeley [1734].

[4] See also [Jesseph, 1993, 206–213].

[5] This is not the place for a discussion of what Leibniz himself thought about his differentials or for a thorough survey of the rich literature on the theme. I limit my references to Bos [1974], Jesseph [1998], Arthur [2008], and the references in these works.

first-order differential. The second-order differential was denoted ddx and it obeyed its own rules, such as a parallel to (1):

$$\text{the quantity } dx + ddx \text{ can be replaced by } dx \tag{2}$$

Actually, the Leibnizians did not stop at second-order differentials, but also worked with third and even higher orders of differentials.

The historians and philosophers of mathematics, who have discussed Berkeley's treatment of Leibniz's differentials, have expressed different opinions as to how they were conceived by Berkeley; some believed he treated them as infinitesimals, while others thought he considered them to be finite. In fact it is difficult to draw any firm conclusion, because Berkeley himself was not clear, as I am now going to show.

Berkeley applied Leibniz's notation dx and dy and characterized these quantities in various places as being "infinitely small" (for instance, in §5) and "infinitesimal Differences" (for instance, in §21). When Berkeley dealt with finite differences, he would explicitly mention that they were finite quantities and in general denote them by a single letter (for instance, in §24). In arguing for his ideas about compensating "errors" Berkeley thought, as far as I see it, that he was working with Leibnizian differentials all through his argument.[6] This is in contrast to his considerations in another example in which he claimed that he looked at the matter in "another light" which consisted of "proceeding in finite Quantities" and only to "make use of one Infinitesimal" at the conclusion (§24).

Although Berkeley claimed to be dealing with Leibniz's differentials when presenting the example for determining the tangent to a parabola, he did not accept Leibniz's rules for calculating with differentials. In particular, he did not accept rule (1), which is fundamental to the treatment of first-order differentials. Instead of following Leibniz's rules, Berkeley introduced his own; as we shall see, they are of a kind that suggests that he conceived of the differentials as being very small rather than infinitely small. This would be in accordance with his statement

> ... to conceive a Quantity infinitely small, that is, infinitely less than any sensible or imaginable Quantity ... is, I confess, above my Capacity. (§5)

Having difficulty in conceiving the infinitely small first-order differentials, Berkeley naturally found the second-order differentials even more absurd. He actually used the intellectual challenge of working with second- and third-order differentials in a theological argument against his "infidel mathematician"

> ... he who can digest ... a second or third Difference [differential], need not, methinks, be squeamish about any Point in Divinity. (§7)

The realization that Berkeley did not accept the rules for calculating with differentials helps explain why some historians interpret Berkeley's argument on compensating "errors" as concerning finite differences. In fact, he himself contributed to this conception, because after having proved what he wanted with the help of compensating infinitesimal "errors" in connection with the tangent to a parabola, he added:

[6] For completeness I should add that at a certain stage in his argument Berkeley set $dx = m$ and $dy = n$ (§22). This does not fit with his mentioned habit of reserving single letters for finite quantities. Still, there is no indication that at this point he started considering dx and dy to be different from Leibniz's differentials, so I understand this change as a simplification of the notation for the further calculations.

> Now I observe in the first place that the Conclusion comes out right not because the rejected ... [quantity] was infinitely small but because it was compensated by another contrary and equal error ... I observe in the last place, that in case the Differences are supposed finite Quantities ever so great, the Conclusion will come out the same ... (§23)

In other words, he claimed that that he could have carried out the same argument, if he had considered the differences to be finite.

I do not pursue this matter further, because for the conclusion I draw from analyzing Berkeley's calculations (to be presented in Section 8), it does not matter how he conceived of the differentials. However, let me sum up and stress that although he wanted the differentials to follow the rules of finite differences, he pretended to be working with Leibnizian differentials in the example treated in this paper. To avoid confusion in working with Leibnizian differentials, which follow one set of rules, and Berkeley's which have different properties, I introduce the symbols Δx and Δy for Berkeley's differentials, while I keep dx and dy when quoting Berkeley.

4. The method of tangents

As mentioned, in his example for determining the tangent to a curve Berkeley chose a parabola, more specifically, the one with the equation $y^2 = px$ (Fig. 1). The first part of his presentation on how the users of the calculus determine a tangent to a curve applies, however, for all curves. To draw the tangent TB to a curve at a given point B with coordinates $x = AP$ and $y = PB$, a method was applied that leads to an expression for the so-called subtangent, which is the line segment TP between the point T at which the tangent intersects the axis AP and the point P.

This method was derived by introducing the line segments PM and RN (N being a point on the curve), denoting them by dx and dy, and claiming that the subtangent TP is the fourth proportional to the line segments dy, dx, and y, from this. The Leibnizians and Berkeley with them concluded that the subtangent TP is equal to ydx/dy (§21).

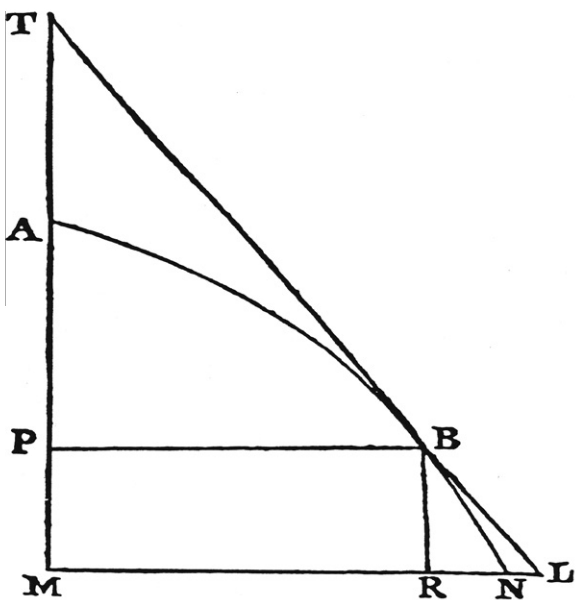

Fig. 1. Berkeley's illustration in §21.

Because TP will turn up many times I will simply name it t in subsequent presentation of Berkeley's ideas. Thus I write the fundamental tangent relation in the calculus as

$$t = \frac{ydx}{dy}. \tag{3}$$

As indicated earlier, Berkeley did not deny that result (3) provides the correct subtangent, but he was very critical about the method by which it had been deduced. He promised, as we saw in Section 2 that he would "shew how Error may bring forth Truth, though it cannot bring forth Science" (§20). By this he meant that he would argue that the deduction of relation (3) contains two "errors", and moreover in the case of the parabola he would show how the "errors" cancel each other.

5. The first "error"

In commenting upon relation (3), Berkeley wrote that the Leibnizians had obtained (3) by considering the triangles BNR and TBP (Fig. 1) to be similar and thereby made a mistake, because it is triangle BLR that is similar to TBP. Hence he introduced the line segment NL, and named it z:

$$z = NL. \tag{4}$$

And then he found what he considered the "true expression for the Subtangent" (§21),

$$t = \frac{ydx}{dy + z}, \tag{5}$$

calling the quantity z the first "error".

That the relation (5) is correct would not have been denied by the Leibnizians, but to obtain the final result they would have disregarded z according to a rule in the calculus corresponding to the one expressed in relation (2). Berkeley found this an "erroneous Rule" (§21), because it is illogical "first to suppose, and secondly to reject Quantities infinitely small" (§18). As I have explained in Section 3, for clarity I prefer to write Berkeley's relation (5) as

$$t = \frac{y\Delta x}{\Delta y + z}; \tag{6}$$

this relation applies, as mentioned, for any curve. For later use, it is convenient to rewrite it with z isolated:

$$z = \frac{y}{t}\Delta x - \Delta y. \tag{7}$$

6. The second "error"

While Berkeley ascribed the first "error" to discarding a line segment in a consideration of similar triangles, he assigned the second "error" to the rules for calculating differentials. To illustrate this, he chose the earlier-mentioned example of the parabola with the equation $y^2 = px$. According to Leibniz, the differentials in this equation are equal, which means that

$$d(y^2) = d(px); \tag{8}$$

furthermore his rules for calculating differentials give $d(y^2) = 2ydy$ and $d(px) = pdx$, hence $2ydy = pdx$, or

$$dy = \frac{pdx}{2y}. \tag{9}$$

Berkeley could not accept the rules leading to this result but made his own calculations instead (§21). He assumed that a neighbor point to (x, y) on the parabola $y^2 = px$ fulfils

$$(y + \Delta y)^2 = p(x + \Delta x); \tag{10}$$

since $y^2 = px$ he then got

$$2y\Delta y + (\Delta y)^2 = p\Delta x \tag{11}$$

or

$$\Delta y = \frac{p\Delta x}{2y} - \frac{(\Delta y)^2}{2y}. \tag{12}$$

Berkeley called the last term the second "error" and kept it in his further calculations. However, he never used its explicit form; hence to make his calculations slightly more transparent I introduce the letter r for his second "error":

$$r = \frac{(\Delta y)^2}{2y}. \tag{13}$$

The relations (12) and (13) show that the term (r in my notation) that Berkeley introduced as a correction to make equation (9) exact can be expressed as follows:

$$r = \frac{p\Delta x}{2y} - \Delta y \tag{14}$$

7. Berkeley's argument for the equality of his two "errors"

To prove that his two "errors" cancel each other, Berkeley carried out some calculations that seem unnecessarily complicated and roundabout. That results are derived by arguments that later are simplified is common in mathematics – in their attempts to create a proof mathematicians seldom find the most elegant at first try. That results first are found by roundabout methods is not unusual either. But in most cases it is possible to understand the reasons behind the calculator's convoluted considerations and meanderings. This is not the case with Berkeley's calculations. In the next section I analyze the technical content of his proof, but first I would like to reproduce Berkeley's own line of argument (though with a different notation), the purpose being to illustrate why Berkeley confused his readers, and, I think, most likely also himself.

In arguing that his two "errors" are equal Berkeley renamed, as mentioned in note 6, the differentials. To avoid the complication of working with a new notation I keep to the one already introduced. Berkeley returned to his considerations concerning similar triangles, expressed in formula (6), but now he applied it in a form corresponding to

$$\Delta y + z = \frac{y\Delta x}{t}. \tag{15}$$

In the specific case of the parabola Berkeley had another means than the differential calculus for obtaining an expression for the subtangent. It had already been determined by the ancient Greek mathematicians by methods applying only finite quantities. In the extant Greek texts the result is mentioned by Archimedes and proved by Apollonius. Berkeley referred to the latter's *Conics*, to be more precise to Book I, Theorem 33, from which he got

$$t = 2x. \tag{16}$$

Hence (15) can be written as

$$\Delta y + z = \frac{y\Delta x}{2x}. \tag{17}$$

Berkeley's next step corresponds to isolating Δx in (14):

$$\Delta x = \frac{2y(\Delta y + r)}{p}. \tag{18}$$

Inserting this in (17), and combining it with the fact that the parabola has the equation $y^2 = px$, he obtained

$$\Delta y + z = \frac{2y^2(\Delta y + r)}{2px} = \Delta y + r, \tag{19}$$

from which he indeed could conclude that $z = r$.

I assume that the readers get puzzled at this point and ask themselves what happened? At least that was my initial reaction when I read Berkeley's argument.

8. An analysis of Berkeley's argument

What intrigued me in Berkeley's argument was that without applying his explicit interpretations of his two "errors" z and r (cf. (4) and (13)), he made calculations that apparently proved that these "errors" are equal. Further analysis was required, and this led me to the conclusions that his interpretations of z and r played no role at all in his argument and regardless of how he went about the calculations he would get $z = r$. I will justify this position in the following.

Berkeley's point of departure was his claim that

$$\frac{y}{t}\Delta x \neq \Delta y \quad \text{and} \quad \frac{p}{2y}\Delta x \neq \Delta y. \tag{20}$$

Temporarily following him in this view I will for a pedagogic reason for a short moment work with the two differences:

$$\delta_1 = \frac{y}{t}\Delta x - \Delta y. \tag{21}$$

$$\delta_2 = \frac{p}{2y}\Delta x - \Delta y. \tag{22}$$

Although it was not at all how Berkeley himself defined or understood his "errors" z and r, he did indirectly introduce them this way, as can be seen from relations (7) and (14); thus we can rewrite the two above equations as

$$z = \frac{y}{t}\Delta x - \Delta y. \tag{7}$$

$$r = \frac{p}{2y}\Delta x - \Delta y. \tag{14}$$

The relations (7) and (14) show that in the case of Berkeley's parabola the equation

$$z = r. \tag{23}$$

is fulfilled precisely when

$$\frac{y}{t} = \frac{p}{2y}. \tag{24}$$

Berkeley's idea was that he had to prove (23) in order to obtain (24). For him the latter was the result he needed because in the case of the parabola the relation (24) is equivalent to the Leibnizians' result (3) with the differentiation carried out according to their rules (cf. (9)). In other words, (24) showed Berkeley that the Leibnizians obtained a correct subtangent to the parabola, although by erroneous means, namely by committing two "errors".

However, one can turn the reasoning around and argue that the equivalence of (23) and (24) also implies that if relation (24) — or one equivalent to it — is assumed then relation (23) that is, $z = r$, automatically follows. This was exactly what happened in Berkeley's proof, because in his calculations Berkeley used the Apollonian result $t = 2x$ (cf. (16)), which for the parabola $y^2 = px$ is equivalent to (24), as the following deduction shows,

$$\frac{y}{t} = \frac{p}{2y} \iff \frac{y}{t} = \frac{py}{2px} \iff \frac{y}{t} = \frac{y}{2x} \iff t = 2x. \tag{25}$$

Hence my conclusion is that the fact that Berkeley, though presumably unaware of it, defined his "errors" as the two differences (7) and (14) and then applied $t = 2x$ meant that he would be led to the result that his "errors" compensated each other, no matter how he had interpreted them. Thus, it is not Berkeley's interpretation of his "errors" outlined in (4) and (13) but the Apollonian result that is essential in his argument.

9. The generality of Berkeley's argument

The generality of Berkeley's argument has been discussed in the literature.[7] Berkeley himself, however, did not explicitly discuss whether his argument for the compensating "errors" in the example of determining the subtangent to a parabola can be generalized, yet I find it likely that he thought his argument would apply in other cases as well.[8] Anyway, since the question of generality has been mentioned, I would like to follow up on my analysis in the last section and briefly touch upon this theme. In a general case, Berkeley would have to argue how the relation

$$t = \frac{y\mathrm{d}x}{\mathrm{d}y}. \tag{3}$$

— where the relationship between dx and dy is calculated according to Leibniz's rules — can be obtained by means of his two "errors". If he were to follow the scheme from the parabola he would be able to do so in the cases in which he had an expression for the sub-

[7] The point was taken up in Grattan-Guinness [1969, 224–225], and raised among other places in Jesseph [1992, note 18, 184].

[8] In connection with another example Berkeley indicated that he thought that a generalization was possible, leaving it to those "who have leisure and curiosity for such Matters" (§29) — see also Jesseph [1993, 212–213].

tangent t obtained by other means than applying infinitesimal calculus — like as for the parabola he had the result $t = 2x$. Moreover, he would presumably also want to find an explicit expression for his "error" r (though, as I have argued, it would not be necessary). He could then go through calculations similar to the ones he did for the parabola and come to the conclusion that his "errors" canceled each other.

The first requirement is a sort of bottleneck because the number of curves whose subtangents were determined by finite means is small. Thus, in relation to Berkeley's own approach, it does not make sense to talk about a generalization.

10. Berkeley and Carnot

A traditional addition to the story about Berkeley's compensating "errors" is that he was not the only one to apply this expression in an explanation of why the calculus gives correct answers. In responding to a prize problem set by the Berlin Academy of Science for the year 1786 about the justification of the infinitesimal calculus, Lazare Carnot presented the idea that the calculus involves errors that compensate each other [Youschkevitch, 1971, 150, 160].[9] Although busy with politics, Carnot returned to the theme over the following decades. Thus, in 1797, he published some of the material from his essay for the Berlin Academy in *Réflexions sur la métaphysique du calcul infinitesimal* (hereafter referred to as *Réflexions*); this book he later revised substantially for a second edition, which appeared in 1813. For the present paper the interesting question is whether Carnot's approach was a continuation of Berkeley's work. In my opinion the answer is 'no,' and I shall briefly argue for this by presenting the context in which Carnot introduced the idea of compensating errors in *Réflexions.*

It was in connection with an example that Carnot mentioned that he had obtained a correct result by "a compensation of errors".[10] The beginning of the example is similar to Berkeley's determination of the subtangent to a parabola. Carnot chose the subtangent to a circle; and in the same way as Berkeley had used Apollonius's result for the subtangent to a parabola, Carnot applied the fact that the subtangent to a circle had been known since antiquity. Carnot's further description of the phenomenon of compensating errors, however, does not resemble Berkeley's approach at all. Berkeley's "errors" were two quantities and his aim was to prove that they were equal. Carnot considered two equations that he claimed to be "certainly wrong" [Carnot, 1797, §9, 17]. As I only want to give an impression of the kernel of Carnot's idea, I do not reproduce his calculations, but refer to the literature listed in note 13. Instead I write his equations in the following form, where t is the subtangent:

$$t = y\frac{\Delta x}{\Delta y}. \tag{26}$$

and

$$\frac{\Delta x}{\Delta y} = \frac{y}{a - x}. \tag{27}$$

Contrary to Berkeley, Carnot did not search for expressions for the errors in these two equations, instead he combined them, whereby he got

[9] The prize went to the Swiss mathematician Simon L'Huilier.
[10] *Une compensation d'erreurs* [Carnot, 1797, §9, 14].

$$t = \frac{y^2}{a - x}, \tag{28}$$

which is a correct expression for the subtangent to the circle. From this process Carnot concluded:

> Therefore it is absolutely necessary that the errors compensate each other by the comparison of the two erroneous equations.[11]

And he continued by claiming that his argument had proved that compensation of errors was a fact [Carnot, 1797, §10, 17]. His task was then to characterize the procedures that made it possible to come from erroneous equations to correct results, and he devoted a large part of the *Réflexions* to this.

Thus, I understand Carnot to attach the expression error to the practice of calculating with infinitesimals rather than to an explicit quantity. This interpretation is in accordance with the following statement by Charles Gillispie:

> The genius of the infinitesimal calculus, in Carnot's account, lay in its capacity to compensate in its own procedures for errors that it deliberately admitted into the process of computation for the purpose of facilitating a solution. ... what Carnot meant by compensation actually eliminated error and made the procedures of analysis as rigorous as those of synthetic demonstration. [Gillispie, 1971a, 75]

My view is also in harmony with Hervé Barreau's analysis of Carnot's procedure, where he described the idiom "method of compensating errors" as unfortunate and continued:

> It seems that nothing is less appropriate than this expression to characterize Carnot's method. In fact to compensate errors, it is necessary first to single them out and compare them, like Berkeley did ... Carnot did never engage himself in any of this ... [12]

Barreau concluded that "method of elimination of errors" would be a more appropriate term to apply to Carnot's practice [*ibid.*; also quoted in Thiele, 1990, 84].

It is difficult to know whether Carnot was familiar with Berkeley's *Analyst* — he might well have been although he did not refer to Berkeley in the *Réflexions*. Carnot might have become familiar with the phrase "compensation of errors" by reading Berkeley or by having seen references to him, but he himself might also have coined the phrase [see also Dhombres and Dhombres, 1997, 159–160]. In any case, Carnot's explanation of why the calculus works is essentially different from Berkeley's argument involving equal errors.

Although readers may have become curious about how Carnot moved on from erroneous equations to trustworthy theorems, it is completely outside the scope of this paper to go into this matter.[13]

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[11] *Il faut par consequent de toute nécessité que les erreurs se soient compensées mutuellement par la comparaison des deux equations erronées* [Carnot, 1797, §9, 17].

[12] *Rien ne convient moins que cette expression, semble-t-il, pour caractériser la methode de Carnot. En effet, pour compenser des erreurs, if faut au préalable, les repérer et les comparer, comme l'avait fait Berkeley ... Carnot ne s'est jamais appliqué a rien de tel ...* [Barreau, 1987, 7].

[13] For presentations of Carnot's treatment of compensating errors see Gillispie [1971a, 75–76; 1971b, 134–143], Youschkevitch [1971, 159–168], Barreau [1987], Thiele [1990], Dhombres and Dhombres [1997, 159–166 and 177–184], Schubring [2005, 340–343].

11. Concluding remarks

I would like to repeat that the point of my analysis of Berkeley's proof was to shed some light upon why he thought that he had found two compensating "errors". I felt this was important because in the literature on compensating errors[14] familiar to me I have never found an explanation of what the mathematical contents of his calculations sum up to. Nevertheless, my conclusion was published some thirty years ago, when Judith V. Grabiner wrote that Berkeley's "demonstration rests on Apollonius, *Conics*, book I, proposition 33" [Grabiner, 1981, note 53, 188]. However, she did not publish the analysis that justified her view – and no other scholar seems to have followed up on her remark. It should be added that it is also important for the conclusion that Berkeley introduced his errors in a way that meant that they can be written in the forms I chose in relations (7) and (14).

My analysis of Berkeley's argument does not change the situation that he himself was convinced that the Leibnizians made two "errors" when applying their method of tangents, one in connection with the formula for the subtangent and one when they calculated differentials. Berkeley carried out calculations that convinced him that he had proved that these "errors" are equal. This was for him extremely satisfactory because it gave him the answer to his wonder as to why the Leibnizians could argue wrongly and still obtain the correct result. What I have attempted to do is give an answer to the wonder how Berkeley's technical arguments could lead to the result that his "errors" are equal.

As I pointed out in the introduction, the aim of this paper was not to discuss the reaction of Berkeley's contemporaries to his theory of compensating errors. This would require a larger investigation, which does not belong in this paper. Yet it is worth noting that Berkeley's *Analyst* did not seem to have made his fellow mathematicians feel that they could halt their own investigations into the foundation of the method of fluxion and the calculus, because Berkeley had shown why the disciplines work. This fits with Guiccardini's impression that Berkeley's idea of compensating "errors" was not considered favorably by any British mathematicians [Guiccardini, 1989, note 3, 173].

Acknowledgments

This paper has benefited tremendously from Henk Bos's valuable comments on earlier versions and from Henrik Kragh Sørensen's highlighting of arguments that needed to be expanded or sharpened. Niccolò Guiccardini has kindly translated my abstract into Italian. I would also like to thank the Max Planck Institute for the History of Science in Berlin whose generous hospitality and excellent library services made the finishing of this article a very pleasant experience.

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[14] Among other places, Berkeley's mathematical argument is paraphrased and commented upon in Grattan-Guinness [1969, 221–225], Youschkevitch [1971, 151–155], Blay [1986, 248–251], Breidert [1989, 102–103], Thiele [1990, 82–83], Jesseph [1992, notes 15, 16 and 18, 182–184; 1993, 200–205], Dhombres and Dhombres [1997, 182–184].

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