The Newton-Leibniz controversy over the invention of the calculus

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1 Introduction

Perhaps one the most infamous controversies in the history of science is the one between Newton and Leibniz over the invention of the infinitesimal calculus. During the 17th century, debates between philosophers over priority issues were dime-a-dozen.[1] Inspite of the fact that priority disputes between scientists were common, many contemporaries of Newton and Leibniz found the quarrel between these two shocking.[2] Probably, what set this particular case apart from the rest was the stature of the men involved, the significance of the work that was in contention, the length of time through which the controversy extended, and the sheer intensity of the dispute.

Newton and Leibniz were at war in the later parts of their lives over a number of issues. Though the dispute was sparked off by the issue of priority over the invention of the calculus, the matter was made worse by the fact that they did not see eye to eye on the matter of the natural philosophy of the world. Newton's action-at-a-distance theory of gravitation was viewed as a reversion to the times of occultism by Leibniz and many other mechanical philosophers of this era. This intermingling of philosophical issues with the priority issues over the invention of the calculus worsened the nature of the dispute.

One of the reasons why the dispute assumed such alarming proportions and why both Newton and Leibniz were anxious to be considered the inventors of the calculus was because of the prevailing 17th century conventions about priority and attitude towards plagiarism. Today, we consider the criterion of printed publication to confer on the author credit for the published work. However, in the 17th century, correspondence and even disclosure in front of reliable witnesses of private manuscripts or instruments had considerable weight; the work need not necessarily have been published. As a result, such private correspondence led to personal claims which could never have been made if the communication, in the first instance, had been made public. Priority also had a subtle relation with the prestige and credibility of an interpretation: the ingenuity of the first inventor went hand in hand with the favourable reception of his work. The second inventor, by contrast had to protect himself from the shade of doubt about his integrity from the

[1] Rupert Hall, *Philosophers at War*, Page 2, Page 80; Domenico Bertolini Meli, *Equivalence and Priority: Newton versus Leibniz, Page 4*. Meli mentions that the second half of the 17th century has been aptly characterised as *the golden age of mud-slinging priority disputes.*

[2] Rupert Hall, *Philosophers at War*, Page 4

suspicion of having stolen a secret from a colleague.[3] There are a number of instances that highlight this attitude towards second inventors and plagiarism, some of which are:

- The Leibniz-Hooke controversy over the calculating machine.[4] When Hooke examined Leibniz's machine very carefully and later criticized it and proposed a simpler model, Leibniz accused Hooke of dishonesty. He claimed that the basis of the construction of Hooke's machine was the same as his own.

- Leibniz's defence when he was accused of plagiarizing Francois Regnaud's work on a method for the interpolation of series by constructing series of differences. Leibniz was so distressed and embarrassed by this accusation that he publicly displayed his private notes to defend himself.[5] This was during Leibniz's visit to London in 1673.

- Newton's assertion that "Second inventors have no right".[6] This was in reference to Leibniz at the peak of the calculus dispute.

In this context where second inventors were almost always treated with suspicion, it is interesting to observe that from 1676, when Newton and Leibniz first corresponded until about 1704 when the dispute was full-blown, neither Newton nor Leibniz considered the other to be guilty of plagiarism. Both seemed content in assuming that the other had independently arrived at his formulation of the calculus. However, by 1712, when the controversy was at its peak, each had accused the other of plagiarism. The relations between the two had soured to such an extent that Boyer views the resulting controversy to be "shamefully bitter".[7] Rupert Hall elucidates this situation very well in his book, *Philosophers at War*.

2 The origins of the calculus

2.1 Debt to the predecessors

These days, we universally ascribe the invention of the calculus to Newton and Leibniz. However, both men owed a very great deal to their immediate predecessors in the development of the new analysis. According to Boyer, the time was ripe, in the second half of the 17th century, for someone to organize the views, methods, and discoveries involved in the infinitesimal analyses into a new subject characterized by a distinctive method of procedure.[8] Some historians have taken this line of argument to the extreme and have sought to establish Barrow as the inventor of the calculus and to represent the labours of Newton and Leibniz merely

[3] Domenico Bertolini Meli, *Equivalence and Priority: Newton versus Leibniz*, Page 4; H.D.Anthony, *Sir Issac Newton*, Pages 67-69

[4] Ibid., Page 5

[5] Ibid., Page 5; Margeret E. Baron, *The Origins of the Infinitesimal Calculus*, Page 273

[6] Rupert Hall, *Philosophers at War*, Page 179

[7] Carl B. Boyer, *The history of the calculus and its conceptual development*, Page 188

[8] Ibid., Page 187

as a translation of Barrow's work into algebraic form.[9] However, this view does extreme injustice to the genius of these two mathematicians. As Rupert Hall rightly puts it, the discovery of the calculus was more than a synthesis of previously distinct pieces of mathematical technique.[10]

2.2 Who did it first? Newton or Leibniz?

Because of the mass of Newton's surviving papers, it has now been established beyond doubt that Newton was the first to arrive at the calculus. He first developed his theory of "fluxions" in 1665-66. By the middle of 1665, Newton was able to set down the standard differential algorithms in the generality with which they were to be expounded by Leibniz two decades later.[11] Further, this demonstrates that Newton could not have plagiarised anything from Leibniz precisely because of the fact that around 1665-66, Leibniz, at the age of twenty, still knew nothing of mathematics.[12]

Now, the other question that remains to be answered is whether Leibniz was guilty of plagiarism. Whereas historians quickly established early on that Newton had arrived at the calculus much earlier than Leibniz, the case of Leibniz is different. Newtonian supporters right into the 20th century leveled allegations of plagiarism against him, some of which border on the ridiculous. For example, Arthur Hathaway casts Leibniz as a German propagandist accustomed to political deceit. He squarely accuses Leibniz of hatching a well-thought out plan to deprive Newton of all credit and carrying it out on a timed schedule. Further, he accuses Leibniz of inaugurating the system of espionage on scientific work in foreign countries by which the usefulness and credit of as much of that work as possible might be transferred to Germany.[13]

At this point, however, it has been established beyond doubt that Leibniz arrived at the calculus independently during the time period of 1673-1676. In the sense that Leibniz's discoveries occur chronologically in time after those of Newton, some historians have considered Leibniz to be the second inventor of the calculus. But, this does not and should not take away from Leibniz the credit that is due him for inventing the algorithmic procedures of the differential calculus.

Now that we know Newton and Leibniz should be considered co-inventors of the calculus, the more interesting questions are why the dispute arose in the first place. The following section examines the case against Leibniz and attempts to answer why Leibniz was accused of stealing Newton's work. A later section discusses the origins of the dispute.

[9] Margeret E. Baron, *The Origins of the Infinitesimal Calculus*, Page 273
[10] Rupert Hall, *Philosophers at War*, Page 7
[11] Ibid., Page 13
[12] Ibid., Page 15
[13] Arthur Hathaway, *The Discovery of the Calculus*, Science 1919, 50:Pages 41-43; Arthur Hathaway, *Further History of the Calculus*, Science 1920, 51:Pages 166-167

3 Leibniz: The case against him

The accusations against Leibniz have their roots in the sequence of events that occurred between 1673 and 1676. These events played a crucial role in the priority dispute that arose later on.

3.1 Leibniz's 1673 visit to London

Before 1672, Leibniz was a novice in Mathematics. When in Paris, he met Christiaan Huygens and studied under his tutelage. In 1673, Leibniz first visited London on some diplomatic mission. At this time, he knew very little of Newton, but had favourably impressed Newton's most intimate acquaintances in the Royal Society, Henry Oldenburg and John Collins. During his two month stay in London, he never met Newton. Neither did he learn anything about the work done by Newton since none of his works were yet in print. However, he did meet Oldenburg and other mathematicians and probably got an inkling of the work on infinite series being done by the mathematicians in the Continent.

While at London, he purchased a copy of the *Geometrical Lectures* of Isaac Barrow, Newton's predecessor in the Lucasian chair of mathematics at Cambridge. This is significant since Barrow had worked on the method of tangents(this is related to the differential calculus) and the book contained a lecture on this topic. Newton and his supporters used this fact in the priority dispute and accused Leibniz of borrowing from Barrow. However, Leibnizians deny that Leibniz ever read this book before his calculus. Hall mentions that Leibniz read this book cursorily and claims that Leibniz could not have been influenced by the book.[14] Feingold presents a strong case that Leibniz must have read Barrow's book.[15] Though this does not necessarily mean that Leibniz plagiarized from Barrow, it highlights the fact that there was a lot of confusion and debate among historians in interpreting this event(and many other events of this period). Some historians were guilty of interpreting this event in ways that best suited their hypotheses. In 1916, J.M.Child used this event to argue that Barrow was the original inventor of the calculus and that Leibniz was in some measure indebted to Barrow's work.[16]

It was during this visit to London that Leibniz was first accused of plagiarism, although on a different case. At Pell's house, he claimed to have a method of differences for series. But, Pell pointed out that this work was already done by Francois Regnaud and had been priorly published by Mouton. Though Leibniz managed to acquit himself by displaying his private notes, this incident was later used by Newton against him.

3.2 Leibniz's correspondence with Oldenburg

Once Leibniz got back to Paris, he started studying the mathematical works of Cavalieri, James Gregory, Pascal, Sluse and others. He also started working on the nature of series and their summations. Further,

[14] Rupert Hall, *Philosophers at War*, Page 55
[15] Mordechai Feingold, *Newton, Leibniz and Barrow too: An attempt at a reinterpretation*, Isis 84(1993), pages 310-338
[16] Ibid., Page 311; Margeret E. Baron, *The Origins of the Infinitesimal Calculus*, Page 273

he was in regular correspondence with Henry Oldenburg. From the mathematical reports and letters that he received from Oldenburg, Leibniz learnt of the British work on infinite series and thus learnt that some of his work on series had been anticipated by the British (notably Gregory and Newton). As a result, Leibniz all along was under the impression that Newton's greatest expertise was in the method of series. The two famous letters of 1676 written by Newton only served to confirm this impression of Leibniz.[17]

By October 1675, Leibniz had developed ideas of his differential calculus. Since, until this time, none of Newton's works were published, Leibniz had no way of knowing that Newton had already hit upon the calculus. The only way he could have known anything about Newton's work was through his correspondence with Oldenburg and Collins. However, Hall mentions that this correspondence in the early summer of 1675 was concerned with the algebra rather than the calculus. Later on, when the dispute was full-blown, Newtonians argued that Leibniz learnt a lot about British mathematics from Tschirnhaus who spent some time in England before he visited Leibniz in 1675. However, the notes made by Leibniz indicate that he only had casual conservations with Tschirnhaus and thus he could not have learnt much from Tschirnhaus.[18]

In June 1676, Newton wrote a letter to Oldenburg in which he described the binomial theorem using which quantities can be reduced to infinite series. He mentions that all mechanical curves can thus be reduced to infinite series and that the areas and lengths of curves, and the volumes and surfaces of solids can be computed from these series. Newton never discusses fluxions in the whole letter. Since Newton does not discuss his fluxional calculus explicitly and given that Leibniz was under the mindset that Newton essentially was a expert in the method of series, Leibniz failed to see any similarity to his own work on the calculus.

Immediately, Leibniz sent off a reply to Newton in which he described his own work on series and also asked for clarifications on certain points of the first letter. In the second letter that Newton wrote in October 1676 as a reply to Leibniz's letter, Newton mentioned that he had obtained a general methods of drawing tangents, of determining minima and maxima and other topics which he did not want to disclose. He concealed the mention of "fluxions" and "fluents" (which are in a way, analogous to differentials and integrals), in an anagram. Thus, these two letters of 1676 did not tell Leibniz very much about Newton's fluxional calculus except that Newton had something similar to his(Leibniz) own calculus. However, Leibniz received the second letter only in 1677. In his reply to this letter, which he sent in 1677, he fully described his differential calculus. Before that, however, he paid a second visit to London.

3.3 Leibniz's 1676 visit to London

Collins, on learning of Leibniz's interest in series, prepared a compendium of Gregory's work and also Newton's fluxional calculus. During his second visit to London, Leibniz got a chance to go through this compendium. This then raises the suspicion of Leibniz having gained important insights from such a perusal.

[17] Rupert Hall, *Philosophers at War*, Page 66
[18] Ibid., Page 59-60

Hall concedes that Leibniz could have taken the first independent steps on differentiation, and then, on seeing Newton's work and having appreciated its value, gone on to "borrow" the development of the calculus in his own notation. He counters such a suggestion as follows: ".. Leibniz took long notes from Newton's *On Analysis*, written in 1669, but these notes deal exclusively with ... series ... Leibniz passed over Newton's brief and obscure allusions to what is tantamount to differentiation .. because there was in them nothing new for him".[19] Thus, if it is accepted that Leibniz developed his differential calculus in 1675, this reasoning sounds very plausible.

3.4 Putting it all together: The accusations

Leibniz finally published his differential calculus in a treatise in 1684, almost 8 years, after his second visit to London. Combined with the fact that the fluxional calculus and differential calculus are very similar in terms of their applicability in solving problems, it can be seen how the all these preceding events can be used to build a case of plagiarism against Leibniz. In 1712, when the dispute had gathered enough momentum, Newton was convinced that the Leibnizian calculus was not an independent discovery, but only an imitation of his own fluxional calculus. His arguments went thus:[20]

1. Leibniz was in London in 1673 from where he went to Paris. From Paris, he was in regular correspondence with Collins and Oldenburg. He learnt a lot of British mathematics, especially those of Gregory and Newton, via this correspondence.

2. During this first visit, Leibniz contended for a method of series which was already published. This was meant to imply that Leibniz had exhibited tendencies to plagiarize.

3. Leibniz learned more from Tschirnhaus in 1675 when the latter visited Leibniz in Paris.

4. Leibniz never acknowledged before 1677 that he had any kind of calculus at all. In 1676, he got from Collins a compendium of letters and treatises. This contained the 1672 letter from Newton to Collins in which the fluxional calculus was completely described. Thus, he had an entire year in which to study Newton's fluxional calculus and suitably modify it and claim it as his own in his letter of 1677.

5. Leibniz had the benefit of the two letters of 1676 from which he could "pick his gold".

6. Leibniz had access to all the materials in Collin's possession during his second visit to London in 1676.

Given all of the above, Newton claimed that Leibniz was able to reconstruct, on the basis of published work of Gregory and Barrow, Newton's fluxional calculus in the form of the differential calculus. Many of these

[19] Rupert Hall, *Philosophers at War*, Pages 73-74
[20] Ibid., Page 72; David Brewster, *The Life of Sir Isaac Newton*, Pages 190-191

arguments were present in the report of the committee that was instituted by the Royal Society to investigate into the calculus dispute.

Perhaps the most serious charge against Leibniz among these is Claim 4. However, Newton is grossly mistaken here. The compendium was never sent to Paris.[21] Instead, he only received a short uninformative summary of the letter. Further, Leibniz accessed the compendium only during his 1676 visit to London. Thus, before 1676, Leibniz could not have known much about Newton's fluxional calculus. Since Newton's Second Letter of 1676 reached Leibniz only in 1677 and further since it did not contain any details of the calculus, Leibniz could not have obtained anything worthwhile(with respect to developing of the calculus) from these 1676 letters either.

Thus, the events of the period from 1673-1676 transpired to put Leibniz in a situation where he could be easily accused of plagiarism. The argument that Leibniz had access to materials in Collins possession and hence would have learnt of Newton's calculus is tough to counter. Without any evidence that would acquit Leibniz, it is indeed very hard to judge what Leibniz might have gleaned out of those documents. In such a situation, any claims of invention would naturally be met with skepticism and suspicion, especially when the fluxional and differential calculus happened to be so alike. Indeed, if we did not know today that Leibniz had independently arrived at his calculus in 1675, it would be hard to acknowledge him as an independent inventor, even if he indeed had.

Hofmann, a Leibniz scholar, attempts to defend Leibniz from the charges of plagiarism. Hofmann says that Leibniz could not have learnt anything useful from his 1673 visit to London because he was virtually ignorant of mathematics at that time and everything would have been over Leibniz's head. He says that Tschirnhaus could not have imparted anything useful to Leibniz during his 1675 Paris visit because Tschirnhaus was too opinionated, locked-up, confused and unsettled. He says that Leibniz did not read Barrow's book until after he discovered the calculus because he(Hofmann) did not find any dated notes in them. He claims that some of the Collins correspondence is irrelevant because they did not intimate any *real* knowledge.[22] Feingold replies:

> And how can one determine with certainty what a genius like Leibniz was capable of comprehending from various books and letters he encountered or discussions he participated in, however vague and confused their context appears to us today? Such reasoning, it seems to me, substitutes preconceived notions for constructive historical knowledge.[23]

Now that the case against Leibniz has been dismissed, let us examine why the controversy arose in the first place. As has been stated before, Newton conceded for a long time, that Leibniz was indeed an independent discoverer of the calculus. Further, Leibniz too for his part was content in his status as an

[21] Rupert Hall, *Philosophers at War*, Page 181
[22] Mordechai Feingold, *Newton, Leibniz and Barrow too: An attempt at a reinterpretation*, Isis 84(1993), Page 331
[23] Ibid., Page 331

independent inventor. So, what went wrong? The following section tries to determine the causes for the outbreak of the dispute.

4 Origins of the dispute

Historians differ on the question as to when the seeds of the dispute were first sown. Hall mentions that some historians have tended to see the priority dispute as beginning in 1676 and assign Newton the role of a suspicious, distrustful and watchful opponent of Leibniz's mathematical evolution.[24] However, as Hall argues, that is a rather extremist view. If Newton indeed had such opinions of Leibniz, Newton would not have written those letters of 1676 to Leibniz.

4.1 Leibniz's first publication of the calculus in 1684

From 1676 till 1684, nothing of interest happened. In 1684, Leibniz published his calculus in *Acta Eruditorum*. Thus, even though Leibniz had his calculus as early as 1675, it was only nine years later that he finally published it. Had he published it right in 1675, Leibniz probably would not have faced the accusations he did, at least, many of the arguments used against him would not have arisen.

Newton's reactions to this publication can be first seen in the *Principia*. Newton started work on the *Principia* in 1684 and it came out in print in 1687. Hall mentions that the form of the mathematical arguments in the *Principia* are far more familiar and convenient than either the fluxional calculus of Newton or the differential calculus of Leibniz,[25] i.e. Newton never explicitly introduced the fluxional calculus in expounding his philosophy of nature. Thus, when Newton introduces the "fluxion lemma"(Lemma II of Book II), and never again uses it in the book, it strikes us as being odd. This is especially so, since in Book I, he had been implicitly differentiating without bothering to explain the process. Hall argues that the reason Newton did this was so that he could insert the now famous scholium of an autobiographical nature in which he asserts that he was in possession of the fluxional calculus for a long time ("ten years ago") before Leibniz published his differential calculus.[26] Newton states that he informed Leibniz via the 1676 letters that he possessed the fluxional calculus. Newton says that in response, Leibniz described to him his differential calculus, which appears the same as his fluxional calculus except for notation. Note that Newton never asserts that he described to Leibniz his fluxional calculus, neither does he assert that Leibniz might have learned anything from him. However, we do not know if at this time, Newton considered Leibniz to be the *second* inventor or if he believed Leibniz to have seen his papers on fluxional calculus. Some historians believe that it is very probable that Newton did indeed consider Leibniz to be a second inventor of the calculus.[27]

It appears that Newton inserted this scholium so that he could state his claim about the invention of

[24] Rupert Hall, *Philosophers at War, Page 68*
[25] Ibid., Page 30
[26] Ibid., Page 33
[27] Ibid., Page ...; David Brewster, *The Life of Sir Isaac Newton*, Page 185

the calculus and assert his independence of Leibniz, since he realized that the fluxional and the differential calculus were very similar. It was especially important for Newton to state his independent claim since he realized the significance of the calculus with respect to solving important problems. After all, he had successfully used his fluxional calculus in providing mathematical justifications in the *Principia*.

Leibniz was happy to accept the assertion of the scholium at face value although neither he nor anyone else knew what the method of fluxions entailed.[28] As Hall repeatedly mentions throughout his book, Newton's work was hardly better known by his countrymen than by Leibniz, simply because Newton never published any of his work. In any case, around this time, everything seems to be fine between Newton and Leibniz.

Some historians have tended to view this event as the beginning of the dispute.[29] However, that is probably a rather strong statement considering that Newton is only making an independent claim over the invention of the calculus. Other events in the following years were far more instrumental in sparking off the dispute.

4.2 The beginnings of the dispute

The next significant event in our story was the arrival in 1693 of the revised edition of John Wallis' *Algebra*. In this book, Wallis printed a brief essay on the fluxional calculus. Hall mentions that this essay once again served to put on record that Newton had long possessed and devised a notation for the differential and integral calculus. There is no claim being made that Leibniz had taken anything from Newton or had even been helped by him.[30] Two years later, in 1695, Wallis published his *Mathematical Works*. In Volume II, which is nothing but the *Algebra*, the essay on the fluxional calculus is presented. However, Wallis misleads his reader by stating that he obtained this method from the 1676 letters of Newton. This can then be meant to imply that Newton had communicated his method of fluxions to Leibniz in 1676 before the latter published his calculus. This, as we know, is blatantly false. Here, perhaps, for the first time, we see suggestions that Leibniz might have plagiarized Newton's work. Indeed, Hall says, "Inadvertently, therefore, Wallis was beginning a process of public deception ...".[31] Not surprisingly, Leibniz takes offense at this implication and says so in his letter to Thomas Burnet.

Hall sheds some light on this behaviour of Wallis. Wallis throughout his life had fought for the claims of Englishmen against foreigners. He had offended almost every foreign mathematician with whom he came in contact by his robust, unabashed xenophobia. Once he had begun to suspect that Newton's typically English inventiveness was likely to be overlooked, he longed to take up his case.[32]

[28] Rupert Hall, *Philosophers at War*, Page 42

[29] G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 110, In *Newton And The New Direction In Science*

[30] Rupert Hall, *Philosophers at War*, Page 94-95

[31] Ibid., Page 95

[32] Ibid., Page 95-96

4.3 The *brachistochrone* problem

In 1696, Johann Bernoulli, who was instrumental in the development of the differential calculus, issued a challenge problem, which is now known as the brachistochrone problem. The problem was published in the Leipzig *Acta Eruditorum* and was addressed to "the shrewdest mathematicians in the world". Copies of the problem were posted to Newton and Wallis. Some historians believe that the problem was posed to demonstrate that the fluxional method was not as powerful enough as the differential calculus.[33] It is argued that Newton realized that this was the case judging by the rapidity with which he applied himself to the problem and solved it inspite of his own declarations that he should be concentrating on the King's business at the Mint and not trifle away his time on mathematics.[34] However, Rupert Hall does not agree with this reading of the situation. If Bernoulli indeed believed that Newton was incapable of solving the problem, then when he received an anonymous solution, he would not have guessed without any surprise whatsoever that the solution came from Newton.[35] At a later time, Leibniz published a review of the challenge problem, where he says that only those who understood the mysteries of "our" differential calculus managed to solve the problem. At this point, another character makes his entrance in the drama. Fatio de Duiller was a Swiss mathematician who had associated for some time in the past with both Leibniz and Huygens, Leibniz's mentor. But, later, he shifted to the Newtonian camp when Leibniz adopted a demeaning attitude towards him.[36] By 1696, he had already became an ardent follower and close friend of Newton. Fatio interpreted Leibniz's statements to mean that Newton had learnt the calculus from Leibniz and resented this. What Newton felt in this matter is not clear. Further, in the list of mathematicians that Leibniz mentioned as being *capable* of solving the problem, Fatio was not present. Fatio perceived this as an insult and did not take it lightly.

Meanwhile, Bernoulli in a private letter to Leibniz, wondered if Newton developed his method after having seen Leibniz's calculus which the latter communicated to Newton in 1677.[37] This was the first suggestion of plagiarism on either side.

However, Fatio has the dubious distinction of bringing out the squabble into the open. In 1699, he published his *Investigations* in which he claims that Newton is the first inventor of the calculus and that Leibniz's zeal in ubiquitously attributing the invention of the calculus to himself will not deceive anyone who has gone through the documents.[38] Once again, it is not at all clear whether Newton supported Fatio in this attack. Leibniz probably believed that Newton was ignorant of this and expected Newton to dissociate

[33]Ibid., Page 105; G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 111, In Newton And The New Direction In Science

[34]Ibid.

[35]Rupert Hall, *Philosophers at War*, Page 106

[36]G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 111, In Newton And The New Direction In Science

[37]Ibid., Page 116; G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus* Page 111, *In Newton And The New Direction In Science*

[38]G.V.Coyne, Page 112; Rupert Hall, *Philosophers at War*, Pages 106-107; David Brewster, *The Life of Sir Isaac Newton*, Page 185

himself from this attack. But, when Newton kept mum, Leibniz published in the *Acta* and dismissed these charges as emanating from a boorish and jealous young man. He referred to Newton's scholium in the *Principia* and accepts their independent developments of the calculus.[39] Fatio attempted to rebut these charges but the *Acta* refused to publish his rebuttal. So, everything went quiet for some time.

Thus, by 1700, the controversy had started. However, it should be noted that upto this time, neither Newton and Leibniz were directly involved in raking up this controversy. In the Newtonian camp, it was Wallis initially and Fatio now who were responsible for starting the squabble. Newton, all along was in the sidelines. However, it is not clear what to make of Newton's silence about the whole issue. On the one hand, he professed to accept Leibniz's independence and even said so in his letters to Leibniz, but on the other hand, he never dissociated from the allegations being leveled by his followers. Hall assigns the best of intentions to Newton's behaviour. He says: "... Newton(to the best of our knowledge) has left no indication of an earlier awareness of Fatio's *Investigations* nor of involvement in its nasty aftermath".[40] In general, it is probably a fair characterization of Newton's role prior to the year 1700. However, Hall remarks later on: "Wallis's own role in the slow warming of the calculus dispute had been to act as an uncritical mouthpiece for Newton ... Wallis was there (*Algebra*) writing - as so many Newtonians were to write - under Newton's instructions".[41]

Out in the Leibnizian camp, Bernoulli vociferously asserted Leibniz's claims to the invention of the calculus. Leibniz also asserted his claims to the invention of the calculus. However, as was mentioned earlier, Bernoulli went so far as to suggest that Newton might have plagiarized from Leibniz. As in the case of Newton, Rupert Hall says that Leibniz did not seek from Bernoulli an accusation of Newton and was anxious to drop the uncomfortable subject when Bernoulli brought it up.[42] However, as we will see, all this would change very quickly in the following years.

4.4 Change in Leibniz's opinion of Newton

The next major incident in the developing quarrel between Newton and Leibniz was Newton's publication of two mathematical treatises (On the Quadrature of Curves, Enumeration of Lines of the Third Order) in 1704. The content of the first treatise drastically altered Leibniz's view of Newton's evolution as a mathematician. When Leibniz read this treatise, he probably realized that Newton's fluxional calculus was not just a series of ad hoc mathematical devices put together, but were as general and powerful as his calculus itself.[43] Further, he realized that it was very identical to his differential and integral calculus except for notation. Thus, Leibniz realized that Newton's friends had all along been claiming priority for this *full majesty* of the calculus and not for the discovery of some inferior methods. Leibniz knew that his calculus was the

[39] David Brewster, Pages 185-186; Rupert Hall, Pages 123-125; H.D.Anthony, *Sir Issac Newton*, Page 69
[40] Rupert Hall, *Philosophers at War*, Page 118
[41] Ibid., Page 129
[42] Ibid.
[43] Ibid., Page 130

beginning of a great system of mathematics and had enormous potential. Leibniz thus far was under the impression that Newton probably had just hit upon extensions of the work of predecessors without having hit upon his discovery in its full beauty.[44] Now, this impression was shattered.

It has been suggested that it was only after the marked success of the Leibnizian calculus that Newton came to regard his method of fluxions as a new subject and an organized mode of mathematical expression rather than simply as a useful modification of earlier rules.[45] Boyer counters this argument by stating that Newton had by 1676 written out three different accounts(as infinitesimals, as ultimate ratios, and as fluxions) of his method thus implying that Newton did indeed understand the significance of his work. However, Boyer concedes that Leibniz probably expressed himself much more vigorously on the subject.[46]

In 1703, George Cheyne, a Newtonian, published a book *On the Inverse Method of Fluxions* which was a treatise on the integral calculus based on Newton and Gregory's achievements in calculus procedures. At the end of the book, he declared that all that has been published in the past 24 years is only a repetition or an easy corollary of what Newton long ago communicated to his friends or the public. It was very partial of Cheyne to thus ascribe all achievements to the British and pass over all the *published* achievements of Leibnizians. This naturally shocked both Bernoulli and Leibniz. Leibniz reacts to this suggestion by portraying Cheyne as no more than a beginner with little understanding of the series. Further, his contempt for Cheyne induced Leibniz into asserting that he never encountered any indication that the differential calculus or an equivalent to it was known to Newton before he(Leibniz) knew it. Here, we see a turnaround in Leibniz's opinions. Earlier, in 1676, he had readily admitted in his 1677 letter to Newton, that he was satisfied that Newton too was master of something analogous. Thus, these set of events had started a process by which he was ready to dismiss evidence that established Newton's equality and independence as an inventor of the calculus.[47] To quote Hall,

> If Newton allowed his disciples to put about highly unjust and damaging claims for his own benefit, Leibniz may have reasoned - perhaps not quite consciously - was it possible any longer to regard him as honest and truthful? Was not the master brushed with the tar of the pupils' incompetence and partiality?[48]

When Newton published his treatises in 1704, Leibniz could not regard this belated emergence of Newton as a trivial event, in the light of the preceding discussion. Further, Leibniz knew nothing of the immediate history of the treatises. This might have aroused suspicions in him about Newton's priority claims. Leibniz published in the Leipzig *Acta* an anonymous review of Newton's treatises. In this anonymous review, which one historian views as being an "imperfect analysis", it is claimed that Leibniz was the inventor of

[44] Ibid., Page 127
[45] Carl B. Boyer, *The history of the calculus and its conceptual development*, Page 208
[46] Ibid.
[47] Rupert Hall, *Philosophers at War*, Pages 131-133
[48] Ibid., Page 133

the calculus. Further he states: “Accordingly, instead of the Leibnizian differences, Mr Newton employs, and has always employed, fluxions, ... He has made elegant use of these both in his *Principia Mathematica* and in other publications since, just as Honore Fabri in his *Synopsis Geometrica* substituted the advance of movements for the method of Cavalieri.”[49] It was well known at that time that Cavalieri was one of the most conceptually inventive of mathematicians while Fabri was a competent second-rater who in the method referred to above, merely borrowed Cavalieri’s method and changed the mode of expression. Given this historical background, the analogy that Leibniz employs immediately leads us to believe that he is implying that Newton had stolen his work. This viewpoint is all the more strengthened given the surreptitious nature of Leibniz’s publication. In fact, this is the interpretation assumed by some historians.[50] However, others see it differently.[51] Hall gives Leibniz the benefit of doubt. Hall argues that the analogy employed by Leibniz also renders itself to a *conceptual* analogy which the latter must have been aware of. He argues that Leibniz probably meant to impress this conceptual analogy rather than imply the analogy of priorities. But, Hall concedes that, even if only in the unconscious manner of a Freudian slip, he had made an assertion that he was the originator and Newton the adaptor.[52] And, as Coyne also rightly admits, Leibniz displays extremely bad taste when he compares Newton with Fabri, who clearly had plagiarized the work of Cavalieri.[53] Though Leibniz would later on deny this implication, the damage had been done.

Newton, for his part, probably did not read the review or failed to discover any malevolence in it. Leibniz, meanwhile, was content to let the ambiguous state of affairs last indefinitely. He had little desire to institute a definitive historical inquiry into the origins of the calculus because the progress of the calculus in the public domain since 1684 had made the preeminence of himself and his pupils obvious.[54] A testimony of the power and excitement that Leibniz’s work held for the contemporaries is the fact that at the age of 40, Pierre Varignon, a French mathematician, set himself to master and teach these new methods and later embark to set out the whole of mechanics(including Newton’s work) in the new mathematical language.[55] Further, Leibniz, and a lot of other mechanical philosophers of the era thought that Newton, who was at that time primarily viewed as a brilliant mathematician, was mistaken in the physical theories he propounded. Accordingly, Hall argues that Leibniz had no reason to fear that Newton’s reputation would enhance and spread outside England due to his physical theories. Thus, Leibniz had no reason to break the peace, though uneasy, that reigned. The next initiative was far likely to come from the Newtonians who were fighting for recognition in a largely Cartesian universe.[56]

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[49]Ibid., Page 138

[50]H.D.Anthony, *Sir Issac Newton*, Pages 69-70; David Brewster, *The Life of Sir Isaac Newton*, Page 186

[51]G.V.Coyne, *Newton’s controversy with Leibniz over the invention of the calculus*, Page 112, In *Newton And The New Direction In Science*; Rupert Hall, *Philosophers at War*, Page 140

[52]Rupert Hall, *Philosophers at War*, Page 140

[53]G.V.Coyne, *Newton’s controversy with Leibniz over the invention of the calculus*, Page 112, In *Newton And The New Direction In Science*

[54]Rupert Hall, *Philosophers at War*, Page 141-143

[55]Ibid., Page 84

[56]Ibid., Page 143

4.5 Keill's attack on Leibniz

The uneasy peace lasted only four years. John Keill, a Newtonian, was the culprit. Keill was a pupil of David Gregory who probably introduced him to Newton. However, at the time that Keill attacked Leibniz in 1708, Newton only had a slight acquaintance with Newton and his writings. Thus, it is argued by Hall, that Keill like Fatio de Duillier, wrote his first defence of Newton without consulting Newton himself.[57] In 1708, Keill wrote a paper *On the Laws of Centripetal Force* published in the *Philosophical Transactions of the Royal Society* which only saw the light of day in 1710. In this paper, Keill claims for Newton the priority of the first invention of the calculus, turning to Wallis's publications of Newton's letters for proof. Further, he asserts that the Leibniz's publications were essentially the same as Newton's work "having changed the name and symbolism".[58] It is interesting to compare the reactions of Hall and Brewster to this letter of Keill. Hall considers this an open accusation of plagiarism which was deliberate, unnecessary and offensive whereas he considered Leibniz's implications in his earlier anonymous review to be accidental and unintended. Brewster, for his part says: "If the reader is disposed to consider this passage as retorting the charge of plagiarism upon Leibnitz(sic), he will readily admit that the mode of its expression is neither so coarse nor so insidious as that which is used by the writer in the Leipsic Acts(sic)."[59] Thus, we see exactly opposite opinions here. Brewster is writing about Newton's life and is probably being partial to Newton. He criticizes Leibniz's underhanded attitude in this incident and another similar one later on. However, it is not clear if Brewster is aware of Newton's role in the Royal Society's report against Leinbiz in 1712. Newton was the President of the Royal Society when Leibniz complained to the Royal Society. Newton then wrote the whole report himself and just got the committee to approve it.[60] Thus the committee was a farce. Brewster, when writing this book in 1831 was not aware of this incident since the membership of the committee was only known 133 years after the report was published in 1712.[61] As regards Rupert Hall, he is probably more impartial writing about 300 years after the controversy and when a lot of facts about the dispute have been cleared up. Throughout the book, he portrays Newton and Leibniz in good light whenever possible and shifts the blame in many a case to their followers. Though this might be true, Hall is probably not being entirely objective either.

Getting back to the story, Hall explains the motivation behind Keill's actions. Apparently, Keill perceived himself the principal target of criticisms in the *Acta Eruditorum* against forces of attraction. As a result, the English, by 1710, felt that they had two grievances against the Germans.[62] Leibniz, though initially favourably inclined towards Newton, had changed by this time, as we have seen. Combined with the

[57] Ibid., Page 144

[58] Ibid., Page 145; David Brewster, *The Life of Sir Isaac Newton*, Page 187; G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 112, In *Newton And The New Direction In Science*

[59] David Brewster, *The Life of Sir Isaac Newton*, Page 187

[60] Rupert Hall, *Philosophers at War*, Page 178

[61] G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 113, In *Newton And The New Direction In Science*

[62] Rupert Hall, *Philosophers at War*, Page 145

fact that he failed to understand Newton, he consistently trivialized Newton's thoughts as a philosopher. Hall claims that it is virtually certain that this determined Newton to give Keill his support since Newton now believed that Leibniz, a dishonorable man, had attacked his own honor and competence.[63] Thus, we see here issues of natural philosophy intermingling with those of the priority dispute resulting in a worsening of the controversy.

4.6 Leibniz's appeal for redress

Offended by Keill's remarks, Leibniz wrote to Hans Sloane, secretary of the Royal Society, requesting that the Society ask Keill to publicly apologize for his insinuations. Sloane sought Newton's advice on the letter, who at that time was the President of the Royal Society. Newton, for his part, took it up personally with Keill. At this time, Keill justified himself by pointing at the earlier reviews in the *Acta*. On reading those reviews, Newton agreed with Keill that he had been everywhere deprived of his discovery. Further he found that his *On the Quadrature of Curves* was considered to be a compilation of earlier work by Leibniz, Cheyne and Craige. It was at this time that Newton gave his support to Keill. When Keill successfully defended himself in front of the society by referring to the *Acta* reviews, the society now approved Keill to submit a written account of the matter.

In the letter that Keill submitted, he modified his earlier claims and instead asserted that Newton discovered his calculus before Leibniz and *opined* that some hints of this calculus were revealed to Leibniz in the 1676 letters. In the letter, Keill formally asserts that he is not pressing any "criminal" charge against Leibniz.[64] This letter was also sent to Leibniz in May 1711. Leibniz then wrote back in December 1711 declaring that Keill's justification by referring to the *Acta* was worthless since "everyone had received his due". He then once again claimed the right to the discovery of the calculus and that no one could claim to have forestalled him. He further appealed to Newton to get Keill, the upstart, to back down.[65]

Brewster is correct in declaring that Leibniz probably worsened the situation by declaring that "everyone had received his due". Surprisingly, Hall ignores this statement of Leibniz in his analysis. Instead, he blames Keill for not putting the matter to rest by refusing to admit Leibniz's vindication of his own independent discovery of the calculus. In any case, the Royal Society decided to appoint a committee to look into the dispute and report back. As was discussed before, the whole investigation was a farce and stagemanaged by Newton. In the report, Leibniz is found guilty of concealing his knowledge of the prior, relevant achievement of others. There was however, no formal accusation of plagiarism against Leibniz in the report. However, as Hall says, ".. if proved, it would defame him as effectively as the worse crime of open theft: that he had first silently ignored, and later explicitly denied, Newton's genuine right as first inventor".[66] The report, along with extracts from relevant documents was published in 1712 under the title *Commercium*

[63]Ibid., Page 167
[64]Ibid., Page 169-170; David Brewster, *The Life of Sir Isaac Newton*, Page 188
[65]Rupert Hall, *Philosophers at War*, Page 176; David Brewster, *The Life of Sir Isaac Newton*, Page 189
[66]Rupert Hall, *Philosophers at War*, Page 179

Epistolicum. As noted earlier, one glaring mistake in the report was Newton's assertion that Leibniz had access to Newton's 1672 letter to Collins. This mistake of Newton was corrected only about a hundred years ago. Hall defends this error on Newton's part as resulting from misunderstanding and that Newton was not deliberately maligning Leibniz.

Leibniz, in response, distributed the *Charta Volans*, an anonymous bulletin, in which he deprecates the *Commercium Epistolicum* and squarely accuses Newton and his disciples of stealing the differential calculus of Leibniz and then committing gross errors in their applications of it.[67] Hall says of this approach of Leibniz as ".. evincing a certain deviousness of character not inconsistent with those anonymous reviews in which he praised himself as another person might."[68]

At this point in the story, we see that there has been a deadlock. Neither party was willing to grant the other any credit for the invention of the calculus and had essentially accused the other of plagiarism. The controversy did not end here. This phase was followed by a barrage of caustic correspondence between the two parties. It carried on far beyond the death of Leibniz in 1716. There was nothing new that was written after a certain point. Accusations and counter-accusations, childish abuse was being hurled around. Of the situation, Halls says: "To examine the last years of the calculus dispute does not increase one's admiration for some of the greatest of mankind"[69]

5 Conclusions

Hall analyzes the whole dispute very nicely in his book. He examines the phenomenon of simultaneous discovery in the context of the existing sociological conditions at that time. Simultaneous discovery is not a rare occurrence in science. It has happened a number of times in the past and might happen in the future too. As recent as the 20th century, the theory of the big bang explanation for the origin of the universe was put forth simultaneously by two physicists, one in Russia and the other in the US.[70] In the Newton-Leibniz case, as we know today, the calculus was effectively discovered simultaneously by both Newton and Leibniz. Hall makes a very strong argument when he says that mathematics, because it offers the possibility of attaining results equally vigorously by different means, and because of its logical character, virtually necessitates the occurence of convergence. Hence, this field was peculiarly likely to be troubled by quarrels and priority disputes, just as, at the opposite extreme, natural history was almost completely free of such disagreeable incidents.[71] Further, the dispute was also to some extent the result of the prevailing conventions of priority in which publication was not necessary. This, combined with the great value attached to personal merit, emphasis on innovation and lack of formalized conventions prompts Hall to state that the

[67] G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 113, In *Newton And The New Direction In Science*

[68] Rupert Hall, *Philosophers at War*, Page 200

[69] Ibid., Page 232

[70] Source: PBS documentary on Stephen Hawking's Universe

[71] Rupert Hall, *Philosophers at War*, Page 6

common occurence of disputes is indicative in a striking way of those faults in the "reward system" of the period.[72]

In this particular case, what exacerbated the situation was Newton's reluctance in publishing his treatises. Probably, had he published his treatises right in the beginning, the dispute might not have taken off in the first place. Even if he had published his treatises when he first got hints of Leibniz's work instead of resorting to scholiums and the like, he might have been spared some of the pain and heartache that followed. In fact, he was repeatedly urged by a number of his friends and even by Leibniz to go into print right till the end of the 17th century. It is not very tough to search for answers into this behaviour of Newton. He had burnt his fingers when he had published his theory of light and colours. He had been subjected to a great deal of labour replying to criticisms and he said that he had sacrificed his peace[73]. Probably, because of this, and also maybe because he wanted his works to be as perfect as possible, he was extremely reluctant to publish his works. Further, he was glad to win private merit within the circle of competent mathematicians and had no wish to contend in a broader field. Hall says that it was a tragedy of the whole dispute that Newton later, changing his mind and seeking to undo the consequences of his earlier inactivity, tried to make this private reputation equivalent to one fully established and recognized by the public.[74] Finally, Newton was forced into the open by two factors. First, by the priority claims being made by his fellow countrymen who sought to establish the superiority of Englishmen over others. Second, by the suggestions by Leibnizians that he relied upon someone else in his intellectual pursuits. This latter character trait of Newton wherein he could not tolerate the idea of indebtedness to anyone[75] probably partly explains the vengeance with which he hit back at Leibniz later on.

Getting back to the phenomenon of convergence and simultaneous discovery, Rupert Hall quotes the sociologist Robert Merton wherein the latter states that the initial stability that arises out of the independent discoveries deteriorates later on. This instability occurs because of the ambiguous situation in which it is difficult to ascertain the role of each but since each *knows* that he had himself arrived at the discovery, because the institutionalized stakes of reputation are high and the joy of discovery is immense. The situation gets worse by reinforcement of group-loyalties and chauvinism.[76] This analysis fits the Newton-Leibniz case very well.

In conclusion, the dispute, though it casts these men of genius in very poor light with respect to the way they quarrelled, however was a necessary result of their personalities and the prevailing sociological conditions. However, this should not cast shadows on the brilliance of the mathematics that these men developed.

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[72] Ibid., Page 7

[73] Ibid., Page 22

[74] Ibid., Page 23

[75] G.V.Coyne, *Newton's controversy with Leibniz over the invention of the calculus*, Page 114, In *Newton And The New Direction In Science*; Mordechai Feingold, *Newton, Leibniz and Barrow too: An attempt at a reinterpretation*, Isis 84(1993), Pages 321

[76] Rupert Hall, *Philosophers at War*, Page 254-255