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An Element of Controversy

The Life of Chlorine in Science, Medicine, Technology and War

Edited by Hasok Chang and Catherine Jackson

from research by undergraduate students at
University College London

British Society for the History of Science

2007

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Front cover illustration: Blueprint for a chlorine chamber for the cure of respiratory diseases. Reproduced by permission of Edward G. Miner Library, Rocherster, New York.

Back cover illustration: Chlorine gas, courtesy of the Department of Chemistry, University College London. Photo by Gretchen Siglar.

Contents

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Chlorine and Prout's Hypothesis

Jonathan Nendick, Dominic Scrancher and Olivier Usher

1. Introduction

Chlorine found itself embroiled in another major scientific controversy, even before the dispute about its elementary nature (Chapter 2) was quite over. This concerned "Prout's hypothesis", the idea that all atomic weights were whole-number multiples of hydrogen's. William Prout (1785–1850) was a physician and physiological chemist, who made pioneering investigations into the chemical nature of urine, gastric juices, and many other animal substances. He also had a reputation as an analytical chemist, so it is not a surprise that his claims about atomic weights attracted some attention. In 1815 Prout anonymously published a paper in which he claimed that all atomic weights were whole-number multiples of the atomic weight of hydrogen.[1] The strikingly simple pattern that Prout identified came as a surprise. The most trusted set of atomic weights at that time, published by the Swedish chemist Jöns Jakob Berzelius (1779–1848), indicated no obvious pattern, and Berzelius had seen no reason to look for one.[2] At the heart of Prout's reasoning was his attraction to the metaphysical idea of the unity of matter. He believed not only that all elements had atomic weights that were integer multiples of hydrogen, but that the reason for this was that all atoms were actually made up of hydrogen atoms, which he thought was the prime matter of

[1] Prout [1815] (1932). In fact, Prout went further than this, proposing that all atomic weights were even-numbered multiples of hydrogen's, with the majority being multiples of four times that of hydrogen. A correction to the 1815 paper was published, also anonymously, in 1816, but it did not affect the hypothesis itself. It is unclear why Prout felt the need to publish these papers anonymously, but it appears to be due to a lack of confidence rather than to any fear of controversy.
[2] Berzelius (1814), p. 362.

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the ancient Greeks, *proto hyle*, or "protyle". When Ernest Rutherford was looking for a name for the newly identified elementary particle that constituted the nucleus of the hydrogen atom, he hit upon the suggestion of "proton" in 1920, in honour of both Prout and his protyle.[3]

Many decades of disputes followed Prout's work, and chlorine was an important focus in that debate. Prout measured the atomic weight of chlorine as exactly 36 times that of hydrogen. Today, the atomic weight of chlorine is known to be approximately 35.5,[4] and the majority of 19th-century chemists were aware of the fact that chlorine did not fit Prout's hypothesis well. The debate over Prout's hypothesis was used by the philosopher of science Imre Lakatos (1922–1974) as one of two main historical examples to illustrate his ideas about scientific research programmes.[5] Lakatos also highlighted the role of chlorine in this debate, as the most salient anomaly for Prout's hypothesis.[6] He claimed that 19th-century chemists were justified in maintaining Prout's hypothesis despite its many difficulties, because they were able to learn new things in their efforts to hold on to the hypothesis. The Proutian work constituted "a research programme progressing in an ocean of anomalies".[7]

In this chapter we investigate the case of chlorine and Prout's hypothesis at two levels, both controversial. First, we trace the history of Prout's hypothesis, building on the extensive and authoritative account by William H. Brock and providing further details especially concerning chlorine.[8] We focus on two early phases of debate. The initial controversy was largely confined to Britain (although it did involve Berzelius in Sweden), where it ceased around 1840 without a clear resolution. Then the work moved over to Continental Europe, where an entirely new generation of chemists became interested in the debate. We also note the

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[3] See Brock (1985), p. 217.
[4] This does *not* actually mean that the modern atomic weight of chlorine is 35.5 times that of the modern atomic weight of hydrogen. This is because the modern system does not take the atomic weight of hydrogen as the unit. Therefore, the modern version of Prout's hypothesis still concerns whole-number atomic weights, but not whole-number multiples of hydrogen's weight. All of this will be explained in detail in Section 2.3.
[5] Lakatos (1970), esp. pp. 138–140.
[6] Ibid., p. 128.
[7] Ibid., p. 138.
[8] Brock (1985).

ironic twists given to the whole history of Prout's hypothesis through the precision measurements of atomic weights developed in the early 20th century. In the second half of the chapter we make an assessment of Lakatos's treatment of the case, which does not seem to square very well with the history that we have learned. This lack of fit will then prompt us to engage in a more general debate about the wisdom of imposing a generalized philosophical framework on the history of science.

2. The history of controversy over Prout's hypothesis

2.1. Britain, 1815–1839

The early debate on Prout's hypothesis revolved around Thomas Thomson (1773–1852), Regius Professor of Chemistry at the University of Glasgow from 1818, who was the first well-respected chemist to champion it. Not only was Thomson an influential figure as a teacher and textbook-writer, but he was also the founder and editor of *Annals of Philosophy*, the journal in which Prout's paper was published. Struck by the simplicity and elegance of Prout's idea, Thomson became an immediate convert. By 1818 he was a firm believer in Prout's hypothesis and set about trying to prove it. The major obstacle to proving Prout's hypothesis was the table of atomic weights that had been published in 1813 by Jöns Jakob Berzelius (1779–1848), which Thomson also published in his journal in 1814.[9] Oxygen and carbon were the only atomic weights in Berzelius's table that convincingly supported Prout's hypothesis, although the figure for nitrogen was also quite close to a whole number. Thomson therefore spent the next seven years conducting a huge number of experiments in order to produce a table of equivalents that would prove Prout's hypothesis true. With this endeavour Thomson became the main defender of the hypothesis in this first period of debate. Prout himself played very little part in the extended controversy following the publication of his papers in 1815 and 1816.

In 1825, Thomson published his results and declared that he had succeeded in proving Prout's hypothesis.[10] He changed all of Berzelius's results so that they were now whole numbers, and gave measurements for elements that were missing from Berzelius's table, notably chlorine. (It

[9] Berzelius (1814).
[10] Thomson (1825).

makes sense that Berzelius would not have included chlorine in his atomic weight table, since he had not yet been convinced that chlorine was an element, as we saw in Chapter 2, Section 6.) Prout had measured chlorine as having an atomic weight of 36 and Thomson's results agreed with this. Thomson attracted considerable support from other British chemists, including William Henry (1774–1836) and Richard Phillips (1778–1851). Henry, who had worked with Thomson while the latter was teaching privately in Edinburgh, rallied to Thomson's side on this debate, stating that even if there were some experimental inaccuracies involved, they should not be "allowed to countervail a general law of so much simplicity, and supported by so many probabilities".[11] Phillips was a friend of Thomson's, and succeeded him as editor of *Annals of Philosophy*. He lent support to Thomson by publishing a table of integer atomic weights in the journal in 1824.[12] Thomson did face opposition from the Glasgow-based chemist Andrew Ure, described by Brock as "an unpleasant character" who was "no friend to Thomson".[13] Ure wrote a scathing review of Thomson's *System of Chemistry* in 1821, in which he questioned the integrity of Thomson's experimental technique.[14]

The significance of Ure's attack, however, paled in comparison to the response from Berzelius, whose view could hardly be ignored given his formidable reputation as one of the leading chemists in Europe at the time. In the year following the publication of Thomson's Proutian treatise, Berzelius announced his intention to examine the case thoroughly:

> In any case, this matter deserves to be investigated in detail, so that, in case Prout's remarks are without grounds, one has to regard it as completely refuted, or, in case it is correct, one can accept it as reliable.[15]

In the same year Berzelius published results for the atomic weight of chlorine as 35.41 and 35.47, thereby placing chlorine at the center of the argument on Prout's hypothesis.[16] Even several years before this episode, Berzelius seems to have had a professional disdain for Thomson, if not a

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[11] Henry (1829), vol. 2, p. 663; cited in Brock (1985), p. 145.
[12] Phillips (1824).
[13] Brock (1985), p. 147.
[14] Ure (1821).
[15] Berzelius (1826), pp. 3–4 (our translation, with help from Hauke Riesch). "Auf jeden Fall verdient dieser Gegenstand so ausführlich untersucht zu werden, dass man, falls Prout's Bemerkung ohne Grund ist, sie als volkommen widerlegt zu betrachten hat, oder, falls sie richtig, dass man sie als zuverlässig annehmen kann."
[16] Berzelius, *Lehrbuch der Chemie*, vol. 2 (1826), cited in Partington (1964), p. 165.

strong personal dislike. In 1821 Berzelius had wondered whether Thomson was "the worst chemist existing at this moment".[17] By 1828 Berzelius publicly accused Thomson of scientific fraud, asserting that Thomson had simply made up his data.[18] Berzelius later regretted his actions, and his accusation of fraud seems unjustified, but there is some evidence that Thomson's strong affinity for Prout's hypothesis introduced a bias in the interpretation and presentation of his data. In a paper of 1818, for example, Thomson openly modified the results of experiments to suit Prout's hypothesis. On the atomic weight for iodine, he followed the experiment of Gay-Lussac, but added:

> This is the number obtained by Gay-Lussac from the combination of iodine and zinc, which he found a compound of 100 iodine + 26.52 of zinc. Now 26.52 : 100 :: 4.125 : 15.625. *I have very slightly modified Gay-Lussac's numbers to make the atom of iodine a multiple of .125*.[19]

Here the atomic weight of oxygen was taken as the unit, and taken as 8 times the weight of hydrogen (on the assumption that water is a one-to-one atomic combination of hydrogen and oxygen), giving hydrogen the value of 0.125. Thomson made similar roundings elsewhere in the same paper. For instance, he reinterpreted Lagerhjelm's experiment on the atomic weight of bismuth, rounding an atomic weight of 8.869 to 8.875 to be a multiple of hydrogen.[20] He assumed that chromic acid — for which he had two experimental weights, 6.547 and 6.541 — had a "true weight" of 6.5.[21] This sort of manipulation of experimental evidence was not limited to his 1818 paper. For example, a similar instance can be seen in his discussion of the composition of the crystals that can form when chlorine is passed through cold water, in the *First Principles of Chemistry*.[22] It would be a mistake, however, to imagine that Thomson was simply a poor and dishonest experimenter whose inadequacies were exposed by Berzelius. Differences between the results obtained by Berzelius and Thomson could have been due to any number of experimental errors. Ironically, as Partington points out, Thomson's results, with the exception

[17] Berzelius, Letter to Gaspard de la Rive, cited in Partington (1964), p. 225.
[18] Berzelius, *Philosophical Magazine* 5 (1828), p. 217, cited in Partington (1964), p. 226.
[19] Thomson (1818), p. 339; emphasis added.
[20] Ibid., p. 347.
[21] Ibid., p. 349.
[22] Thomson (1825), pp. 83–84.

of chlorine, are all in better agreement with modern values than Berzelius's![23]

Following the direct confrontation between Berzelius and Thomson, a significant contribution was made by Edward Turner (1796–1837), the first Professor of Chemistry at the University of London. Turner, unlike Phillips and Henry, had no particular allegiance with Thomson, but he was a great admirer of Thomson's. When he published his *Introduction to the Study of the Laws of Chemical Combination and the Atomic Theory* in 1825, Turner undoubtedly supported Prout's hypothesis:[24]

> Dr Prout published an essay … in which he showed that the atomic weights, or equivalents of several substances, are multiples by a whole number of the atomic weight of hydrogen gas. Dr Thomson took up this idea, and in his recent admirable treatise on the 'First Principles of Chemistry' has proved that it applies generally.[25]

In the years following this assertion, however, Turner changed his view considerably. The differences between Thomson's and Berzelius's atomic weights forced him to reconsider his position and persuaded him that some kind of arbitration was needed in order to settle the matter:

> This [Prout's] hypothesis is of so much importance if true, and may give rise to so much error if false, that its accuracy cannot too soon be put to the test of a minute experimental enquiry.[26]

With this in mind, he set about re-investigating the atomic weight of barium (a result of Thomson's that had been specifically criticized by Berzelius) by conducting experiments on the chloride of barium. It ought to be pointed out that Turner's views had not yet changed to such an extent that he was ready to reject Prout's hypothesis, or to take the side of Berzelius and Ure. Indeed, the introduction to his 1829 paper "On the Composition of Chloride of Barium" criticized the unprofessional behaviour of both Berzelius and Ure, stating that their acrimonious and aggressive tone had destroyed "that confidence which their well-founded reputation for sagacity and skill would otherwise inspire."[27] Having said this, Turner then continued to find errors in Thomson's calculations and

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[23] Partington (1964), p. 226. See also Table 2 in Section 2.3 below.
[24] Partington (1964, p. 227) states erroneously that Turner's *Introduction* was anti-Prout.
[25] Turner (1825).
[26] Turner (1829), p. 292.
[27] Ibid.

concluded provisionally in favour of Berzelius, although he added that a full conclusion would have to wait until the dispute over the atomic weight of chlorine (on which that of barium depends) was settled.

In 1833, in the 4th edition of his textbook of chemistry, Turner published his own table of atomic weights, which appeared to refute Prout's hypothesis.[28] In the same year he also published a follow-up to his 1829 paper on barium, in which he corrected Thomson's values for the atomic weights of lead, chlorine, silver, barium, and nitrogen. By this time Turner had completely reversed his views on Prout's hypothesis, concluding that the weight of chlorine was 35.42 and that:

> Dr Prout's hypothesis, as advocated by Dr Thomson,—that all atomic weights are simple multiples of that of hydrogen,—can no longer be maintained.[29]

This was a serious blow: Turner had defected to the anti-Proutian camp, claiming to have uncovered serious flaws in Thomson's *First Principles of Chemistry*, a book which he considered to supply the only real body of evidence that the Proutians could offer in support of their hypothesis.[30] What Turner did was to restate the flaw already documented in Thomson's method, and to argue that this error undermined the entire corpus of Thomson's work on atomic weights. As Turner pointed out, subsequent to his proof of Thomson's error with barium chloride, Thomson admitted to the fallacy and:

> accordingly changed the equivalent of barium from 70 to 68. The inevitable consequence of this change must be apparent to every one who is acquainted with the method of analysis so frequently resorted to by Dr Thomson. Many of the experiments described in his First Principles of Chemistry are now at irreconcilable variance with each other, and, if relied upon at all, subvert the conclusions which they once appeared to establish.[31]

In other words, the identification of the problem with Thomson's barium chloride experiment required a far more comprehensive reappraisal of Thomson's figures than he provided, if they were to be internally coherent once again. It might be expected that this would have settled the

[28] Turner (1833a), p. 971.
[29] Turner (1833b), p. 544.
[30] Turner (1832), p. 109.
[31] Turner (1833b), p. 523. The retraction was made in Thomson (1831).

debate; Turner had obviously spent considerable time and care over these experiments, and they had forced him to reappraise his original views.

Still, Turner's work failed to generate an anti-Prout consensus. James R. Partington argues that even Turner's results were, with the exception of that for chlorine, not incompatible with Prout's hypothesis if his experimental errors are taken into account.[32] Although the internal consistency of the results obtained by each of the various chemists discussed here was excellent, typically showing a variation of less than 0.02% for Turner, Berzelius and Frederick Penny,[33] the systematic errors seem to have been much larger. As Brock has pointed out, the largest possible deviation from an integer is 0.5 — a small percentage of the atomic-weight values for heavier elements.[34] The systematic error in the results of everyone involved in the debate was at least of this order in some experiments. In any event, Prout's hypothesis still had considerable support both from Thomson and from his disciple Phillips. As late as 1839, Penny suggested that Thomson's Proutian estimates were still "in general use among British chemists".[35] Prout himself in 1831 anticipated a modification to the hypothesis that would explain fractional atomic-weight values:

> The original opinion to which I was led . . . was . . . that the combining or atomic weights of bodies . . . must necessarily be multiples of some one unit; but as the atom of hydrogen, the lowest body known, is frequently subdivided when in combination with oxygen, &c. there seems to be no reason why bodies still lower in the scale [i.e., lighter] than hydrogen . . . may not exist, of which other bodies may be multiples, without being actually multiples of the intermediate hydrogen.[36]

The debate in Britain effectively came to a close in 1839, not in a consensus but in a stalemate, when Phillips and Penny published mutually contradictory papers on the subject. Phillips's paper contained results of his own experiments, which agreed with those of Thomson, and therefore disagreed with those of Turner.[37] Meanwhile, Penny came down on Turner's and Berzelius's side of the fence and, mimicking Turner,

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[32] Partington (1964), p. 228.
[33] See figures published in Turner (1833b) and Penny (1839).
[34] Brock (1985), pp. 154–156.
[35] Penny (1839), p. 32.
[36] Prout (1831), pp. 129–130.
[37] Phillips (1839).

stated: "The favourite hypothesis, of all equivalents being simple multiples of Hydrogen, is no longer tenable."[38] It would appear from this that the debate ought to have continued. Surprisingly, however, Penny and Phillips had what amounted to the last words spoken in the debate, and there was little discussion of Prout in Britain after 1839.

Why did this happen? There are a number of possible reasons for the fact that no further significant research into Prout's hypothesis took place in Britain after 1839. The first possible cause is that British chemists came to realize the truth of what Charles Daubeny had said in 1831: experimental chemistry was not yet accurate enough either to verify or to refute Prout's hypothesis.[39] However, if there was a feeling that experimental chemistry would be unable to settle the dispute, then it was largely an unspoken feeling. There were other, more practical, reasons why the debate might have fizzled out. One likely cause is that many of the chemists who had been working on Prout's hypothesis were coming to the end of their careers, or even their lives by 1839. Henry died in 1836, Turner in 1837. Thomson lived until 1852, but by 1840 he had started to wind down his involvement in chemistry. He continued to write papers, but the rate of his work was becoming slower, and he was choosing his areas of research more carefully. Presumably there was also a feeling on Thomson's part that he had done all that he could do on this debate, having spent seven years researching atomic weights. Phillips carried on working until his death in 1851, but with the absence of Turner, and the non-participation of Thomson, there might have been little impetus for him to carry on alone. Prout himself, having contributed little to the debate apart from writing the original paper, exhibited no further interest after writing to Daubeny. The only British chemist who had contributed to the debate and who was of a younger generation was Penny (born in 1816). But with no one else of his generation showing an interest, there was hardly much of an incentive to continue his research. This failure of interest in Prout's hypothesis to cross the generation gap in Britain is perhaps not surprising — there is often a tendency for younger generations to be uninterested in things that greatly interested their predecessors. This tendency makes what happened in Continental Europe from 1840 onwards all the more unusual and interesting.

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[38] Penny (1839).
[39] Daubeny (1831).

2.2. Continental Europe, after 1840

By 1840 the debate over Prout's hypothesis in Britain was effectively finished, but in France it was just getting started. Jean-Baptiste Dumas (1800–1884), France's most noted chemist at the time, published a paper jointly with the young Belgian chemist Jean-Servais Stas (1813–1891) titled "Researches on the True Atomic Weight of Carbon".[40] This was an attempt to ascertain whether Berzelius's figure of 76.42 (O=100) for the weight of carbon was too high.[41] Leo Klosterman states that the paper came about as a result of a report on an analysis of resinous oils commissioned by the *Académie des Sciences*. Dumas had also shown interest in Prout's hypothesis in 1828 despite the fact that he had measured the atomic weight of chlorine as 35.8, a figure that did not fit the hypothesis well.[42] The 1840 paper concluded that Berzelius's figure needed to be corrected to 75 (O=100), a figure that Dumas and Stas realized was identical to Prout's prediction of C=6 (H=1).

Dumas was instantly impressed by this result, and it led him to believe that all atomic weights would have to be measured again in order to correct any errors. Despite the long years of debate that had raged in Britain, Dumas was ignorant of, or uninterested by, any table of atomic weights other than that of Berzelius. This shows the regard with which Berzelius was held on the Continent and the relative disregard of British chemists at the time. This disregard had not gone unnoticed in Britain, and many believed that British chemistry simply was not as good as that on the Continent. Daubeny commented on this by citing John Herschel's complaint in his *Introduction to the study of Natural Philosophy* that Britain had fallen behind the rest of Europe in all branches of science. Daubeny, predictably, blamed the Government:

> the inferiority . . . is in part attributable to the culpable apathy, which the government of our own country has been wont to exhibit with reference to abstract science in general.[43]

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[40] Dumas and Stas (1841).
[41] An online biography of Thomson: "Significant Scots — Thomas Thomson" (http://www.electricscotland.com/history/men/thomson_thomas.htm, most recently accessed on 15 May 2007) claims that Dumas was asked to investigate the weight of carbon by Thomson on a trip to Glasgow, but we have been unable to confirm this.
[42] Klosterman (1985), p. 73.
[43] Daubeny (1831).

Whether through an ignorance of the debate in Britain (unlikely), or through a belief that the British chemists had done little to resolve the problem (much more likely), Dumas embarked upon his own programme of research with the aim of proving Prout's hypothesis true.

His first action was to persuade former students and associates to join him in researching atomic weights. Dumas, even more than Thomson, had a network of young chemists who might be said to be his allies, and he used them to his advantage.[44] In fact, the similarities between Dumas and Thomson are striking. Dumas had also set up a practical teaching laboratory in Paris, the first of its kind in France, similar to Thomson's in Glasgow. Their support for Prout's hypothesis, in both cases, began quite suddenly and lasted for the rest of their lives. And both men decided to embark upon an exhaustive set of experiments with the express aim of proving Prout's hypothesis true.

Dumas quickly came up with a favourable result for the atomic weight of oxygen, 8 (H=1).[45] The German chemists Otto Erdmann and Richard Felix Marchand confirmed Dumas's result.[46] A former student of Dumas's, Jean Charles Galissard de Marignac (1817–1894), now working in Geneva, measured the atomic weights of chlorine, potassium and silver and obtained favourable results, with chlorine as 36 (H=1). Marignac emphasized the crucial importance of chlorine:

> Of all the elementary substances, chlorine is perhaps that for which it is most important to know the equivalent [atomic weight] with certainty; because, once that has been determined, the atomic weights of the majority of metals may be calculated with sufficient ease and precision.[47]

Marignac also saw that the fact that Berzelius's table of equivalents listed the weight of chlorine as 442.65 (O=100) meant that the fate of Prout's hypothesis might rest on the accuracy of this figure.

[44] See Klosterman (1985) for a detailed discussion of Dumas's research school.
[45] Dumas (1843), p. 189.
[46] Brock (1985), p. 174.
[47] Marignac (1842), p. 145, translation by Jonathan Nendick. "De tous les corps simples, le chlore est peut-être celui dont il est le plus important de connaître l'équivalent avec certitude; car, une fois celui-là déterminé, les poids atomiques de la plupart des métaux peuvent être calculés avec assez de facilité et de précision."

At this point, things were looking very good for Dumas and Prout's hypothesis, while looking quite bad for Berzelius, who had been forced to concede that his value for the weight of carbon was erroneous. Berzelius was still unwilling to accept Prout's hypothesis on the strength of relatively few new results, however, and wrote to Marignac urging him not to read too much into these results.[48] The situation changed, however, due in no small part to Stas, Dumas's collaborator on the 1840 paper. Stas was originally taken by the simplicity and elegance of Prout's hypothesis, but he came to realize that no matter how much he wanted it to be true, the results from his own further research simply did not support the hypothesis. In 1849 he published "Recherches nouvelles sur le véritable poids atomique du carbone" in which he reaffirmed the value of 75 (O=100) for carbon, but in the course of two decades of experimental work Stas gradually lost confidence in Prout's hypothesis, and eventually rejected it as an "illusion" in 1860.[49]

Meanwhile, Marignac was also becoming doubtful. Just a year after he found a value of 36 for the weight of chlorine, he discovered an error in his calculations and corrected his value to 35.456, agreeing with Berzelius's old value.[50] Around the same time the French chemist Auguste Laurent also obtained a value for chlorine that agreed with Berzelius and used it to reject Prout's hypothesis.[51] Chlorine was becoming a major sticking point for supporters of the hypothesis. Laurent's colleague Charles Gerhardt obtained a similar result to Marignac's in 1845, but "corrected" it to 36.[52]

Marignac had not yet given up on Prout's hypothesis. In the same paper that gave 35.456 for chlorine, encouraged by the closeness of this figure to 35.5, he suggested that if one half the weight of chlorine was taken as unity, then the hypothesis could be saved. E. J. Maumené later made the same suggestion independently of Marignac.[53] To some extent, Prout himself had anticipated this modification in his letter to Daubeny,

[48] Berzelius, letter to Marignac, mentioned in Klosterman (1985), p. 76, but no reference given.
[49] Stas (1849); Stas [1860] (1932).
[50] Marignac (1843).
[51] See Klosterman (1985), p. 76.
[52] See ibid., p. 77.
[53] Maumené (1846).

as we have already noted. In the later episode chlorine was directly responsible for a major modification to Prout's hypothesis, whereas Prout did not mention any specific element in his letter. Marignac soon encountered fresh difficulty with his subsequent measurements for the atomic weight of silver, which did not agree with even his reformulation of Prout's hypothesis.[54]

In 1859, desperately trying to save Prout's hypothesis, and taking Marignac's measurement of the atomic weight of silver as his basis, Dumas suggested that one quarter the weight of hydrogen be taken as unity.[55] But in the following year Stas declared that even taking one-quarter hydrogen as unity could not save all the experimental results. Stas now declared: "I have reached the complete conviction . . . that Prout's law, with all the modifications due to M. Dumas, is nothing but an illusion, a pure hypothesis expressly contradicted by experiment."[56] Marignac, commenting on this paper, conceded the force of Stas's argument, but was reluctant to give up Prout's hypothesis entirely. From his own experiments Marignac gave the atomic weight of chlorine as 35.456, which was very close to Stas's 35.46.[57] Speculative debate continued for years afterwards, due mainly to the reluctance of both Dumas and Marignac to accept defeat. Dumas took his belief in Prout's hypothesis to his grave. Stas eventually persuaded Marignac to accept that Prout's hypothesis was false in 1866, but Stas himself admitted in 1887 that the number of elements with integer atomic weights was too high to be a mere coincidence.[58] Prout's hypothesis may not have been strictly true, but there was something to it.

The history of Prout's hypothesis in the 19th century is complex and eventful, and the role played by chlorine in the debates is a difficult one to pin down. There is a clear division between two periods of debate. While there is little or no gap chronologically speaking between the two periods, there exist both a generation gap and a geographical division. With the exception of Penny, the British chemists who were engaged in debate over Prout's hypothesis were all born before 1800. In contrast, if we look at the three main Continental contributors to the debate, Dumas

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[54] Marignac (1846).
[55] Dumas (1859); see Klosterman (1985), p. 77, for further discussion.
[56] Stas [1860] (1932), p. 45.
[57] Marignac [1860] (1932); the atomic-weight values are summarized on p. 51.
[58] See Klosterman (1985), p. 79.

was born in 1800, Stas in 1813 and Marignac in 1817 (two years after the publication of Prout's first paper). In the first time period, the fact that Continental chemists ignored the efforts of the British chemists can, at least in part, be put down to the fact that British chemistry was considered inferior by chemists on both sides of the divide. When Dumas and his fellow Continental chemists started to show interest in the hypothesis, British efforts had come to a halt, with the young generation of British chemists displaying a lack of interest in the problem which had occupied their predecessors. The one link between the two periods was the indefatigable Berzelius, who appeared to have a need to be involved in all aspects of chemistry, wherever it was taking place and whoever else was taking part.

In addition to generational and geographical differences, there was also a marked difference in the role that chlorine played in the two debates. In the British debate, the atomic weight of chlorine barely played any role at all, except in helping to determine the atomic weight of barium. Chlorine was not considered to be of any greater or lesser importance than other elements. Berzelius's table of atomic weights — the single greatest impediment to the acceptance of Prout's hypothesis during the first period of debate — proved such a difficulty not merely because his measurement of chlorine disagreed with the notion of integer multiples of hydrogen, but because the vast majority of his measurements did. Chlorine was nothing special.

In the second period of debate, however, chlorine was recognized as being one of the most important elements to investigate accurately in order to verify or refute Prout's hypothesis. Ironically, though he was the first to explicitly state that a great deal depended upon the weight of chlorine, Marignac did not accept that Prout's hypothesis was false even when it became well established that chlorine did not fit. Instead, he used the fact that many of the measurements, including his own, were close to 35.5 to modify the hypothesis so that all elements were made up of integer multiples of half the weight of hydrogen. Later, Stas used his measurement of chlorine to refute Dumas's further modification of the hypothesis which proposed one-quarter hydrogen as unity.

2.3. Twentieth-century aftermath

We have seen that the debate on Prout's hypothesis reached a stalemate by the late 19th century: it had too many exceptions to be regarded as a true generalization, but it was so close to the mark in too many cases to be dismissed completely. In the early decades of the 20th century, unexpected developments from microphysics would revive the interest in Prout's hypothesis, and it eventually became "the corner-stone of modern theories of the structure of atoms."[59]

The rejuvenation of Prout's hypothesis was most of all due to the work of the English physicist Francis William Aston (1877–1945), whose mass spectrometer allowed a direct and precise measurement of the weights of individual atoms (positive ions, to be precise) by electromagnetic deflections.[60] Aston's measurements revealed the existence of chemically identical atoms that had distinct masses, which the English chemist Frederick Soddy (1877–1956) called "isotopes" in 1913.[61] Aston's measurements seemed to indicate that the atomic weights of individual isotopes obeyed Prout's hypothesis, while the atomic weights determined by macroscopic-chemical methods were merely "fortuitous statistical effects due to the relative quantities of the isotopic constituents".[62] For example, in a case closely investigated by Aston early on, neon was shown to be a mixture of two isotopes with atomic weights of 20 and 22, giving an average atomic weight of 20.2 simply because neon-20 happened to be about 9 times as abundant as neon-22 on earth. In the case of chlorine, there are two isotopes with atomic weights approximately 35 and 37. Chlorine found on earth is a mixture of these isotopes roughly in the ratio of 3:1, giving the average atomic weight of 35.5. By 1919 Aston was ready to declare the truth of a reconstituted Prout's hypothesis.

Further experimental investigation by Aston, however, revealed that even for isotopes "the whole number rule" was not exactly true, which was understood in later physics by reference to the interconversion of mass and energy. Perhaps the most egregious exception to Aston's version of Prout's hypothesis was hydrogen itself. Aston had inferred the truth of his whole-number rule by taking the atomic weight of oxygen as 16. On that basis, however, precise measurement gave the value of 1.008 to hydrogen. Aston's measurements led to the system of atomic weights

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[59] Frederick Soddy, quoted by Lakatos (1970), p. 140.
[60] On Aston's work and its implications, see Brock (1985), pp. 201–216; Aston's own retrospective account can be found in Aston (1960).
[61] See Brock (1985), p. 200.
[62] Aston, quoted in Brock (1985), p. 210.

currently in use, in which the atomic mass unit is defined as one-twelfth of the mass of carbon-12, not as the mass of hydrogen. It is effectively the mass of one half proton, plus the mass of one half neutron, plus the mass of an electron. It also turns out to be almost exactly one-sixteenth of the average mass of oxygen in its naturally occurring mixture of isotopes.

All of that should give us a good pause before we make any retrospective celebrations about how modern values on the weights of isotopes vindicate Prout's hypothesis. If the weight of the lighter isotope of chlorine is very nearly 35 in the modern system, then it is *not* anywhere near 35 times the atomic weight of hydrogen, since 35 times 1.008 is 35.28. In modern physics we say that weights of protons and neutrons are not exactly additive when they are put together to make atomic nuclei.

Table 1 shows some selected modern atomic weights, in column *a*. On the right-hand side (column *b*), we give the same numbers expressed in the system in which the weight of hydrogen is taken as unity. The results in the table confirm T. M. Lowry's point that "integral atomic weights are much more common when O=16 and H=1.0078 than when H=1 and O=15.876."[63] If Prout's original hypothesis were correct, integer values would appear in column *b* of our table, not in column *a*. We can see that the values in column *b* are mostly quite far away from integer values. In effect, the modern system of atomic weights based on carbon-12 was designed to make the maximum number of isotopes have atomic weights that are closest to whole numbers.

Let us take a closer look at several selected elements and the evolution of their accepted atomic weight values, shown in Table 2. This overview gives us a rather humbling lesson: none of the four sets of 19th-century measurement stand out as particularly close to or far from the modern values, and it seems that it was a vain hope that any of these values would give a conclusive experimental proof or refutation of Prout's hypothesis.

[63] Lowry (1936), p. 483.

Table 1. Selected elements with modern values for atomic weights[64]

Element	*(a) Modern atomic weight (with 12 for C-12)*	*(b) Modern atomic weight (with 1 for H)*
Hydrogen	1.0079	1 (by definition)
Carbon	12.011	11.9169
Nitrogen	14.0067	13.8969
Oxygen	15.9994	15.8740
Sulphur	32.06	31.8087
Chlorine	35.453	35.1751
Potassium	39.098	38.7915
Manganese	54.938	54.5073
Iron	55.874	55.4361
Copper	63.546	63.0479
Silver	107.868	107.0225
Barium	137.34	136.2635

Table 2. Experimental values of atomic weights of selected elements, expressed on H=1 scale (including modern values)[65]

	Prout (1815)	Thomson (1825)	Berzelius (1827)	Turner (1832/3)	Modern value (M)	M/2**	2M**
Lead	104	104	107.458	103.5	205.57	102.785	
Nitrogen	14	14	14.186	14	13.8969		
Iron	28	28	54.363	28	55.4361	27.7180	
Barium	70	70*	137.325	68.7	136.2635	68.1318	
Silver	108	110	216.611	108	107.0225		214.045
Manganese	56	28	57.019		54.5073	27.2537	
Chlorine	36	36	35.47	35.45	35.1751		

* In Thomson (1831), he revises this to 68.
** Atomic weights were frequently out by a factor of two, because the number of atoms in the compounds that were being tested was not always known. For this reason, the final two columns — half and double the modern value — are included where relevant.

[64] The values in column *a* have been taken from Ohanian (1985), p. 561.
[65] These values were compiled from Ohanian (1985), Freund (1904), Turner (1832), Turner (1833b) and Thomson (1825).

3. Lakatos, Prout and historiographical controversy

3.1. Lakatos and rational reconstructions

In this section we analyze and evaluate Imre Lakatos's "rational" approach to the history of science, with reference to the case study on Prout's hypothesis. In his paper on "The History of Science and Its Rational Reconstruction", Lakatos makes a distinction between internal and external history, and argues that the historian of science should be primarily concerned with internal history. Lakatos does concede that "any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) 'external history'."[66] However, "external history is irrelevant for the understanding of *science*."[67] Lakatos' definition of internal history is not the standard one. As Ian Hacking notes:

> Internal history is usually the history of ideas germane to the science and attends to the motivations of research workers, their patterns of communication and their lines of intellectual filiation. Lakatos's internal history is to be one extreme on this spectrum. It is to exclude anything in the subjective or personal domain. What people believed is irrelevant: it is to be a history of some sort of abstraction from what is said.[68]

What Lakatos is aiming for then is a history of the growth of impersonal knowledge. He proposes that if a particular episode in the history of science does not quite fit with a rational interpretation of it, then actual history should be altered in our account in order to render it rational: "internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasions, their radically improved version."[69] As Tomas Kulka summarizes:

> Lakatos first argues that each methodology implies normative guidance for historiography, for rational reconstruction. [Lakatos says:] 'The basic idea of this criticism is that *all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational reconstruction to which they lead*.' The content of the rational reconstruction . . . is to be contrasted with the actual history.[70]

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[66] Lakatos (1971a), p. 91.
[67] Lakatos (1971a), p. 92; emphasis added.
[68] Hacking (1981), p. 138.
[69] Lakatos (1971a), p. 106.
[70] Kulka (1977), p. 337. The quotation within this quotation is from Lakatos (1971a).

It is Lakatos's view that the best historiographical methodology is the one that manages to internally account for the most history of science. This desire to internalize the historical account stems from Lakatos's view of rationality; this rationality, in turn, defines what is internal. Thus, internal history "provides a rational explanation for the growth of objective knowledge."[71] Lakatos then argues that his own "methodology of research programmes" produces an account in which the most history of science can be seen as internal, compared to other philosophies of science that serve as historiographical frameworks.

Lakatos's idea of rational reconstructions has been criticized widely and harshly by many historians and even philosophers. We will advance our own critique later in the chapter, but for now we want to start by defending Lakatos's view against some common yet misconceived attacks. The most obvious criticism that can be aimed at Lakatos is that the job of a historian should be to write history as honestly as possible. Lakatos's rational history would seem to prevent this from the outset, since he wants to account for the history of science in an entirely rational way. Where historical events do not happen to fit his conception of acceptable scientific methodology he simply rewrites history to fit with his philosophy. When Lakatos writes history he seems dishonest, and what is worse, *intentionally* dishonest. It is one thing to write a poor history through ignorance, but it is quite another to change historical fact to support a particular view. As Larry Laudan puts it, Lakatos is making the claim that "the 'rational historian' should construct *a priori* an account of how a particular episode should have occurred. There need be *no* resemblance whatever between the 'internal' account so constructed and the actual exigencies of the case under examination."[72]

There is something too easy about this kind of criticism. Lakatos is not saying that the historian should lie about what actually happened. Famously he states:

> One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history "misbehaved" in the light of its rational reconstruction.[73]

[71] Lakatos (1971a), p. 91.
[72] Laudan (1977), p. 168.
[73] Lakatos (1971a), p. 106.

Furthermore, Lakatos's particular objective must be remembered: he is trying to chart the growth of objective knowledge and is proposing a particular historiographical approach in order to do this. While it may seem obvious to most people that there is something fundamentally wrong with consciously falsifying history, we must find a more substantive criticism. It is not enough to say "well you can't just rewrite history." We must find a reason for saying this.

A more serious criticism is that Lakatosian history cannot teach us anything that we didn't already know. Thomas Kuhn insists: "data can, and must be permitted to, react back on expectations, make trouble for them, play a role in their transformation."[74] This cannot happen when Lakatos re-writes history. As Kuhn explains, Lakatos's point is "not simply that the historian selects and interprets, but that prior philosophy supplies the whole set of criteria by which he does so. If that were the case, however, there would be no way at all in which the selected and interpreted data could react back on a methodological position to change it."[75] Lakatos's method seems to leave no room for an alteration of philosophical position. According to Kuhn, the Lakatosian requirement to make history fit with methodology is so strong that it prevents any change in historical outlook:

> If 'internal' were an independent term unequivocally applied, as it is for the historian, then one could hope to learn something about rational methodology from the study of internal history. But if 'internal history' is simply the rational part of history, then the philosopher can learn from it about scientific method only what he puts in. Lakatos' meta-methodological method is in danger of reducing to tautology.[76]

Kulka makes a similar point, in the continuation of the earlier passage:

> How could a history serve as a test of the methodology on the basis of which it has been reconstructed? A historical test so conceived has a very strong circular element. For it is obvious that history interpreted according to a specific methodological criteria [sic] would always tend to vindicate that rational reconstruction which is explicitly based on this methodology rather than any rival one.[77]

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[74] Kuhn (1980), p. 182.
[75] Kuhn (1971), p. 142.
[76] Ibid., p. 141.
[77] Kulka (1977), p. 338.

This criticism is not quite cogent. We must recall that the reconstructionist has a grasp of actual history when he is altering it to fit his rational reconstruction. It just so happens that this actual history is written in the footnotes. This means that we *can* put Lakatos's historical research programme to the test. We can examine a particular historical episode and see how well Lakatos's method can account for it. We can see how much of the history has to be rewritten in order to fit the methodology. If a large amount of history has to be altered, then we can start to have doubts about this particular historiographical approach. Lakatos himself made this point very clearly:

> If in the light of a rational reconstruction the history of science is seen as increasingly irrational *without* a progressive externalist explanation (such as an explanation of the degeneration of science in terms of political or religious terror, or of an antiscientific ideological climate, or of the rise of a new parasitic class of pseudoscientists with vested interests in rapid 'university expansion'), then historiographical innovation, proliferation of historiographical theories, is vital.[78]

3.2. Lakatos's reconstruction of the Prout case

Let us see, then, how well Lakatos can handle the history of Prout's hypothesis. One clear reconstruction that Lakatos makes is Prout's view on the atomic weight of chlorine:

> Prout, in an anonymous paper of 1815, claimed that the atomic weights of all pure chemical elements were whole numbers. He knew very well that anomalies abounded, but said that these arose because chemical substances as they ordinarily occurred were *impure*: that is, the relevant the 'experimental techniques' of the time were unreliable, or, to put it in our terms, the contemporary 'observational' theories in the light of which the truth values of the basic statements of his theory were established, were false.

Only in a footnote does he tell us that this is not true:

> Alas, all this is rational reconstruction rather than actual history. Prout denied the existence of any anomalies. For instance, he claimed that the atomic weight of chlorine was exactly 36.[79]

Hacking notes this, and wonders what Lakatos's aim is in altering Prout's belief:

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[78] Lakatos (1971a), p. 119.
[79] Lakatos (1970), p. 138.

> Lakatos made Prout into a significant figure who *knew* that chlorine has a weight of 35.5 but still promulgated his hypothesis of integers. A footnote corrects this by saying that Prout thought Cl was 36. Prout had so fudged the numbers that he got 36 and believed it. . . . Lakatos's point would have been perfectly well served by the facts rather than his fiction, for many able analytic chemists, especially in Britain, did persist in Prout's hypothesis after it was 'known' that Cl had to be about 35.5. It was unnecessary for Lakatos to spruce up the example by distorting the facts.[80]

Although Hacking thinks that Lakatos has no reason to "spruce up the example", Lakatos always has a reason for such things. By implying that Prout did not think that chlorine had a value of 36, Lakatos is able to put forward his rational methodology more strongly. With Prout knowing that Cl is 35.5 Lakatos is showing us an example of someone holding on to the "hard core" of his research programme despite apparent refutation. Thus in Lakatos's reconstruction, Prout's behaviour was rational according to the methodology of scientific research programmes. Generally the Proutians, Lakatos argues, held onto their hypothesis not through irrational acts such as distorting experiments,[81] or *ad hoc* hypotheses,[82] but merely because they had a progressive research programme generating new experimental techniques and results.

The problem is that Lakatos actually misses certain "irrational" acts that were committed to help prevent the refutation of Prout's hypothesis. He also neglects the role of irrational factors in the acceptance or rejection of the hypothesis. The acceptance of Prout's hypothesis was contingent upon external factors, for example geographical location. As Stas noted:

> In England the hypothesis of Dr Prout was almost universally accepted as absolute truth. The work executed by Professor Thomas Thomson of Glasgow in order to base it on analytic experiments, greatly contributed to this result. Nevertheless, the same thing did not hold for Germany or France. The immense prestige which surrounded the name of Berzelius and legitimate confidence inspired by his work on the weights of atoms were incontestably the cause of this.[83]

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[80] Hacking (1981), p. 140.
[81] As suggested by Klosterman (1985), p. 77.
[82] As suggested by Kendall (1949), p. 4.
[83] Stas [1860] (1932), p. 42.

Another important external factor that we miss in Lakatos's account is the role of scientific communities. We have already noted the influential positions that both Thomson and Dumas occupied in British and French chemical communities. From Lakatos we may well learn that these communities were engaged in a specific "research programme", but we would not take into account the sociological factors that surely impacted upon acceptance. Indeed, Lakatos's admission that "the history of science cannot be *fully* understood without mob-psychology"[84] becomes particularly pertinent at this point. "Mob-psychology" seems to have played a significant role in the case of the Proutians, and we need this kind of external explanation to fully understand the episode.

If we look at the historical details we see that the episode is highly irrational, under a Lakatosian definition. Belief in the hypothesis was determined by community, geographical location, and personal factors; furthermore, the whole episode has elements of dishonesty, uncertainty, and *ad hoc* modification of theories. To fully understand the episode we must have an external account. While it is possible for Lakatos to account for the episode by way of rational reconstruction (Proutians held on to their research programme in a sea of anomalies because it was still progressing), it is hard to avoid the impression that Lakatos only goes for this explanation because we now know that Prout's hypothesis is more or less correct when we consider isotopes and avoid making hydrogen the unit of atomic weights (see Section 2.3). Suppose that Prout's hypothesis was entirely false but had been thought to be correct at that time; in that case Lakatos would probably endorse an external account telling us that people only stuck with the programme *because* of their irrational acts such as the "correction" of results. In any case, the Prout episode cannot be fully understood "without a progressive external explanation."[85] Without such an explanation we do not understand why many scientists decided to abandon the programme, or why some held on to it despite strong evidence that, at the time, contradicted the hypothesis.

An even more serious point of criticism may be raised: does the history of Prout's hypothesis fit with Lakatos's model of a research programme which continued to be used because it was more progressive than the alternatives? The evidence suggests that it does not. First, there

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[84] Lakatos (1970), p. 140, footnote 3.
[85] Lakatos (1971a), p. 119.

is scant evidence to suggest that the Proutian research programme was progressive at all in the British phase, at least in terms of how Lakatos understands progressiveness. For Lakatos, it is the generation of novel predictions, confirmed by experiment, which defines progressiveness. Where are the confirmed novel predictions that Proutians made? Thomson's experiments that confirmed Prout's hypothesis were often, as we have seen, in effect rigged to give results supporting the hypothesis — hardly the sort of confirmation Lakatos would have wanted. Second, in terms of the rationality of theory-choice, Prout's hypothesis, if it was a Lakatosian research programme, was not actually competing against a rival research programme. Prout's opponents did not have a rival theory; they simply doubted that Prout was correct. This means that the alternative could not possibly have been "progressive" in Lakatosian terms, as a non-existent theory cannot produce novel predictions. Therefore, in Lakatos's framework, it was not possible to rationally choose anti-Prout over Prout, even though the rational reasons for defending Prout had disappeared by 1840.

3.3. Critique of Lakatosian historiography

Having made a sympathetic exposition of Lakatos's historiography, and examined the quality of the rational reconstruction that Lakatos makes of the Prout episode, we are now in a good position to attempt an informed general critique of Lakatos.

By rewriting history Lakatos is showing science to be a rational pursuit unhindered by personal belief and human subjectivity. He argues that if we do not employ his notion of research programmes when writing history we are condemned to Kuhnian or other similar irrational accounts of scientific progress. Lakatos's account aims to show how science has been a rational activity, with choice governed by whether a research programme is progressive or not. Progress is defined by whether there is an increase in the empirical content of a research programme, which in turn hinges on whether there are corroborated novel predictions. This also provides the meta-criteria by which to judge rival historiographical research programmes, which can be rationally compared and evaluated: "Thus progress in the theory of scientific rationality is marked by the discovery of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational."[86]

There are some problems with using novel predictions as criteria of progress and rationality. It is not straightforward to define "novel", and there are well-known quandaries about whether successful novel predictions should be given greater significance than the accommodation of known facts. And as Laudan points out, the amount of empirical content is a difficult thing to measure:

> Because comparisons of content are generally impossible, neither Lakatos nor his followers have been able to identify *any* historical case to which the Lakatosian definition of progress can be shown strictly to apply.[87]

All these problems, however, are something of a side issue, compared to the fundamental objection to the employment of a timeless, *a priori* conception of rationality. Even Lakatos himself seems to object to a fixed concept of rationality: "is it not then *hubris* to try to impose some *a priori* philosophy of science on the most advanced sciences? . . . . I think it is."[88] But this is exactly what Lakatos does when he employs his notion of rationality. This problem of fixed rationality threatens the very purpose of Lakatos's philosophy of science, as Alan Chalmers argues in relation to Lakatos's treatment of Newton:

> There are two reasons why I regard this position I here attribute to Lakatos as untenable. Firstly, having granted that it is perfectly intelligible to present methods and standards as progressively changing in the light of practice on one occasion, as Lakatos does with his study of Newton's physics, it is implausible to assume that similar changes cannot happen on other, subsequent occasions. Secondly, it is possible to provide examples of changes in standards within physics after Newton. For instance, a standard implicit in nineteenth-century physics involved its determinist character.[89]

Lakatos asserts that Newton did in fact change scientific standards, admitting the possibility of change, but then says that the standards introduced by Newton are unchangeable. Chalmers's objection to this claim is surely valid.

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[86] Lakatos (1971a), p. 118.
[87] Laudan (1977), p. 77.
[88] Lakatos (1971a), p. 121.
[89] Chalmers (1990), p. 21.

Even if we grant that there could be a timeless form of rationality, Lakatos provides surprisingly scant defence of the particular conception of rationality that he adopts. As Peter Machamer and Francesca di Poppa point out:

> an argument is needed to prove that rationality is in fact constituted by theory choice that increases empirical content. Such an argument would have to be analytic, *a priori*, transcendental, or somehow based on premises that are detached from history's actual happenings, separated from any empirical basis.[90]

Lakatos does not provide such an argument. In fact he does not even make an argument for the superiority of rationality over irrationality. When it comes to historiography, Lakatos states that internal explanations are better *because* they concern the rational aspects of science, but the superiority of a "rational" as opposed to an "irrational" explanation is merely assumed, not argued for. Lakatos does say that the history of science should be internal as this is what reveals the rationality of science, but he still does not argue for why such a rational history should be preferred. We may grant him that it is internal history that defines what is external; "internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history."[91] It is not necessary, however, to go along with Lakatos's next step, which is to declare that internal history is more important than external history.

It is impossible to argue that Lakatos's historiography is wholly incoherent or illegitimate, despite all the objections we have articulated. Yet it is still possible to make the *plea* that history should not be falsified in order to fit a particular conception of rationality. Lakatos himself states that if the external account is the only way to make sense of history then we must look for alternative historiographical theories. Our discussion of the Prout case has shown that Lakatos's rational reconstruction does not fully explain the episode, and that an external account is needed. Lakatos's account does not consider the complexities of the case, and therefore misses out on some fundamental insights into the workings of science, such as the effects of scientific communities on acceptance of theories. A rationalist historiography sterilizes history. As Laudan puts it:

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[90] Machamer and di Poppa (2001), p. 467.
[91] Lakatos (1971a), p. 105.

> once an episode has been so re-cast by the rational reconstructionist, he proceeds to appraise its rationality, according to an appropriate model of rational choice. Whatever the outcome of the appraisal, however, *the historical episode itself remains untouched and unexplained.*[92]

Lakatos's methodology does have its benefits: it is useful for determining what should be internal and what should be external in an historical account. That is to say, Lakatos's criteria enable us to recognize when science has slipped out of the bounds of a particular type of rationality, and thus allows us to examine what may have been the cause of this. In this way we are able to understand the scientific process better, and that is exactly what Lakatos's method should be used for. However, Lakatos's suggestion that we should alter historical facts if they do not fit can be dropped, and within historiography there is no compelling reason to give internal explanations a higher status than external explanations.

4. Conclusion

Through most of the 19th century Prout's hypothesis was in a kind of limbo, being neither fully accepted nor fully rejected at any point. The complexity of this period in the history of chemistry is far too great to be fitted into Lakatos's philosophical framework. Kuhn's model may be a better framework for understanding the Prout debate, as it allows for historical changes in the notion of rationality itself. But it is not illuminating to identify Proutians and anti-Proutians as followers of competing paradigms (or indeed, research programmes). While there were some fairly serious differences of basic assumptions between Thomson and Berzelius that clouded some of the debate between them and made direct comparison of their experimental data difficult, this stops short of the full incommensurability we would expect if they belonged to competing paradigms. Moreover, the breakdown of belief in Prout's hypothesis in the 1830s does not look like the breakdown of a paradigm. Large parts of the chemical community still retained the hypothesis; there was no period of extraordinary science; and Prout's hypothesis was resurrected shortly afterwards in France.

It is more instructive to regard the debate over Prout's hypothesis as a debate *within* the paradigm of atomic chemistry. Proutians and anti-

[92] Laudan (1977), p. 169.

Proutians alike used the same terms, and carried out the same sort of experiments. Experiments were carried out with particular types of outcomes in mind, under the assumption of fixed proportions in chemical combinations. Prout's hypothesis went in and out of fashion and remained a subject of considerable debate; however, at no stage did this debate threaten the fundamental tenets of the paradigm of atomic chemistry. In this context we can also see that Berzelius's robust criticism of Thomson's approach was somewhat unfair. Thomson's manipulation of the figures is best seen as an example of "puzzle solving" within a paradigm, rather than as deception or fraud. Proponents and opponents of Prout's hypothesis were not entirely dogmatic. Berzelius was at least prepared to countenance changing his views about Prout's hypothesis. Thomson, to his credit, was also prepared to acknowledge his mistakes; when Berzelius and Turner questioned his experimental method with regard to barium chloride, he accepted their criticism and published a retraction and detailed correction. What all this suggests is that the Proutian and anti-Proutian camps were actually able to agree on a great deal. Their disagreements were on a fairly minor aspect of the broad atomist paradigm, within which they were all engaged in subtly different forms of problem-solving activity.

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