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Discussions

AN HISTORICAL NOTE ON ERNST MACH

I

A number of philosophers including Peter Clark ([1976] & [1982]) and Paul Feyerabend [1980] in this journal and Larry Laudan [1976] and Michael Gardner [1979] elsewhere have tried to suggest that perhaps Ernst Mach's opposition to atoms and Ludwig Boltzmann's kinetic theory of gases was scientific and justified. Much as I respect Mach's independent judgment and expecially his warnings about excessive idealisation in physics I cannot agree.

As an historian of science, author of a biography of Mach [1972], and as co-editor of forthcoming volumes on the correspondence respectively of Mach and his principal methodological critic Ludwig Boltzmann I do not believe that the attempt to whitewash Mach's position on atomism will stand up. It is understandable that philosophers may be reluctant to believe that philosophical views, assumptions, or prejudices can stand in the way of scientific progress, but sometimes the evidence is too strong to be easily argued or willed away.

Perhaps Mach's opposition to 'billiard ball' atoms should have included support for atoms as changing, perishable 'corpuscles' such as Boltzmann strongly advocated, but it never did. And perhaps Mach's opposition to Boltzmann's kinetic theory should have been grounded on real technical difficulties such as what Clark calls 'the problem of the anomalous specific heat ratios' or real defects in his use of idealised molecular models rather than seemingly automatic rejection of anything and everything based on the atomic theory, but it never did. In *principle* Mach was remarkably tolerant. He allowed that the atomic theory might have 'provisional value' in science and were there no need to reconcile the philosophical assumptions of psychology with those of physics, he would have no objection to physicists continuing to use the atomic theory. But in *practice* he was adamant. He made no use of the atomic theory in his own scientific work during his mature years, not even concerning shock waves where the theory might have been useful.

In my [1972] book on Mach I failed to mention the many technical objections to Boltzmann's kinetic theory which accompanied the many philosophical attacks by Mach, Ostwald, and their followers. My reason was that virtually all of the technical objections were highly mathematical and most appeared in English journals, and there was and is no evidence which has been made available to me that Mach had ever read the technical objections or was even aware of them. His criticisms of Boltzmann's statistical interpetation of the second law of thermodynamics was quite

independent of scientific objections, apart from the largely philosophical reversibility and recurrence objections of Loschmidt and Zermelo. To partly compensate for my 'oversight' the forthcoming edition of Boltzmann's correspondence will include the technical objections by Culverwell, Bryan, Burburry, Watson, and others including Boltzmann himself which originally appeared as letters to the editor in *Nature* during the middle 1890s.

Perhaps Mach was too clever to be a 'simple-minded positivist' as Paul Feyerabend supposes.[1] Perhaps no philosopher is literally simpleminded, but his reduction of the physical world to sensations and his belief that all real knowledge is scientific was surely positivistic as it was understood at the time, and he did share the simpleminded opposition to atoms which many positivists held. His reasons were not primarly physical; they were philosophical, if one may label the opinions of a posititivist as philosophical.

What were Mach's philosophical objections? Peter Clark supposes that his opposition to use of the atomic theory was based on his notion of 'Denkökonomie' as if one should always pick the simplest theory in science and is if one could imagine a simpler approach than atomism. Mach's opposition to the reality of 'billiard ball' atoms (a very different question) was that as eternal and unchanging 'substances' they were metaphysical and incapable *in principle* of being observed. On the other hand, it is not clear why he seemed unable to appreciate that by the late nineteenth century a large number of physicists and chemists were already thinking of atoms in terms of divisible and transient corpuscles which were not necessarily subject to Mach's philosophical objections. (His partial paralysis in 1898 and withdrawal from university life in 1901 naturally did not help him keep up with the latest opinions in physics, but his fairly rapid return to writing and publishing would suggest that he should have known what was going on. His strong interest in Einstein's special theory of relativity during 1909 and 1910 would indicate he was still capable of following recent developments.)

There is a belatedly published story by Ludwig Boltzmann's last assistant, Stefan Meyer [1950] which I include in my biography ([1972], pp. 319–23) that Mach changed his mind about atoms after looking through a spinthariscope and observing the scintillations, but since Mach wrote and published attacks on the reality of atoms as late as 1910, if there was a conversion it presumably must have been after that year, that is, between 1910 and his death in 1916. The quotation against the reality of atoms is to be found in a 'Kampfschrift' against Max Planck and assorted critics ([1910] & [1919], pp. 10–11):

> The results of the atomic theory can be just as manifold and useful if one is not in such a hurry to treat atoms as realities. Therefore all honor to the beliefs of physicists! But I myself cannot make this particular belief my own (trans. Blackmore, [1972], p. 321).

[1] Note from Paul Feyerabend, postmarked 5 January 1984.

The only evidence I have been able to find from Mach's published or unpublished writings that he had begun to have second-thoughts about atoms seems to surface in his last 'Notizbuch' from 1910 to 1914. On page 37 he writes with a shaky left hand, presumably during 1911:

Atome bewegen sich im Raum und Zeit. Was ist Raum und Zeit? Raum ist von der Materie abstrahiert, von der Ausdehnung der Materie. Zeit von der Veraenderung der Materie.

And page 132, most likely from 1914 in an extremely faint and shaky left hand. (He was seventy-six years old and his paralysis would still not let him write with his right hand):

Atome nicht *occult*?[1]

II

Concerning kinetic theory, it may be well to keep in mind that there is a difference between atomists who objected to one or more technical aspects of Boltzmann's work (such as virtually all of the contributors to the kinetic discussion in the British Isles) but who in general favoured the kinetic theory and anti-atomists who opposed the kinetic theory *in principle* (such as Mach, Ostwald, and their followers). Also, it might be helpful to remember that especially on the Continent physicists with a strong knowledge of mathematics were still rare birds. (Even the extensive interest in kinetic theory in England and Ireland, which presumably had been stimulated by Maxwell, was led primarily by mathematicians with an interest in physics rather than by professional physicists, though Lord Kelvin would be an exception.) Most Continental physicists (such as Mach) were experimentalists who did not feel competent to handle complex mathematics. They could carefully follow neither the technical arguments of Boltzmann nor his critics. To justify this weakness it was natural to appeal both to a form of thermodynamic reductionism and to positivist arguments that the purpose of science was merely to discover relations between physical appearances and to remind each other that atoms and molecules were still unobservable (though Loschmidt and Kelvin had calculated the approximate size of some molecules in a manner consistent with Avogadro's law as early as the middle 1860s and by the 1890s new elements to fill in the periodic chart were being discovered almost every year).

[1] Mach's Notizbücher are located in the Ernst-Mach-Institut, Eckerstrasse 4, Freiburg im Breisgau. There are sixty-five in number from the 1860s until 1914. They include drafts of papers, diagrams, comments, addresses, lists of names to send preprints and books to, but after Mach's partial paralysis in 1898 the remarks are shorter and increasingly more difficult to read. Mach typed his correspondence after 1898 and he continued to send and receive many letters after 1914 until his death two years later. None of his Notizbücher and very few of the letters written by him have been published, but Friedrich Stadler has prepared a book of essays which includes the entirety of the last Notizbuch as an appendix. The book will be published in Vienna and should appear in 1986.

There can be little question that more physicists at least in Central Europe were familiar with Mach's anti-atomistic books such as *Erhaltung der Arbeit* [1872], *Die Mechanik* [1883], and *Principien der Wärmelehre* [1896] than with technical objections to Boltzmann's kinetic theory published abroad in English. As already mentioned, semi-philosophical objections, which still interest many people including Sir Karl Popper [1974], such as those of Loschmidt on reversibility of motion [1876] and Zermelo in his extension of Nietzsche's 'eternal recurrence' speculation [1896] were widely known. It was Boltzmann's answers which were not sufficiently circulated or comprehended (and even today his notorious 'H-theorem' can seem mindboggling).

Mach's anti-atomism and opposition to a mechanical 'explanation' of thermodynamics, however, *preceded* both the arguments of Boltzmann and those of his critics and at least on the Continent helped to dispose physicists and many chemists against Boltzmann's work before they had even read it. Mach stopped using the atomic theory in his work in 1863 and attacked it in 1872. Boltzmann began publishing in the late 1860s, replied to Loschmidt's objections in the middle 1870s, became well-known for his ideas in the 1880s, and was strongly criticized on mathematical, physical, and philosophical grounds during the 1890s.

Wilhelm Ostwald, a co-founder of physical chemistry and long the most influential man in the discipline with his efficient laboratory and vast numbers of students, freely admitted that Mach among the living and Robert Mayer among the dead had influenced his thinking the most (Ostwald, [1901]). Ostwald's anti-atomism became conspicuous in 1892 when he published a chemistry textbook with no reference to atoms. His attempt to substitute energy equations for everything atomistic provoked Boltzmann and the mathematician Felix Klein into a devestating reply at Lübeck during a scientific conference in 1895, but the influence of Mach and Ostwald was so great by then that most physicists and many chemists in Germany, Austria, and France still seemed to oppose Boltzmann. Continuum physics appeared to be the wave of the future, either Ostwald's 'energeticism', Mach's 'physical phenomenology', or Lorentz's 'electromagnetic world-view'. Atomism seemed dead apart from the continuing discoveries of Ramsay and Rayleigh and J. J. Thomson's apparent discovery of something which was soon called an 'electron'.

As for the technical objections to Boltzmann's work which were much discussed in 1895 and 1896 in England, while they seemed to fit in with the general opposition to dis-continuity physics at the time and the anticipation that the triumph of continuum physics was at hand, should not be overvalued, particularly by us who have the benefit of hindsight. As John Nyhof has pointed out in his still unpublished doctoral dissertation [1981] technical objections such as those about anomalous specific heat ratios were scarcely fatal, as seems rather evident from the fact that both Planck and Einstein from 1900 on accepted and used Boltzmann's major ideas, that is,

well *before* such technical objections had been met. Had Continental physicists been aware of Lakatos's theory of 'degenerating research programmes' they might have labeled Boltzmann's kinetic theory with the expression, but they would have been as wrong then as Peter Clark appears to be in his [1976] paper. When the continuum euphoria had died down (largely as a result of Planck's quantum theory and Einstein's theory on Brownian motion) and it became clear that atoms were here to stay, the tide understandably began to turn against the anti-atomism of both Mach and Ostwald until Ostwald himself recanted in 1908 and perhaps Mach in 1914.

To be sure, not all of Boltzmann's work has been retained and there are still occasional critics, but in terms of the larger picture he has contributed so much of value to modern physics, and by no means least to the understanding of Planck and Einstein themselves that to complain about difficulties in retaining the entire corpus of his labour would be like rejecting Kepler's discoveries or importance because of his occasional mysticism, technical oversights, or his 'fourth law'.

As for Mach's specific objections to Boltzmann's statistical interpretation of the second law, there don't seem to be any except opposition to anything which seems to presuppose the reality of atoms and a distrust of what Mach termed 'Lottospiel' or in English bingo. In point of fact, Mach was not persuaded that the second law was essentially different from the first. He did repeat Loschmidt's objection that if all mechanical motion was reversible and entropy was not, then if the second law was valid something was wrong with traditional mechanics. But he never seems to have replied to Boltzmann's statistical interpretation of the second law as an answer to Loschmidt, at least not in a mathematical manner. Indeed, apart from a few isolated disparaging remarks he did his best to ignore the entire kinetic or mechanical development after the discovery of the first law. In fact, when he published his *Wärmelehre* in 1896 (largely to counter Boltzmann's Lübeck triumph over Ostwald the previous year) his book was severely criticised for just this failing. J. E. Trevor, who liked Mach's other books, could not help mentioning in his review [1897]:

> Mach's treatment of the early history of the theory of heat, say of the period preceding Gibbs arouses only admiration, but the remainder of the book has by far neither the same completeness nor the same finish. Horstmann's practical application of thermodynamic method to chemical action is neglected, Massieu's characteristic functions, Helmholtz's theory of free energy and its application, V. Oettingen's antithetic developments and the magnificent work of Gibbs are all but little more than cited. This is indefensible, especially since the author brings his treatment so far down into the present as to touch upon the recent discussion on 'energetics'. Then again, the assemblage of both new and reprinted philosophical sketches at the close, instructive as they are, is very disconnected; the book as a whole is neither a collection of scientific papers nor a well-rounded critical treatise on its subject,—we get the impression that a splendid work, partially finished has been dumped upon the market in company with the materials for its completion.

In short, Mach's opposition to both the reality of atoms and to

Boltzmann's kinetic theory of gases was philosophical and was not even accompanied by a serious scientific argument against either one. On the other hand, Mach's reputation as an experimental physicist and philosophical forerunner of logical positivism seems safe. And while his contributions to theoretical physics such as his definition of mass and what Einstein would call 'Mach's principle' may still be controversial, there can be little doubt that whether Einstein correctly understood his ideas or not that he did considerably influence the young genius, particularly concerning absolute space, time, and motion. Mach's central role in understanding modern physics and philosophy of science is not about to disappear, regardless of how misguided he may have been about the reality of atoms or Boltzmann's kinetic theory of gases.

Let me close with a passage from the British biographer Morris W. Travers ([1951], pp. 251–2) who remembered from personal experience how anti-atomistic philosophy could affect British thinking about science, in this case chemistry:

> . . . when in 1901 he (Ostwald) published his *Vorlesungen über Naturphilosophie*, in which his anti-atomistic theory was developed, respectful attention was given to his views. Most of the members of Ramsay's staff bought a copy of the book, and read it; and the subject was keenly discussed at an evening meeting held at his house, and, of course, generally in the laboratory. It must be admitted that the atmosphere at University College became momentarily strongly pro-Ostwald. It is difficult to say how deeply chemists generally were impressed by Ostwald, but physicists were not impressed at all. One must say that the tendency among physicists at the time was in the direction of considering not only matter but energy as particulate; and, indeed, it was difficult to attach any meaning to Ostwald's idea that heat energy, electrical energy etc., were definitely differentiated without adopting some such hypothesis. Ostwald was really trying to swim against the stream; but while chemists were not at first strongly influenced against his views by developments in the field of physics generally, Rutherford's discoveries, and the enunciation of the disintegration theory in 1902, which demanded the acceptance of the atomic theory, dealt Ostwald a deadly blow. Rutherford's views quickly gained acceptance by the younger men. While the disintegration theory was completely revolutionary, it appeared to be very simple, and at the same time to coordinate a number of very striking phenomena. It involved no particular structure of the atom; and it violated no principle, except that the atom was the ultimate and indivisible particle of matter, which was just an article of faith. While some stalwarts refused to accept Rutherford's views, these were not among Ostwald's adherents, rather the reverse.
>
> The writer had the privilege of being a guest at a private dinner given to Madame Curie and Lord Kelvin by Lord Reay, President of University College, London, in June 1903. Madame Curie did her best to convert her dinner partner to the disintegration theory, without success, as a letter written by Lord Kelvin to Ramsay a little later shows (August 22nd, 1903):-
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> 'We are all at our "wits end" in respect to the emission of heat by radium, and are forced into thinking, if not actually harbouring very wild conjectures. The intervention of ether waves into thermodynamics was not fully thought out 50 or 60 years ago. It certainly does not come under "reversible reactions" contemplated, and expressly defined by Carnot in his law. Waves of ether may conceivably supply the enrgy radiated out of radium; but I cannot at present see how they do it, if they do it, or whence they get the energy they conjecturally supply.

'The hypothesis of evolution in the atom or transformation of its substance, coupled with the supposition that energy emitted by the radium is taken out of the store in the atom seems to me to be utterly impossible . . .'

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SOME RECENT OBJECTIONS TO THE BAYESIAN THEORY OF SUPPORT

Bayesianism today makes a serious claim to be regarded as the logic of scientific inference (one of its very obvious strengths is that it adjudicates both statistical and non-statistical inference in a uniform way, for example).

[1] My thanks to Stephen Brush for his acute comments which have contributed significantly to the revision of this historical note.