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Response to Book Review: *The Kind of Motion We Call Heat*[1]

S. G. Brush[2]

I thank the Editor for offering the chance to reply to Laszlo Tisza's review of my book, *The Kind of Motion We Call Heat*. Since Tisza prefers to discuss his own version of the history of science rather than mine throughout most of his review, I will do the same in my reply.

Tisza's approach to history is revealed near the beginning of his review when he argues that it is unnecessary to discuss the geological background of the principle of irreversibility. Refusing even to consider the evidence for the existence of this background, he complains that it is a "new idea which only generates perplexities." Instead, he claims that irreversibility "is inherent in the method of calorimetry." Having decided what the historical origin of the concept *should* have been, he has no interest in finding out what it actually was.

Now let us turn to the history of what Tisza calls "caloric thermodynamics." According to him, the caloric theory was a phenomenological theory based on the principle of conservation of heat, and it remained valid with only minor qualifications after the incorporation of conversion experiments. He considers the question of whether heat is a substance or a form of motion to be secondary or even irrelevant. This position is clearly stated in his earlier book, which he now cites as the "conventional account":

> The microscopic aspect of the caloric theory never received a quantitative mathematical elaboration. These aspects constitute an intuitive imagery rather than a real theory and, however ingenious, they are of no interest for our analysis concerned with the cumulative elements of the theory. In fact, the significant developments of the period 1760 to 1850 are all phenomenological [ones] that can be discussed without concern for the prevailing speculative ideas, whether right or wrong. The impressive rigor of

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[2] Department of History and Institute for Physical Science and Technology, University of Maryland, College Park, Maryland.

> classical thermodynamics [which Tisza considers to have begun with Joseph Black] was ensured by keeping clear of microscopic speculations (Ref. 1, p. 17).

The historical accuracy of Tisza's account can easily be checked by any reader with access to a good library. For example, a quantitative mathematical elaboration of the microscopic aspect of the caloric theory can be found in Laplace's paper of 1821, incorporated in Book XII of his treatise on celestial mechanics. Laplace worked out a detailed model in which gas particles radiate and absorb caloric, and derived the ideal gas law from this atomic model.[(2),3] That paper was part of Laplace's general research program in which the properties of matter were to be related to short-range forces between atoms; that program influenced a considerable amount of physical and chemical research in France in the early 19th century.[4] Similarly, both Dalton and Avogadro developed their atomic theories in terms of hypotheses about the repulsion of caloric atmospheres condensed around atoms.[5] One may of course argue that the "significant developments" achieved by these scientists—Laplace's explanation of adiabatic compression and the speed of sound, Dalton's law of partial pressures and his theory of chemical combining weights, etc.—*could* have been accomplished by a purely phenomenological approach. But I fail to see how our understanding of the history of science can be advanced by ignoring the microscopic speculations and intuitive imagery that have often played an important role in the development of theories.

Contrary to Tisza, the problem of the nature of heat was an important one to many writers on the caloric theory. For example, William Cleghorn's *De Igne* (1779), generally regarded as the first comprehensive exposition of the caloric theory, presents arguments against the hypothesis that heat depends on motion and in favor of his hypothesis that it is a substance.[6] A more extensive discussion along similar lines can be found in George Gregory's *Economy of Nature*.[(6),7] Moreover, it was generally assumed that the caloric fluid consists of particles that repel each other and are attracted to matter; thus the caloric theory involved "microscopic speculations" almost as much as did the kinetic theory. The views of Cleghorn, Laplace, Dalton, Avogadro, and other caloric theorists such as Bryan Higgins, J. M. Socquet, and C. L. Berthollet are discussed at length in Robert Fox's mono-

[3] Also see Ref. 3, Vol. 1, pp. 11–13.

[4] See Ref. 4, pp. 166–174, and Ref. 5.

[5] Dalton: see Ref. 4, pp. 110–114, and Ref. 6. Avogadro: see Ref. 4, Chapter 6, and Ref. 7.

[6] See the reprint and translation in Ref. 8; the arguments about the nature of heat are on pp. 14–15, 42–43.

[7] See extracts in Ref. 3, Vol. 1, pp. 66–70.

graph.[4] Tisza fails to mention any of these scientists in his discussion of the history of caloric theory.[1] Instead, he picks out only those men who contributed "cumulative elements of the theory" (thermodynamics) as it was established after 1850.

Tisza, along with most other writers on this subject, states that the caloric theory was generally accepted until about the middle of the 19th century (Ref. 1, pp. 13, 17). I tried to show in Chapter 9 of my book that, as a result of the adoption of the wave theory of light, together with the belief that (radiant) heat and light were essentially the same type of phenomenon, most scientists shifted gradually after 1825 from the caloric theory to a "wave theory of heat." Tisza does not challenge any of the evidence for this statement, but simply rejects it as incompatible with his view that speculations about the nature of heat *should* not have influenced the scientific status of the caloric theory. He then criticizes a statement I did not make, namely that the wave theory had a "real role in the establishment of the principles of thermodynamics." I claimed only that the "wave theory of heat . . . made it seem natural to treat heat as a form of mechanical energy" (p. 305), that it "was a partial but not a *sufficient* basis for thermodynamics" (p. 328), and that Clausius "could see the wave theory of heat as a possible route to the mechanical theory even though he does not seem to have followed that path himself" (p. 171). As for the influence of the wave theory on William Thomson, Tisza makes it appear that I have merely postulated an influence to fit my reconstruction; but the quotation following my statement that "someone has told him about the wave theory of heat" makes it quite clear that in 1851 Thomson thought the evidence about radiant heat waves was important for thermodynamics.[8] Thus the alleged discrepancy between my pp. 566–579 and p. 331 disappears.

Tisza's other main criticism is that my "philosophical convictions" have intruded too much upon the historical narrative, especially in my treatment of controversies between kinetic theorists and their phenomenological critics. Perhaps he is right. I do think that the development of the kinetic theory of gases in the 19th century was one of the major achievements in the history of science—otherwise I would not have written this book!—and that the criticisms of the antiatomists were misguided. Moreover, I am convinced that the writing of history always contains some element of subjectivity, if only in the *selection* of topics and "facts" to be treated—and that an author should not try to conceal his bias from the reader.

[8] "The Dynamical Theory of Heat, thus established by Sir Humphry Davy, is extended to radiant heat by the discovery of phenomena, especially those of the polarization of radiant heat, which render it excessively probable that heat propagated through vacant space, or through diathermane substances, consists of waves of transverse vibrations in an all-pervading medium."[10]

Again, Tisza accuses me of trying to relive the dramatic battles of the past rather than "removing the residues of outdated controversies" and analyzing only those earlier writings that are presently considered valid. Here his accusation is certainly right. Even if we could all agree on who won those battles, I think it would still be of great historical interest to see how they were fought, though a modern scientist might not want to take time to follow the details. But Tisza's review itself demonstrates the need to review those battles, for it appears that he does not accept my conclusion—by no means an original one!—that the outcome of the debate was generally favorable to the kinetic theory. He argues, in effect, that the phenomenologists conceded defeat unnecessarily after Perrin's experiments on Brownian movement; rather than admitting that the acceptance of theories based on unobservable atoms is inconsistent with phenomenology (as Mach would have said), they should simply have allowed phenomenology to become microscopic as well as macroscopic.[9] They would then have been able to assimilate the quantum mechanical atom while still rejecting the classical atom.

That is not what happened, at least in the part of the scientific community with which I am familiar. Once the limits of validity of the classical kinetic theory and the magnitude of quantum corrections had been established, it was found that the Maxwell–Boltzmann theory, as extended by Chapman and Enskog, covered a large range of situations in the real world. There was a major revival of interest in the kinetic theory after World War II; some aspects of this revival have been described in my earlier book (Ref. 3, Vol. 3, pp. 39–80). The phenomenological approach to statistical thermodynamics, in which Tisza has been one of the leaders, has produced valuable results, but certainly has not made kinetic theory obsolete.

I can leave that issue in the hands of the readers of the *Journal of Statistical Physics*; what concerns me here is the result of the phenomenological approach when applied to the history of science. According to Tisza, it means, for example, that the caloric theory was never overthrown but only "refocused and streamlined" by Clausius, while caloric was conceived as "an ancestor of the energy concept" (Ref. 1, pp. 12, 15). It also means that the kinetic theory was criticized "mainly on the inability of this theory to account for spectroscopy and for chemical binding," and that those criticisms were fatal before the advent of quantum mechanics.

There is very little evidence for these claims. On the contrary, Clausius specifically rejected the caloric theory in his first thermodynamics paper: "facts have lately become known which support the view, that heat is not a

[9] The atom could not be *directly* observed at the time of Perrin's experiments or even when quantum mechanics was established. Tisza's requirements for a phenomenological theory thus seem to be much weaker than Mach's.

substance, but consists in a motion of the least parts of bodies."[11] (I have indicated on pp. 576–577 of my book how he used this motion even in constructing his thermodynamic theory.) J. B. Stallo, one of the sharpest critics of the kinetic theory, admitted that the evidence from the spectroscope was for the most part *favorable* to the kinetic theory; he had to quote Maxwell, a supporter of the theory, to explain the rather indirect way in which spectroscopic evidence might be considered to undermine it.[12] Another supporter, Boltzmann, seems to have been the only person who seriously expected the theory to explain chemical binding.[13] The other arguments against kinetic theory (not mentioned by Tisza) seem to be rather weak compared to those against the caloric theory (i.e., the caloric theory as it was actually presented in the 19th century, not Tisza's cleaned-up version).

It would not be appropriate to discuss this point at length in a reply to a book review; I bring it up mainly to show that the old controversies are not quite dead, and that the interpretation of scientific history cannot be left to adherents of any one viewpoint, whether phenomenological or atomistic.

Two minor criticisms call for replies:

1. Tisza says I reproach Ehrenfest for focusing on the weaknesses of kinetic theory despite being a supporter of it. That was not the intent of the rather infelicitous sentence he quotes about the reversibility and recurrence paradoxes; the Ehrenfest article was published some years *after* the anti-atomists used those paradoxes to attack kinetic theory.

2. Tisza notes that "conservation" does not appear in the index. True; but there are 17 separate page references for the index entry "Energy Conservation Law."

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