



McGill
University

Foundations & Philosophy of Science Unit

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Professor Clifford Truesdell

Dear Truesdell

Thank you very much for your extraordinary letter of the 9th. Doubly extraordinary, for it is a distillate of your long experience with statistical mechanics and thermodynamics, and you inform me that you have been kind enough to contribute to the Festschrift that Agassi and Cohen are preparing for me. This was a very agreeable surprise.

Your assessment of the current state of research into the statistical mechanical foundations of thermodynamics is depressing. (Incidentally, my own assessment, in my Philosophy of Physics (1973), was equally negative--but of course my information is 2nd hand, for I have never worked in that field.) I must own that physicists are not too impressed with logic, and are ever ready to sacrifice it to the obtainment of "results". (Richard Feynman, otherwise so inventive, is a clear case: anything goes provided you can get what you are after--which is never real understanding, always computing power.)

However, there must be further reasons explaining the failure to obtain a rigorous deduction of T to SM. One of them you have alluded to yourself, namely that physicists usually don't know what is it that they should reduce: they don't really know classical thermodynamics, but only equilibrium thermostatics. Another reason may be that, since we are all taught that the reduction is a fait accompli, nobody regards it as a research problem, so no physicists work on it. And the third reason, also suggested by yourself, is that maybe the whole program is a wild goose chase: maybe T is irreducible to (classical) SM. I am ready to believe this, but for a reason you won't like, namely that atoms and molecules are not correctly described by classical mechanics.

It would be very salutary if you were to publish a paper on this subject either in Philosophy of Science or in the Brit. J. Phil. Sci., for philosophers have been repeating ad nauseam that the reduction of T to SM is the paradigm or paragon of theory reduction, and my criticisms have gone unheard.

What you say about Prigogine is distressing. I knew the man's work in quantum mechanics is phoney; I have discussed some matters with him, in private and in public, and he does not have the slightest idea about them. But I took it for granted that his work on irreversible thermodynamics and SM was, though mathematically sloppy, full of insights. One less hero.

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