DISCUSSION ON THE TEACHING OF THERMODYNAMICS

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Several of the papers presented at this conference were concerned with axiomatic treatments of thermodynamics. The following statement by H. Buchdahl (Australia), namely, '*Even if axiomatic thermodynamics is physics, it should not be taught*', evoked considerable comment. The discussion was started by Veazey (Luton) who complained that the statement is incomplete in that it does not say at what level, or for what students, the subject is to be taught. To understand axiomatics, the students must have some previous knowledge. One can see no reason why advanced level students cannot profit from such a study. It would be a good topic for an advanced degree. There is, however, no place for axiomatics for introductory students. It was then argued by Le Fevre (London) that axiomatics is just as much legitimate physics as statements about engines you can't build. Even if one does not like to *start* the subject with an axiomatic treatment, there is no need to say that such a treatment should not be taught.

It was then suggested by Silver (Glasgow) that Buchdahl's statement should be altered to read, 'Even if axiomatic thermodynamics is *not* physics, it *should* be taught'. Giles (Canada) who only half an hour previously had described an axiomatic treatment, admitted that he would not teach such a subject in a first course to elementary students. It was, however, pointed out by Landsberg (Cardiff) that all young people must learn the rules of inference, which *are* axiomatic. No matter what method is used in teaching, the theoretical principles cannot be exact physics. Even Euclidean geometry fails to take into account the curvature of the earth; the second law in its Carathéodory formulation breaks down when a system is very small, etc. He therefore suggested another rearrangement of the words of Buchdahl's statement, namely, 'Even if axiomatic thermodynamics is taught, it could not be physics'.

The statement made by B. Cimbleris of the Nuclear Energy Commission in Brazil, namely, that '*the quantity* $\Delta(U + P_0V - T_0S + \Sigma\,\mu_i n_i)$, *which represents the maximum amount of work during a reversible process in which the system exchanges heat and mass with its environment, should be given a prominent role in the teaching of thermodynamics to engineers*', evoked the comment from Le Fevre that the last two words, 'to engineers' should be eliminated, inasmuch as thermodynamics is a single subject. Zemansky (New York) felt that the statement, although a useful one to engineers, referred to a situation that was not fundamental, but he was immediately opposed by Horne (Michigan, USA) on the ground that perhaps we need a new definition of thermodynamics. In his opinion the expression in question dealt with a real process of a real system in which the temperature inside and that outside differ. This is also true of a living system, to which thermodynamics ought to apply as well.

Tisza (M.I.T., USA) made the point that the origins of thermodynamics are very diversified, and that open systems are just as simple and important to treat as closed systems. He objected to the statement that the expression in question was of value to engineers only, on the grounds that Landau and Lifshitz made use of it extensively and that engineers often contribute (sometimes in a sloppy manner) excellent notions that prove of value to mathematicians and mathematical physicists. He concluded by pointing out that the word 'fundamental' is often used to mean 'what we are used to'.

Landsberg then proceeded to defend the statement, which appears in his textbook published in 1961, to the effect that '*In so far as the time coordinate is absent, nothing happens in thermodynamics*'. He maintained that real processes are not always discussed in thermodynamics, sometimes one deals only with ideal abstractions. A quasistatic process, for example, being a succession of equilibrium states, may go forwards or backwards and is therefore reversible. In the real world, one may approach this condition as close as one pleases, but if one postulates that one reaches it, then nothing could happen. He would prefer to think of a quasistatic process as a curve in phase space.

It was pointed out by Kestin (Providence, USA) that in real or irreversible processes the initial state might be an equilibrium state and also the final state, but between the two terminal states, there may be no possibility of drawing a curve in phase space. For irreversible processes the concept of a field is essential. Zemansky tried to give an experimental interpretation of a quasistatic process as one in which instruments behave in a manner that enables the experimenter to take meaningful readings. Such a process is slow and a good enough approximation to a quasistatic process to enable one to use the appropriate equations.

Gurney (Hong-Kong) objected to Landsberg's equating the words 'quasistatic' and 'reversible'. He pointed out that the motion of a blackboard eraser across the table at constant velocity is quasistatic but, because of the large amount of friction, is hardly reversible. A testing machine may stretch a sample of material beyond the elastic limit in a process that is quasistatic but not reversible. [There are some writers who regard a reversible process as one which satisfies two conditions: quasistatic and non-dissipative.]

Landsberg reiterated his belief that his statement was true because an *approximation* to a quasistatic process is not to be confused with an *ideal* quasistatic process. Tisza maintained that time *does* appear in thermodynamics but in hidden fashion. Thermodynamics is always in close contact with experiment. When an event occurs in an experiment, such as the opening of a valve, this involves the removal of a constraint within a given duration of time. Time is really there and plays a role in the consideration of the relation between the thermodynamic equations and the experimental realization. A similar situation occurs in quantum mechanics, where there is a closer connection between a measurement and the quantum-mechanical description of the system undergoing the measurement.

Zemansky then proceeded to defend his 'provocative statement' reading as follows: '*The expression for the work of a thermodynamic system should be chosen so that the definition of internal energy should* not *include external potential energy.*'

He pointed out that the expression for the work in increasing the magnetization of a stationary paramagnetic bar is $-H\,dM$, whereas the work in moving a paramagnetic bar from one point in a field H to another point where the field is $H + dH$ is $+M\,dH$. Since a change of internal energy is defined to be adiabatic work, the first point of view provides an energy U and the second the energy $U - HM$. Since $-HM$ is the external potential energy of a system of magnetic moment M by virtue of its presence in a field H, the second expression is seen to contain as part of the internal energy the external potential energy. It has been the custom to accept these two expressions for work as equally legitimate because all thermodynamic equations based on the two points of view are identical.

Zemansky said that two expressions for work exist also in the case of gas. The first is the well-known one, $+P\,dV$. The second is the work needed to move a small gas balloon from one point in a pressure field (provided, for example, by a tall cylinder containing a dense liquid) to another point of higher pressure, namely, $-V\,dP$ where the minus sign signifies that work must be done *on* the gas. The first expression provides an internal energy U and the second an internal energy $U + PV$. Again, both expressions yield identical equations, but no one would consider seriously the adoption of the second point of view.

Kestin pointed out that the expression $-V\,dP$ is known in engineering as 'technical work' and is widely used. He emphasized that, if one tells him the system and what the system does, he will accept any expression that fits the conditions so specified. R. O. Davies (London) supported the expression $-H\,dM$ for the magnetic work on the ground that it allowed a more acceptable correlation between the statistical mechanics of paramagnetic systems and thermodynamics. Zemansky agreed whole-heartedly.

There then ensued a discussion of the statement by Hornix, namely, '*It is desirable to replace the Kelvin and Clausius formulations of the second law through a set of statements which expresses the "accessibility structure" of phase space in a simplified physical way*'. Hornix (Nijmegen) objected to the classical statements on the ground that they are the result of engineering experience, whereas thermodynamics requires a more sophisticated mode of presentation.

Barron (Bristol) wondered whether the Hornix statement would be of much value to the thousands of students that we are soon to confront in the classroom. He felt that the very words 'accessibility structure of phase space' would be enough to finish at least three quarters of the first-year students. The laughter that ensued indicated considerable sympathy toward Barron's point of view. Silver clinched this point by saying that he was sick and tired of the patronizing attitude of some physicists, expressed by the contention that 'the engineering background of thermodynamics is of historical interest only'. He went on to say that anyone who believes the preceding contention fails to understand some important parts of thermodynamics. Le Fevre hoped that the Hornix method gave clearer statements to students concerning the states to which systems tend to go, and Hornix replied that this is what he accomplished with students. The sophisticated language that Barron decried was meant for teachers, not for students.

The final argument arose over the statement of Silver, '*In the teaching of engineering thermodynamics*: (a) *irreversibility should be introduced at the outset, and* (b) $\mathrm{d}Q = \mathrm{d}u + p\,\mathrm{d}v - \mathrm{d}W_f$ (*where* $\mathrm{d}W_f$ *is the work done against friction*) *should be deduced from mechanics and conservation of energy subsequently identifying* $\mathrm{d}Q$ *as the energy transferred by virtue of a temperature difference*'. Just how this is done was shown in a careful and explicit manner by Silver in the presentation of his thesis. Discussion was started by Frank (Bristol) who agreed with Giles that in the presentation of thermodynamics entropy should come early and temperature later. One of the troubles indicated by students, he said, is that they believe they know what temperature is and they believe that they will *never* know what entropy is. Entropy is what Carnot called heat, and entropy is what is conserved in reversible processes. Frank avoids the concept of quantity of heat because even Carnot was not sure what he meant, although he did state that 'chaleur' and 'calorique' meant the same thing.

Le Fevre agreed with most of the ideas contained within the formulation of Silver but disagreed with regard to the moment when the increasing property of entropy should be introduced. He felt that one should arrive as quickly as possible at the entropy statement and then use it to infer the existence of frictional forces and other causes of irreversibility, instead of bringing in friction first and then entropy changes.

After congratulating Silver on his presentation and expressing complete agreement, Zemansky asked permission to object to some of the remarks made by Frank. He said he had no patience with the point of view that entropy should be brought in early. If it could, it would be most desirable, but there are so many concepts such as temperature, work, energy, heat, engine cycles, the second law, etc., that *must* be understood first. To deal with the foundations of thermodynamics *as though* you don't know what temperature, work and heat are is nonsense. The entropy change should be introduced in an *operational* manner, so that the student will know how to measure it and how to calculate it. If a reservoir at T parts with heat Q, the entropy change is calculated on the basis of a knowledge of T and Q, not on statistical considerations. McGlashan (Exeter) indicated his agreement with Zemansky.

Landsberg agreed that the concept of temperature should be taught first and the difficult concepts of entropy and chemical potential last. He suggested that there was a big difference between the order of events when teaching large numbers of ordinary students, and the reformulation of logical structures of thermodynamics such as those suggested by Giles and others, which may be suited to research workers, or possibly to advanced students.

Silver referred to a remark by Landsberg to the effect that one could look at thermodynamics in a variety of ways, analogous to the ways in which a man might look at a woman. Silver insisted that he looks at thermodynamics as an engineer who wants to produce children, so he looks at her in a very definite and pragmatic way (Laughter). Hornix emphasized that, in the introduction of the concept of empirical temperature and its measurement everyone concedes that it is necessary to associate with each isotherm a definite number. In a similar manner, it is necessary to associate numbers with adiabatics. These numbers are analogously empirical entropies. When this is done in such a manner that entropy is additive, one gets a system similar to that of Giles. What we have to learn from the axiomatic point of view is that things are in some respects more simple than we suspected at first, because of the history of the subject. We have to try to become more independent of the historical approach.

Frank struck back at Zemansky by objecting to the latter's insistence that 'simple' ideas like temperature and heat be treated first, and the difficult concept of entropy be reserved for later. Frank insisted that the only thing simple about heat is the fact that it is treated early in Zemansky's book, whereas what makes entropy difficult is that it appears late in this book (Laughter). In the first really valuable publication on this subject, namely in Carnot's book, entropy was called heat, and if it was called heat, it would seem to be the simple concept that I believe it could be made. [Only a few people believe that Carnot anticipated Clausius by having an idea of the meaning of entropy. They maintain that when Carnot used the word 'chaleur' or 'feu', he meant ordinary heat; whereas with the word 'calorique' he meant 'entropy'. This belief is more hero worship of Carnot than practical sense because in *Le Pouvoir Motrice du Feu*, Carnot states definitely once and for all that 'chaleur' and 'calorique' mean the same thing.]