1. Essay Review
Nought but Molecules in Motion
Jos Uffink[1]

> "The air", you said in language fine,
> Which scientific thought expresses,
> "The air– which with a megadyne
> On each square centimetre presses–
> The air, and may I add the ocean,
> Are nought but molecules in motion."
>
> Atoms, you told me were discrete,
> Than you they could not be discreter,
> Who know how many Millions meet
> Within a cubic millimetre.
> *They* clash together as they fly,
> But *you!* – you cannot tell me why. [2]

The foundations of statistical and thermal physics is not a field that has been able to attract the attention of philosophers of physics in large crowds. On the contrary, there seems to be widespread consensus that philosophically exciting problems in physics are found only in quantum theory and, to a lesser extent, in the theory of relativity. To anyone accustomed to the idea that classical physics is basically a finished, clear-cut and philosophically unproblematic body of knowledge it may come as a surprise how many serious problems this field has in store and how unexploited this rich field has remained until now. A book on the subject by the well-known philosopher of physics Lawrence Sklar is therefore most welcome. The state of neglect in which the subject finds itself is perhaps no better illustrated than by the fact that a large section in the opening chapter of the book is devoted merely to a summary of the famous survey article by Paul and Tatiana Ehrenfest of 1911 on the statistical approach in mechanics. This work has in all the intervening years lost little of its urgency; nor has it been rendered superfluous in the sense that the many problems it raises have been solved by modern developments.

This is not to say, however, that nothing worthwhile has appeared in the intervening years. Oliver Penrose's survey paper of 1979 on the foundations of

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[2]From J. Clerk Maxwell, *Lectures to Women on Physical Science II*, 1874. In L. Campbell and W. Garnett, *The Life of James Clerk Maxwell*, (London: Macmillan 1884), p. 410.

statistical mechanics counts about 500 entries in its list of references. However, most of the effort that physicists have devoted to the subject has resulted in the development of different schools, each with its own programme and technical apparatus. Unlike quantum theory or relativity, there is in this field no common mathematical backbone that is accepted by most of the participants. Critical study of the foundations of thermodynamics and statistical mechanics has instead been dominated by mathematicians and others with a keen eye for exactness and logical clarity. The names of Zermelo, Poincaré, Tatiana Ehrenfest and Truesdell spring to mind. The contributions along these lines have often been devastating: an astonishing part of what is usually presented under the name of statistical mechanics and thermodynamics is mathematically untenable, unproved or in confusion. As early as 1878 Kelvin complained that the modern student of thermodynamics is somewhat liberally perplexed with "questions unanswerable by theory or experiment, and propositions which escape the merit of being false by having no assignable meaning" (Kelvin, 1878, §2). Khinchin, in his book of 1949 described the attempts to derive the Boltzmann relation $S = k \log W$ as "an aggregate of logical and mathematical errors superimposed on a general confusion in the definition of the basic quantities" (Khinchin, 1949, p. 142); and not long ago the mathematician Arnold opened an article by simply stating that "every mathematician knows it is impossible to understand an elementary book on thermodynamics." (Arnold, 1990, p. 163) There are, of course, physicists who would be tempted to say the same about some books on mathematics.

On the other side of the spectrum are the more visionary and freely speculative works of Eddington, Prigogine, Eigen and Roger Penrose, applying the concepts of statistical mechanics in the construction of an encompassing picture of the universe. Further important contributions have come from the historians of physics. In particular the careful analyses of Brush, Klein and Kuhn have provided much insight in the original views of Maxwell and Boltzmann. Each of these types of approaches to the foundations of statistical and thermal physics have their own merits and natural limits. It is, I believe, in the midfield of physics, mathematics and history of science that the knowledge and skills of a philosopher would be extremely useful, amalgamating the insights from these disciplines by conceptual analysis to provide clarification of the fundamental issues.

I am not sure whether the present book is intended in this way. Sklar's stated aim is to bring an elementary, organized and comprehensive survey of the philosophical and physical issues in "this confused field, a field desperately in need of all the understanding that can be thrown upon it." (p. 6-7.) But he also has some disclaimers. He considers his ambition amply fulfilled if the book will stimulate *others* to apply 'the talents of conceptual clarification' to these issues. His own answers, he stresses, should be regarded as mere tentative suggestions. These reservations are, in part, only natural, given that the field lacks a tradition of philosophical analysis on which one can build. But I must also say that I regret this self-imposed restraint by the author. Indeed, sometimes it seems that his attitude is closer to that of a reporter than a philosopher, and I wish that some problems in the book were analyzed in greater depth or more

systematically.

Now of course this is not a fair criticism to make. The untrodden jungle will not be mowed overnight into a neat country garden, and one cannot blame the pioneer for preparing a provisional path only or for exploring a direction that eventually leads to a dead end. Moreover, the range of subjects that is covered in this book is truly amazing. Where else can one find a book which takes us from the KAM theorem to the mind-body problem, from Big-Bang cosmologies to the Gibbs-Duhem inequality, from causation and counterfactuals to chaos, while easily passing the cosmological argument, the spin-echo experiment and the Wheeler-Feynman theory of radiation on the way?

Thus, although my review will be somewhat critical, this should not hide my admiration for the author's feat in writing the book. Still, I take it to be the task of a reviewer to point out cases where the book remains unsatisfactory or where another direction might be more promising. Naturally, I too will have to restrict myself, and I will concentrate on what seems to me to be the core of the, indeed, comprehensive book: the foundations of statistical mechanics, both equilibrium and non-equilibrium, and their relation to thermodynamics. Thus, the more general and open-ended chapters on cosmology, the direction of time and statistical explanation will remain untouched.

1   Thermodynamics

The book starts with an 'historical sketch' introducing the theory of thermodynamics and the works of Maxwell, Boltzmann and Gibbs on kinetic gas theory and statistical mechanics. This introduction, as the author warns us, is extremely abbreviated and selective. Indeed, the text is mainly a summary, very condensed and not always accurate. In the case of statistical mechanics, the omission of detail and analysis is made good by three later chapters comprising almost 150 pages of further study. But not so for thermodynamics. This theory gets no more than the cursory description offered in the brief sketch of this chapter. For a book that describes itself as being devoted to the *pair* of theories of thermodynamics and statistical mechanics, this uneven division of attention is remarkable.

Sklar describes thermodynamics as resting on a 'principle of conservation' and a 'principle of irreversibility', the latter being summarized in the famous Second Law. The meaning of irreversibility or the relationship between the various forms of the second law is not examined. This is a pity, because even here the situation is by far not as transparent as one would want.

Let me only note here that the thermodynamical usage of the term 'irreversible' can have several distinct meanings, which may differ from what is usually understood by that term in a mechanics context. For example, one tradition, going back to Clausius, calls a process irreversible if the initial state of a system cannot be restored except by interactions with other systems which then suffer compensating changes. Applying this same criterion to mechanics, it seems obvious that most mechanical motions would also be irreversible since they can only be reversed by means of collisions with other bodies which receive

a compensating recoil. Others, Born for example, defines the term such that 'irreversible' is synonymous to 'non-quasistatic' (Born, 1921. A process is called quasi-static if it is carried out so slowly that the system remains arbitrarily close to equilibrium at all stages of the process.) It is therefore not obvious, at least to me, that a principle of irreversibility in thermodynamics implies or expresses a temporal asymmetry, or is in conflict with the reversibility of mechanics, as Sklar assumes.

Now, obviously, the reason for the absence of a serious analysis of this subject is simply that the author did not intend to write on the foundations of thermodynamics, but of statistical mechanics alone. Still, I feel this absence is unfortunate, since so much of the conceptual problems in statistical mechanics derive from its supposed relationship to thermodynamics. It is no coincidence that the term 'second law' is the most-often cited item in the index of this book, with 'time-asymmetry' coming in second place.

A second remark concerns the sometimes peculiar style of this book. What should one make of a sentence like this:

> "Overall, the theory is extended into a universally comprehensive scheme in which the exchange of energy between that which is macroscopically overt and that which is transmuted into internal energy of microscopic constituents becomes formalizable in the thermodynamic scheme." (p. 22)

It is not just that I find the formulation ugly (e.g. because of the double use of 'scheme') and unnecessarily vague, depicting an exchange between two nondescript 'thats'. It puzzles me how an agent which is apparently able to receive energy can be transmuted into a form of energy as a result of this exchange. I wonder how many readers will recognize this bit of alchemy for what is intended, namely a characterization of the Gibbsian theory of equilibrium thermodynamics.

Furthermore I should say that passages as these:

> "[. . .] one would have to give a proof to the effect that if the intercomponent interaction is sufficiently well behaved, then in the thermodynamical limit of the number of components and size of the system going to infinity but with density held constant the use of law of large number theorems will allow us to prove that for the type of functions derived from the ensemble that we use to calculate observed macroscopic parameter values, the probability distribution for the parameter values calculated for an individual system in the ensemble will in fact cluster overwhelmingly around the mean value that we calculate from the ensemble." (p.202)

are not a joy to read. No doubt, formulations like this, in which a mathematical goal is described in words, are inspired by the desire to write an elementary and non-technical text. But I fear that in many cases the result is in fact accessible only to those who are able to reconstruct the intended meaning because they already know the technical details.

2 Probability and explanation

The next two chapters of the book are devoted to the interpretation of probability and the philosophy of explanation, in particular of statistical explanation.

These chapters form a unit in itself. They are meant to equip the reader with the necessary philosophical tools to address the foundations of statistical mechanics in chapters 5 up to 7.

On the whole, this seems to be well-done, as far as I can tell. My only objection here concerns the representation of the frequency interpretation of probability. This is the view that interprets the probability of the outcome of an experiment as the limiting frequency with which this outcome occurs in an infinite series of repetitions of the experiment. Sklar presents this view as an attempt to obtain a definition of probability by using the laws of large numbers (in either the weak or strong version). Puzzles, objections and 'disturbing thoughts' are brought forward regarding the frequency and other interpretations, mostly bearing precisely on their supposed dependence on the laws of large numbers. At one point it is even said that a certain interpretational aspect "can't be quite right" simply because it is not implied by "even the strongest laws of large numbers" (p. 102).

But that is a misrepresentation of the goal of interpretation, and of the frequency view in particular. The terms 'weak' and 'strong laws of large numbers' designate the purely mathematical theorems derived by Jacob Bernoulli and by Borel and Cantelli respectively. Now it is, I believe, a philosophical truism that the interpretation of a mathematical formalism cannot be derived from the formalism itself. Thus the laws of large numbers, surely, do not mediate or guarantee the frequentist interpretation. And in fact, the main proponent of the frequency view, Richard von Mises, made it very clear that in his view the idea that stable relative frequencies obtain in mass phenomena, converging to a limit when their number is increased, is a 'brute fact', which one can only derive or motivate by an appeal to experience (Von Mises, 1981). The point has been stressed recently in particular by Van Lambalgen (Van Lambalgen, 1987). Thus, to criticize the view for saying things not said by the laws of large numbers is, in my opinion, to turn the whole issue on its head. An interpretation of probability does not have the task of returning to us only those conclusions that are already implied by the theorems of the theory. We would have more cause for complaint if it did.

This does not mean, of course, that the frequency interpretation is entirely free of genuine problems. But here some of the objections and conundrums raised in the chapter are apparent only, rather than inherent in the subject.

3   Equilibrium and rationalization

Now we come to the real core of the book, the foundations of statistical mechanics proper. Sklar presents an admirable up-to-date overview of the developments and results in modern statistical mechanics. He divides the topic in equilibrium and non-equilibrium theory, a division that I found very congenial, although it means that the discussion from specific points of views (e.g. ergodic theory, or the subjectivist approach of Jaynes) becomes split over the different chapters.

The first question to ask here is what we want from a study of the foundations of statistical mechanics, in this case the theory of equilibrium. For

Sklar, the answer is *rationalization*. Rationalization is definitely the buzz-word throughout this entire book. Still, I am not quite sure what it means. It is often put on the same footing as 'explanation', 'justification' or 'understanding', and seems to differ from a mere question of interpretation, as in the previous chapter on probability. Specific problems in need of rationalization are: "why the recipe [for the description of equilibrium] works so well as it does?" (p. 158) or also: "why equilibrium is the way it is" (p. 179).

It is not clear to me what kind of answer is expected to settle the quest for rationalization. It would have been helpful if Sklar had mentioned an example of a fully rationalized theory so as to give us a better grasp of what it is that we lack in statistical mechanics in comparison to other theories in physics. Do we understand any better e.g. why the Schrödinger equation works so well as it does? Do we know why gravity is the way it is?

However this may be, it is clear that Sklar is not prepared to be satisfied by just any answer to the above questions. In a case where a proposed answer rests on a further assumption, the next question is raised: "just *exactly* how does [this assumption] function to rationalize, justify or explain its use?" (p. 159.) This is asking quite a lot, perhaps a bit too much, from any physical theory. Personally, I think it would already be a formidable task to obtain a logically coherent formulation of the theory, before embarking on this ambitious program of rationalization.

In any case, a fruitful discussion of the question why equilibrium is the way it is, presupposes an answer to the question of what equilibrium is, i.e. how the concept is represented in the theory of statistical mechanics. Sklar limits his discussion to the micro-canonical ensemble. In this case one assumes that all macroscopic quantities can be represented as functions on phase space. Further, one assumes a probability distribution which is derived from the restriction of the standard Lebesgue measure on phase space to a fixed energy hypersurface. Then, the usual thermodynamical relations between the macroscopic quantities should be recovered for the expectation values of the corresponding phase functions. This is the recipe of equilibrium theory that Sklar wishes to rationalize. It does not settle yet, however, what is meant by the term 'equilibrium', or by 'equilibrium state', 'equilibrium condition' etc.

I regret that Sklar has not put this question at the start of his exposition, because there are two quite different conceptions associated with these terms and we have to switch repeatedly between them in the book. One is the idea of Boltzmann that equilibrium refers to a particular *subset* in phase space, in which the velocity distribution takes (approximately) the Maxwell-Boltzmann form. Here, one can always tell from the momentary microscopic state of a system whether or not it is in equilibrium. In the course of time, the system may fluctuate in and out of the equilibrium set, although, as Boltzmann indicated, this should be very unlikely for a macroscopic system.

The second view is the one used by Gibbs. Here equilibrium is associated with a particular *probability distribution* over phase space. Thus the term characterizes an ensemble, and since one particular system can be a member of many ensembles one can, strictly speaking, no longer say whether an individual system is in equilibrium or not. Further, the state of any system in the

equilibrium ensemble may fluctuate wildly in the course of time. But now this does not mean that the ensemble leaves equilibrium. Thus, while Boltzmann takes over the idea from thermodynamics that equilibrium is a property of an individual system, but relinquishes the idea that it is stationary in time, Gibbs retains time-independence but gives up the notion that it is a property of a system.

Sklar never confuses the two meanings, but he often switches between them without warning or leaves it undecided what he means. Thus on p. 177 the Boltzmann view is adopted and one speaks of an equilibrium microstate of a system; 3 pages later we are told that the only appropriate application of the term 'equilibrium' is to an ensemble. The first clear statement of the distinction, however, has to wait until p. 350 in chapter 9, too late to be of help in the analysis of equilibrium theory.

Another complaint is against the recurring mention of equilibrium as being an 'attractor' state. It is well-known, and Sklar mentions it, that the latter term from the modern study of dynamical systems applies only to dissipative systems, i.e. to systems whose mechanical energy leaks away in the form of heat. Given the historical background of statistical mechanics, as the theory whose aim it is to understand heat as a form of mechanical energy, a dissipative system is probably the last sort of system that one would wish to use for a statistical mechanical description of thermal equilibrium.

4   Ergodicity

The main candidate rationalizer for equilibrium theory is ergodic theory. Sklar takes us through the many meanings of the term, and the maze of criteria and escape-clauses in the results obtained in ergodic theory, and the pitfalls in what they might mean. He also relates how difficult it has been to actually prove that one of the criteria of ergodicity actually holds in any realistic mathematical model of a physical system. In the main, his treatment is clear, able and careful.

One point, however, should be corrected. It concerns the statement that a system of two or more hard spheres is provably ergodic (i.e. metrically in-decomposable), a proof that Sklar presents as the climax of thirty years of mathematical effort.

It is well-known that Sinai announced this result in the early 60s, and al-though the proof was not published, many subsequent writers believed him on his word. But, as far as I am aware, the proof has not appeared yet, and in a recent article on the question Sinai himself states that his announcement has been 'premature', and claims only that his result holds for a system consisting of two spheres (Sinai and Chernov, 1987). But that is hardly a realistic gas model. The climax, thus, is still to come.

Has the rationalization of equilibrium by ergodic theory been a successful one? Sklar's attitude, throughout the book, is one of utmost caution. He often warns the reader that we cannot be assured of the validity of the assumptions necessary to obtain what we want. And in the cases where the validity of such assumptions is unproblematic, we may still be unable to obtain from them

all that we would want. Even in the rare cases where we do seem to obtain completely what we want, Sklar still has an argument waiting to show that after all we may not truly like what we first wanted.

One of the many problems on which Sklar's discussion zooms in is the measure zero problem. Most, if not all of the 'nice' theorems that one obtains from ergodic theory hold for all states of a dynamical system except for a set of measure zero. This would establish these results rather firmly, if only one could show that measure zero sets are non-existent, ignorably small, or in some other way exceptional. The first option is clearly wrong, but the others are hard to substantiate. It is well-known that if one compares 'smallness in measure' with other natural criteria by which one can judge the 'size' of sets, e.g. by their cardinality, dimension or categoricity, the comparisons do not match. Sets of measure zero can be surprisingly large by many other standards. Sklar discusses the subject quite well and warns us to be satisfied only when we can

> "fully justify our claim that we have grounds, over and above purely postulational ones, for assuming that we have a right to take as zero the real probability that a system in the world could be in the deviant set [of measure zero]."

Again, although the measure zero problem is a very real one, I fear that Sklar is asking too much. Multi-layered justifications like these, in which one shows that we can claim that we have grounds to assume that we have the right to make an assertion are nowhere to be had in physical science.

Still, in spite of his extreme caution, Sklar comes to a favorable judgment on the role of ergodicity in the rationalization of the statistical mechanics of equilibrium. "Ergodicity, where it can be demonstrated, does certainly provide us with something of a deep conceptual significance." (p. 178) and "If the system is ergodic, we can, at least to a degree, back up the Boltzmannian picture with an argument that this picture is in some sense a correct representation of the facts about the system." Hence: "there will be no harm in saying that an explanation of some kind of why equilibrium is the way it is has been provided." (p. 180)

My own judgment here is somewhat less optimistic. Surely, the most obvious example of an ergodic system (in all of the many meanings of that term) is the one-dimensional ideal harmonic oscillator. But that system does not show any equilibrium behaviour at all. On the other hand, the 'Boltzmannian picture' of an equilibrium state corresponding to an overwhelmingly large subset in state space was derived for an ideal gas, consisting of non-interacting particles. But that is a clear example of a system that we know to be *not* ergodic. Thus, equilibrium and ergodicity are not so easily connected. Sklar in fact also points out that something else, not implied by ergodicity, is to be added to save the explanation. This is the 'Khinchin program' on the thermodynamical limit. Unfortunately, the results in this program still depend on the assumption that the total energy is the sum of 1-particle energies. This, as Sklar stresses, is something one would like to get rid off. What he does not note, however, is that the assumption as it stands clearly entails that all the 1-particle energies are integrals of the equations of motion, and thus *violates* ergodicity, instead of

helping it.

5   Non-equilibrium

The next two chapters concern non-equilibrium statistical mechanics. Here again I have great admiration for the breadth that Sklar covers. A discussion is given of the approach known as the BBKGY-hierarchy, of Lanford's approach to obtaining a rigorous derivation of the Boltzmann equation, the 'master equation' approach, extension of ergodic theory into the hierarchy of conditions known as C-systems, K-systems etc., as well as of many others. And similar to the previous discussion, the question is whether these approaches allow us "to understand why non-equilibrium systems behave as they do". In most approaches to non-equilibrium theory the evolution of an ensemble is described by an equation similar to the Boltzmann equation. Thus, if we start with an ensemble in an initial non-equilibrium state (probability distribution), it will approach the equilibrium state in the course of time.

The main theme of the chapters is that these equations are obtained with the help of some 'rerandomization posit'. That is, the evolution is not derived from the underlying dynamical laws of mechanics alone, but from a modern variant of the molecular chaos hypothesis. The hypothesis takes various guises in the various approaches: either one continually replaces the distribution by a coarse-grained one, or one replaces an n-particle correlation function by a product of lower correlation functions, etc. The main problem is that these rerandomizations or averaging processes are added by hand, and not related to the dynamical evolution of the systems. Here, the original goal of basing the theory on mechanics seems to be forsaken. Sklar rightly compares these transitions to the projection postulate of quantum theory, another theory which harbours two different types of evolution for the same system.

The approach of Lanford is essentially different from the others in the sense that it aims at proving the approximate validity of the Boltzmann equation from the underlying dynamics alone and an appropriate limit on the number of particles and their density. This work, which can also be seen as an investigation of the consistency of the two types of evolution has however only obtained results for an amazingly short period of time.

I would like to point out one source of confusion in the chapter. The so-called 'one-particle distribution functions' employed by Bogolyubov and Lanford are denoted by the same symbol f_1, and sometimes referred to as if they were the same object, whereas the two are in fact very different. Bogolyubov's f_1 is a probability distribution which is obtained from the original probability distribution f over all the coordinates of phase space by marginalization to the coordinates of a single particle, say the first:

$$f_1(p_1, q_1) = \int f(p_1, \ldots p_n; q_1, \ldots q_n) \, dp_2 \ldots dp_n \, dq_2 \ldots dq_n$$

Of course, one needs to assume permutation invariance of f, or else average over the 1-particle distribution functions for all other particles, in order that it

may represent any particle, instead of the first only. The 1-particle function f_1 used by Lanford, on the other hand, is obtained from the microstate of a gas by counting how many of its particles have their positions and momenta in an infinitesimal parallelopiped box in the μ-space. Thus if $x = (p_1, \ldots, p_n; q_1, \ldots, q_n)$ is the momentary state of the system and Π_j, for $j = 1, \ldots, n$, is the coordinate function on phase space defined by $\Pi_j(p_1, \ldots, p_n; q_1, \ldots, q_n) = (p_j, q_j)$ and if A is the infinitesimal box of size $d^3p\, d^3q$ around the point (p, q) in μ-space, then

$$f_1(p, q)\, d^3p\, d^3q = \frac{1}{n} \#\{ j \ : \ \Pi_j(x) \in A \}$$

where $\#$ denotes the number of elements in a set. Lanford examines the behaviour of this function as $n \to \infty$ in the Boltzmann-Grad limit. Notice that this function describes a distribution of particles, not of probability. The Lanford f_1 depends on the exact state x of a single, individual system, and is thus itself a stochastic variable on phase space. The Bogolyubov f_1 by contrast is a probability distribution which (partially) characterizes the ensemble, not an individual system.

6   The reduction of thermodynamics

We now come to an issue which probably has the longest tradition in the study of the foundations of statistical mechanics, the claim that thermodynamics can be reduced to statistical mechanics. Again, Sklar takes a reserved and cautious attitude on the problem, warning us that the situation is fraught with subtleties and unclarities. Yet it seems that for the most part he does agree with the claim, since he concludes: "We can summarize the situation by saying that statistical mechanics successfully reduces thermodynamics by replacing the structural constraints on the world imposed by the latter theory [. . .] with a structural constraint on probabilities of the initial systems characterized at the microlevel." (p. 368)

Let us first consider the notion of reduction itself. Reduction is a particular relationship between two theories. Sklar presents two views on that relationship, the positivist and the identificationary view. The former holds that reduction relies on 'bridge laws' that connect the concepts of the two theories. This view Sklar discards because it would trivialize the problem. One could simply postulate the bridge law that theory T_1 holds just in case theory T_2 holds in order to successfully reduce any theory T_1 to any theory T_2 whatsoever. The contrasting view on reduction focuses on the ontology, on the realm of really existing entities described by the theories. Here one would say that a theory is reduced to another if all entities it describes can be identified with entities from the other's domain. The virtue of this latter account of reduction above the positivist view is, according to Sklar, that it *explains*. To say, e.g., that a light wave corresponds to an electromagnetical wave by means of a bridge law is merely to state a regularity. But on the ontological view one can do more: the question 'why are light waves always accompanied by electromagnetical waves?' receives an answer: it is because they are identical.

I think that the above rejection of the positivist view on reduction is a bit rash. I am not qualified to speak on behalf of the positivists, but it would seem to me that what Sklar calls 'bridge laws' should be compared to a kind of dictionary. They establish links between the elementary terms or concepts of the two theories which enable one to deduce (or approximately deduce) the propositions of the reduced theory from those of the reducing theory. They do not operate directly on the level of the theories as a whole and simply postulate the deducability of one theory to the other. The fear that one could trivially reduce any theory to any other by means of suitably chosen bridge laws is as plausible to me as that one could translate *the Pickwick Papers* into *Madame Bovary* by an appropriate editing of the English-French dictionary.

Perhaps it is Sklar's preference for the ontological rather than a semantical view on reduction that steers his intuition in another direction than my own. But I find his judgment on the question whether thermodynamical concepts are reducible to those of statistical mechanics often completely opposite to mine. He considers the topic of reduction in some detail for four thermodynamical notions: heat, pressure, temperature and entropy. I discuss, for brevity, heat and temperature only.

Can the notion of *heat* be successfully reduced to a concept in statistical mechanics? Sklar regards this case as the least problematic one: we are directed unequivocally to the view that heat is associated with a portion of the energy of the system (p. 349). To me, this misses the point. It is, of course, nice that heat can be identified, ontologically speaking, with a form of energy (or, perhaps, rather with a form of energy *exchange*). This means we need not refer to an independently existing entity, such as caloric, phlogiston, etc. But that does not reduce the *concept* of heat; it does not take away that one still needs to make a distinction between the particular form of energy 'formerly known as heat' versus other forms of energy (exchange).

Perhaps the point is made clearer by going back to pure thermodynamics, in the formulation of that theory given by Carathéodory. Indeed, one of Carathéodory's main goals was to eliminate the concept of heat as a fundamental notion from thermodynamics, precisely because he believed that it could be defined in terms of energy. And his formulation of the theory, at first sight, succeeds in doing so very well. However, as noted by Hornix (1970), one now finds that the notion of 'adiabatic' is indispensable and refuses to be defined in other terms. What we are left with is that, in some guise or another, the distinction between the two kinds of energy transfer, whether we call them work versus heat, or a transfer through adiabatic versus non-adiabatic walls, or, as is also sometimes done, as exchange by mechanical versus non-mechanical means, is essential to the fundamental structure of thermodynamics. But how does one reduce this distinction to statistical mechanics?

In fact, it was already noted long ago by Maxwell that according to the molecular view there cannot be a real distinction between heat and work, their only difference being that in "the communication of energy that we call heat" the motion of the molecules is irregular or confused, whereas in the case of work their motion is ordered (Maxwell, 1878a). But then, Maxwell reminded us, the distinction between confusion and order "is not a property of material

things in themselves but only in relation to the mind which perceives them" (Maxwell, 1878b). Now one need not agree, perhaps, with this last point. Statistical mechanics has more at its disposal than the concepts of mechanics and the molecular view alone, and one can reasonably expect that the distinction between heat and work can be framed in a mind-independent way with the help of the concepts of probability theory. Still I will not surprise the reader by saying that in this field one cannot safely believe anything until one can see it done. It is precisely the sort of analysis one would hope to find in a book like this.

In any case, the mere statement that heat can be identified with a form of energy does not settle its successful reduction to statistical mechanics. One should at least be able also to specify *which* form, using the terms of the reducing theory only. Note, for comparison, how different the above situation is from the case of the reduction of optics to electromagnetism. There one can specify exactly which part of the spectrum of electromagnetic waves corresponds to visible light.

A second concept of thermodynamics for which Sklar discusses the alleged reduction to statistical mechanics is that of *temperature*. He mentions how puzzling it is that people still say that temperature is nothing but the mean kinetic energy of a gas, as if it was as straightforward as that (p. 340).

The main point against this all too simple reduction is that temperature in statistical mechanics is introduced as a parameter in the canonical distribution, not as a function on phase space. Thus, it must be seen as a property of the ensemble, and not of any particular system. Still, leaving this important point aside, Sklar judges the reductive association of the thermodynamical notion of temperature to a statistical-mechanical one to be successful. The case is illustrated by showing that a more general use of the temperature concept is allowed within statistical mechanics than would be possible inside thermodynamics. This use is shown in "a good example of concept extension" (p. 353), a typical byproduct of a successful reduction.

The example is the well-known case of 'negative absolute temperatures' that can be attributed to a system of magnetic dipoles in an external magnetic field. The crucial point is that the energy of such a system has a maximum value which is reached when all dipoles are aligned antiparallel to the external field. In this state the entropy is minimal. Thus, if one starts with that state and takes energy out of the system by means of a quasistatic process, its entropy increases, so that, using $T := (\partial E/\partial S)_V$, its temperature T will be negative. Sklar argues that this is an application for which the original concept of temperature in thermodynamics was never intended, and made possible only through its reduction to statistical mechanics.

Again, my own judgment here is also antiparallel. Let me first recall that thermodynamics is characterized by an overall effort to remain completely neutral on questions of the microscopic constitution of the material systems it describes. Thus, there is no obstacle whatsoever to its application to magnets rather than gases. (Applications which indeed go back at least a century.) Further, it may be true that the founding fathers of thermodynamics never dreamt of systems with negative absolute temperatures. But that is not relevant to the

issue. The fact remains that the principles on which thermodynamics rests rely only on the idea of 'equal temperatures' (in the zeroth law) and on the ordering of temperatures (in Clausius' version of the second law, where the distinction between 'hot' versus 'cold' is introduced). There is no law that says that the ordering of equilibrium states by means of temperature must acknowledge end points: i.e. there is no law forbidding negative temperatures. Thus, the entire thermodynamical formalism can be applied to the current example without any problem.[3] And indeed, the first mention of negative absolute temperatures that I know of is by Tatiana Ehrenfest in 1925, who discusses them as a theoretical possibility allowed by the usual axioms of orthodox thermodynamics.

Quite in contrast with this, it is for the reducing theory, the kinetic approach that wishes to relate temperature with the intensity of 'intestine motion' of a body, that the example comes as a surprise. If temperature is, as so many people believe, nothing but the mean kinetic energy of the molecules, then how could it possibly become negative? Also the more advanced theory of classical statistical mechanics does not deal with the example quite so easily.

A theoretical description of the system of dipoles would employ a Hamiltonian containing a magnetic energy term only:

$$ H = - \sum_{i=1}^{N} \vec{B} \cdot \vec{\mu}_i, $$

where $\vec{B}$ is the external magnetic field and $\vec{\mu}_i$ denotes the dipole moment of particle i. (One could add a term to allow for mutual interactions between the dipoles, to obtain a more interesting system.) Notice especially that there is no kinetic term which is quadratic in the canonical momenta. This, of course, is responsible for the essential property that the energy of the system has an upper bound. However, it also means that, at first sight, one is at a loss to specify the phase space for this system, with its canonical coordinates. Thus, it is not immediately clear how one can implement the Hamilton equations, Liouville's theorem, and all the further mathematical structure on which the results of classical statistical mechanics and ergodic theory depend.

Fortunately it is possible to recover these results, by adopting a phase space which is radically different from the usual one. One can take the magnetic orientations of the individual dipoles themselves as furnishing the coordinates of a microstate. The phase space is then taken to be an N-fold Cartesian product of unit spheres. The role of the 'canonical coordinates' is taken over by ϕ_j and $\cos\theta_j$, where ϕ_j and θ_j are the spherical angles of the dipole vector (or 'classical spin') $\vec{\mu}_j$, and the Hamilton equations for the above Hamiltonian lead to the precession of the moments with the Larmor frequency. It is remarkable that the possibility of this description in statistical mechanics has been discovered rather recently, and how different it is from the usual assumption of a Euclidean phase space, an assumption which is often the very first mentioned in expositions

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[3]The only obstacle to the application is that negative temperatures could not be measured with a gas thermometer. But that is not a very serious objection. In fact, one of the main and often emphasized advantages of the concept of absolute temperature introduced by Kelvin is precisely that it liberates the concept of temperature from a particular choice of thermometer.

of statistical mechanics. I wonder whether Hamilton or Gibbs ever dreamt of applying mechanics on a spherical phase space.

Another approach to a successful statistical description of the system is, of course, obtained in quantum theory. But that move upsets the structure of the theory even further.

Thus I would argue here, that while the example is encompassed effortlessly by thermodynamics, it is describable by statistical mechanics only after hard work and a non-trivial extension of the theory. If concept extension is the true mark of a successful theory reduction one is tempted to conclude, somewhat perversely, that thermodynamics is not successfully reduced to orthodox statistical mechanics at all, but rather vice versa.

7 Conclusion

Summing up, I would like to repeat my admiration for the book. It delivers what it promises: a wide-ranging overview of many aspects in the foundations of statistical mechanics, far wider than I was able to discuss. I do not always find Sklar's approach to these topics the most fortunate, neither do I always agree with the conclusions he reaches. But that is only natural in a field as open and uncultivated as this one. There is obviously still a lot of work to be done before the foundations of this part of classical physics reaches the level of maturity that the debate on the foundations of quantum physics has reached already long ago; i.e. a general agreement on what the disagreement in the field is about. I wholeheartedly join Sklar in his hope that his book will inspire more philosophers of science to subject the field to critical conceptual analysis.

Acknowledgments

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