Abstract

What I am concerned with in this essay is a case that has often been taken up with respect to the problem of the reducibility of thermodynamics to statistical mechanics or of the logical hierarchy between them, namely, the case of the derivation of the equation for the state of ideal gases (Boyle-Charles law) from the kinetic theory of gases. This is commonly considered to be one of the most successful examples of theory reduction, in that through the efforts of Maxwell and Boltzmann the basic but phenomenological law of classical thermodynamics was brought to bear the Newtonian mechanical description of the movement of molecules which were supposed to constitute gases, and through this then the so-called mechanistic view of the world was laid firmly at the foundation of modern physics. As for the relation between thermodynamics and (statistical) mechanics, the most attractive and often discussed theme must be the one concerning the second law and irreversibility, which won't be taken up here. The main reason that I have chosen to focus on the former case, instead of the latter, is that it is not so much a question of abstract concepts of theoretical origin, such as 'entropy', of which it is hard to establish a consensus of usage even among physicists, but rather it is a question of concepts such as 'heat', 'temperature' or 'pressure', which could in one sense be said to be formed by the very logic of our ordinary sense experiences, to which this case refers. In other words, it is expected that the task of explicating the logical status of the presuppositions which are tacitly adopted in this derivation (which physicists often pass by as unquestionable) can serve as a case study for investigating such problems on the philosophy of science as that of the relation between scientific language and ordinary language, the relation between scientific realism and anti-realism, or the relation between conceptual reducibility and 'emergence'.

The points that result from this essay are as follows. First, the most important presupposition introduced in this reduction and in some sense upon which the whole
success or failure of this reductionistic procedure hinges (briefly, that temperature is nothing but the mean kinetic energy of molecular motion) bears a more or less conventional character, the main rationales justifying its introduction being ‘formalism’ (i.e. formal equivalence between basic laws of thermodynamics and statistical mechanics) and ‘consequentialism’ (i.e. the increase in applicative power of the whole theoretical system resulting from this reduction). Secondly, if it is true that the logical status of this presupposition remains suspended, then the alleged successful reduction will also need reconsidering, at least in principle, which then makes our understanding of the so-called phenomenalistic properties such as temperature problematic again. Is temperature a mere ‘secondary quality’ which doesn’t have its root on the microlevel ‘true reality’, or should it be treated as something like an ‘emergent’ property which claims to have its own ‘raison d’etre’ apart from the latter? That is the question.

1. Introduction

It is a long time since the limit of reductionism-to-elements of modern science and the problems of its mechanistic-instrumentalistic grasp of nature came to be pointed out. Then what critics often have on their minds to represent this methodology is a so-called mechanistic view of the world. This means, in short, a position that undertakes to reduce all natural phenomena to the spatio-temporal displacement (i.e. movement) of the ‘mass points’, which are supposed to be basic elements that constitute them. This position was outlined in Newton’s *Principia* and afterwards developed to a mathematically formalized systematic theory, by Euler, Lagrange and Hamilton, that marked the completion of classical mechanics. And it is a well known fact that the thought of so-called ‘Laplace’s Demon’ which appeared at the outset of the 19th century embodies the height of its thought.

On the other hand, the study of macroscopic thermal phenomena (the theory of heat), which was originated by Galileo and further developed from the 18th through the 19th century, although wavering between the substance (caloric) theory of heat which purports to ascribe thermal phenomena to entities proper to them and the kinetic theory of heat which purports to reduce them to the motion of matter, held a relative autonomy against the above mechanical method, as a whole. However, after the establishment of the equivalence (the possibility of mutual transformation) between heat and mechanical work by the discovery of the first law of thermodynamics (i.e. the law of conservation of energy), it came to be merged into mechanics, as thermodynamics. Among others, the enterprise of statistical thermodynamics by Maxwell and Boltzmann, which purported to explain thermal phenomena away by means of the mechanical motion of microscopic molecules, was one that had the obvious intention of reducing thermodynamics to mechanics in a
genuine sense.

But this reductionistic strategy hasn't necessarily achieved complete success so far. In particular, the Bolzmann's effort which tried to deal with the irreversibility of natural phenomena at a macroscopic level expressed in the second law of thermodynamics as a 'surface' phenomenon which was caused by the mere fact that the microscopic molecules, themselves occupied in the reversible movement describable by Newtonian mechanics constituted a huge ensemble, or as a state of affairs which was brought about by the 'lack of knowledge' of us human beings who were, unfortunately, not so omnipotent as Laplace's or Maxwell's demon, though successful to a certain extent in building a theory for practical use, still has left room for arguments in principle and has in fact offered materials for various philosophical interpretations.

However, what I am concerned with in this paper as a primary subject is not this problem on irreversibility but another case that has often been taken up with respect to the problem of the reducibility of thermodynamics to mechanics or of the logical hierarchy between them, namely, the case of the derivation of the equation for the state of ideal gases (Boyle-Charles law) from the kinetic theory of gases. The main reason for this is that it is not so much a question of abstract concepts of theoretical origin as 'entropy', of which it is hard to make consensus of usage even among physicists, but rather of concepts such as 'heat', 'temperature' or 'pressure', which could in one sense be said to be formed by the very logic of our ordinary sense experiences, to which the latter case refers. In other words, it is expected that the task of explicating the logical status of the presuppositions which are tacitly adopted in this derivation (which physicists often pass by as unquestionable) can serve as one of the case studies for investigating such problems on the philosophy of science as that of the relation between scientific language and ordinary language or that of the relation between scientific realism and anti-realism.

2. The derivation of the Boyle-Charles law from the kinetic theory of gases

The above-mentioned Maxwell's and Bolzmann's task of deriving the Boyle-Charles law, which enjoyed the status of being the basic law in classical thermodynamics, from the Newtonian description concerning the movement of molecules which were supposed to constitute gases, is usually considered to be a typical successful case of the reduction of an old theory by means of a new one. For example, Kubo Ryogo, one of the representative statistical physicists in Japan, once

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2 Of course, there is a certain gap between the cases in which these concepts are used in physics (eg. F, K) and in ordinary discourse (eg. warm, cold). This point will be taken up later in section 4.
stated the following in his book *Statistical Mechanics*.

Although it may have to be considered to be first confirmed with respect to the phenomena of gaseous reactions such as the law of definite proportions that the concept of atoms is quite useful to explain macroscopic laws, it should be the merit of the kinetic theory of gases that it fixed the concept firmly at the foundation of physics by explaining wonderfully such physical laws as the equation for the state of ideal gases, namely the Boyle-Charles law. Through this means, physicists set out to investigate the atomic (microscopic) world beyond macroscopic physics. The kinetic theory of gases was just the one which represents the mechanistic view of the world that intends to reduce everything to mechanics.  

Also in Ernest Nagel's *The Structure of Science*, which is said to be a classical work analyzing the logical structure of scientific explanations, this case is detailed as a model case of successful "Reduction of Theories". ([13] Chap. 11)

As for Nagel's analysis, we will later examine it in detail, but now we begin by presenting this case briefly. Suppose *N* number of ideal gas molecules are contained in a cubic container of a length *L*. Each molecule is supposed to be a spherical, perfectly elastic body of mass *m* and each collides with other molecules and the walls of the container perfectly elastically. Let the size of each molecule be so small that it can be neglected compared with the mean distance between them (which means the potential energy of intermolecular force also can be neglected), and each molecule should be light enough to escape the effect of gravity, so that they are not subject to any mechanical action other than the impulsive force they receive when colliding with one another or with the walls. Now take notice of one molecule among others and suppose its movement can be described regardless of the mechanical interaction with other molecules (the supposition of mutual independence of molecules). And let one corner of the container be the origin and settle *x*, *y* and *z* axes along its three sides extending therefrom and crossing perpendicularly with one another. Then, provided we name the velocity of this molecule in the positive direction of the *x* axis *v_x*, this molecule collides with the wall located in the position *x = L* perpendicularly to the *x* axis *v_x/2L* times per unit time and transmits the impulse *2mv_x* to it each time there's a collision, from which it follows that the average force imposed on this wall by the molecule (which is equal to the impulse per unit time) turns out to be *2mv_xv_x/2L = mv_x/L*, and by dividing this value by the area of the wall *L^2*, we get the pressure *mv_x^2/V* (*V* is defined as *L^3*).

Next we consider the *N* number of molecules at a time. In this case we cannot

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3 Kubo [1], p. 3. Translation into English was done myself.
assume \( v_x \) to be constant as above, as the exchanges of kinetic energy brought about by mutual collision of the molecules make the velocity components of each molecule vary from moment to moment. However in that case, we can disregard such velocity exchanges between molecules by considering the statistical average for a large number of them. For, as the law of conservation of momentum assures that the gain of momentum in the \( x \)-direction for one molecule at a collision time is equal to the loss of it for another, the gains or losses of momentum for individual molecules cancel out in considering the total impulse imposed on the wall by many molecules, so that we can acquire an expected value for the total pressure simply by 'superposing' the result in the case of one molecule obtained above by \( N \) times. (This is exactly why this method is a 'statistical' one.) Now, introducing the notation \( v_{xk} \) to express the velocity of the \( k \)-th molecule in the \( x \)-direction ('\( k \)' is a natural number not larger than \( N \)), the total pressure \( P \) imposed on the wall turns out to be as follows.

\[
P = m \sum_{k=1}^{N} \frac{v_{xk}^2}{V} = mN\langle v^2 \rangle /3V = \frac{2N}{3V} \cdot \frac{m\langle v^2 \rangle}{2}, \quad \text{.....................(#)}
\]

where we put the mean square of molecular velocities as

\[
\langle v^2 \rangle = \frac{\sum_{k=1}^{N} v_{xk}^2}{N} = \frac{\left( \sum v_{xk}^2 + \sum v_{yk}^2 + \sum v_{zk}^2 \right)}{N} = \frac{3}{N} \sum v_{xk}^2,
\]

as the isotropy of molecular motion requires that

\[
\sum v_{xk}^2 = \sum v_{yk}^2 = \sum v_{zk}^2.
\]

Then, introducing an assumption that the mean kinetic energy of molecular motion should be proportional to an absolute temperature \( T \) of the gas and positing that

\[
\frac{1}{2} m\langle v^2 \rangle = \frac{3}{2} kT, \quad \text{..............................(##)}
\]

we obtain \( PV = NkT \) from the foregoing expression for \( P \), from which finally we reach the expected Boyle-Charles Law \( PV = KT \), simply by newly using \( K \) instead of \( Nk \). This is how usually the case concerned is introduced in many textbooks of statistical mechanics.

3. Nagel's analysis

We are now in a position to examine Nagel’s analysis of this model case in *The Structure of Science* (1961). As we alluded to before, in the chapter “The Reduction of Theories” of this book, he systematically deals with the problem of which conditions must be met for one theory to be said to be successfully reduced to another. The task of structure analysis of scientific theories as is typical of Nagel
seems somewhat out of fashion today, when the 'hermeneutic-sociological turn' in
the philosophy of science initiated by Kuhn's *The Structure of Scientific Revolutions*
(1962) still holds its cogency in some sense. Nevertheless this task is indispensable,
especially when one tries to examine critically the modern scientific view of the
world from within, and not being merely satisfied with historically relativizing it as
the discourse only proper to modern times.

Anyway, among the several points of argument he proposes in this chapter, we
now focus our attention only to one which we hold to be the most important, that
is, the problem of what a logical status the foregoing assumption (###) —— this
eventually comes down to the ontological implication that temperature is nothing
but the mean kinetic energy of molecular motion —— has and how the adoption of
the assumption can be justified in this derivation, upon which in some sense, from
our point of view, the whole success or failure of this reductionistic feat hinges.

The import of this problem becomes clear from the following consideration. If,
as Nagel believes so, 'reduction' requires strict 'deduction' ([13] p. 352), then ——
from the logical maxim that in formal inferences whatever terms are not included in
their premises cannot appear in their consequences —— it turns out trivially impos-
sible to reduce such empirical laws of thermodynamics which include the term
'temperature' as Boyle-Charles law to the theory of statistical mechanics which does
not. Therefore, the introduction of the assumption of the syntactical function to
relate between terms which reducing theory (statistical mechanics) and reduced
theory (thermodynamics) each has of its own —— we call this assumption the
"additional postulate" according to Nagel ([13] p. 355) —— becomes necessary for
the smooth performance of the reduction. And even if we don't commit ourselves
to the Hempel-like deductionistic position to which Nagel adheres, we can't in any
case do without introducing a sort of 'correspondence rule' which in some way relates
between the macroscopic terms of thermodynamics and the microscopic terms of the
kinetic theory of gases.

However, on the other hand, this postulate is nothing but 'additional', which
means it is neither included in nor derived logically from the reducing theory.
That's why we have further to ask about its logical status and the rationale of its
employment.

Now, what Nagel gives as the possible candidates for its logical status are these:
(1) analytical statement (in his words, "logical connection") (2) convention (3)
But the possibility (1) drops off before entering into further consideration, for it is
already precluded for the trivial reason mentioned two paragraphs before.

Then as for (2), this means the possibility of construing the postulate as a
correspondence rule which "institutes a correspondence", "by deliberate fiat" ([13]
p. 354) between the theoretical term of the reducing theory and the observational
one of the reduced theory. One main reason sustaining this possibility against (3) is that the postulate itself can’t be subject to empirical test. For, in order to test it, it becomes necessary to measure the quantities concerned which appear on both sides of the equation (###) independently beforehand and only after that to compare them, whereas such a procedure is doomed to fall into begging the question if the theoretical concept of ‘mean kinetic energy of molecular motion’ (the left-hand side of the equation) is put to quantitative measurement for the first time by this very rule of correspondence. According to this construction, the function of this correspondence rule consists of, so to speak, ‘bridging’ between both theories which have remained mutually independent to some extent so far, and through this incorporating the phenomenological law of the reduced theory newly into a more abundant interpretative context. However, in this case, there couldn’t be any rationale justifying the introduction of this rule itself, other than a ‘consequentialistic’ criterion of an increase in the consistency and applicative power of the whole theoretical system resulting from fulfilling this reductionistic procedure by introducing it, so that its logical status remains still temporary and unstable.

Next as for (3), in order that this possibility of construction holds, ‘temperature’ and ‘mean kinetic energy of molecular motion’ ought to be put to quantitative measurement independently, contrary to what was said in the preceding paragraph. According to Nagel, the proponents of this construction think it possible. They say the quantitative measurement of ‘mean kinetic energy of molecular motion’ can be done, not necessarily by depending on the measurement of ‘temperature’, let alone by direct measurement of itself, but by putting it into relation to directly measurable quantities other than ‘temperature’⁴, which procedure hence supposedly establishes an indirect connection between temperature and kinetic energy and so enables us to ascertain whether both respectively measured quantities concerned are in proportion to each other, just as the postulate requires. This would mean, in other words, releasing the postulate from the duty of a correspondence rule into a mere empirical hypothesis by means of adopting different rules. If such a way should actually be possible, and also provided that the postulate will have been proved to be sufficiently valid for the task of carrying out the present reduction —– we now leave it unquestioned that empirical proof cannot claim to be necessary anyway —–, then all this would become a strong reason for the justice of the reduction. According to Nagel, this is the way most technical textbooks introduce the postulate concerned and suppose its procedure to be no problem. ([13] p. 356)

What’s more, he thinks it is difficult to decide which of the two constructive possibilities (1) and (2) actually reflects the fact, because “the cognitive status of an

⁴ Nagel alludes to ‘viscosity’ and ‘heat flow’ as the candidates in place of temperature ([13] p. 356), but I cannot judge here their suitability for the assigned job.
assumption often depends on the mode adopted for articulating a theory in a particular context" and "it is therefore not possible to decide in general whether the postulate is a coordinating definition or a factual assumption, except in some given context in which the reduction of thermodynamics to mechanics is being developed" ([13] p. 356f.). What he means is that these two constructions are not necessarily incompatible and that the postulate holds both characters depending on the given context.

However, in my judgement, after looking over several textbooks on statistical thermodynamics ([1, 2, 3, 4, 5, 6, 7, 8]), contrary to Nagel's diagnosis, it seems that there is more trait of a convention than of an empirical hypothesis to this additional postulate. Let us explain this point in a bit of detail. Although we have already outlined the way the postulate concerned is introduced into the reductive procedure in section 2, it was, to tell the truth, sort of a textbook presentation which is more or less a product of rational reconstruction post hoc. A more historically faithful (but somewhat simplified) presentation of the course in which this postulate was actually reached would be like this: On the one hand, there was already known an empirically-phenomenistically established Boyle-Charles law waiting to be reduced (or explained) by some more basic theory. It can be expressed as $PV = kT$. On the other hand, there was a formula theoretically derived from kinetic theory $PV = 2N/3 \cdot m\langle v^2 \rangle/2$ (identical with (1)), as we derived in section 2 (which is sometimes called 'Bernoulli's formula' today). Now, if you pay attention to the formal equivalence between the left-hand sides of these two formulas, you will be naturally led to equate the right-hand sides, too, and easily obtain the expected postulate, $\frac{1}{2}m\langle v^2 \rangle = \frac{3}{2}kT$, after a simple replacement of notation. Once having reached it this way, you will be well justified to qualify it in turn as a keystone for carrying out the anticipated reduction in such a way as a sort of rational reconstruction. This is presumably the logic which led Maxwell's and Boltzmann's (or maybe Clausius's5) thought, and hence which also underlies most of today's textbook introduction of the postulate that strikes us prima facie as somewhat abrupt.

But, here we have to be more deliberate because there seems to be a smell of question-begging in this course, at least from a logical point of view. Originally, the 'pressure $P$' that appears in the first formula stood for a macroscopic quantity measurable by laboratory experimentation, while the same notation in the second formula stood for the summation of supposed microscopic effects on the wall by molecules under the proposed "billiard ball model" (Brush [11]) of the kinetic theory. Now strictly speaking, it should be a matter which can only be settled after successful completion of the reduction of thermodynamics to the kinetic theory,

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whether these 'two pressures' can really be identified. Before that they are rightly said to still leave the possibility of remaining 'incommensurable' quantities, in the sense in which Kuhn and Feyerabend used this term where they questioned the identifiability of Newtonian 'mass' and relativistic 'mass'. While the same term 'pressure' is used in each instance, the connotation differs altogether and the coincidence of their denotation is still in question. Hence, it will be to blame for begging the question, to use such a postulate in the context in which the reducibility itself between the two theories is at stake.\(^6\)

Of course, it must be granted that several lines of circumstantial empirical evidence were found there to sustain such a reductionistic feat. For example, as we stated in the introduction, the discovery of the conservation of energy and the convertibility between heat and mechanical work by Mayer or Joule encouraged the kinetic theory of heat and inspired the belief among the physicists of those days that there must be some intrinsic relation between the temperature of matter and the mode of behaviour of its constituents. This becomes more understandable if we note the fact that there was a strong tendency among them to explain away, by all means, thermal phenomena which had remained a mere matter of experiment from more basic principles in such a way as Galileo's 'demonstrational science'.\(^7\) But these factors themselves don't directly corroborate the identification between 'two pressures' described above. As of that point of time, such an identification relied more or less on anticipation. Most of today's literature only repeats this way of introducing the postulate without detailing with how and when this anticipation turned into an established fact after that. This is why we think this postulate should be characterized not so much as an empirical fact but rather as a convention.

4. The philosophical interpretation of this 'additional postulate'

If it is true that the additional postulate concerned bears a more or less conventional character, then it has to inevitably make itself open to various philosophical interpretations. We'll now consider this side of the problem in the following.

First of all, the conventional side of this postulate suggests the possibility of interpreting it reductionistically as a semantical (or else, an ontological) 'definition' of macroscopic and empirical qualities on the basis of the information about the

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\(^6\) We can find similar arguments against such a formalistic reduction in Rohrlich [20].

\(^7\) See Tomonaga [8] p. 49. He describes their aspirations for the reducing theory and their consequent high expectations for molecular theory as a promising candidate, using such expressions as "venture" or "bet". Naturally as a physicist, he takes it favorably, as a matter of course, from a pragmatic point of view, while we, as philosophers of science, are reconsidering it more disinterestedly here from a logical point of view.
microscopic structure of matter, like this: temperature is nothing but the mean kinetic energy of molecular motion. (And surely this is the most common interpretation.) In this case then, temperature should turn out not to be a real attribute of matter, but something like a ‘secondary quality’ which reveals itself only in relation to our subjective or intersubjective experiences. For individual gas molecules, which are supposed to be the ‘exclusively true’ reality according to this interpretation, engage themselves only in a mechanical movement (spatio-temporal displacement) as a ‘primary quality’, which makes it nonsensical for us to speak of the ‘temperature’ of each single molecule.

But, even if such inference is valid, from which it won’t necessarily follow immediately that our sensation of temperature such as ‘warm’ or ‘cold’ should finally be reduced to statistical mechanics (plus neurophysiology\(^8\)). For the so-called empirical temperature, which is defined through the zeroth law of thermodynamics,\(^9\) is being introduced operationally by means of the empirical concept of thermal equilibrium and insofar can be said to be an ‘objective’ measure, which therefore doesn’t necessarily coincide with our subjective sensation of temperature. For example, we ‘feel’ the water of a well as cold in summer and as warm in winter, which doesn’t mean, however, that the temperature of the water of the well is higher in winter than in summer. As for the absolute temperature which appears in the Boyle-Charles Law, it surely bears more of a character of theoretical constitution. But the situation isn’t so far away between the two in that they are both being defined through an operational procedure which can be shared intersubjectively. What is reduced by the present reduction (##) is, at the best, such an ‘objective’ temperature in physics, not so far as the subjective sensation of temperature.

\(^8\) Here we call the theory which covers up to where our sense organs are stimulated by outer matters ‘statistical mechanics’, and the theory which covers our information processing mechanism reaching therefrom ahead to the activation of cerebral neurons ‘neurophysiology’, only for convenience.

\(^9\) The zeroth law of thermodynamics states, “If a matter A thermally equilibrates with a matter B and simultaneously with a matter C, then B and C are in equilibrium, too.” Hence, if we define ‘empirical temperature’ as a quantity that remains the same between any two objects mutually in equilibrium, then this can be claimed to be an ‘objective’ measure. And what’s more, ‘difference in temperature’ can be defined like this: if heat flows from a matter A to a matter B, when contact between them is maintained, then the temperature of A is higher than that of B. Taking for granted the atomic or molecular constitution of matter, and also provided that we consider the reductionistic interpretation now in question to be no problem, such temperature as is defined this way surely can be said to be reduced to the kinetic theory. For the empirical fact that heat flows from high to low temperature can be explained by the knowledge of the kinetic theory that, upon collision between a molecule of relatively high kinetic energy and one of relatively low kinetic energy, brought about by contact between two macroscopic objects of different temperature, energy is always transferred from the former to the latter. As to this point, see Oshida & Fujishiro [2].

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However, even in that case, if we were to further introduce a definition for the sake of physicalistic reduction in such a way that our touching something and feeling it as 'warm' means nothing but the state of affairs that heat flows from that thing of relatively high temperature to our skin of relatively low temperature, then our subjective sensation of temperature would also turn out to be said to be reduced to statistical mechanics (plus neurophysiology). Yet then, the logical status of and the legitimacy for introducing such a 'definition' (or else, bridge law) should have to be newly brought into question, which, as you see, is something like the same situation as the present additional postulate.\(^{10}\)

What's more, let us here note one more problem of a semantical sort which results from the above reductionistic interpretation. If, by definition, temperature should be equated with the mean kinetic energy of molecular motion, then that will also make the meaning of the word 'temperature' identical to the meaning of the phrase 'the mean kinetic energy of molecular motion', by definition, that is to say, both these expressions turn out to be synonymous. However, it would lead to a queer situation like this: suppose a layman in physics says, "the temperature of this milk seems to be 0 degrees (Celsius) or so". Then what he is 'really' doing is referring to the mean kinetic energy of molecules constituting his milk, without intending or being conscious of that himself? On the one hand, such a comprehension surely sounds somewhat unnatural. But on the other hand, what if your familiar everyday concept of temperature is 'in fact' a mere representation without any referent, something like the case of 'round square'?

One possible way around such a difficulty might be, as Garfinkel shows (Garfinkel, p. 443f.), to distinguish the reduction of explanations (or theories) from the ontological reduction. Namely, we might be able to think this way: even if it is true that something (i.e. a macroscopic object) is 'nothing but' something else (i.e. a microscopic object) from the ontological point of view, from which it won't necessarily follow that the description of the former 'something' (on a macroscopic level) should also be reduced to the description of the latter 'something' (on a microscopic level) from the explanatory point of view. To put it another way, the former description won't come down to a 'redundant' one which can be replaced by the latter. Paraphrasing this suitably for the present case, even if reductionists are correct in that temperature is ontologically nothing but the mean kinetic energy of molecular motion, that won't necessarily forbid us to speak of this one and the same object from several viewpoints on different levels, each of which claims to have its

\(^{10}\) Such a physicalistic definition should indeed be questioned, in that it identifies the so-called mental qualia with a physical process immediately, without argument. The problem of the possibility of such a reduction of the mental is one of the central problems in today's analytic philosophy of mind. See, for example, Churchland [17] and Nagel, T. [18].
own context of discourse.

On the other hand, however, it is possible to be suspicious of such an ontological reducibility itself, at least in principle. So next we'll deal with the second possible interpretation of the postulate from an anti-reductionistic position, as opposed to the first.

As a comparatively moderate first step in this second anti-reductionistic direction, we can think of an empiricist line of interpretation to the effect that it considers this postulate as a correspondence rule, the function of which is to bestow empirical significance upon the theoretical concept of statistical mechanics (i.e. the mean kinetic energy of molecular motion) by means of relating it to the measurement procedure for the observable quantity of temperature. In fact, this line matches the spirit of the research program of logical positivists. In this case, using a symbolic expression, it is not that ‘meaning’ ascends from the microlevel to the macrolevel, but, on the contrary, that it descends from the macrolevel to the microlevel. In other words, according to this interpretation, what the postulate is assigned to do is stipulate the procedure, not for replacing the ‘merely phenomenalistic’ macrolevel quality by the ‘existing’ microlevel one, but, daring to say, for ‘filling’ the microlevel quality as ‘theoretical construction’ with a meaning that comes from the macrolevel quality in the ‘life world’ (Husserl) as the basis of ‘evidence’ of scientific knowledge.

This first step of anti-reductionism itself, however, still remains a cognitive (or epistemological) assertion, not being necessarily committed to the negation of ontological reducibility or anti-realism. But such a moderate position is easily radicalized to a second step in this second anti-reductionistic direction, which has in fact exemplified itself historically, for example (putting aside actual chronological order), as the opinion of Mach, to the effect that it is the very macroscopic thermodynamics which can claim to be a real science founded firmly on the base of experience. He says, “purely physical phenomena are therefore abstractions, introduced, either intentionally or forced by necessity, in order to obtain better perspectives” or “the view that mechanics should be considered as the foundation for all other remaining branches of physics and that every sort of physical phenomena must be explained mechanically, is in our judgement a prejudice.”

5. The ‘emergence’ of thermodynamics

Differently from Mach’s times, however, it would be difficult to maintain such an extremely phenomenalistic or anti-realistic position without questioning it today, when there can no longer be any room for doubt as to the atomic constitution of

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11 Mach [12], at the top of Chap. 5, ‘The Relations of Mechanics to Physics’. Translation was done myself from the original text in German.
matter. So we are now faced with the necessity of groping for a third way, which doesn’t commit itself to such a radical anti-reductionism, and besides, which also draws a distinction from the ‘robust reductionism’ described above.

One possible move is to give thermodynamics credit for being a discipline holding a relative autonomy from mechanics and addressed to its own subject phenomena. This means, so to speak, no more than trying to apply the claim of ‘the multilayered (or hierarchical) structure of being’, which has been offered so far with respect to biological, psychological or social phenomena, also to the present case of the relation between mechanics and thermodynamics. And then, the properties such as ‘temperature’, ‘pressure’ or ‘heat’ turn out to be sorts of ‘emergent’ properties.

According to Nagel’s formulation, a property \( P \) possessed by a complex system \( O \) can be held emergent in the following case: suppose \( O \) to be constituted out of certain elements \( a_1, \ldots, a_n \), when and only when these elements are mutually standing in some relation \( R \). (symbolically, \( O = R(a_1, \ldots, a_n) \)); and suppose these elements possess properties belonging to the classes \( A_1, \ldots, A_n \) respectively; and also suppose they may enter into relations with one another different from \( R \) (say, \( S \)), to form complex systems (say, \( Q \)) other than \( O \). (symbolically, \( Q = S(a_1, \ldots, a_n) \)) Here, let’s assume in addition that we have what is called “complete knowledge” concerning the elements, namely, that we know all the properties belonging to each \( A_k \) when the elements exist “in isolation” from one another, and furthermore that we know all the properties exhibited by systems other than \( O \) when they enter into relations other than \( R \), as well as all the properties of the elements themselves in those systems. Now then, despite this assumption of complete knowledge, if it is impossible to predict (that is, deduce) therefrom that, when each \( a_k \) is mutually standing in the relation \( R \), \( O \) will exhibit the property \( P \), then \( P \) is said to be an “emergent property”, \( O \) being an “emergent object” ([13] p. 367f.).

12 This idea of emergence as ‘nonpredictability’ originally comes from C. D. Broad [24], and here Nagel reformulates it in his way, albeit for critical discussion, on which we depended for the time being. But, of course, Nagel’s conception of the concept of emergence should not be final and much closer scrutiny will be required to work out what exactly this concept can imply. We indicate here only the locus of the problem concerning Nagel’s conception and the direction along which it may well be explored hereafter. Seemingly his conception refers to so-called cognitive emergence, in the sense that the concept applies only to the dynamics within our cognitive states, not reflecting any structure of real things, for he appeals to such cognitive concepts as “complete knowledge” or “prediction” in formulating his conception. And if so, it will be the case that the emergence can imply nothing more than the confession of our ignorance at the present stage of science and that it is doomed to be overcome one after another by the development of reductionistic theories accompanying the progress of science. However, on the other hand, he seems to try to retain positive, not merely temporary, significance of the concept, when he puts the adjective ‘complete’ on the word ‘knowledge’ and identifies ‘to predict’ with ‘to deduce with strict logical rigor’ ([13] p. 371). For this indicates that something emerges on a higher level ‘necessarily’ (this might be equated with ‘ontologically’).
We now hereafter proceed to consider the present case, based on this Nagel's basic spirit. To be sure, we are able to 'predict' a holistic property of a gas system (i.e. temperature) from the knowledge of the microscopic properties of its constituent molecules (i.e. mean kinetic energy), using the additional postulate concerned. But, this applies only to the case in which the molecules have already constituted an ensemble put under a certain 'structural' correlation called the Maxwell-Bolzmann distribution, which forces them to interact with one another under a certain velocity constraint, and not to the case where they exist 'in isolation from one another' flying around freely, so that the information used here concerning the molecules, as the starting point to carry out the reduction, cannot claim to be the 'complete knowledge' in the sense Nagel meant it to be. For the very concept of the 'mean' kinetic energy of molecular motion has already preempted some bit of this holistic quality of the gas system. Even if we were to have such an intelligence as that comparable with Laplace's demon and could fully grasp the states of individual molecules at one time, it would be nonetheless impossible to predict the temperature of the system from such an enormous amount of data, were it not for the additional postulate concerned, because, given that the temperature is defined through the postulate, even the 'Demon' would have to impose 'coarse graining' upon those microscopic data in order to derive it, in the way of 'averaging' the kinetic energy of individual molecules. And if so, all his effort would come down to just the same effect as we, of limited intelligence, would attain, by giving up acquiring the microscopic data and applying a statistical method to the gas ensemble from the first. Properties obtained as a result of taking the average or coarse graining are not the ones belonging to the constituents as 'primary qualities'. They are, so to speak, something which should be placed not so much on the side of reality but rather on the side of theoretical construction, that we, as scientists, assign to the constituents only post hoc. Put another way, the constituent molecules are said to precede the holistic properties ontologically, indeed, to the effect that, unless there existed if and only if even an omnipotent scientist at the ultimate stage couldn't pose any algorithm to deduce it from the full information of lower level, not merely temporarily if we cannot afford to do that as of now. Whether 'emergence' should be dealt with in terms of its cognitive or subjective implications, or whether it should be considered as only reflecting the triviality that it is the discrepancy between both repertories of vocabulary of reducing and reduced theory which makes it impossible to deduce analytically the laws of the latter from those of the former, or else we can expect that there is some positive and indispensable aspect in it which reflects the hierarchy and complexity of the natural world; that is precisely the focal point of the controversy which has existed up to the present day between reductionists and anti-reductionists. Concerning such a problem, the proceedings of a symposium held under the title "Emergence and Supervenience: Alternative to Unity by Reduction" at the 1996 meeting of the Philosophy of Science Association in the U.S. (see [20, 21, 22]), and also some papers included in [23], are instructive.

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individual molecules, it wouldn’t make sense to think of the mean kinetic energy of molecular motion, and accordingly, nor of the concept of temperature of the gas system. But, on the other hand, they are also said to be subordinated to the whole system, to the effect that, once becoming members of the gas ensemble, the individual molecules are inevitably put under the structural constraint of the Maxwell-Bolzmann distribution and so are forbidden random movement. One might be able to notice here a situation analogous to the ‘hermeneutical circularity’ between parts and the whole, say, in the context of interpreting texts.

Thus, even if we were to overlook the problem of the reducibility of thermodynamics to statistical mechanics, concerning the validity of the introduction of the additional postulate concerned, the emergence of the latter from mechanics eventually blocks the overall reducibility of the former to mechanics, and so establishes its emergence in the full sense.

CONCLUSION

judging from all the consideration hitherto, the temperature exhibited by an ensemble of gas molecules can be said to be a holistic and emergent property, which cannot be entirely reduced to the microscopic properties of individual molecules. And if we dare to generalize this point, keeping in mind that it is only one specific case of investigating the hierarchical structure between thermodynamics and mechanics, we think it proper to maintain that thermodynamics also claims to be a branch autonomous from mechanics with its own ‘emergent’ subject matter, and hence with its own ‘raison d’être’.

References


