Jordan's Derivation of Blackbody Fluctuations

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Abstract

The celebrated Dreimännerarbeit by Born, Heisenberg and Jordan contains a matrix-mechanical derivation by Jordan of Planck's formula for blackbody fluctuations. Jordan appears to have considered this to be one of his finest contributions to quantum theory, but the status of his derivation is puzzling. In our Dreimenschenarbeit, we show how to understand what Jordan was doing in the double context of a Boltzmannian approach to statistical mechanics and of the early 'statistical interpretation' of matrix mechanics.

1 Introduction

In 1926, Born, Heisenberg and Jordan published their famous Dreimännerarbeit (three-men paper) giving a first definitive form – that of matrix mechanics – to Heisenberg's quantum mechanics of 1925 (Born *et al.* 1926; Heisenberg 1925). In this article we shall be concerned only with the final section of the 1926 paper, which was authored by Jordan and contains a derivation solely from within the matrix mechanical framework of Einstein's 1909 formula for blackbody fluctuations.

A derivation of Einstein's formula from a unified framework had long been sought, and yet Jordan's success in this regard in the 1926 paper received surprisingly little attention from his contemporaries, and perhaps as a consequence of this has been largely neglected in more recent scholarship. Duncan and Janssen (2008) (hereafter simply 'Duncan and Janssen') review a number of criticisms of Jordan's contribution to the Dreimännerarbeit in an attempt to explain the lack of attention paid to a derivation Jordan himself later considered his most important contribution to quantum mechanics (*ibid.*, p. 639).

While the answer to this riddle for Duncan and Janssen has somewhat to do with unfamiliarity of matrix mechanics at the time, especially in its novel application to radiation

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formulae, it is ultimately blamed on residual technical and conceptual puzzles in the proof. For instance, the authors argue that Jordan's claim to have resolved wave-particle duality with his derivation generated a merely lukewarm reaction mainly due to the derivation's looking 'suspicious because of the infinities one already encounters in this simple example of a quantum theory of free fields' (*ibid.*, p. 663).

Though certain other minor problems accompany the proof, Duncan and Janssen's reconstruction of the entire calculation, including both the classical and quantum cases, seems primarily motivated by the following analysis (pp. 662–663; emphases original):

> [T]he authors do not distinguish in their notation between (in modern terms) operators and expectation values of operators in energy eigenstates. Here we have to keep in mind that this distinction had not crystallized when the paper was written. The authors had no clear notion yet of operators acting on states. They did not even have the general concept of a state. [...] In the absence of a general state concept, they did not distinguish between pure states and mixed states either. This did trip them up. The formula they derived is for the mean square *quantum uncertainty* in the energy of a subsystem in an energy eigenstate of the system as a whole, which is a *pure state*.

This lack of a concept of 'state' is partially responsible, according to the authors, for Jordan's finishing his proof a step too early with the calculation of the quantum spread instead of finishing with a calculation of the average quantum energy in a canonical ensemble – the quantity required to make a direct comparison to Einstein's 1909 derivation.[1]

Duncan and Janssen are not alone in finding fault with Jordan's proof, as demonstrated by their examination of various criticisms launched against Jordan soon after the publication of the Dreimännerarbeit. For instance, his own coauthors were less than enthusiastic about his derivation. Heisenberg worried about the proof's applicability to a quantum system with infinite degrees of freedom, and Born later wrote a paper with Fuchs calling the proof incomprehensible (Duncan and Janssen, pp. 640–641). Einstein, too, had issues with the matrix mechanical calculation of his radiation formula. Duncan and Janssen believe, based on surviving letters of Jordan, that Einstein was bothered by the use of zero-point energy in Jordan's proof and remained unconvinced that the probabilities of very large fluctuations could be inferred by the same method as for the ground state case (Duncan and Janssen, pp. 648–649).

None of these criticisms would ultimately stand, however. Heisenberg quickly came around to Jordan's way of thinking about the fluctuation calculation, Born had to (in effect) retract his paper with Fuchs, and Einstein's problems with the proof were addressed by Jordan in later papers (e.g., his 1928 paper with Pauli; cf. Duncan and Janssen p. 649).

[1] Duncan and Janssen note (pp. 644, 663) that Lorentz had also omitted this final step in his classical derivation of the fluctuation formula's wave term (cf. Note IX of Lorentz 1916). In our opinion this is not quite accurate (cf. Section 3.6 below), although there is a direct connection between the two methods of calculation, which bolsters our claim that Jordan was following closely the Boltzmannian approach.

The most important criticism motivating Duncan and Janssen's re-derivation, then, is this: Jordan's final ensemble is not a thermal one. How, then, can it be a proof of Einstein's energy fluctuation formula for blackbody radiation? As they note in their Section 3.4, this objection was raised already in 1926 by Adolf Smekal, who remarked that one could not do this derivation without taking into account the radiation-matter interactions giving rise to emission and absorption processes (Smekal 1926). Without these interactions one is left with a collection of normal modes that do not exchange energy, ergo do not exhibit thermal fluctuations. As Duncan and Janssen put it (p. 644, emphasis original):

> To derive a formula for *thermal* fluctuations, one needs to consider a thermal ensemble of states. Both Lorentz and Jordan, however, only considered individual states and failed to make the transition to an ensemble of states. A clear indication of the incompleteness of their derivations is that the temperature does not appear anywhere in them.

To summarise: to Duncan and Janssen the outstanding problem with Jordan's derivation is that his calculation of fluctuations is based on time averaging for a single state. This cannot be a thermal fluctuation, as the spread in values comes from a stationary quantum state without any thermodynamic properties. To resolve this problem, Duncan and Janssen argue that a further step in the proof is needed: averaging over a thermal ensemble of states at a well-defined temperature.

In this paper, we shall construct an alternative interpretation to Duncan and Janssen of what Jordan is doing in the final section of the Dreimännerarbeit, using two arguments. In Section 3 we sketch how Jordan's derivation not only fits nicely into a *Boltzmannian* framework, but furthermore that understanding the derivation in this way rather than in a modern, Gibbsian framework (as is employed by Duncan and Janssen) makes sense of why Jordan omits the ensemble averaging that a Gibbsian derivation would require for completeness. This constitutes the first argument for our interpretation. The second, presented in Section 4, concerns the history of interpreting matrix mechanics: we argue that Born, Heisenberg and Jordan's earliest *interpretation* of their new matrix formalism is then crucial for providing a notion of thermal fluctuations in Jordan's derivation. We provide brief concluding remarks in Section 5. But first, we present a bit of historical context in Section 2 regarding the puzzle of Einstein's energy fluctuation formula.

2 Historical Context

In his first 1909 *Zeitschrift für Physik* paper, Einstein sought a justification for Planck's distribution law on independent theoretical grounds, and in so doing arrived at his famous blackbody formula for the mean square fluctuation of the energy of radiation in a subvolume V and in a small frequency range $[\nu, \nu + \Delta\nu]$,

$$\langle \Delta E^2 \rangle = \frac{c^3}{8\pi\nu^2} \frac{\langle E_\nu \rangle^2}{V \Delta\nu} + h\nu \langle E \rangle \ . \tag{1}$$

He begins his argument (cf. Section 5 of Einstein 1909) by noting that if one wishes to have Planck's radiation formula stand on solid theoretical foundations, two things must be done. One the one hand, adhering primarily to the Jeans law relating resonator energy to radiation pressure allowed Einstein to use the general equation for mean energy squared he had derived a few years before,

$$\langle \Delta E^2 \rangle = kT^2 \, \frac{d\langle E \rangle}{dT} \ , \tag{2}$$

to find an expression for the Rayleigh–Jeans frequency regime:

$$\langle \Delta E_\nu^2 \rangle = \frac{c^3}{8\pi\nu^2} \frac{\langle E_\nu \rangle^2}{V \Delta\nu} \ . \tag{3}$$

This application of the Rayleigh–Jeans distribution law gives $\langle \Delta E^2 \rangle \propto \langle E \rangle^2$, as one would expect for a wave-like mechanism capable of producing interference phenomena. Since Planck had derived this equation using Maxwell's classical theory, its theoretical foundations were perspicuous. On the other hand, Einstein recognised the need to modify statistical mechanics in the blackbody case by forbidding arbitrary energy states for the oscillating mechanism (the so-called 'resonators') responsible for energy exchange between the radiation field and the blackbody. Indeed, only quantised energy states would satisfy the empirically established Wien distribution law. Again applying equation (2), the mean energy squared in this regime then can be written as

$$\langle \Delta E_\nu^2 \rangle = h\nu \langle E_\nu \rangle \ . \tag{4}$$

And as before, what was expected from classical calculations – in this case, of energy fluctuations in systems of non-interacting particles (e.g. for calculating density fluctuations in an ideal gas) – is found for the Wien regime: a contribution $\langle \Delta E^2 \rangle \propto \langle E \rangle$. Applying (2) to the Planck distribution yields directly Einstein's formula (1).

The deeply-felt puzzle of terms appearing side by side in (1) which were nevertheless derivable from contradictory physical pictures was succinctly described by T. Ehrenfest to P. Ehrenfest (as reported at the start of his 1925 paper; pp. 1–2 of English translation):

> Einstein had derived his equation for energy fluctuations of a volume element in the black radiation field from Planck's radiation formula and got this result: the magnitude of these fluctuations is incompatible with the conception of the radiation field as a superposition of light waves. But on the other hand, it is well known that one can derive the Planck radiation formula for blackbody radiation from wave concepts if one quantizes only the Rayleigh–Jeans normal modes of the blackbody governed by Planck's energy ensemble statistics. Thus the following internal contradiction arises – not so different from the question of the 'true' nature of radiation fields: starting from wave concepts one attains, via Planck's equation, light fluctuations that are incompatible with wave concepts. – How do we resolve this contradiction?

A variety of resolutions to this problem were proposed (some are discussed in Duncan and Janssen: cf. p. 638 and references therein), among them attempts to jettison the wave or particle pictures rather than reconcile them, to find problems with Einstein's derivation, or even to adopt a guiding wave interpretation of quantum phenomena.

Lorentz (1916)[2] and Ehrenfest (in the above-quoted 1925 paper) were among those who tried to resolve this interpretational issue, each in their own way. While Lorentz was (to our knowledge) the first to demonstrate rigorously that the wave model leads to the Rayleigh–Jeans fluctuation formula even though the energy distribution was given by Planck's law, one of Ehrenfest's motivations for addressing the paradox was to undermine Einstein's assumption regarding the statistical independence of individual subvolumes – an assumption that leads to equation (2), and had already been suggested as a possible culprit by Lorentz (1916, Lecture 5, Section 44) and more forcefully by Ornstein and Zernicke (1919). We shall investigate Lorentz's and Ehrenfest's approaches in more detail below, as we believe they shed light on important aspects of Jordan's approach.

Early adherents to the quantum hypothesis believed that the only acceptable explanation for the occurrence of both a wave-like and particle-like term in Einstein's formula would be a re-derivation of it carried out within a unified framework. In his Chicago lectures not three years after the publication of the Dreimännerarbeit, Heisenberg put it this way (Heisenberg 1930, p. 96):

> The classical particle theory thus results only in the first part of the formula. The classical wave theory of radiation, on the other hand, leads exactly to the second part [...] Thus, the quantum theory proper is necessary for the derivation of the formula, in which it is naturally immaterial whether one uses the wave or the corpuscular picture.

This comment directly precedes discussion of Jordan's 1926 derivation of the energy fluctuation formula. Not only did Jordan produce the 'right answers' using matrix mechanics alone, but he did so without abandoning Einstein's statistical independence assumption and without putting in by hand the quantisation of emission and absorption processes. But the import of what Jordan had accomplished can only fully be understood if one interprets Jordan as approaching the energy fluctuation calculation from a Boltzmannian perspective. And while we note that at the time of the Dreimännerarbeit there was less of a matter of principle seen between the Boltzmannian and Gibbsian approaches and more of a matter of difference in applicability (notwithstanding the Ehrenfests' 1911 analysis of the connection between these two approaches), the key puzzles raised by Duncan and Janssen are easily understood from within the Boltzmannian tradition. To the demonstration of this we now turn.

[2]This is the edition of a wonderful series of lectures Lorentz had given in 1912 at the Collège de France. They were published in 1916 after Lorentz had added a series of very detailed Notes.

3 Statistical Mechanics and Thermal Fluctuations

As emphasised by Duncan and Janssen, Jordan's derivation of thermal fluctuations seemingly makes no use of thermal properties. Einstein's derivation of the fluctuations is intended to come from a blackbody in thermal equilibrium. How can Jordan's derivation even be comparable to Einstein's? From a modern approach to statistical mechanics, this comparison would seem perplexing. The correct solution would require Jordan's derivation to be averaged over a (thermal) canonical ensemble, as Duncan and Janssen show.

However, in the context of statistical mechanics in 1925, we claim Jordan's calculation can be understood as a thermal calculation in terms of the Boltzmannian approach, given in the Ehrenfests' exposition of 1911. We will outline the salient features of this approach largely following Ehrenfest and Ehrenfest (1911, Sections 12–13), itself based on Boltzmann's combinatorial argument in Boltzmann (1877), updating the notation and language, and for the moment will remove as far as possible any reference to the ideal gas model on which it was based.

This will be followed by the Ehrenfests' application of this approach to the ideal gas model, and an examination of how the differences between how the ensemble averaging approach, advocated by Duncan and Janssen, and the time averaging approach, associated by the Ehrenfests with Boltzmann, treated the problem of fluctuations. We will then reconsider the wave model calculations by Lorentz (1916), Ehrenfest (1925) and Jordan (Born *et al.* 1926), to show how Jordan's calculations can be understood as following a Boltzmannian notion of thermal fluctuations.

3.1 The Boltzmannian Framework of 1911

In 1911, the Ehrenfests produced a celebrated exposition of Boltzmann's approach to statistical mechanics, and contrasted it with Gibbs' approach. Their exposition was based upon Boltzmann (1877), although they placed much greater emphasis upon the role of ergodicity than Boltzmann did in his original papers. It should be noted that both the Ehrenfests and Boltzmann based their arguments upon the ideal gas model of point particles.[3] As a result, their exposition includes features such as phase spaces and identical particle symmetries, which may raise the question of whether the model can be applied to other systems such as a wave model (for example, see Lecture 4, Section 36 of Lorentz 1916 for a discussion of this problem). Our exposition in this section will differ from the Ehrenfests' by picking out the aspects of their Boltzmannian framework that are not specific to the ideal gas model.

A macroscopic dynamical system is taken to have a large number N of constituent objects, with independent degrees of freedom. The individual objects i each have a state space μ_i and the global state space is $\Gamma = \Pi_i \mu_i$. The system follows a trajectory through

[3]The use of point particles in this model must be viewed as an idealisation, as collisions between the particles are required to achieve energy exchanges between particles, allow the system to approach equilibrium and render the assumption of ergodicity at least possible.

Γ. A macrostate space, Z, is constructed by dividing each individual state space up into a large number M of notional cells, labelled by j, each of equal state space volume. The microstates within a given cell ought to have approximately the same energies. The size of the cells should be such that $N \gg M \gg 1$. A macrostate in Z corresponding to a microstate in Γ is given by the occupation numbers, n_j, for the cells: the number of individual objects in the cell j in their individual μ-spaces. It is important to remember that these n_j are not probabilities for a microstate occurring, but are the relative frequencies of individual objects for a given global microstate. Owing to both the finite volume of the cells and any permutation symmetries amongst the objects, individual macrostates in Z will correspond to whole regions of microstates in Γ.

Following this approach, the thermal equilibrium state, for a given total internal energy U of the dynamical system, is identified as the macrostate z^* of Z that occupies the largest Γ-state space volume among all macrostates with (approximately) that energy. Under some reasonable assumptions, z^* will not only be the largest macrostate, but will dominate the Γ-space at that energy, so it is overwhelmingly likely that any microstate picked at random will lie in z^*.

The Boltzmann entropy S_B of a microstate is defined as proportional to the logarithm of the volume in Γ-space of the macrostate with which it is associated: $S_B = -k \ln |z|$.

3.2 The Ideal Gas Model

We will now fill in the details of the ideal gas model, again following Ehrenfest and Ehrenfest (1911). Boltzmann's ideal gas model is envisaged as a box, containing a very large number N of molecules. For convenience, we will ignore any internal degrees of freedom of the molecules, and treat them as point particles, with no interaction with the other molecules except through collisions. The global state of the gas itself is then just given by the $3N$ position co-ordinates and $3N$ momentum co-ordinates of all the molecules. This is the Γ-space. The μ-space, is given by 3 position co-ordinates and 3 momentum co-ordinates. The Z-space comes from dividing the μ-space into cells of size $\delta^3 x \delta^3 p$, and a state in the Z-space consists of just the total number of molecules that lie in each cell. We draw especial attention to the point that the cells are not simply a division of the box into different locations: the cells are dividing the state space into narrow bands of position *and* momentum. An essential part of the setup is that there must be a very large number, M, of cells, but not so many that there are not still a very large number of molecules, on average, per cell:

$$N \gg M \gg 1 \ . \tag{5}$$

For the most likely distribution, Boltzmann employs a combinatorial argument. Let n_j be the number of molecules in cell j. The volume in the Γ-space occupied by a state in

Z-space is just

$$|z| = \frac{N!}{n_1!n_2!\ldots n_C!}(\delta x \delta p)^{3N} \ . \tag{6}$$

With the following constraints on the total number of molecules and total energy,

$$N = \sum_j^C n_j, \quad E = \sum_j^C n_j \epsilon_j \tag{7}$$

(where $\epsilon_j = p_j^2/2m$ is the energy of a molecule in cell j), the largest value of $|z^*|$ is for the Maxwell–Boltzmann distribution $n_j^* = Ae^{\beta\epsilon_j}$, where A and β are determined by N and E.[4] The aggregate behaviour of a gas in such a macrostate can be compared with the ideal gas law, and the identification $\beta = -1/kT$ can easily be made.[5]

This exhibits all the key properties of the Boltzmannian model. Although an isolated system, there exists a most likely, or equilibrium, macrostate, which is regarded as a thermal state. The essential feature to which we call attention is that an individual microstate, within such a thermal macrostate, can have well-defined thermal properties such as entropy and temperature attached to it.

In this model the values of n_j are not static: the molecules are moving between different locations all the time, so the z state will change. This leads to dynamical fluctuations in the value of n_j over time, varying around the average value of n_j^*. Historically, Boltzmann had generally discussed such possibilities only in the context of responses to the reversibility and recurrence objections to statistical mechanics, arguing that the domination of the z^*-state will ensure that they rarely move far from the n_j^* values. By the time of the Ehrenfests' exposition, however, work on fluctuations by Einstein, Smoluchowski and others had started to examine fluctuations statistics in more detail, requiring an analysis of 'the probability of a certain deviation from the most probable state' (Ehrenfest and Ehrenfest 1911, Section 25).

3.3 Averages and Fluctuations

At first sight, the calculation of the fluctuations in energy in each cell, in the particle model, seems straightforward (see, for example, Note III of Lorentz 1916). The energy in the cell

[4] Stirling's approximation is typically used in this calculation. The expression must fail for high values of ϵ_j as the total energy is fixed and finite. The calculation will correspondingly fail when n_j^* is low and Stirling's approximation does not hold.

[5] More generally, the temperature of a microstate, in this framework can be identified using the appropriate generalisation of

$$\frac{1}{T} = \left.\frac{\partial S_B}{\partial U}\right|_V \ ,$$

where S_B and U are the entropy and internal energy of the global microstate, respectively. See Lorentz (1916, Lecture 1, Section 7, and Lecture 4, Section 36). A temperature can generally be defined for a microstate in this way only when it is in the thermal equilibrium macrostate.

is $n_j\epsilon_j$. Given the domination of the z^* thermal state, if we pick a molecule at random, the likelihood it will be found in cell j is just $p_j = n_j^*/N$. Now if we assume the molecules are distributed independently, with these probabilities, then the probability of getting $\epsilon = n\epsilon_j$ energy in cell j, is $P_j(n) = \frac{N!}{(N-n)!n!}p_j^n(1-p_j)^{N-n}$.

The mean value will be $\langle\epsilon\rangle = \sum_{n=0,N} P_j(n)n\epsilon_j = n_j^*\epsilon_j$, as we might have expected. The variance is $\sigma_j^2 = \sum_{n=0,N} P_j(n)(n\epsilon_j)^2 - (n_j^*\epsilon_j)^2 = n_j^*\epsilon_j(1-p_j)$. Now provided $n_j^* \ll N$ we have the result that $\sigma_j^2 \approx \langle\epsilon\rangle$. This is the first term in Einstein's fluctuation calculation. This relationship has been called the 'signature of particles', and it can be seen that it is remarkably insensitive to details of z^*.

The problem is that the probabilities used in this calculation seem to have no connection to the dynamical time averaging described in the previous section. Boltzmann's own work is ambiguous on this topic. Uffink (2014), for example, notes four different technical meanings of probability Boltzmann used throughout his research, without clearly distinguishing them. The probabilities in the fluctuation calculation above are ultimately drawn from phase-space volumes: the probability of a macrostate being occupied is taken to be proportional to its phase-space volume. For this to produce the same results as a dynamical time averaging requires an additional *ergodic* assumption: that the length of time a system spends in a macrostate is proportional to its phase-space volume.

Both Lorentz (Lecture 2, Section 18) and the Ehrenfests (Sections 10–11) highlight two distinct interpretations of probability: an ensemble probability, and a time averaging probability. In the time averaging view, the probability of the system being in a macrostate of Z is just proportional to the length of time it spends in the corresponding region of Γ, over a long time. In this dynamical view of probabilities, fluctuations would refer to the change in the macrostate of an individual system over time. Such a system can be isolated, but must be going through a time-varying dynamical process.

This stands in stark contrast to the ensemble approach. This approach is concerned with a probability distribution ρ over the global state space Γ. The entropy has the form $S_G = \int \rho \ln \rho$, so it is a property of the ensemble rather than an individual microstate. Thermal equilibrium corresponds to choices of stationary ensembles that maximise this entropy. Thermal fluctuations in this approach are calculated not by looking at an individual state's evolution over time, but by calculating the variation in a value over the ensemble. A system that is not changing in time may still show an ensemble variation.

Lorentz attributed the time average view to Einstein, and associated Boltzmann more with an ensemble view, based upon Boltzmann's equating probabilities with volumes in phase space, which is equivalent to a uniform ensemble. The Ehrenfests, however, argued that Boltzmann was best understood in a dynamical time averaging picture, and the use of the ensemble average was justified by appeal to the ergodic hypothesis[6]. Both Lorentz and the Ehrenfests note that if the ergodic hypothesis holds, the time average and the ensemble average yield the same values. In practice, calculating long-term time averages may be

[6]It should be noted that the Ehrenfests repeatedly expressed doubts about this hypothesis.

intractable for complex dynamical systems. Substituting the ensemble average is often more convenient. Lorentz (p. 38), when calculating particle fluctuations, explicitly states that the two approaches are equivalent (presumably making an assumption of ergodicity).

Both the Ehrenfests (Sections 20–24) and Lorentz (Lecture 3) associate the ensemble point of view with Gibbs. For convenience, we will follow the Ehrenfests by referring to a time-varying approach as Boltzmannian, and an ensemble approach as Gibbsian.

Ergodicity only gives the uniform, microcanonical distribution in phase space. Thermal ensembles, such as those employed by Duncan and Janssen, are typically canonical distributions, giving the ensemble probability $p_g \propto e^{-E_g/kT}$ to a microstate g. While this looks very much like the Maxwell–Boltzmann factor $n_j \propto e^{-\beta E_j}$, the similarity can be misleading. The canonical distribution is the probability of a microstate g occurring in an ensemble over the the global state space Γ. The Maxwell–Boltzmann distribution is a set of relative frequencies with which individual molecules have particular properties within an individual equilibrium microstate.

However, the Ehrenfests use the Maxwell–Boltzmann factor to address the role of the canonical distribution in the Boltzmann approach (Section 25), acknowledging that it can 'furnish a more convenient computational scheme' than Boltzmann's. They argue that when a system is treated together with a very large heat bath, and the whole system is assumed to be ergodic, then the calculation of the Maxwell–Boltzmann factor applies to the system of interest. They do not identify this with an ensemble probability p_g, but instead with 'the relative length of time during which the state of [the system] lies in the region' of the microstate space around E_j. An open system in contact with a heat bath exchanges energy with the heat bath, so will move through its own global energy levels over time. Their view of the canonical distribution is still a time average, but now for an open system.

Finally, we note that for many large systems the canonical distribution becomes sharply peaked around the mean energy (cf. also Lorentz 1916, Lecture 3, Section 24).[7] In these cases, the canonical distribution can be well approximated by a microcanonical distribution.

So, we are left with a situation where the microcanonical ensemble in the Gibbs approach is expected to yield the time average values in the Boltzmannian approach for a closed system, and the canonical ensemble in the Gibbs approach is equivalent to the time average values in the Boltzmannian approach for a system in contact with a heat bath, under the assumption of ergodicity. Furthermore, in the right circumstances the canonical and microcanonical ensembles are expected to yield the same values, so all four approaches seem empirically equivalent. For these reasons, many statistical mechanical calculations proceed pragmatically, without explicitly identifying which approach is intended.

[7] For example in the ideal gas model, with a large number of particles, the density of states rises combinatorially with energy while the canonical probability suppresses high energy states exponentially.

3.4 The Wave Model

We now ask, what would a Boltzmannian view of a wave field look like?

We start – following Lorentz (1916), Ehrenfest (1925) and Jordan (Born *et al.* 1926) – with an oscillating field, and use Ehrenfest's simplification of a vibrating string to reduce the number of degrees of freedom (although we shall emphasise that the picture we might intuitively start with from such a description, of a harmoniously vibrating guitar or violin string, can be misleading). The string is uniform, of length L, satisfying the equation

$$\frac{\partial^2 u}{\partial t^2} = c^2 \frac{\partial^2 u}{\partial x^2} \ , \tag{8}$$

where $u(x,t)$ is the displacement of the string at time t and location x, with boundary conditions $u = 0$ at $x = 0, L$. The standard Fourier treatment of this is as an infinite sum over normal modes

$$u_n(x,t) = A_n sin(\omega_n t + \phi_n) cos(\omega_n x/c) \ , \tag{9}$$

with $\omega_n = n\pi c/L$.

The energy in the system is given by

$$H = \int_0^L \frac{1}{2} \left[\left(\frac{\partial u}{\partial t} \right)^2 + c^2 \left(\frac{\partial u}{\partial x} \right)^2 \right] dx \ , \tag{10}$$

and following well-known calculations, each mode decouples, contributing an energy $E_n = \frac{1}{2} A_n^2 \omega_n^2$.

The total energy over the whole string is, of course, constant, as is the energy in a given oscillatory mode over the whole string. However, the amount of energy within a small frequency range, in a small region, is not constant due to interference beats. If we simply take two modes with very close frequencies $\omega' \approx \omega$ and look at a single position, the energy in the oscillations will vary between $(A + A')^2\omega^2$ and $(A - A')^2\omega^2$, around its mean value of $\frac{1}{2}(A^2 + A'^2)\omega^2$. Jordan described the situation in the following terms (Jordan 1927a, p. 642, our translation):

> Let us imagine that a small volume V is separated out of a large cavity filled with blackbody radiation, in such a way that only radiation in a narrow frequency range ν, $\nu + d\nu$ can proceed unhindered through the large and small volume. Between the light waves in this narrow frequency range entering the small volume from the large one there will now result slow beats; at the rate of these beats the subvolume V breathes slowly in and out radiation energy in the frequency range $d\nu$.

To study these interference fluctuations, we divide the string up into a large number of small segments of length $\delta l \ll L$, with a given segment centred on x_j. We focus on

a band $\delta\omega$ of high frequency oscillations, centred on a frequency ω_j whose wavelength is much smaller than the size of the segment: $\omega_j \gg c/\delta l$. The spread in frequencies is narrow compared to their value, so $\delta\omega \ll \omega_j$. The picture we are interested in, therefore, is not the harmonious vibrations of a guitar or violin string: it is more like looking at the ripples passing through a small patch on the surface of a lake. Localised wavepackets travel along the string, moving in and out of the different locations, constructively and destructively interfering with other wavepackets as they pass.

To follow the Boltzmannian approach, we would identify a macrostate with specific quantities of energy in the individual cells. The thermal states are found by fixing the total energy of the system, and looking for the distributions of A_n that maximise the size of the macrostate. Once that is achieved, the entropy of the macrostate is identified with the logarithm of the volume of that macrostate, and the temperature is given by $\frac{1}{T} = \frac{\partial S_B}{\partial U}\Big|_V$.

This macrostate represents thermal equilibrium, but there would be no reason to suppose that this would correspond to a stationary microstate. On the contrary, the very fact that the macrostate is required to be large indicates that it would correspond to a great many microstates, each corresponding to an instantaneous configuration of the string. Only an exceptionally few microstates, with carefully correlated phases, could be expected to go through simple cycles. For most configurations, corresponding to uncorrelated phases for the normal modes, there might be expected to be complex patterns of travelling wavepackets. As the system cycles through these dynamically changing microstates, the instantaneous value of E_j in the subvolume will fluctuate around the mean value $\langle E_j \rangle$, in a more complex version of the two-frequency beat.

We will here omit most of the details, which can be found in Lorentz (1916), Ehrenfest (1925) and Born *et al.* (1926, pp. 375–385). Our presentation will most closely follow that of Ehrenfest. The energy in cell j is given by:

$$E_j = c^2 \int_{x_j-\delta l/2}^{x_j+\delta l/2} dx \sum_{\omega_m=\omega_j-\delta\omega/2}^{\omega_j+\delta\omega/2} \sum_{\omega_n=\omega_j-\delta\omega/2}^{\omega_j+\delta\omega/2} A_m A_n \omega_m \omega_n \Big(C(\omega_m,\phi_m,x,t)C(\omega_n,\phi_n,x,t) + \\ + S(\omega_m,\phi_m,x,t)S(\omega_n,\phi_n,x,t) \Big) \ , \quad (11)$$

where $C(\omega,\phi,x,t) = cos(\omega t+\phi)sin(\omega x/c)$ and $S(\omega,\phi,x,t) = sin(\omega t+\phi)cos(\omega x/c)$. The spatial term is dealt with using an approximation based on the width of the band of frequencies $\delta\omega$ being much smaller than the central frequency ω_j, simplifying the expression to

$$E_j = c^2 \sum_{\omega_m=\omega_j-\delta\omega/2}^{\omega_j+\delta\omega/2} \sum_{\omega_n=\omega_j-\delta\omega/2}^{\omega_j+\delta\omega/2} \Omega_{m,n} A_m A_n \omega_m \omega_n cos\Big((\omega_m-\omega_n)t + \phi_m - \phi_n \Big) \ , \quad (12)$$

where $\Omega_{m,n}$ is what remains of the spatial dependence (and has the property that $\Omega_{n,n} = \delta l/2$).

Averaging over time, the mean energy in the cell j becomes just

$$\langle E_j \rangle = \frac{\delta l}{2L} \sum_{\omega_n - \delta\omega/2}^{\omega_n + \delta\omega/2} A_n^2 \omega_n^2 \ . \tag{13}$$

The fluctuation about this mean,

$$\sigma_j^2 = \langle (E_j - \langle E_j \rangle)^2 \rangle = \langle E_j^2 \rangle - \langle E_j \rangle^2 \ , \tag{14}$$

needs the quadruple sum:

$$E_j^2 = c^4 \sum_{\omega_k = \omega_j - \delta\omega/2}^{\omega_j + \delta\omega/2} \sum_{\omega_l = \omega_j - \delta\omega/2}^{\omega_j + \delta\omega/2} \sum_{\omega_m = \omega_j - \delta\omega/2}^{\omega_j + \delta\omega/2} \sum_{\omega_n = \omega_j - \delta\omega/2}^{\omega_j + \delta\omega/2} \Omega_{k,l}\Omega_{m,n} A_k A_l A_m A_n \omega_k \omega_l \omega_m \omega_n$$
$$cos\Big((\omega_k - \omega_l)t + \phi_k - \phi_l\Big) cos\Big((\omega_m - \omega_n)t + \phi_m - \phi_n\Big) \ . \tag{15}$$

This, then, would be the Boltzmannian view of energy fluctuations in a closed-system radiation field.

3.5 Analogies and Disanalogies between the Gas and Wave Model

Before examining the fluctuation calculation in more detail, we will review some of the similarities and differences with the particle model. It is not our intention to defend this picture of the wave model as having been the correct Boltzmannian account of thermal fluctuations in a classical wave field. No such account was ever systematically carried out, for good reasons. Our intention is to lay out the principal features of what such an approach would look like, and suggest that Jordan's calculation fitted this approach.

The microstate of all the particles is replaced by the configuration of the wave field. The cell in position and momentum space is replaced by a cell in position and frequency space. In the gas model, the energy in the cell is proportional to the number of particles in the cell n_j. In the wave model, the energy in the cell is proportional to the amplitude-squared of the oscillations A_j^2. Energy changes occur dynamically in the particle model by particles moving through the cells, and also by collisions between particles. In the wave model, the energy changes occur through complex interference patterns, producing shifting wavepackets moving between the cells, like ripples on the surface of a lake. In the Boltzmannian approach, averages are taken to be time averages rather than ensemble averages, and fluctuations around the mean value are understood as dynamical variations over time.

We turn now to the disanalogies between the two cases. In the wave model, there is a question over the independence of different cells. For low frequency, large wavelength

oscillations where the model resembles the harmonious violin string, the different cells are obviously not independent. Furthermore, oscillations at the same frequency at different locations will also be strongly correlated. Viewed at the level of a single normal mode, there appear dependencies across the cells. However, for the high frequency, short wavelength oscillations it is worth recalling that the model is closer to the ripples on the surface of a lake than a musical string. Each cell in the wave model covers a frequency band, which may include a large number of normal modes, each with independent phases and amplitudes. A general pattern of deformations and ripples will be produced by the multiple independent amplitudes and phases of the different normal modes. Provided the frequency band is large enough,[8] with $\delta\omega \gg \pi c/L$, this would make it possible to fix the instantaneous state of each cell as a more or less independent variable.

Of course, there remains the continuity condition for the wave field at the boundary between physically neighbouring cells in the wave model. But this does not enter into the energy calculation itself, and the continuity simply means that the travelling wavepackets move from one cell to another, in the same manner as particles move between their physical locations.

A noticeable difference between the two models is that there is no equivalent to the conservation of particle number in (7). The analogy can be partially restored by noting that in the ideal case of non-interacting particles, the total number of particles with a given momentum is constant. This then is analogous to the amplitude of the global wave mode at a given frequency, which is also constant. The transfer of energy between locations is due to the changing distribution of the number of particles due to their movement, or the shifting amplitudes of the local oscillations due to travelling wavepackets.

This raises the problem as to how energy can be exchanged between modes, without which ergodicity cannot hold. The analogy to the particle case would suggest two ways this could happen. The first possibility from the particle picture is exchanging energy between particles through collisions. Unfortunately, the only way this could be included in the wave model would be to introduce non-linear coupling between the normal modes. This is a possible consideration for sound waves in a crystal lattice, where the linear wave equations might be regarded as an idealisation, but in the case of the electromagnetic field no such option is available. The second possibility is, of course, contact with a heat bath: exchanges of energy allow the different normal modes to change amplitude and phase. This certainly presents a significant disanalogy, and one that also forms part of Smekal's objection: without the ability to exchange energy between normal modes, a wave field that was far from the equilibrium macrostate would not be able to reach it.

However, the most significant disanalogy to the particle case is failing to identify the thermal macrostate. The reason for this is well known: the statistical mechanical treatment

[8] Although this condition does not seem to be used in any of the wave model calculations, it is not inconsistent with them. It would be the natural analogy to the requirement in the particle model that each cell should contain a large number of particles, which is also not used in the fluctuation calculation and is only required for the calculation of the thermal macrostate.

of the classical wave model contains a fatal flaw. In the particle case, the energy in a cell is by definition $n_j\epsilon_j$, where n_j must be an integer. As the total energy is fixed and finite, cells with very high values of ϵ_j cannot have a high occupation number, and cells with ϵ_j above the total energy cannot be occupied at all. Although the energy spectrum has no upper bound, so that in principle there are an infinite number of high energy cells available, the finite value of the total energy imposes a cut-off above some value of j. In the wave case there is no such restriction, as A_j^2 is real, not an integer. No matter how large ω_j becomes, there is always a sufficiently low value of $A_j^2 > 0$ that is possible for any finite total energy. But there is also an infinite number of high frequency modes to share out this energy, so the most likely distribution will end up distributing infinitesimally small portions of energy to each mode, throughout the full spectrum. Applied to the wave field as a whole, this leads to the Rayleigh–Jeans distribution, $\rho(\omega) = \frac{8\pi}{c^3}\omega^2 kT$, which is unbounded for high ω and cannot be normalised.

Historically, the Planck distribution had already been proposed before this problem was identified. Restricting the energy of the normal modes to only occur in integer multiples solves the problem in an analogous way to the particle case: too high frequency modes cannot have even a single quantum of energy. The Planck distribution law follows. But now the paradox shown by Lorentz and Ehrenfest arises: their calculations are independent of the energy spectrum; thus, even using such a quantisation scheme, the fluctuation spectrum of a subvolume would still appear to follow the one derivable by thermodynamical considerations from the Rayleigh–Jeans distribution (equations (2)–(3) above), not the one derived by Einstein from the Planck distribution.

3.6 Fluctuations in the Wave Model

We have suggested that energy fluctuations in a classical wave field could be understood as thermal fluctuations, without needing a thermal ensemble, provided one is working in the Boltzmannian framework for closed systems put forward by the Ehrenfests. We have also noted that in the ideal gas model, the dynamical assumption of ergodicity leads to an equivalence between ensemble and time averaging, which is generalisable to include open systems, so even in the Boltzmannian framework the more tractable calculations based upon ensemble averaging can be used.

However, in the wave model, the dynamical behaviour is more straightforward than in the particle model, and the time averages can be calculated directly from the expressions in equations (13) and (15). The ergodic assumption is simply not needed in this case. A closed-system time average would hold the amplitudes and phases of the modes fixed, and average over time. An open-system or an ensemble average would allow the phases and amplitudes to vary as well. So, what approach did Lorentz, Ehrenfest and Jordan take for their fluctuation calculations in the wave model?

Lorentz gives two calculations. The first (Lorentz 1916, pp. 71–72) is a simple sketch of a two mode interference beat, intended to simply demonstrate that the scale of the

fluctuations is proportional to the mean energy squared. Although at first he uses language suggestive of temporal fluctuations ('The energy is [...] sometimes $(a_1 + a_2)^2$, sometimes $(a_1 - a_2)^2$, sometimes any value between these two'[9]), he then refers to a variation in the amplitudes ('if the amplitudes a_1 and a_2 should both vary in the ratio of 1 to s') which could only result from either an open-system approach or an ensemble approach. In his more detailed calculation (Note IX, in particular Section 3), he explicitly refers to ensemble averaging ('The average values used in the following are the ones referring to the ensemble of systems Σ, [...] we assume [...] that the individual systems differ from each other in the values of the quantities b_1, b_2, q_1, q_2 [the amplitudes and the phase angles]'). He then averages the trigonometric functions in (his version of) equation (15) using a phase-angle average, rather than a time average,[10] and states that the resulting expression 'has the same value for all systems Σ, so that from now on we may limit ourselves to considering one and the same system'. That is, since the resulting expression is in fact independent of variations in the amplitudes across the ensemble (note that the amplitudes of each individual mode would automatically be constant for a closed-system time average), there is no need to average explicitly also over the amplitudes, but in fact Lorentz has calculated an ensemble average, not a time average. It is also notable that, while several times he calls attention to the equivalence of time-averaging and ensemble-averaging in the discussion of particle fluctuations, he makes no such assertion for the wave fluctuations.

Ehrenfest gives calculations for two different cases and is explicit about how to understand each. His Case III describes the 'timelike fluctuations [...] in the course of a day i.e. when the blackbody remains isolated' (p. 5), while his Case II are the fluctuations associated with subjecting the blackbody as a whole to a 'Planck lottery' each day, changing the total energy according to the Planck distribution.[11] Ehrenfest provides a different notation for the two kinds of averaging: round brackets for time averaging over the day, and square brackets for averaging over the Planck lottery. It would be tempting, but incorrect, to associate the Planck lottery with an ensemble average at this point. Ehrenfest is quite clear that the Planck lottery is also a temporal average, just over many days, with the system allowed to interact with a heat bath (p. 4). The two different averages are therefore both within the Boltzmannian framework described by the Ehrenfests: Case III is the closed-system average, and Case II is the open-system time average of their Section 25.

Ehrenfest's picture is clouded by the fact that for the Case III calculation he resorts to a phase-angle average over the course of many days, in order to get to the final fluctuation formula. This might seem odd for a calculation that is otherwise clearly intended to be fluctuations over the course of a day. At first sight it would also seem unnecessary, and

[9] The French adverb is 'tantôt'. All translations from Lorentz (1916) are ours.

[10] Although he does not explicitly state he is doing a phase-angle average, Lorentz refers to the need for the trigonometric functions to involve only linear combinations of the modes' phase-angles. For a time average this requirement is irrelevant, while for a phase-angle average it is essential.

[11] Ehrenfest's Case I is the uncontroversial fluctuation formula in the energy of the blackbody as a whole, while his Case IV is Einstein's fluctuation formula for the subvolume. Neither is explicitly calculated.

that a closed-system time average would do just as well. It is instructive to consider why it would not. When averaging terms from equation (15) with expressions like

$$cos((\omega_i - \omega_j)t + \phi_i - \phi_j)cos((\omega_k - \omega_l)t + \phi_k - \phi_l)\,, \tag{16}$$

phase-angle averages and time averages are *not* equivalent. Although simple symmetries where, for example $i = k, j = l$ are easily accounted for, and leave equal terms in the two approaches, degeneracies where $|\omega_i - \omega_j| = |\omega_k - \omega_l|$ leave non-zero terms in the time average (proportional to $cos(\phi_i - \phi_j - \phi_k + \phi_l)$) that would vanish in the phase-angle average. While neither Lorentz nor Ehrenfest explicitly refer to this problem, both are using phase-angle averaging at the point where it becomes an issue.

What of Jordan's calculation? As the quote from (Jordan 1927a) in Section 3.4 shows, Jordan certainly describes the fluctuations as a temporal variation. Duncan and Janssen, in their reconstruction of Jordan's calculation, note that he uses a time averaging throughout. This forms the basis of their criticism: they argue that he should have been using an ensemble averaging instead. Indeed, they perceptively notice the existence of the difference between phase-angle and time averaging. They call attention to it (footnote 59 on p. 653), because Jordan uses a time average and does not appear to have noticed the problem of the degeneracies. (Duncan and Janssen also note that Jordan uses a time average at all other points of the calculation, where the phase-angle average is equivalent (*ibid.*, footnote 58 on p. 652)). Did Jordan make a fatal error here? Duncan and Janssen think not, as they believe that the degeneracies (which are expressed in a slightly different but equivalent form in Jordan's calculation) can be avoided in more realistic models. It is also possible that the terms could be neglected: if the phase-angles are assumed to be randomly distributed and uncorrelated between the different modes, then the resulting sums of the form

$$\sum_{i,j,k,l} A_i A_j A_k A_l cos(\phi_i - \phi_j - \phi_k + \phi_l) \tag{17}$$

might be expected to be quite small provided there are a large number of modes within the frequency band. So it may be argued that for a typical configuration, the additional terms should be negligible. To our knowledge, though, no such argument was presented at the time.

We fully agree with the diagnosis provided by Duncan and Janssen. For us, however, Jordan's technical error in missing the effect of the degeneracy on the time average is less important than what it reveals about the calculation Jordan was making. At the point where even Ehrenfest resorts to a phase-angle average, Jordan tellingly persists with a closed-system time average, characteristic of the Boltzmannian approach.

The upshot of all these calculations is to leave us with the wave fluctuation formula

$$\sigma_j = \frac{< E_j >^2}{2\delta l \delta\omega}\,, \tag{18}$$

containing only the second of the terms in Einstein's calculation (for the model of the vibrating string).[12] Both Lorentz and Ehrenfest note the paradox that they achieve this fluctuation formula even in situations where the energy spectrum should be the Planck blackbody distribution, and therefore should produce the Einstein fluctuation formula.

Here is the point at which Jordan's matrix mechanics calculation enters. Formally, Jordan's proof again follows the structure of his classical wave calculations, in a Boltzmannian closed-system framework, but using matrix mechanics produces the Planck formula for fluctuations. Jordan seems to provide a route to get both terms in Einstein's fluctuation law, maintaining statistical independence of cells, without introducing a separate hypothesis about quantisation of energy exchanges.

But a new problem now rears its head: whether the closed-system Boltzmannian approach can be appropriate for the quantum case. The normal mode of a vibrating string has a fixed total energy, but is still a dynamic object, indicating the string is moving in time. In the classical wave model, the superposition of a great many high frequency oscillations allows complex interference patterns and travelling wavepackets, all the while maintaining the fixed energy of the normal modes. But a quantum state with fixed energy does not seem to have this quality: it is stationary. Without a dynamical change giving a time-varying picture within each cell, there seems no basis for the time averaging required by the closed-system Boltzmannian view of thermal fluctuations.

4 Fluctuations and the 'Statistical Interpretation'

The familiar fact that a stationary quantum state is time-independent (apart from an irrelevant global phase factor), and that, indeed, so are the reduced states of any of its subsystems, seems thus to make the Boltzmannian strategy that we have sketched in the previous section inapplicable to the quantum case. At least from a modern point of view, there simply are no temporal fluctuations of the quantum state of the subsystem. In order to calculate a thermal fluctuation, it appears inescapable that one should look at an *ensemble* of systems in thermal equilibrium with a heat bath, and Duncan and Janssen accordingly go on to show that one can adapt Jordan's calculation to include Gibbsian ensembles and indeed derive the fluctuation formula in this way.

As already mentioned above, Duncan and Janssen also point out that the notion of a quantum state is not even present in the Dreimännerarbeit. Indeed, the earliest one can talk of a 'quantum state' entering the work of the Göttingen physicists in a more or less recognisable form is only with Born's papers on collision phenomena (Born 1926a,b).[13]

[12] It might seem strange that this fluctuation law can be derived without needing to calculate the thermal equilibrium macrostate. However, it is worth noting that the same is true of the particle fluctuation calculation in Section 3.3 above.

[13] Arguably, not even in Born's work do we have the 'quantum state' as understood today. As we shall briefly sketch below, the 'statistical interpretation' went through various further developments before it became recognisable as the 'standard' or 'orthodox' interpretation in the hands of von Neumann.

On the other hand, it is precisely assuming the modern notion of quantum state that the above reasoning appears to prevent Jordan from invoking a Boltzmannian strategy. In its absence, we are left with the choice between seeing Jordan's calculation as a purely formal exercise, or else arguing that Jordan had a different way of interpreting the formalism.

It is the purpose of this section to argue precisely that Jordan (or indeed Born, Heisenberg and Jordan) had such an interpretation in mind, and in fact one that makes sense of the claim of having derived thermal fluctuations in a Boltzmannian framework, i.e. temporal fluctuations arising through the dynamics of the system. As already noted, this requires a notion of a 'local state' of a cell undergoing changes in time, even though the global quantum state is stationary. In order to find this, we shall need to delve into the development of the 'statistical interpretation', both in Born's own work and in related work especially by Heisenberg and by Jordan himself.[14]

4.1 Matrix Mechanics and its Limitations

Let us briefly recall the main tenets of matrix mechanics. The theory is designed to describe systems that, like in Bohr's model of the atom, are in one of several possible *stationary states* with energies E_i, and in general perform quantum jumps between these states, absorbing or emitting radiation with frequencies ν_{ij} related to the energies through Ritz's combination principle:

$$E_i - E_j = h\nu_{ij} \ . \tag{19}$$

The basic mathematical objects one considers in the theory are certain two-dimensional arrays of the form

$$q = \begin{pmatrix} q_{11}e^{2\pi i\nu_{11}t} & q_{12}e^{2\pi i\nu_{12}t} & \dots \\ q_{21}e^{2\pi i\nu_{21}t} & q_{22}e^{2\pi i\nu_{22}t} & \dots \\ \vdots & \vdots & \ddots \end{pmatrix} \ , \tag{20}$$

with $q_{ij} = q^*_{ji}$ and thus Hermitian (because of (19)), which are meant to generalise the classical Fourier series of the form

$$q_n e^{2\pi in\nu t} \ . \tag{21}$$

Specifically, the position matrix q and the momentum matrix p have to be solutions of the (classical) equations of motion, under the additional 'quantum condition' $pq - qp = h/2\pi i$. Solving the equations yields in particular values for the energies of the stationary states and the spectral frequencies.

Note that while the indices i label stationary states, there is no mathematical description of the stationary states themselves. The ith row or column of a matrix, for instance, can be associated with the ith stationary state, but the objects describing physical systems are the entire two-dimensional matrices, so that the stationary states enter *collectively*

[14] For more detailed discussions of the development of the statistical interpretation, see Bacciagaluppi and Valentini (2009, Chapter 3) and Bacciagaluppi (2008).

into the description of physical systems. Thus, while through an appropriate extension of correspondence arguments one can relate diagonal elements to 'time averages' in the corresponding stationary state,[15] and identify squared amplitudes $|q_{ij}|^2$ with transition probabilities,[16] the theory does not strictly speaking have the resources to describe a system as *being* in a given stationary state, let alone as *performing* a quantum jump between two given states. As Born and Heisenberg stated in 1927 at the fifth Solvay conference (Born and Heisenberg 1927, as quoted in Bacciagaluppi and Valentini 2009, p. 383):

> The most noticeable defect of the original matrix mechanics consists in the fact that at first it appears to give information not about actual phenomena, but rather only about possible states and processes. [...] it says nothing about when a given state is present, or when a change is to be expected.

Accordingly, Born made two rather different attempts in 1926 to overcome these perceived limitations of matrix mechanics.

4.2 The Development of the 'Statistical Interpretation'

Born's first attempt, together with Norbert Wiener, was the development of an 'operator calculus' generalising the notion of a matrix in such a way as to enable the treatment of aperiodic processes (Born and Wiener 1926a,b). The main example treated by Born and Wiener using this formalism is in fact the free particle. Note that while Born and Wiener's operators act on a space of functions, these are not yet identified with 'states' of the system. However, Born and Wiener show that the real part of the 'column sum' (the generalisation of the sum of the column elements of a matrix) of the position operator for the free particle has the form $t\sqrt{2E/m}$, where E and m are the energy and mass of the particle, and thus represents a state of 'inertial motion' (!).

A few months later, Born adopted instead Schrödinger's formalism of wave mechanics to treat the collision of a particle and an atom (Born 1926a,b), both assumed to be initially in stationary states (i.e. asymptotically for $t \to -\infty$). By solving perturbatively the time-independent Schrödinger equation, Born managed to derive the final asymptotic state, which in general is an (entangled) superposition of various stationary states of the atom and outgoing plane waves. The physical picture Born had in mind was still the same as in matrix mechanics: one of systems always in stationary states. This led him to interpret the final wave function as describing a *statistical ensemble* of systems in different stationary states, and to interpret the (squared) amplitudes as the probabilities of occurrence of the various states. Before and after the collision, both the particle and the atom are in a stationary state, but during the interaction the only thing that can be said is that they

[15] See also Jordan's remarks on measuring time averages in the next subsection.

[16] For an in-depth discussion of transition intensities in matrix mechanics, in fact on how the problem of intensities turns out to have been a driving concern in the development of the theory, see the beautiful paper by Blum *et al.* (this issue).

perform a quantum jump, and there may be no anschaulich description of either system. Since the initial state is a product of given stationary states, the 'state probabilities' defined by Born in terms of the coefficients of the final state are automatically also transition probabilities for the quantum jumps from the given initial states to the various possible final states.[17] The most complete presentation of the 'statistical interpretation' to which Born was led by this work was presented in his subsequent paper on the adiabatic principle (Born 1926c).

Schrödinger and Heisenberg were both furious with Born, for obvious but opposite reasons: the former because Born had taken the formalism of wave mechanics but imposed on it Heisenberg's interpretation, the latter because Born had managed to use Schrödinger's formalism to calculate transition probabilities from first principles, something that matrix mechanics had not been able to do.[18] Pauli, however, noticed that Born's calculation could be translated into the language of matrix mechanics, and suggested to Heisenberg that he develop that idea. Pauli had realised that one can indeed treat true change also in matrix mechanics, but, as Born and Heisenberg later put it in their Solvay report (as quoted in Bacciagaluppi and Valentini 2009, p. 383):

> [i]n order to have true processes, as long as one remains in the domain of matrix mechanics, one must direct one's attention to a *part* of the system; this is no longer closed and enters into interaction with the rest of the system.

This idea was in fact developed independently by both Heisenberg and Jordan, in two very similar papers (Heisenberg 1927, Jordan 1927b).[19] Heisenberg and Jordan consider two weakly coupled atoms with (at least) one energy difference in common. Because of the coupling, the energy matrices of the two atoms are no longer diagonal in the energy basis of the composite system, and thus they are time-dependent (and show a slow 'exchange of energy'). The main aim of the two papers is to demonstrate how this result is in fact equivalent to the picture of (correlated) quantum jumps in the two atoms. Indeed, just as Born had calculated the transition probabilities for the quantum jumps in terms of the coefficients of the final wave function, Heisenberg and Jordan now calculate them in terms of the elements of the transformation matrix between the two energy bases, thus providing a rigorous calculation of the transition probabilities by purely matrix mechanical means.

[17] The state of the incoming particle is formally a plane wave, but Born still refers to it as 'uniform-rectilinear motion' (and he always uses 'state' for a stationary state and never for the wave function).

[18] As Born put it in the paper on the adiabatic theorem: '[The fundamental ideas of the matrix form of the theory initiated by Heisenberg] have grown directly out of the natural description of atomic processes in terms of "quantum jumps" and emphasise the classically-geometrically incomprehensible nature of these phenomena. It is settled that both forms of the theory arrive at the same results for stationary states; the question is only how one should treat non-stationary processes. Here, Schrödinger's formalism turns out to be substantially more convenient, provided one interprets it in the sense of Heisenberg. I therefore wish to support a merging of both points of view, in which each fulfils a very particular role' (Born 1926c, p. 168, our translation).

[19] Heisenberg (1927) is cited by Duncan and Janssen, but only in connection with the question of whether the fluctuations calculation indeed applied to the radiation field or only to a crystal lattice.

One of Jordan's remarks in his paper is very illuminating specifically with regard to Duncan and Janssen's complaint that he is calculating only a quantum spread. Indeed, Jordan considers three methods one could use to measure the transition probabilities between the relevant states (0 and 1) of one of the atoms (say A_1). His methods A and C both involve measurements distinguishing between the states 0 and 1 of A_1 after the interaction.[20] His method B, however, involves measuring 'in a direct way the average, average square etc. of quantities that for the isolated A_1 are constant over time' (Jordan 1927b, p. 664, our translations here and below). He later gives an explicit example of method B, taking the energy W_1 of A_1. He claims indeed that the average value of the energy can be measured directly ('*in principle* without inducing quantum jumps, say through weighing', p. 666, footnote 2, original emphasis), yielding the result $W_1(1) \cdot |T(01,10)|^2 + W_1(0) \cdot |T(01,01)|^2$, or (p. 666, original emphasis):

> in the words of the discontinuous description: energy of state 1 times probability of this state plus energy of 0 times the corresponding probability. Similarly for the *quadratic fluctuations* of the energy, one obtains from the matrix formulas the same results as from the elementary picture of the discontinuous description.

Thus for Jordan, the quantum spread of the energy has indeed a direct interpretation in terms of dynamical transitions between the stationary states of the subsystem.

The further development of the 'statistical interpretation' is of no direct relevance to us now, but can be very briefly summarised as follows: Dirac and Jordan independently developed the 'transformation theory', which generalised to arbitrary bases the technique developed by Heisenberg and Jordan in the two papers just mentioned. In Born and Heisenberg's Solvay report, this generalisation is then combined with an explicit discussion of interference (arguably still deficient in Born's adiabatic paper) to yield an interpretation of quantum mechanics in which systems perform transitions between *measured* values of *arbitrary* quantities. As late as Born and Heisenberg's report (October 1927), the wave function is still not seen as the 'state' of the system, and is possibly merely a mathematical tool for calculating probabilities (which possibly also explains why they seem to consider choosing a new wave function after a measurement as unremarkable). The modern notion of a quantum state as characterising an ensemble of identically prepared systyems in terms of the associated expectation values for quantum mechanical observables, as well as the modern form of the collapse postulate, are then introduced by von Neumann in a paper that Born presented two weeks later at the 11 November session of the Göttingen Academy of Sciences (von Neumann 1927).[21]

[20] Assuming a given state (say 0) before the interaction, measuring whether A_1 is in the state 0 or 1 after the interaction of course tells us the probabilities of the transitions from 0 to 0 and from 0 to 1.

[21] For details, see again Bacciagaluppi and Valentini (2009, Chap. 3) and Bacciagaluppi (2008). Von Neumann's contribution will be analysed in a separate paper.

4.3 The Picture of Temporal Fluctuations

What is directly relevant and crucial to our discussion is that, at least by the end of 1926,[22] Born, Jordan and Heisenberg are all thinking of quantum systems in interaction as *exchanging quanta of energy as they perform quantum jumps between different stationary states*. And this is true even when the composite of the interacting systems is *itself in a stationary state*. This is precisely the picture that is needed in order to make sense of Jordan's calculation as referring not to what we now call the quantum spread in the energy of a segment of the string, but to true *temporal fluctuations* in the energy of the segment.

Can we assume that this is the picture that they and in particular Jordan had in mind also one year earlier when they submitted for publication the Dreimännerarbeit? Jordan does not make any explicit connections between his treatment of quantum jumps in Jordan (1927b) and his earlier derivation of Einstein's fluctuation formula. Heisenberg, however, as we shall see, does make a direct connection between his 1927 paper and the fluctuation calculation in the Dreimännerarbeit.[23]

The hesitation one might have in drawing a definite conclusion comes from the apparent disagreements between the three authors as to Jordan's fluctuations calculation, well described by Duncan and Janssen, so that e.g. it may appear illegitimate to cite Heisenberg in support of our interpretation of Jordan. However, the doubts about the validity of Jordan's calculation appear to have centred on whether the calculation was indeed applicable to the radiation field, as opposed to e.g. a crystal lattice (Heisenberg to Born, Jordan and Smekal, 29 October 1926, as quoted by Duncan and Janssen, p. 644):

> Our treatment of fluctuation phenomena is undoubtedly applicable to the crystal lattice [...] The question whether this computation of fluctuations can also be applied to a radiation cavity can, as Mr Smekal emphasises, not be decided at the moment, as a quantum mechanics of electrodynamical processes has not been found yet. Because of the formal analogy between the two problems (crystal lattice–cavity) I am personally inclined to believe in this applicability, but for now this is just a matter of taste.

However, we must distinguish between doubts as to the propriety of quantising the electromagnetic field and the applicability of Jordan's procedure to both radiation and the crystal or string, and doubts about the way one should understand it conceptually. On this last point there need not have been any disagreement, and also Heisenberg appears to understand the physical picture behind the calculation as one in which the subvolume is performing quantum jumps. Indeed, as he emphasises in the same letter, this picture

[22] Born's paper on the adiabatic theorem was submitted in October 1926, Heisenberg and Jordan's papers on quantum jumps both in November 1926.

[23] And Jordan connects explicitly their two papers of 1927: 'A detailed explication of "method B" can be found in a paper by W. Heisenberg, whose manuscript I got to see after completion of this note' (Jordan 1927b, p. 666, footnote 3).

(that of quantised energy exchanges) is the one that leads automatically to the fluctuation formula via thermodynamical considerations (i.e. via eq. (2)). The merit of the novel calculation is to derive it *directly*,[24] thus supporting the idea that matrix mechanics is the correct theory for describing quantum jumps (*ibid.*, as also quoted in Duncan and Janssen, p. 646):

> For the crystal lattice the quantum-mechanical treatment undoubtedly means essential progress. This progress is not that one has found the mean square fluctuation; that one already had earlier and is obvious on the basis of general thermodynamical considerations if one introduces quantum jumps. The progress, in fact, is that quantum mechanics allows for the calculation of these fluctuations without explicit consideration of quantum jumps, on the basis of relations between q, q', etc. This amounts to a strong argument for the claim that quantum-mechanical matrices are the appropriate means for representing discontinuities such as quantum jumps (something that does not become equally clear in the wave-mechanical way of writing things). The calculation of our Dreimännerarbeit thus provided an element of support for the correctness of quantum mechanics.

With that in mind, then, here is the smoking gun where Heisenberg explicitly states that what he is doing in late 1926 is an extension of the fluctuations calculation (reinterpreted in the sense of a crystal lattice[25]) from the Dreimännerarbeit (Heisenberg 1927, p. 506, our translation):

> As an example one may finally state how the calculations about fluctuations in a crystal lattice given by Born, Jordan and the author (*loc. cit.*) are included in the present considerations. They are energy fluctuations in a small subvolume of the crystal. Let the subvolume be separated out of the crystal in the unperturbed system. Then in the perturbed system one has resonance with respect to all eigenvibrations whose frequency is the same for crystal and subvolume in the unperturbed system. This is approximately the case for all eigenvibrations whose wavelengths are small compared to the linear dimensions of the small subvolume. Only for such eigenvibrations does the fluctuation problem thus make sense. From the considerations of Section 1 one can predict without [explicit] calculation, that the calculation of the mean square fluctuation and of all higher fluctuation means according to quantum mechanics must yield the same result as the Bose–Einstein statistics of light quanta. [...] For the case of the mean square fluctuation the calculation is carried out explicitly in the above cited paper 'Quantum Mechanics II' [= the Dreimännerarbeit].

[24] As Jordan (1927a, p. 642, footnote 2) puts it: 'completely avoiding all statistical concepts'.

[25] Note that Heisenberg refers to 'sound quanta' as being exchanged between the two resonant atoms of his model.

This is thus arguably the picture that Jordan had in mind: the individual subvolumes of the blackbody are constantly performing quantum jumps between stationary states, and his calculation refers to the corresponding temporal fluctuation in the energy of the subvolume.

5 Conclusion

In this Dreimenschenarbeit we have constructed, and defended, an alternative interpretation to Duncan and Janssen (2008) of what Jordan was doing in the final section of the Dreimännerarbeit (Born *et al.* 1926). We have done so in two ways. First, by demonstrating that Jordan's matrix mechanical derivation of Einstein's (1909) mean energy squared fluctuation formula fits within a Boltzmannian framework. In this statistical mechanical picture, we believe it becomes clear why Jordan omits certain proof steps that a Gibbsian derivation (such as Duncan and Janssen carry out) would require for completeness. Second, we have argued that Born, Heisenberg and Jordan's earliest interpretation of their new matrix formalism implies a dynamical picture of temporal fluctuations, which is precisely what is required to underwrite a Boltzmannian reading of Jordan's derivation of the blackbody fluctuation formula.

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