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Planck's heritage and the Bose statistics

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1. The genesis of the ideas which eventually merged into the Bose-Einstein statistics has been analysed widely in the literature[1]. To our knowledge, however, little attention has ever been paid, surpisingly enough, to the historical roots of the feature which most typically characterizes the new statistics, that is the formula for the statistical counting.

In this paper we would like to contribute to filling this blank, by indicating where these roots are to be found and analysing the way in which the information elaborated by the early authors was transmitted to Bose and Einstein.

A convenient starting point for our analysis is provided by Pais'(³) account of the first of Bose's papers(⁴) on the statistics. According to Pais, of the four essential ingredients Bose had introduced -his particles are massless and have two states of polarization, their number is not conserved, and they obey a new statistics- he was only aware of the first two.

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[1] NDLR : See the notes at the end of this paper.

Conservation of the number of particles was necessarily introduced by Einstein in the paper(5) in which Bose's analysis was applied to a gas of non relativistics particles; so that it can be fairly concluded, as Pais does, that Einstein must have taken notice of the lack of the same constraint in Bose's paper.

It can therefore be stated that, by the time the first of Einstein's papers on quantum statistics had appeared, every aspect of it was clarified ... except for the nature of the new statistics.

This is not surprising, if one recalls that Bose's procedure was not the most apt to exhibit this aspect. In particular, the factor :

$$w_s = \frac{(n_s + z_s - 1)!}{n_s\,!\,(z_s - 1)!} \tag{1}$$

which is so essential in the new statistics that we feel free to introduce it without even defining the notation, is *not* to be found in Bose's famous paper(4), nor in the first of Einstein's papers on the ideal gas.

Chronologically, the first universally known paper on the Bose-Einstein statistics in which the factor w_s is introduced, is Einstein's second paper(6), as the expression of the "number of complexions for the s^{th} infinitesimal domain" "according to Bose" ; the expression is compared with the one which holds "according to the hypothesis of the statistical independence of the molecules".

The difference, according to Einstein, confirms what had been pointed out, in particular by Ehrenfest, after the appearance of the first two papers on the new statistics, i.e. that in his gas theory as well as in Bose's radiation theory molecules and quanta could not be considered as statistically independent.

In commenting this point, Einstein went so far as to speak of a "mutual influence of the molecules which for the time being is of a quite mysterious nature".

If one wants to fix the moment in which a relevant component of the scientific community took notice of the fact that Bose and Einstein had introduced a profound revolution in the basic concepts of the statistics of particles, this may well be the one.

What is here at stake, as pointed out by Ehrenfest, is the loss of statistical independence. As we know now, this is the effect of the quantum-mechanical treatment of systems of integral-spin identical particles, and, as such, of quantum indistinguishability. Obviously, when Einstein's paper first appeared, the explanation had to be found somewhere else, as documented by Einstein's sentence quoted above. Therefore, the role of indistinguishability remained at first in the backgroung. It is interesting to recall that a debate on the role of the identity of particles had been going on for several years among various authors, including Planck and Ehrenfest, in connection with the Gibbs paradox arising in the statistics of the ideal gas[2]. The common origin of the two sets of problems, those associated with the statistics of light quanta and those relating to particle identity in gas theory, was to some extent hinted at by Ehrenfest(1), but became evident to most physicists only after the renewal of the debate on both items which followed the introduction ot the new statistics (1,2,8). Since it was Einstein's second paper to start the process, it is of primary importance, for anyone wishing to reconstruct the history of the Bose-Einstein statistics, to establish the way in which the basic factor(1) was transmitted to Einstein from its first inventor.

It is also interesting to investigate how its objective meaning happened to shift in the process of transmission and the way it was considered at the beginning and at the end of its journey.

These are the subjects of our investigation. We wish to formulate its central historiographic thesis in terms of the metaphor of the "letter from the past". We have all at some stage read in a novel that these letters, which usually contain information which has not become public in the meanwhile in some other way, can have an impact on their late readers and modify the course of events. Since in the meanwhile the context has also changed, the role played by the information changes as well.

Like a letter from the past, the basic factor of the Bose-Einstein statistics conveyed information which had long been ignored. Once the factor had again found a use, the information was deciphered and found to play a different role in the new context.

This is the kind of story we would like to tell, with the idea that perhaps it could suggest a mechanism for the transmission and the growth of scientific knowledge which may have operated in other occasions in the history of physics.

2. There is one little puzzle in what has just been said, i.e. the credit that Einstein gives to Bose for the introduction of the factor w_s. The puzzle has, however, an easy solution : quite simply, Einstein is most likely making use of a second paper by Bose(9), which is not so well known[3]. It is at this stage that we meet the question mark which is at the origin of our investigation : for, in Bose's paper, the factor w_s is not derived nor commented, but simply taken, as it stands, from a paper published by Debye(11) in 1910. But this is not the end of the story, since, in turn, Debye quotes the factor from the first edition of Planck's lectures on the theory of heat radiation -which sends us back, more or less straightly, to Planck's classical paper of 1900(12).

Thus, apparently, the "letter" was "written" by Planck and travelled 24 years before it was read by the inventors of the new statistics..

Now that we have discovered the path followed by the letter, let us try to follow it through, to elucidate the points mentioned above : what it meant in the views of the various authors and how its objective meaning happened to shift during the journey.

3. The article in which the factor that interests us makes its first appearence in our story is Planck's famous second paper of 1900(12). In order to be able to recognize the objective meaning it had there and to speculate about the way Planck must have looked at it, we need analyse the paper in some detail. In it, as he had done in the series of papers he had already devoted to the theory of heat radiation(13), Planck considers oscillators and radiation enclosed in a cavity with perfectly reflecting walls. His problem is to determine how the energy is distributed over the oscillators at thermal equilibrium. Once the mean energy U of the oscillators at the frequency ν is given, the spectral density ρ_ν is computed by means of the relation :

$$\rho_\nu = \frac{8\pi\,\nu^2}{c^3}\,U \tag{2}$$

which Planck had derived much earlier on the basis of the oscillator's classical electrodynamics.

Let us, with Planck, denote by E the (mean) energy shared by the N oscillators having frequency ν. Planck (and "this is the most essential point of all the calculation"[4]) considers E as "composed of a very defined number of equal parts"[5], $\varepsilon = h\nu$: dividing E by ε, one obtains therefore the number P of the "energy elements" which are to be distributed among the N resonators :

$$E = P\varepsilon \tag{3}$$

Now, says Planck, "it is clear that the distribution of the P energy elements over N resonators can only take place in a finite well defined number of ways". Each such mode is called by Planck a "complexion", ("using an expression introduced by Mr. Boltzmann for a similar quantity")[4].

A possible complexion, for the case N = 10, P = 100, is explicitly indicated by Planck with the symbol :

1	2	3	4	5	6	7	8	9	10
7	38	11	0	9	2	20	4	4	5

where the oscillators are labeled by the upper index and the number of energy elements pertaining to each of them in the complexion is written underneath. Now, says Planck, "the number of all possible complexions is clearly equal to the number of all possible sets of numbers which one can obtain for the lower sequence for given N and P". "To exclude any misunderstanding -adds Planck- we remark that two complexions must be considered different if the corresponding sequencies of numbers contain the same numbers, but in different order"[4]. This is a clear enough statement on the distinguishability of the resonators.

On the other hand, Planck does not emphasize another feature of this descriptive scheme which is even more crucial : the fact that also the exchange of an energy element between two resonators does not lead to a different complexion.

Already in the fact that Planck fails to make this observation, one can find a hint for the thesis that Planck was not in the least inclined to consider the energy elements as the objects of a statistics.

As this stage Planck states that[4], "from the theory of permutations(6) we get for the number of all possible complexions

$$(4) \qquad \frac{N.(N+1).(N+2)\ldots\ldots(N+P-1)}{1.2.3.\ \ldots\ldots\ldots\ldots\ldots\ldots\ P} = \frac{(N+P-1)!}{(N-1)!\ P!} \text{ "}$$

To our knowledge, it was never stressed that only in the second edition of his book Planck, referring to Eq. (4), wrote explicitly : "hence we have 'combinations with repetitions of N elements taken P at a time'."

This is in agreement with the reconstruction of Mehra and Rechenberg(2), according to which he extracted his counting from Boltzmann, attracted by the fact that such an expression provided a probabilistic basis for the entropy of the black-body radiation previously found ; the attention given to the form of Eq. (4) would then have given rise to a purely analogical train of thought.

Concerning this point, Mehra and Rechenberg[7] recall that Planck, commenting on the way in which he adapted his reading of Boltzmann to the needs of his own problem, made the statement : "I can characterize the whole procedure as an act of despair ...".

A further argument in favour of the hypothesis that the objects of the statistics could not be for Planck the energy elements ε can be derived from the observation that for Boltzmann the subdivision of the total energy into discrete quantities ε was a purely mathematical artifice, and that, of course, in the limit as $\varepsilon \to 0$ any trace of the "elementary" energy elements disappears : in taking the probabilistic scheme from Boltzmann, Planck would then not easily be inclined to attribute to his energy elements a statistical meaning.

In the following we shall recall the way Ehrenfest and Kamerlingh Onnes re-analysed the form (4) as the one arising from the computation of the number of distinct ways in which P-*indistinguishable* objects can be distributed over N-*distinguishable* containers (this method has since become standard and completely legitimates the result). It will then become clear that two procedures are conceptually distinct.

In order to understand Planck's attitude, which cannot be easily deciphered from direct sources, it may be helpful to recall that the novelty in the counting procedure was not to his eyes the crucial point his work had introduced in the foundations of the theory ; Planck's concern was addressed to the definition of probability, and, in a subsequent paper(17), he stated that to set

$$S = k \log W + \text{cost}$$

"leads to a definition of the above probability ; that is to say, in the foundations of the electromagnetic theory of radiation, we do not find any indication enabling us to speak of such a probability with a precise meaning". "As regards the usefulness of the assumption arrived at in this way, its simplicity and its close affinity with a statement in the kinetic theory of gases speak immediately in its favour".

In conclusion it seems that Planck, more than at emancipating himself from Boltzmann's statistics, aimed at establishing a link between his and Boltzmann's procedures.

Only for the latter, in fact, one could find, in the mechanistic description of nature, a firm foundation for a probabilistic approach.

If this was indeed Planck's way of looking at the relation between his and Boltzmann's procedures, it is even more doubtful that he would easily conceive a replacement of the energy elements for the oscillators as basic objects of the statistics.

The problem relating to indistinguishability went unobserved for a few years. Planck's subsequent papers(18) do not add anything relevant from this point of view.

Even the most alert of the early criticists of Planck, Ehrenfest(19), concentrated his attention on other aspects, like the irrelevance of Planck's resonators as far as the "blackening" of the radiation and the novelty of the hypothesis $\varepsilon = h\nu$ with respect to the usual statistical procedures are concerned. The latter point concentrated the attention of most of Planck's early readers, and rightly so.

Indistinguishability remained hidden in the background and it took a few years before a keen observer was able to spot it.

4. By the time Debye wrote the paper that interests us here(11), it was clear enough that Planck had indeed quantized his resonators(20). While it was clear to Debye that something had to be "quantized", he did not like Planck's use of resonators, which he considered inessential and artificial.

Remember that Planck had derived his equation (2) from the classical electrodynamics of a charged oscillator, with no reference whatsoever to the counting of the cavity's normal modes. But, of course, Eq. (2) can be derived immediately in a much simpler way once the number of normal modes for a cavity of volume V in the frequency interval $d\nu$ has been computed as

$$N(\nu)d\nu = \frac{8\pi\nu^2}{c^3} V d\nu \tag{5}$$

(We may recall that this computation was first made by Rayleigh and Jeans in 1905)[8]. If we now indicate with U the mean energy of a mode at frequency ν, the total energy $\rho_\nu V d\nu$ contained in the frequency interval $d\nu$ will be $UN(\nu)d\nu$: by equating the two expressions one then gets Eq. (2).

Debye's idea was to quantize directly the cavity normal modes[9], by assuming that each oscillation receives a share of energy in the form of quanta $h\nu$. This procedure has been analysed elsewhere(22) ; we reproduce here for completeness the account given in ref. (15).

Let $f(\nu)$ quanta belong to the oscillation of frequency ν ; then

$$U = f(\nu)h\nu \text{ and}$$

$$\rho_\nu V d\nu = f(\nu).h\nu N(\nu)d\nu, \tag{6}$$

so that

$$\rho_\nu = \frac{8\pi\nu^2}{c^3} h\nu f(\nu) \tag{7}$$

and the problem of determinating ρ_ν is reduced to that of determining $f(\nu)$.

On the right hand side of Eq. (6), $f(\nu).N(\nu)d\nu$ represents the number of energy elements to be distributed over the $N(\nu)d\nu$ oscillations ; $N(\nu)d\nu$ and $N(\nu).f(\nu)d\nu$ corresponds therefore respectively to Planck's N and P. Debye writes then formally Planck's formula for W :

$$W = \frac{(P + N - 1)!}{(N - 1)!\, P!} \approx \frac{(N + P)!}{N!\, P!} \tag{8}$$

in the form

$$W = \frac{(Nd\nu + Nfd\nu)!}{(Nd\nu)!\,(Nfd\nu)!} \tag{9}$$

which he had found in the first edition of Planck's book(²³).

Then he determines the function f which maximizes the entropy, subject to the condition of fixed total energy, and finds

$$f(\nu) = \frac{1}{\exp(\alpha h\nu) - 1} \tag{10}$$

where the constant α is subsequently specified as $1/kT$ by the thermodynamical relation $(dV = 0)\ dS = dU/T$.

To realize that Debye had introduced an essential novelty in the matter, we need only consider the objective support his treatment gave to Einstein's views on the quantization of the energy of the radiation (this has been stressed, for instance, by Kastler(²²)). It is interesting to note that, in his 1906 paper on Planck's radiation theory(²⁴), Einstein had introduced a quantization procedure which anticipated to some extent Debye's main idea. However, Einstein's oscillators were to be imagined as "Ionen" (ions), and not as the cavity's normal modes. The lack of a marked interest, on the part of Einstein, for the computation à la Rayleigh-Debye is considered by Kastler as one of the great missed opportunities in the early development of quantum radiation theory.

With the novelty introduced by Debye is linked, as we shall comment in the following, the basic shift of meaning which Planck's heritage underwent in its transmission to Bose.

On the question of indistinguishability, however, Debye has nothing to say, and does not appear to be conscious of the existence of the problem.

5. Planck's "letter" took its final leap from Debye's paper to Bose. This does not mean that it was not read by other people in the meanwhile. Among them we shall first briefly quote the keenest of Planck's readers, the Polish physicist Natanson (this section developes slightly the discussion in ref. (22)).

In a paper which appeared in Physikalische Zeitschrift(²⁵), Natanson set himself the task of computing in

how many ways P energy elements can be distributed over N "receptacles of energy" (Energiehaltern), so that N_j receptacles contained j energy elements, subject to the restrictions $\sum_j N_j = N$ and $\sum_j j \cdot N_j = P$, if it is assumed that each energy element has the same probability "of reaching a given receptacle". But then Natanson suggests that one should, a priori, distinguish three cases :

1) receptacles and energy elements are both indistinguishable. In this case a microscopic state is defined once the number of receptacles containing a given number of energy elements is specified ;
2) receptacles are distinguishable, energy elements indistinguishable. In this case a microscopic state is defined once the number of energy elements for each identifiable receptable is specified ;
3) both receptacles and energy elements are distinguishable (this is Boltzmann's case).

Planck's procedure, continues Natanson, is based on the tacit assumption (tacit, at least, we may add, as far as the indistinguishability of the P elements is concerned) that we are in case 2).

This may well be taken, says Natanson, as the foundation (Grundlage) of the theory. It so happens that it is confirmed by the experiment. But apparently no one has as yet paid sufficient attention to the fact that it is only the experiment which can tell us, a posteriori, what is the correct assumption in a given physical situation.

Natanson's paper appeared in 1911. What kind of a reception did it receive ? The widest reference to Natanson's work can be found in Hund's book(²⁶) and in the treatise by Mehra and Rechenberg(²) ; hints concerning possible early readers of Natanson's are however practically nil. Hund's conclusion is that "in 1924 Natanson's arguments had been forgotten". One may wonder whether they had really ever received any attention at all.

What about Planck himself ? There is an important reference[10] to Natanson's paper in Planck's report(27) at the first Solvay Conference (1911). Concerning the formula for W (Eqs. 4, 8), he says that "it is measured by the number of ways (complexions) in which the oscillators can be distributed over the energy domains corresponding to integer multiples of ϵ ..." (our translation from the French original).

Planck makes at this stage (in a footnote) the following statement : "This calculation is completely unambiguous and in particular does not conceal anymore anything of the indetermination of which L. Natanson has recently written ...". Planck concludes the statement in the text in this way: "... and this number is equal to that of distributions of P energy elements over N oscillators, if one considers only the number and not the individuality of the energy elements an oscillator receives in each of the distributions considered". The last sentence certifies that Planck has taken notice of the most relevant aspect of his counting that Natanson had pointed out. His interest, however, does not seem to focalize in the matter of indistinguishability (after all one deals here with energy elements, and these *are* indistinguishable), but rather on the internal consistency of his procedure, and the reference to Natanson seems to be mainly made to stress that Planck original view does not lead to ambiguities of any kind.

Natanson's paper is also quoted in the Appendix to the English version of the second edition of Planck's book, containing "a list of the most important papers on the subjects treated in this book", compiled by the translator Morton Masius, "with Professor Planck's permission". However Planck's text does not make any comment or explicit reference to Natanson. It is perhaps also interesting to note that, while some papers in Masius' list, Debye's paper among them, are briefly introduced and commented, Natanson's paper is merely quoted.

Would it be considered a wild speculation to infer that Masius, not unlike some of his contemporaries, suspected that the paper *had to be* important but had not found within himself and the scientific community sufficient motivation to devote to it the time requested to understand exactly *why* and where *it was* important ?

Although a definite conclusion about the role played by Natanson's paper in the years which immediately followed its publication can hardly be achieved on the basis of such scarce hints, we are here formulating the hypothesis that the point Natanson had caught was not felt of central importance by the bulk of the influent physicists who had dedicated some interest to Planck's theory.

Ever since its first appearence, Planck's factor (4) conveyed with it, objectively, the information concerning the indistinguishability of the P energy elements.

Natanson, eleven years later, deciphered this message and gave the scientific community the opportunity of transforming the objective information into a subjective awareness.

Apparently this opportunity was missed by the majority of the influent members of the community.

But even if we cannot completely exclude that the knowledge of the content of Natanson's paper had spread among a limited number of readers, Hund's(26) conclusion that Natanson's arguments had been forgotten by the time Bose wrote his first paper on the statistics seems unavoidable.

6. There is one good reason why it should have been so. As Kuhn has stressed in his book(20), a decisive turn in the course of events that led the scientific community to accept the idea that indeed a quantum revolution had taken place was impressed by the new attitude about Planck's theory which Lorentz developed immediately after the Rome Conference of 1908. Criticisms from the experimentalists and even from Wien had convinced him once and forever that the Rayleigh-Jeans law was untenable, and, in 1909, prompted him to accept the quantization of the energy exchanged between Planck's resonators. Kuhn(20) comments that "after adopting that position, Lorentz quickly became a leader in developing and propagating the

quantum theory" ; the relevance of Lorentz's attitude for the development of the theory is connected by Kuhn with Lorentz's unquestioned prestige within the scientific community.

For the sake of our analysis, it seems important to stress that Lorentz's "conversion", and the way it took place, constituted perhaps the most crucial hindrance for a direct transmission of our "letter from the past". Lorentz firstly(28) adhered entirely to Planck's approach, set himself the task of giving an adequate presentation of Planck's procedure and even ended the derivation of the law that Planck had not carried through in this 1900 paper(12) as too "complicated to perform explicitly". Lorentz, introducing Eq. (4), states with no further comment, that it gives the number of different ways in which the P units of energy can be distributed over the N elements. Subsequently, in a paper of wider scope(29), he restricts any reference to Planck's work to the single annotation : "Planck's derivation of his own law differs from the present approach", and exhibits, for the first time, the two-step derivation of Planck's formula which most students know. The derivation indeed proceeds via the application of Boltzmann's maximization procedure to the quantity

$$W = \frac{N!}{N_1!\ N_2!\ N_3!\ \ldots}$$

where N is the total number of the receptacles of energy, in Natanson's terms, and N_s is the number of receptacles containing s quanta. The procedure, with the constraint on the energy :

$$E = \sum_s N_s\, h\, \nu_s$$

and the number of receptacles :

$$N = \sum_s N_s$$

leads of course to Boltzmann's distribution (with equally spaced discrete levels ; Boltzmann had himself followed this procedure for pedagogical reasons ; as final step, however, he had let the level spacing tend to zero(20)).

The second step consists in computing the average energy as

$$\bar{E} = \frac{\sum_s N_s\, h\, \nu_s \exp(-E_s/kT)}{\sum_s N_s \exp(-E_s/kT)}$$

which leads, as Einstein had already shown in 1907, in his paper on specific heats(30), to Planck's formula for the energy of the oscillators.

It seems fair to conclude that most physicists willing to accept the quantum discontinuity insofar as it was supported by Lorentz's authority would be inclined to accept his specific approach as well. But the point is that this approach masks completely the role of indistinguishability (a discussion of the technical reasons for that would be of interest in itself), and diverted from it the attention of the new adepts.

7. Planck's message, when it reached Bose, was not accompanied by the key Natanson had provided for its deciphering. However, other readers of Planck's work had provided in the meanwhile more or less equivalent keys. We will briefly discuss their contributions and then examine the possibility that Bose could be acquainted with them.

In the first place, a paper of 1911 by Ehrenfest (31) should be mentioned, in which it is for the first time explicitly pointed out that to arrive at Planck's formula one must release the hypothesis of statistical independence (... "die Annahme" that "das individuelle Elementarquantum entfällt auf die verschiedenen Resonatoren mit gleicher Warscheinlichkeit und die einzelnen Elementarquanten sind als voneinander unabhänging verlegbar anzusehen" ... "führt nicht zu Planckschen Strahlungsformel"). Apart from its general interest, Ehrenfest's paper represented an important step both in the development of the author's ideas on the subject and for the story we are trying to reconstruct.

A second relevant contribution is associated with the name of another Polish physicist, Mieczyslaw Wolfke, who was then working in Zürich, where he had some contact with Einstein. His work and figure are described in the treatise by Mehra and Rechenberg(2), and readers are referred to that for further details. As stressed by these authors, Natanson had not concerned himself with a physical interpretation of his indistinghuishable energy elements. Wolfke associated with them "light atoms" and analysed Planck's law from this point of view in a series of papers, published in 1913 and 1914. It is interesting to note that in the subsequent debate with Krutkow(2), a physicist then active at Leyden, we find, clearly delineated, the elements which formed the core of the more authoritative analysis that Ehrenfest and Kamerlingh-Onnes(32) carried out in the same years. This analysis was presented in some detail in(15) and we reproduce it here for completeness. One of the motivation for their work, as Ehrenfest and Kamerlingh-Onnes say in a footonote, was to "find an explanation of the form (N − 1)! in the denominator" of Planck's formula (4). Planck, say the authors, "refers to the train of reasoning followed in treatises on combinations". In these treatises, Planck's expression -they go on- "is arrived at by the aid of the device 'transition from N to N + 1', and this method taken as a whole does not give an insight into the origin of the final expression". Ehrenfest and Kammerlingh-Onnes consider, first, an example ($N = 4$, $P = 7$). In this case, one of the possible distributions is that in which the resonator R_1 "contains" the energy 4ϵ, R_2 the energy 2ϵ, R_3 no energy and R_4 the energy ϵ. This is what is indicated by the symbol

$$\| \,\epsilon\epsilon\epsilon\epsilon \mid \epsilon\epsilon \mid \mid \epsilon \,\|$$

Now, continue the authors, with general values of N and P, the symbol will contain P times the sign ϵ and $(N - 1)$ times the sign |. The question is, how many different symbols may be formed from the given number of ϵ and |. To answer it, observe that there are altogether $P + (N - 1)$ signs ϵ and |. If we consider these $(N - 1 + P)$ elements as so many distinguishable entities, they may be arranged in $(N - 1 + P)!$ manners between the ends ‖. However, in the symbol, the elements ϵ, and the elements |, are not distinguishable among themselves, so that, once a particular arrangement is obtained, a permutation of the P signs ϵ or of the $(N - 1)$ signs | transforms the symbol in itself. The number of different symbols is thus obtained by dividing $(N - 1 + P)!$ by $(N - 1)!\,P!$, which gives Planck's formula, Eq.(4).

Ehrenfest and Kamerlingh-Onnes have thus, no doubt, provided us with the clearest possible derivation of Planck's formula. Concerning the physical meaning to be attached to the procedure, their point of view differs from that of Natanson ; indeed, in the Appendix of their paper, they insist that "the permutation of the elements ϵ is a purely formal device, just as the permutation of the elements |". This is of course true if one is exchanging *quantities*, numbers, which can in no way be associated with *objects* such as particles. Natanson does not think in terms of an exchange of objects either ; as a matter of fact, he does not suggest any physical interpretation. However, he does not exclude one a priori, and it seems that it is only by allowing for the possibility of a physical interpretation that we realize that we are indeed dealing with a different statistics. This is the point that Ehrenfest and Kamerlingh-Onnes seem to miss[11].

According to Ehrenfest and Kamerlingh-Onnes, "a physical interpretation" of the "formal device" has been given "more than once" by "a misunderstanding". "As a matter of fact -continue the authors- Planck's energy elements" were often "almost entirely identified with Einstein's light quanta ... the confusion which underlies this view has been more than once pointed out". We pause for a moment only to say that the authors quoted here are Ehrenfest (1911) and Krutkow. But, how can the product of the confusion be measured ? This leads us to the second important point contained in this paper. In his 1905 paper, Einstein had considered the thermodynamical properties of a radiation described by Wien's formula

$$\rho_\nu = \alpha\nu^3 \, e^{-\beta\nu/T} \tag{11}$$

and derived, in particular, the expression

$$S - S_0 = \frac{E}{\beta\nu} \ln \frac{V}{V_0} = k \ln\left(\frac{V}{V_0}\right)^{E/h\nu} \tag{12}$$

for the dependence of the entropy of radiation of energy $E = V\rho d\nu$ for a change from volume V_0 to volume V. (The second expression in Eq. (12) is in modern notation and uses $\beta = h/k$). Now take Einstein's formula, Eq. (12), and rewrite it in "Planckian" terms. To begin with, call P the numbers of the light quanta, as the objects which are akin to Planck's energy elements. On the other hand, the number of normal modes at a given frequency in a cavity is proportional to its volume. The normal modes represent the physical states of the system over which the P light quanta are distributed. The number of normal modes at frequency ν plays therefore the role of the number N of receptacles of energy available, and, on account of what has just been said, it will be

$$V \propto N \tag{13}$$

so that Eq. (12) will be rewritten as

$$S_2 - S_1 = k \ln\left(\frac{N_2}{N_1}\right)^P \tag{14}$$

(when a change from volume V_1 to volume V_2 is involved).

Read in terms of Boltzmann's relation

$$S = k \ln W + \text{const.}, \tag{15}$$

Eq. (14) gives then

$$\frac{W_2}{W_1} = \left(\frac{N_2}{N_1}\right)^P \tag{16}$$

On the other hand, in Planck's formula, Eq. (4), N represents again the number of receptacles of energy. From it one can then derive, with a similar meaning of the symbols, the formula corresponding to Eq. (16) in Planck's scheme :

$$\frac{W_2}{W_1} = \frac{(N_2 + P - 1)!}{(N_2 - 1)!\, P!} : \frac{(N_1 + P - 1)!}{(N_1 - 1)!\, P!} \tag{17}$$

Ehrenfest and Kamerlingh-Onnes stress that the profound difference between Eqs. (16) and (17) is explained by the fact that "Planck does not deal with really mutually free quanta ε"(32).

Ehrenfest and Kamerlingh-Onnes could have easily observed, especially if one considers the attention devoted in their paper to combinatorial aspects, that the distinction is between the number of ways in which P distinguishable and indistinguishable objects may be distributed over N receptacles, but they did not. Rather, by stressing that Einstein's hypothesis leads necessarily to Wien's, Eq. (11), and not to Planck's law, and that Planck's "formal device" cannot be interpreted in the sense of Einstein's light quanta, they wanted to indicate, like Ehrenfest had already done in his 1911 paper, that Planck's counting implied the loss of statistical independence (see, for instance, Klein (16, 1962) : "What Ehrenfest showed, in effect, was that particles which have to be counted according "to Planck's equation (4)" are not independent particles in any ordinary sense"). The authors did not give an explicit answer to the question they had posed. As stressed by Klein(1), the question formed, at the time the paper was published, a puzzle ; a puzzle that had to wait for the quantum mechanical postulate on the systems of integral-spin identical particles for a formal general answer. On the other hand, as it is perhaps interesting to note, the paper by Ehrenfest and Kamerlingh-Onnes provides the clue for the solution, though not within the context of an all-pervasive theory : indeed, the combinatorial analysis of Planck's formula clearly exhibits, to the eyes of today's reader, the essential role of indistinguishability (for bosons !) in determining the loss of statistical independence.

Is there any sign that the authors' analysis received any attention by the experts of the field in the following years ? The hints are here even scantier than in the case of Natanson. None of the treatises on the subject quotes early readers of the paper. It does not appear in Masius's bibliography for the good reason that it appeared in press later, though in the same year. And it is not quoted in any of the classical papers on the Bose-Einstein statistics.

We cannot draw the conclusion that it went completely unobserved. But there is one very good reason why it might have : the paper was communicated in the meeting of Oct. 31, 1914, of the Amsterdam Academy (in Dutch) ; its German translation appeared in Annalen der Physik in issue 7 of volume 46 of 20 April 1915(2). By that time Europe was at war. In the spring of 1915 there were probably not so many German speaking physicists in the Central Empires capable of spending or willing to spend their time on a scientific paper. The war lasted three and half more years, and, in the words of Mehra and Rechenberg (see ref. (2), pag. 259) it "affected the international relations of science more deeply than any other event during the previous hundred years. Scientific researchers active in enemy countries had been cut off from one another almost completely". And, we may add, by the time the war was over, problems which had just come into focus when the conflict broke out had very likely been forgotten. When the war was over, physicists either resuming their activity or just starting their work concentrated almost entirely on the problems of atomic physics[12].

In particular, there weren't many willing to spend their time on a problem, that of black-body radiation, considered as solved. Of course, there were exceptions. Among them the prominent case is Einstein. As far as the statistics of light quanta and gases are concerned, however, the contributions by Wolfke and de Broglie between 1921 and 1924 are even more important. For this reason their contributions must be quoted in any attempt[13] to write the history of the Bose-Einstein statistics.

Since, however, their part in the problem of the transmission of knowledge from Planck to Bose does not seem to have been of any importance, we will not deal with their contributions here[14].

8. A very accurate and complete account of Bose's scientific background and early career is given in(2). We briefly summarize it for the points of greater interest for the problem we are dealing with. Bose started his scientific career at the University of Calcutta around 1915. Only "limited European scientific literature and few advanced books were available at the Calcutta libraries due to war conditions"(2). Among the very few things of interest to us, Bose, who knew some German, and had himself translated Einstein's famous paper of 1916 on general relativity, could have been acquainted by the early twenties with Planck's book and with Planck's Festschrift

of 1918, which contained, among other things, Wien's article on "Die Entwicklung von Max Planck's Strahlungstheorie", and Nernst's "Quantentheorie und neuer Wärmesatz". By 1924, Bose had moved as a Reader of Physics to the University of Dacca. It was in deep connection with his teaching and tutoring activities that he developed his attitude towards Planck's theory. As Einstein and Debye, among others, had already pointed out, Planck's derivation of his law did not stem from a self consistent set of axioms. Bose was aware, in particular, of Einstein's criticisms, which he had read in his 1917 paper. However, even Einstein, as Bose wrote later in the introduction of his famous paper, had referred to classical theory by applying Wien's displacement law and a correspondence argument. Bose felt uneasy about this lack of a self-consistent framework, and this uneasiness wa augmented by teaching. He wanted to find a purely quantal derivation. As stressed in (2), a turning point in the course of events that led Bose to his answer was provided by a visit paid to him in Dacca by his friend and collegue Saha. According to Ref. (2), Saha "pointed out to Bose the work of W. Pauli (1923) and of Einstein and Ehrenfest (1923)". Still according to Mehra and Rechenberg, the discussion with Saha encouraged Bose to study carefully the papers by Peter Debye (1910-1923), Einstein (1917) and Compton (1923)[15].

No doubt, Compton's analysis of his effect in terms of light quanta was decisive. Bose's great idea was of course to derive Planck's law as the law of thermal equilibrium for light quanta. This is so well known that there is no need to spend any time on it. But there is the further aspect, which will lead us, eventually, to the crucial point of all our investigation, that is, the statistical aspect. In this respect, Mehra and Rechenberg's quotation of Debye (1910) and, to a lesser extent, of Pauli and Einstein and Ehrenfest, as acquaintances of Bose, call for some comment. No doubt, "the discussions with Saha encouraged Bose to study carefully" these papers ; no doubt, he became acquainted with them in the short period between March (Saha's visit) and 15 June 1924 (on which date he sent his second paper to Einstein). But, we ask, is it not possible that his attention to these papers developed gradually in the sense that he was ready to take full account of them only after giving an answer, in his own way, to his central question ?

This is, at any rate, what we would like to suggest. We feel encouraged to assume this point of view by the fact that not only Debye (1910), Pauli, and Einstein and Ehrenfest are not quoted in the first of Bose's papers, but that his way of attacking the statistical problem is logically independent of Debye's way[16], although he probably thought it was close to Planck's. According to this point of view, we may conclude that Bose's first paper does not add anything significant on the problem of indistinguishability.

Corollary to it is the thesis that his second paper is historically very relevant as the one in which, through Debye, the transmission of Planck's heritage on indistinguishability, although unconscious, is actuated.

We feel conforted in our thesis that Bose availed himself of the subset of the papers suggested as relevant to him by Saha, consisting of Debye (1910), Pauli and Einstein and Ehrenfest, only when writing his second paper[17], by the very fact that "Debye" is the word with which its begins, and the names of Pauli, Einstein and Ehrenfest appear several times in the paper. "Debye has shown -the article begins- that Planck's law can be derived by means of statistical mechanics" (9). This initial quotation prepares the ground for Bose's later assumption concerning one of the basic ingredients he needs here, namely, of course, the expression for the thermodynamical probability of the radiation. After perusing Debye (1910), he was willing to give up the procedure of his preceding paper, which he probably never considered as central. Debye existed in the literature and it appeared fair to make reference to an existing derivation. Of course, Bose did not want to follow Debye through ; he was up for his central idea of dealing with a gas of photon. So that the only thing that he actually picked up from Debye was his factor (9)

$$W = \frac{(N d\nu + N f d\nu)!}{(N d\nu)!\,(N f d\nu)!}$$

with a slight change in notation (the factor $8\pi\nu^2/c^3$ is written as A_ν while the notation $N_\nu\, d\nu$ is used for Debye's

N(ν)f(ν) dν), and with the more substantial change consisting in taking into account all frequencies, so that Bose has

$$W = \prod_{\nu} \frac{(A_\nu + N_\nu\, d\nu)!}{A_\nu!\; N_\nu\, d\nu!} \tag{18}$$

In a sense, but only in a sense, Debye and Bose are indeed doing the same thing. To our knowledge, this point was first pointed out by Kastler(22).

The basic objects that enter into Debye's Eq. (9), and Bose's formula, Eq. (18), are the number of radiation states in the frequency interval dν (Ndν and A_ν in the two notations) and the number of energy elements to be distributed over the N(ν) dν states (f(ν).N(ν)dν) and N_ν dν in the two notations). The closeness of the two procedures has been pointed out in particular in(22). However, for Debye the number of states is viewed, "classically", as a number of normal modes, and the energy elements are nothing but energy elements ; for Bose, the number of states is a number of photon states, and the number of energy elements is the number of photons in the state[18].

But then, of course, Planck's indistinguishability of the energy elements has now become the indistinguishability of particles, which is by no means formal. This fact remained hidden (this is in fact the central point of our argumentation), to the eyes of Bose and of many of his early readers, because the debate on indistinguishability that had flourished around Planck's formula had for a long time been forgotten and therefore not transmitted with Planck's "letter". It took some time before it was recognized that the new statistics indeed implied (or was based on) indistinguishability, and the way this happened could form the object of a story of itself. This story is of no concern to us here. We would instead like to come back to the metaphor to stress once again that the message on indistinguishability had been recodified in Bose's second paper, and once spotted, gave new impulse to the course of ideas in physics.

We wish to thank A. Desalvo for useful discussions. We also thank J.L. Heilbron for his sharp criticisms which contributed to ameliorate a first draft of this article in several points.

NOTES

[1] See, for instance, M.J. Klein(1), J. Mehra and H. Rechenberg (2), A. Pais(3).

[2] These developments have been analysed by several authors. See, for instance, refs. (1), (2), (7).

[3] See however, for instance, E.C.G. Sudarshan(10).

[4] Translation as in ter Haar(14).

[5] In the text "endlichen gleichen Teilen".

[6] In the text "Kombinationslehre".

[7] See also S. Bergia(15) ; M.J. Klein (1962, 1977)(16).

[8] See also H.A. Lorentz (1903)(21).

[9] It must be noted that Ehrenfest(19) had already suggested that an unambiguous derivation of Planck's law should be based on the quantization of the cavity normal modes (we thank J.L. Heilbron for reminding us of this aspect of Ehrenfest's paper). However, he did not develop the approach in any detail (see Kuhn(20)).

[10] We thank J.L. Heilbron for pointing out to us this important point.

[11] Our opinion about the different views held by Natanson and Ehrenfest and Kamerlingh-Onnes is supported by Hund(26).

[12] For a comprehensive analysis of the period, see Heilbron(33).

[13] On these aspects, see, for instance, (34), (35), (36), (37), (38), (39), (40).

[14] This omission is viewed by J.L. Heilbron (private communication) as an example of "Whig interpretation of the history of science", defined by S.G. Brush (by analogy with H. Butterfield's *The Whig Interpretation of History*) as a history in which "each person or event in the past is to be judged from the standpoint of the present". We thank J.L. Heilbron for calling our attention to the concept and A. Desalvo for referring us to Brush's original definition. Of course, we do not agree with Heilbron's criticism.

[15] It should be noted that Saha had been himself working in problems of statistical mechanics (thermal ionization of gases)(2).

[16] To define the thermodynamic probability w_s of N_s quanta of frequency ν_s, Bose most likely referred to the structure of the equation

$$W = \frac{N!}{N_0!\ N_1!\ N_2!\ \ldots}$$

valid for the case of N gas molecules distributed over the elementary "space" regions 0, 1, 2, ... (N_0, N_1, N_2,... molecules in each separate space element), but replaced the quantities appearing in it by different entities. Precisely, calling A_s the number of cells at disposal for the quanta of frequency ν_s, P_0 the number of vacant cells, P_1^s the number of cells containing one energy quantum $h\nu_s$, etc, he wrote :

$$w_s = \frac{A_s!}{P_0^s!\ P_1^s!\ P_2^s!\ \ldots}$$

For an analysis of the statistical meaning of this procedure, see Pais(3), Tagliaferri(38), Bernardini(39), Bergia (35).

Bose could have found his formula for W in Planck's book, with which he was acquainted (ref. 2, p. 564), most likely in its second edition, whose English translation had been available since 1914 (we thank J.L. Heilbron for calling our attention to this point).

[17] According to W.A. Blanpied(41), Bose had sent "a paper on the subject to the Philosophical Magazine" already during 1923 ; when "six months later the editors of that journal informed him that (regrettably) the referee's report on his paper was negative", he sent "the rejected manuscript" to Einstein. This reconstruction corroborates our thesis that Bose's first paper was independent of Debye's paper of 1910.

[18] It can be observed that this "translation" illuminates the connection between the Einstein light-quantum idea and Debye's view in terms of discrete excitations of waves,

which neither Debye nor Einstein had hinted in 1911. The connection is of course made possible by the correspondence existing between density of cavity modes and density of phase space cells. The clue for an understanding of this correspondence was first provided between 1923 and 1924 by Louis de Broglie(42) with the introduction of the wave-particle dualism (both for a material gas and for a gas of light quanta).

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