

On the Explanation for Quantum Statistics

Simon Saunders

Abstract The concept of classical indistinguishability is analyzed and defended against a number of well-known criticisms, with particular attention to the Gibbs' paradox. Granted that it is as much at home in classical as in quantum statistical mechanics, the question arises as to why indistinguishability, in quantum mechanics but not in classical mechanics, forces a change in statistics. The answer, illustrated with simple examples, is that the equilibrium measure on classical phase space is continuous, whilst on Hilbert space it is discrete. The relevance of names, or equivalently, properties stable in time that can be used as names, is also discussed.

Keywords: classical indistinguishability, Gibbs paradox, quantum statistics

Einstein's contributions to quantum theory, early and late, turned on investigations in statistics - most famously with his introduction of the light quantum, now in its second century, but equally with his penultimate contribution to the new mechanics (on Bose-Einstein statistics) in 1924. Shortly after, his statistics was incorporated into the new (matrix and wave) mechanics. But there remained puzzles, even setting to one side the question of his last contribution (on the completeness of quantum mechanics). A number of these centre on the concept of particle indistinguishability, which will occupy us greatly in what follows.

To keep the discussion within reasonable bounds, and for the sake of historical transparency, I shall use only the simplest examples, and the elementary combinatoric arguments widely used at the time. For similar reasons, I shall largely focus on Bose-Einstein statistics (and I shall neglect parastatistics entirely). Hence, whilst not a study of the history of quantum statistics, I shall be speaking to Einstein's time.

But if not anachronistic, my way of putting things is certainly idiosyncratic, and calls for some stage-setting.

1 The Puzzle

These are the puzzling features to be explained: distinguishable particles, classically, obey Maxwell-Boltzmann statistics, but so do *indistinguishable* (permutable) particles. In quantum mechanics, distinguishable particles also obey Maxwell-Boltzmann statistics; but *not* so indistinguishable ones. There is evidently something about the combination of permutation symmetry and quantum mechanics that leads to a difference in statistics. What, precisely?

Were the concept of indistinguishability unintelligible from a classical perspective, the puzzle would hardly arise, or not in this form; indistinguishability in itself, in that case an inherently quantum concept, would be the obvious culprit. And, indeed, the notion of classical permutability has for the main part been viewed with suspicion (because already incompatible with classical principles, philosophical or physical, almost always unstated). After all, classical particles can surely be distinguished by their trajectories (an argument I shall discuss at length). The very concept of particle indistinguishability only came to prominence through investigations in quantum statistics (Ehrenfest (1911) and Natansen (1911)). It was natural to view particle indistinguishability as an intrinsically quantum mechanical concept.¹

And yet the same concept is important to another puzzle that arises already in classical mechanics - the *Gibbs' paradox*. In particular it explains the subtraction of a term $k \ln N!$ from the classical (Boltzmann) entropy for N identical particles² (to give the entropy as an extensive function of state); so much is required if there is to be zero entropy of mixing of two samples of the same gas. Division of the classical phase space volume by $N!$, as follows if identical classical particles are permutable (so that phase space points related by a permutation are identified), supplies the needed correction. It was explained in just this way (using his own definition of the entropy function) by Gibbs (1902 p.206-07).

But Gibbs' view of the matter found few supporters, and was rapidly overtaken by events; so much so, that by mid-century, in reply to a related question raised by Schrödinger, it was not even judged worthy of mention:

In conclusion, it should be emphasized that in the foregoing remarks classical statistics is considered in principle as a part of classical mechanics which deals with individuals (Boltzmann). The conception of atoms as particles losing their identity cannot be introduced into the classical theory without contradiction. (Stern 1949).

(Stern did not say in what the contradiction consists).

For a text book still in wide use:

It is not possible to understand classically why we must divide by $N!$ to obtain the correct counting of states (Huang, 1963 p.154).

(although classical permutability implies it directly).

For a statement by Schrödinger on the subject::

¹ Although Schrödinger had by the close of this period (in his last paper prior to the series on wave mechanics) shown how one can dispense with it, taking as distinguishable objects the modes of a system of waves as representing the states of a gas (he was of course drawing at this point heavily on de Broglie's ideas). It was an advantage of the approach, thought Schrödinger, that the states of the gas thus conceived obeyed Maxwell-Boltzmann statistics (see Schrödinger (1924) and, for further discussion, Dieks (1990); I shall come back to this point at the end).

² Meaning that differences between them, if any, are irrelevant to the dynamics.

It was a famous paradox pointed out for the first time by W. Gibbs, that the same increase of entropy must not be taken into account, when the two molecules are of the same gas, although (according to naive gas-theoretical views) diffusion takes place then too, but unnoticeably to us, because all the particles are alike. The modern view [of quantum mechanics] solves this paradox by declaring that in the second case there is no real diffusion, because exchange between like particles is not a real event - if it were, we should have to take account of it statistically.³ It has always been believed that Gibbs's paradox embodied profound thought. That it was intimately linked up with something so important and entirely new [as quantum mechanics] could hardly be foreseen. (Schrödinger 1946 p.61).

(implicitly suggesting that the paradox could not be resolved on purely classical grounds).

And from a recent monograph devoted to the concept of classical particle indistinguishability:

Prior to the description of a state by means of probability measures states were identified with point measures. In this deterministic setting indistinguishable objects are not conceivable. (Bach 1997, p.131)

I shall come back to Bach's treatment of classical indistinguishability shortly.

I do not wish to suggest that the concept was universally rejected; Leon Rosenfeld⁴ spoke with approval of Planck's appeal to indistinguishability to explain the needed subtraction (in the 4th edition of *Theorie der Wärmestrahlung* of 1921), comparing it with the discussion by Gibbs. This, thought Rosenfeld, was a 'simple and clear interpretation' but one (he went on to say) that was rejected by Ehrenfest and Trkal (as 'incomprehensible'⁵).

However, Rosenfeld understood Gibbs' conception of statistical mechanics as 'fundamentally idealistic' (Rosenfeld 1959 p.244); in contrast:

The argument of Ehrenfest and Trkal is clearly inspired by the materialist (others prefer to say 'realist') attitude characteristic of Boltzmann's thought: the problem of statistical mechanics is to give a complete and logically coherent deduction of macroscopic laws of thermodynamics from those of mechanics, applied to the atomic model of matter. *Considered from this point of view, it is evidently decisive*; and it is thus particularly significant that it does not at all impress Planck. (Rosenfeld 1959, p.243, emphasis mine).

³However, in quantum mechanics as classically, certainly there *is* real diffusion (of particles initially confined to a sub-volume, on removal of a partition separating the two samples of gas, into the total volume).

⁴Another exception is Hestenes, who has also defended the concept of classical indistinguishability (Hestenes (1970)); but, with certain reservations, his approach is similar to that of Rosenfeld and Bach, discussed below, and I shall not address it separately.

⁵But note the word does not occur in the English-language version of their paper.

(Planck, Rosenfeld went on to explain, did not share Boltzmann's 'materialist' preoccupations.)

I take it to have established a *prima facie* case that the concept of classical indistinguishability, at least at a realist, microphysical level of description, has been greatly neglected, and for the most part dismissed out of hand. In what follows I shall first dissect with rather more care the arguments of its critics (Section 2), and then go on to defend the principle directly (Section 3); only if it is granted as *classically* intelligible does the puzzle about quantum statistics arise in the way that I have stated it. An answer to the puzzle follows (Section 4). Indeed, it is not too hard to find, prefigured as it was in the work of Planck (1912) and Poincaré (1911, 1912), who traced the origin of the new statistics to the discreteness of the energy. (More generally, I shall trace it to the discreteness of the measure to be used, in equilibrium conditions, on Hilbert space.)

2 Against classical indistinguishability

The clearest argument in the literature against classical indistinguishability is that the principle is not needed (what I shall call the 'dispensability argument'); a thesis due to Ehrenfest and Trkal (1920), and subsequently defended by van Kampen (1984). The objection that classical indistinguishability is incoherent is more murky, and has rarely been defended explicitly; for that I consider only van Kampen (1984) and Bach (1997).

Ehrenfest and Trkal considered the equilibrium condition for molecules subject to disassociation into a total N^* of atoms, whose number is conserved, with recombination into different possible numbers $N, N', N'' \dots$ of molecules of various types. The upshot: a contribution $k \log(N^*/N!N'!\dots)$ to the total entropy which, when written as a sum of entropies for each type of molecule, supplied in each case the necessary division by $N!, N'! \dots$ etc. (but with a remaining overall factor N^* - a number, however, that did not change, so contributing only a constant to the entropy). As a result one has extensive molecular entropy functions (albeit a non-extensive entropy function for the atoms), and can determine the equilibrium concentrations of the molecules accordingly.

Van Kampen's argument was similar, and in certain respects simpler; let us go into this in more detail. Consider a gas of N^* particles, defined as a canonical ensemble with the probability distribution:

$$W(N^*, q, p) = f(N^*)e^{-\beta H(q, p)}$$

where (q, p) are coordinates on the $6N^*$ dimensional phase space for a system of N^* particles (with f as yet undetermined). Let the N^* particles lie in volume V^* , and consider the probability of finding N with total energy E in the sub-volume V (so $N' = N^* - N$ are in volume $V' = V^* - V$). Assuming the interaction energy between particles in V' and V is small, the Hamiltonian H_{N^*} of the total system can be written as the sum $H_N + H_{N'}$ of the two subsystems. It then follows that the probability density $W(N, q, p)$ for having N particles at the point $(q, p) = q_1, p_1; q_2, p_2; \dots; q_N, p_N$ should be calculated by the procedure:

first select N out of the N^* particles to be located in V , and then integrate over all possible locations of the remaining $N^* - N$ particles in V' , and repeat, allowing for different selections. The result is:

$$W(N, q, p) = \text{const.} \binom{N^*}{N} e^{-\beta H_N(q, p)} \int_{V'} e^{-\beta H_{N'}(q', p')} dq' dp'. \quad (1)$$

If one now goes to the limit $N^* \rightarrow \infty$, $V' \rightarrow \infty$, at constant density N^*/V' one obtains the grand canonical distribution:

$$W(N, q, p) = \text{const.} \frac{z^N}{N!} e^{-\beta H_N(q, p)}$$

where z is a function of that density and of β , with the required division by $N!$.

In this derivation the origin of the $1/N!$ is clear; it derives from the binomial in Eq.(1) in the limit $N^* \rightarrow \infty$, i.e.:

$$\binom{N^*}{N} = \frac{N^*!}{(N^* - N)!N!} \rightarrow \frac{N^{*N}}{N!}$$

where the integral over the volume V' supplies a further factor $V'^{N^* - N}$ (i.e. $\text{const.} V'^{N^* - N}$). Thus it is only *because* permutations of the N^* particles yield physically distinct states of affairs that one must divide through by $N!$ (to factor out permutations that do not interchange particles inside V , with those in V'). The same point applies to the model of Ehrenfest and Trkal: it is only *because* permutations of atoms are assumed to lead to distinct states of affairs, that one must factor out permutations that merely swap atoms among the same species of molecules.

Why so much work for so little reward? Why not simply assume that the classical description be permutable (i.e. that points of phase space related by a permutation of all N^* particles represent the same physical situation)? That, essentially, is what Gibbs' proposed. Van Kampen considered the matter in the following terms:

One could add, as an aside, that the energy surface can be partitioned in $N!$ equivalent parts, which differ from one another only by a permutation of the molecules. The trajectory, however, does not recognize this equivalence because it cannot jump from one point to an equivalent one. There can be no good reason for identifying the Z -star [the region of phase space picked out by given macroscopic conditions] with only one of these equivalent parts. (van Kampen 1984, p.307).

(I shall come back to this argument somewhat later.) Gibbs' views to the contrary he found 'somewhat mystical' (van Kampen 1984, p.304). Moreover:

Gibbs argued that, since the observer cannot distinguish between different molecules, "it seems in accordance with the spirit of the

statistical method" to count all microscopic states that differ only by a permutation as a single one. Actually it is exactly opposite to the basic idea of statistical mechanics, namely that the probability of a macrostate is given by the measure of the Z-star, i.e. the number of corresponding, macroscopically indistinguishable microstates. As mentioned...it is impossible to justify the $N!$ as long as one restricts oneself to a single closed system. (van Kampen 1984, p.309).

These are the incoherence arguments, as we have them from van Kampen.

The dispensability argument can be challenged head on. The extensivity of the entropy, if it can be secured, even for contexts in which it has no direct experimental meaning, hardly counts against the metaphysics, or philosophical point of view, or physical interpretation, that underwrites it; for it is in all cases desirable. Certainly it is possible to define a classical thermodynamic entropy function that is extensive, and demarcates precisely the thermodynamically allowed transformations of initial into final states (whether or not by a quasi-static process), even for closed systems, let the statistical mechanical account of it fall where it will. Using the methods just outlined, one will be hard put to identify the 'ur-particles', whose number strictly does not change, in all physical cases. The analysis of Ehrenfest and Trkal relied on the immutability of atoms, but why not, as countenanced by Lieb and Yngvason (1999 p.27), contemplate nuclear interactions as well? So long as initial and final states are comprised of non-interacting systems, classically describable, each in a (thermal) equilibrium state, classical thermodynamical principles apply, however violent (and non-classical) the transformations that connect them. These principles ensure the existence of entropy functions, additive and extensive for each constituent classical equilibrium subsystem, now matter how various. There is no difficulty in defining the latter, in classical statistical mechanics, assuming permutability, but it is far from clear how this may be achieved (or even that it should be possible at all) when it is rejected, and one restricts oneself to the methods of Ehrenfest, Trkal, and van Kampen.⁶

Van Kampen's incoherence arguments were more rhetorical. It is true that unobservability *per se* is not a good reason, in statistical mechanics, for identifying microscopic configurations; but Gibbs said only that 'if the particles are regarded as indistinguishable, it seems in accordance with the statistical method...' (Gibbs 1902, p.187), and for a further indication of what he meant by the latter, his conclusion was that 'the question is one to be decided in accordance with the requirements of practical convenience...' (p.188). Gibbs spoke as a pragmatist, not as a positivist, nor as someone muddled on method.

I will approach van Kampen's remaining incoherence argument indirectly. Alexander Bach, whose monograph *Indistinguishable Classical Particles* goes a long way to rehabilitating the concept of indistinguishability in classical statistical mechanics, voiced a related objection that he himself found compelling.

⁶Elements of this argument are due to Justin Pniower (see his (2006) for his somewhat stronger claim that extensivity is indeed an empirically falsifiable principle).

As a result, he thought it important to distance his concept of classical indistinguishability from this other, indefensible kind. The latter takes particle indistinguishability all the way down to the microscopic details of individual particle motions, whereas, according to Bach, it ought to concern only statistical descriptions (probability measures). In the sense Bach intended this restriction, it is simply not the same concept as indistinguishability in quantum mechanics,⁷ which does go all the way down to the microscopic level and the details (such as they are) of individual particle motions. If Bach were right on this point, the concepts of classical and quantum indistinguishability would differ fundamentally.

Bach is led to this view because:

Indistinguishable Classical Particles Have No Trajectories.

The unconventional role of indistinguishable classical particles is best expressed by the fact that in a deterministic setting no indistinguishable particles exist, or - equivalently - that indistinguishable classical particles have no trajectories. Before I give a formal proof I argue as follows. Suppose they have trajectories, then the particles can be identified by them and are, therefore, not indistinguishable. (Bach 1997 p.7).

Bach's formal proof proceeds by identifying the coordinates of such a pair (in 1-dimension) as an extremal of the set of probability measures $M_+^1(\mathbf{R}^2)$, from which the 'diagonal' $D = \{ \langle x, x \rangle \in \mathbf{R}^2, x \in \mathbf{R} \}$ is deleted (because the particles are assumed impenetrable); and by characterizing classical indistinguishability as a feature of the state, namely states in $M_{+,sym}^1(\mathbf{R}^2)$, whose extremals are of the form

$$\mu_{x,y} = \frac{1}{2} (\delta_{\langle x,y \rangle} + \delta_{\langle y,x \rangle}), \quad \langle x,y \rangle \in \mathbf{R}^2 \setminus D$$

(i.e. states concentrated on the points $\langle x,y \rangle, \langle y,x \rangle, x \neq y$). It concludes with the observation that no such symmetric state is an extremal of $M_+^1(\mathbf{R}^2)$, hence no such state assigns coordinates to the particle pair.

But why not say instead that the coordinates of classical indistinguishable particles on the contrary attach to points in the *reduced state space*? I.e., for two particles in 1-dimension, they are extremals not of $M_+^1(\mathbf{R}^2)$, but of $M_+^1(\mathbf{R}^2/\Pi_2)$, where \mathbf{R}^2/Π_2 is the space obtained by identifying points in \mathbf{R}^2 that differ only by a permutation. Passing to this quotient space defeats Bach's formal argument. This can also provide a starting point for the definition of the quantum theory of indistinguishable particles (by quantization on the reduced configuration space).⁸

⁷It is closer to De Finetti's concept of 'exchangeability', called 'purely classical' by van Fraassen (1991, p.414).

⁸As shown by Leinaas and Myrheim (1977). It is of interest that of the two objections to Schrödinger's use of functions on configuration space made by Einstein at the fifth Solvay conference, one of them was that points related by permutations were not identified (Einstein (1928); whether he would have welcomed Leinaas and Myrheim's clarification is not so clear).

We should locate clearly our point of difference with Bach. His argument that identical particles cannot have trajectories, for otherwise particles would be identifiable by them, was intended to show not that classical indistinguishability makes no sense, but that it only makes sense if description- relative (and, indeed, is restricted to a level of description that does not describe individual trajectories). Hence his equation:

$$\text{Indistinguishability} = \text{Identity of the Particles} + \text{Symmetry of the State (Bach 1997 p.8).}$$

We can agree with Bach that indistinguishability is a matter of the symmetry (permutability) of the description, but not with his further point, that a symmetric description is impossible if it is so detailed as to specify the trajectories. For why not allow that an equivalence class of trajectories in configuration space (under the equivalence relation ‘is a permutation of’) indeed specify a single trajectory? - not, of course, in configuration space, but in *reduced* configuration space. We are clearly going round in circles.

The case against classical indistinguishability that it is unnecessary is moot, that it is incoherent is question-begging. What of van Kampen’s criticism that it is unmotivated? Granted that macroscopic unobservability is not in general a condition, in statistical mechanics, for identifying putatively distinct states, there remains another condition which is - which is in fact much more universal. Indeed, it can be formulated and applied across the gamut of physical theories. The condition is this: insofar as permutations are mathematical symmetries of the equations, adequate to a given set of applications (for a closed system), then they should be treated just like any other group of symmetries - that is, points in the state space for such systems related by the symmetry transformation should be identified (and we should pass to the quotient space). This point is a familiar one in the context of space-time symmetries, for example translations in space, where the quotient space is the space of relative distances. Why not treat permutations just like any other symmetry group, and factor them out accordingly?⁹

3 Demystifying Classical indistinguishability

The answer, presumably, is that we surely *can* single out classical particles uniquely, by reference to their trajectories. But there is a key objection to this line of thinking: *so can quantum particles, at least in certain circumstances, be distinguished by their states*. No matter whether the state is localized or not, the ‘up’ state of spin, for example, is distinguished from the ‘down’, and may well be distinguished in this way over time. In such cases, particle properties can be used as names.¹⁰

⁹For the general method and its rationale, see my (2003a,b).

¹⁰This point was earlier made by Shankar (1980 p.283-88, particularly p.284; I am grateful to Antony Valentini for bringing this to my attention). I will come back to this matter in the final section.

In the case of fermions it might even be thought that such an identification is always possible. Thus Pauli recounts his discovery:

On the basis of my earlier results on the classification of spectral terms in a strong magnetic field the general formulation of the exclusion principle became clear to me. The fundamental idea can be stated in the following way: The complicated numbers of electrons in closed subgroups are reduced to the simple number one if the division of the groups by giving the values of the four quantum numbers of an electron is carried so far that every degeneracy is removed. An entirely non-degenerate energy level is already ‘closed’, if it is occupied by a single electron: states in contradiction with this postulate have to be excluded. (Pauli 1946 p.29).

Electrons may be simply identified by their quantum numbers (and as such the permutations have nothing to act upon). As Stachel (2002) has recently remarked, extending Einstein’s famous ‘hole argument’ to a purely set theoretic setting (whereupon the symmetries analogous to diffeomorphisms are permutations), one can talk of the pattern positions themselves as the objects (which are not themselves permutable, no more than sets of quantum numbers); he too recommends that we identify electrons by their quantum numbers.

This amounts to identifying electrons as 1-particle states. Of course there are plenty of situations where (because energy degeneracies are *not* always eliminated) this does not suffice to point to any unique electron,¹¹ but these concern further symmetries, unrelated to permutations *per se*; symmetries which may also be present in the classical case and lead to exactly the same difficulty (in such situations one cannot refer to a unique classical trajectory either).¹² The real distinction in the two cases is this: in quantum mechanics, given an (anti)symmetrized state constructed from a given set of orthogonal vectors $\{\varphi_k\}$, $k = 1, \dots, N$, one can individuate one particle from the remaining $N - 1$ by its state, and one can in principle, when the Hamiltonian factorizes, track the time evolution of that particle (that state); but nothing comparable is possible if the state is a *superposition* of such (anti)symmetrized states.

That marks a profound difference from the classical case, but it does not affect the comparison we are concerned with: a state of definite occupation numbers is nevertheless permutation invariant, and the particles it describes are still indistinguishable. To this the classical analog is clear: just as we may speak of quantum states, rather than particles having states, so we may speak of classical trajectories, rather than particles having trajectories. But equally, if we do talk of the particles (that may be in various states, or have various trajectories), that are otherwise the same, then we should do so identifying permutations of particles among states, for there is no further fact as to which

¹¹A point recently made by Pooley (2006); here Pooley also argues against the similarity of permutation symmetry in quantum mechanics with general covariance in space-time theories, a point I shall come back to at the end.

¹²But in every case one can still *discern* between the electrons, or classical particles; see my (2003a, 2003b, and particularly 2006) for further discussion.

particle is in which state, or which has which trajectory, relevant to the dynamics. Returning to Van Kampen's 'incoherence' argument that 'the trajectory... does not recognize this equivalence because it cannot jump from one point to an equivalent one' draws its effect, so far as it goes, from mixing the two kinds of description. Our conclusion, again: indistinguishability (permutability, invariance under permutations) makes just as much sense classically as it does in quantum mechanics.

The matter can even be pushed as a point of logic, for the requirement of indistinguishability, understood as permutability, would seem to make no difference to a language that is void of proper names. Help yourself to such a language, bracketing for the time being any scruples you may have on its ultimate adequacy; then you are in much the same position as if you had restricted yourself to complex predicates totally symmetric in all of their arguments (at least if you restrict attention to finite numbers of objects). For it can be proved (see the Appendix):¹³

Theorem: Let \mathcal{L} be a first-order language without any proper names (0-ary function symbols). Let T be any \mathcal{L} -sentence satisfiable only in models of cardinality N . Then there is a totally symmetric predicate $Gx_1...x_N \in \mathcal{L}$ such that $\exists x_1...\exists x_N Gx_1...x_N$ is logically equivalent to T .

The intuitive point is indeed very simply made if we consider only purely existential sentences, like $\exists x_1...\exists x_N Fx_1...x_N$, which is obviously logically equivalent to $\exists x_1...\exists x_N \bigvee_{k=1}^N Fx_1...Fx_N$, whatever the predicate F . And sentences like this are, one would have thought, perfectly sufficient to describe the configuration of a system of particles in space.

But that makes the restriction (if it amounts to no more than the renouncing of names) seem *purely* metaphysical (and note that it applies equally to descriptions of *non-identical* particles).¹⁴ Indeed, according to Huggett (1999), the principle of indistinguishability is none other than *antihaecceitism*, an old doctrine of scholastic philosophy, and one that is surely devoid of empirical significance. His conclusion was endorsed by Albert (2000):

There is a certain fairly trivial sense in which it ought to have been obvious from the outset (if we had stopped to think about it) that the facts of thermodynamics cannot possibly shed any light on the truth or falsehood of the doctrine of Haecceitism. The question of the truth or falsehood of the second law of thermodynamics is (after all) a straightforwardly empirical one; and the question of Haecceitism, the question (that is) of whether or not certain observationally identical situations are identical simpliciter, manifestly

¹³For further discussion, see Saunders (2006).

¹⁴If 'essential' attributes of particles - charge and mass and so on - were also specified by the state, one would indeed have a theory in which all particles whatsoever are permutable. (I shall come back to this point in Section 5.)

is not. Nevertheless, it might have turned out that the statistical-mechanical account of thermodynamics is somehow radically simpler or more natural or more compelling or more of an explanatory success when expressed in a Haeccisstic language than it is when expressed in a non-Haeccisstic one. And the thing we've just learned (which seems to me substantive and non-trivial and impossible to have anticipated without doing the work) is that that is not the case. (p.47-48)

But whilst I have some sympathy with Huggett's equation, it should be obvious from the discussion of Section 2 that all is not well with this way of putting it. To suggest, as Huggett did (citing van Kampen (1984)), that extensivity of the entropy is only a 'convention', is clearly unsatisfactory. But that to one side, it is obvious that permutability *can* have empirical question, indeed *straightforward* empirical consequences, for if the state-space is Hilbert space, rather than phase space, it forces a change in particle statistics! How can a change in metaphysics have that consequence? And with that we are back to our puzzle: What is responsible for the difference between quantum and classical statistics?

Permutability, we should conclude, is not a metaphysical principle, nor merely a convention; if not in itself an empirical claim, it makes a contribution to others that are. But we need not pursue the question of the precise status of this principle, given only that it makes classical sense; whereupon we are returned to the puzzle as stated.

4 The explanation for quantum statistics

To begin at the beginning:

The distribution of energy over each type of resonator must now be considered, first, the distribution of the energy E over the N resonators with frequency ν . If E is regarded as infinitely divisible, an infinite number of different distributions is possible. We, however, consider - and this is the essential point - E to be composed of a determinate number of equal finite parts and employ in their determination the natural constant $h = 6.55 \times 10^{-27}$ erg sec. This constant, multiplied by the frequency, ν , of the resonator yields the energy element $\Delta\epsilon$ in ergs, and dividing E by $h\nu$, we obtain the number P , of energy elements to be distributed over the N resonators. (Planck 1900 p.239).

It is noteworthy that permutability (indistinguishability) seemed perfectly natural to Planck in this setting: for why distinguish situations in which one entity is allocated to one resonator, rather than another, when the entity is merely an 'energy element'? Boltzmann likewise identified permutations of energy elements, both in his 1868 derivation and that of 1877 (using the combinatorics

factor below) - but differed in the crucial respect that he took the limit in which the energy elements went to zero.¹⁵

How many ways can P energy elements be arranged among N resonators? This question is important, if each such arrangement is equiprobable (as we assume). Call the number W_I . For it Planck took from Boltzmann the expression:

$$W_I = \frac{(P + N - 1)!}{P!(N - 1)!}. \quad (2)$$

For (this derivation is due to Ehrenfest) an arrangement can be given as a sequence of symbols (where $n_i \in \{0, 1, 2, \dots, P\}$, $i = 1, \dots, N$):

$$\underbrace{p \dots p}_{n_1} \underbrace{p \dots p}_{n_2} | \dots | \underbrace{p \dots p}_{n_N}$$

of which there are P symbols ‘ p ’ in all (so $\sum_{s=1}^N n_s = P$), and $N - 1$ symbols ‘ $|$ ’. Given such a sequence one can say exactly how many energy elements n_k are in the k -th cell (the ‘occupation number’ of each cell), but not which energy element is in which cell. If no such further facts are either relevant or available, W_I is then simply the number of distinct sets of occupation numbers (of distinct arrangements, in Planck’s sense). There are $(P + N - 1)!$ permutations of $P + N - 1$ symbols in all, but of these, those which only shuffle ‘ p ’s among themselves, or ‘ $|$ ’s among themselves, do not give us a new set of occupation numbers; so we must divide by $P!$ and by $(N - 1)!$.

Now adopt a change in notation and physical picture; let the P quanta be called ‘particles’, and the N oscillators ‘cells in phase space’, with the new notation ‘ N ’ and ‘ C ’ respectively. The question may now seem to arise as to which particle is in which cell; to which the answer is there are C^N possible choices, one for each set of ‘occupation numbers’ n_k , $k = 1, \dots, C$, with a multiplicity to allow for permutations of particles among different cells. In this time-honoured way obtain:¹⁶

$$W_D = C^N = \sum_{\substack{\text{occupation numbers} \\ \text{s.t. } \sum_{s=1}^C n_s = N}} \frac{N!}{n_1! \dots n_C!}. \quad (3)$$

There is one further complication: the numbers C , N are associated with particular regions of phase space, parameterized by other variables (usually the

¹⁵I refer to Bach (1990) for a detailed study of this history.

¹⁶The second equality is obvious by inspection. It is a special case of a more general theorem (the multinomial theorem), which says that for arbitrary quantities $z_1 \dots z_C$

$$(z_1 + \dots + z_C)^N = \sum_{\substack{\text{all } C\text{-tuples of integers } n_1 \dots n_C \\ \text{s.t. } \sum_{s=1}^C n_s = N}} \frac{N! z_1^{n_1} \dots z_C^{n_C}}{n_1! \dots n_C!}$$

As defined below, Boltzmann’s count of complexions is obtained for $z_1 = \dots = z_C = 1$, his volume measure for $z_1 = \dots = z_C = \tau$. (For this and the combinatoric expressions that follow, see e.g. Rapp (1972).)

energy). Thus, in the case of the Planck distribution, by the frequency (so C_k cells in the k^{th} frequency range, etc.). If E_k is the energy of the k^{th} region, then C_k is the corresponding degeneracy (number of cells all in that energy range). Let N_k particles lie in region k , and let there be j regions in all; then the constraint on the total energy E_{tot} is that $\sum_{k=1}^j N_k E_k = E_{tot}$; if particle number too is conserved (with N_{tot} particles in all), then the total number of arrangements is:

$$W_D = \sum_{\substack{\text{sequences } N_1 \dots N_j \\ \text{s.t. } \sum_{s=1}^j N_s = N_{tot}, \sum_{s=1}^j N_s E_s = E_{tot}}} \frac{N_{tot}!}{N_1! \dots N_j!} \prod_{k=1}^j C_k^{N_k}. \quad (4)$$

Here the permutation factors arise as distinct regions of phase space correspond to different choices as to which N_1 (of N_{tot} particles) are assigned to region 1 (with C_1 cells), which N_2 (of $N_{tot} - N_1$) to 2 (with C_2 cells), and so on.

Eq(4) was also written down by Boltzmann, in his 1877 memoir. Contrast it with the analogous expression for W_I :

$$W_I = \sum_{\substack{\text{sequences } N_1 \dots N_j \\ \text{s.t. } \sum_{s=1}^j N_s = N_{tot}, \sum_{s=1}^j N_s E_s = E_{tot}}} \prod_{k=1}^j \frac{(N_k + C_k - 1)!}{N_k! (C_k - 1)!}. \quad (5)$$

It is a spurious simplification of (4) to suppose that the degeneracy of each energy E_k is unity (i.e. $C_k = 1$ for each k), and to go on to identify the N_k 's with the occupation numbers n_k of (3). Under that assumption, the limiting agreement between (4) and (5) is rather hard to see. It is that in the limit in which $C_k \gg N_k$

$$\frac{(N_k + C_k - 1)!}{N_k! (C_k - 1)!} \approx \frac{C_k^{N_k}}{N_k!} \quad (6)$$

whereupon $W_I \approx W_D / N_{tot}!$. But away from this limit, the two are quite different; moreover, the quantity $W_D / N_{tot}!$ is not a combinatorial count of anything (it is not an integer). Rather, we should interpret it as the expression:

$$\frac{W_D}{N_{tot}!} = \frac{1}{\tau^{N_{tot}}} \sum_{\substack{\text{sequences } N_1 \dots N_j \\ \text{s.t. } \sum_{s=1}^j N_s = N_{tot}, \sum_{s=1}^j N_s E_s = E_{tot}}} \prod_{k=1}^j \frac{(C_k \tau)^{N_k}}{N_k!} \quad (7)$$

i.e. *the reduced phase space volume* (in the units $\tau^{N_{tot}}$) corresponding to the stated constraints on N and E . Each term in the product is the reduced N_k -particle phase space volume corresponding to the C_k cells in the 1-particle phase space, each of volume τ .

The breakdown of the approximation (6) is responsible for the entire difference between classical and quantum statistical equilibria. Evidently to understand it we need only investigate it for a single (arbitrary) phase space region k

(so from this point on we drop the subscripts on ' N_k ' and ' C_k '.) For low numbers the approximation can be illustrated graphically. We take the simplest case of $N = 2$ particles in 1-dimension (so with a 4-dimensional phase space). First the distinguishable case.

Distinguishable particles Divide each 1-particle phase space into C cells, as did Boltzmann, say $C = 3$. Then there are $C^N = 3^2 = 9$ different ways the two particles can be distributed in this region of the 2-particle phase space. Thus, supposing the two particles are named ' a ', ' b ', the region in Γ^2 corresponding to the arrangement in which particle a is in cell 2 and particle b is in cell 3 is the region shaded (Fig.1, suppressing one dimension of Γ^1). If each such arrangement is equiprobable, one obtains Maxwell-Boltzmann statistics.

Fig.1: Distinguishable particles

On the alternative way of putting it, in terms of phase space volume, if τ is the size of each cell in the 1-particle phase space, the volume of the N -particle phase space is $(C\tau)^N$, or τ^N times the number of all arrangements of N particles in Γ^N . That is, we may equally take Boltzmann's thermodynamic probability as phase space volume (Boltzmann himself often spoke of it in this way).

These two quantities, the count of arrangements, and their corresponding volume, are quite generally proportional to the count of available states in the Hilbert space of N distinguishable particles, corresponding to the classical phase space region Γ^N , each with a 1-particle Hilbert space of C dimensions. For $N = 2$ and $C = 3$, as above, we have a 2-particle Hilbert space $H^2 = H^1 \otimes H^1$, where each 1-particle space is spanned by three orthogonal states $\varphi_1, \varphi_2, \varphi_3$. There are again $C^N = 3^2 = 9$ orthogonal two-particle states spanning H^2 .

There is therefore one orthogonal 2-particle state in H^2 (represented by dots in Fig 1) for each arrangement in Γ^2 , each with the same phase-space volume by Boltzmann's assumption, and each with the same Hilbert space measure (counting each state - each dot - as equiprobable). Under these assumptions, the measures on the quantum and classical state spaces are the same (the number of dots is proportional to the total phase space volume); so distinguishable quantum particles also obey Maxwell-Boltzmann statistics. Of course there are other important differences, notably, that the entropy no longer has an arbitrary additive constant (corresponding to the arbitrary choice of the unit τ); its value at absolute zero, in particular, is $Nk \ln C_0$, where C_0 is the dimensionality of the subspace of lowest energy E_0 . For another, that for sufficiently small temperatures, only particles in the lowest energy states contribute to the specific heats of solids (as discovered by Einstein in 1907) - but this bares more on the discreteness of the spectrum of the energy.

Indistinguishable particles In the case of classical indistinguishable particles we should use instead the measure on the reduced phase space for this region, i.e. Γ^N/Π_N . For $N = 2$, $C = 3$ this amounts to going over to Fig.2.

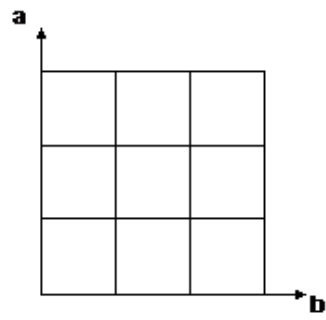


Fig 1: Distinguishable particles

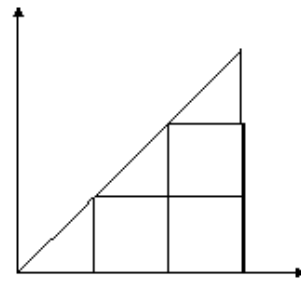


Fig 2: Indistinguishable particles

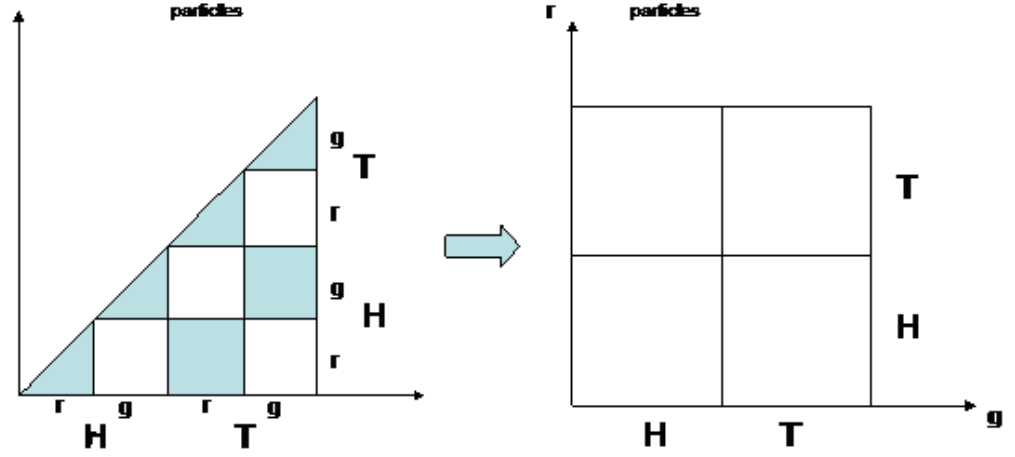


Fig 3: Recovering distinguishable particles

The volume goes down from $(C\tau)^N$ to $(C\tau)^N/N!$; the number of arrangements *also* goes down, *but not by the same factor*. And correspondingly, the volume of each arrangement is no longer the same.

Fig.2: Indistinguishable particles

This point is obvious by inspection of Fig.2: the volume is now $(C\tau)^N/N! = 9\tau^2/2$, but of course there are not $C^N/N! = 4.5$ arrangements - rather, there are precisely six different ways of distributing two indistinguishable particles over three 1-particle cells, without regard for which is in which cell (one for each dot in Fig.2); but clearly the reduced phase space volumes of three of the arrangements are twice those of the others (the ones along the diagonal - this is why in the classical case the statistics remains Maxwell-Boltzmann. Only the ratios of volumes, the relative probabilities, matter to the statistics). Suppose $C = 2$ (so ignoring the top row and rightmost column); take in illustration two fair coins, with the two regions of the 1-coin phase space labelled ‘ H ’ and ‘ T ’ respectively; then the outcome $\{H, T\}$ (an unordered pair) is twice as likely as either $\{H, H\}$ or $\{T, T\}$, just as for distinguishable coins.

The count of distinct arrangements is given by (2). In quantum mechanics, this count goes over (for any basis) to the count of orthogonal totally symmetrized states in Hilbert space (the dimension of the subspace corresponding to the macroscopic constraints). In our example, this includes the 3-dimensional subspace spanned by the vectors $\varphi_k \otimes \varphi_k$, $k = 1, 2, 3$, as before, but now directly summed not with the 6-dimensional subspace spanned by $\varphi_k \otimes \varphi_j$, $j \neq k$, but with the 3-dimensional subspace spanned by $\varphi_k \otimes \varphi_j + \varphi_j \otimes \varphi_k$, $j \neq k$. The crucial difference with the classical case is: *there is no other measure on the state space but this*. And using this measure, the diagonals must have the *same* probability as the off-diagonals - therein lies the difference with classical theory (and the reason why, for two quantum coins, the probabilities for $\{H, T\}$, $\{H, H\}$. and $\{T, T\}$ are all the same). Arriving at a quantity like $(C\tau)^N/N!$, rather than $(N + C - 1)!/N!(C - 1)!$, is not an option.¹⁷

It is worth making this point again in terms of the occupation numbers. For each arrangement of distinguishable particles, there are $N!/n_1! \dots n_C!$ distinct assignments of the N particles over C cells, so as to place n_k in cell k , $k = 1, \dots, C$. The total number of such arrangements is C^N , which as we have seen (Eq.(3)) is:

$$\sum_{\substack{\text{occupation numbers} \\ \text{s.t. } \sum_{k=1}^C n_k = N}} \frac{N!}{n_1! \dots n_C!} = C^N.$$

Why then, if division by $N!$ compensates for unwanted permutations, do we not obtain in this way the same result as did Planck? In fact, to get the Planck

¹⁷My thanks to David Wallace for conversations on this point.

expression, the factor $N!/n_1!...n_C!$ is set equal to *one*, weighting each Planck arrangement the same, thus obtaining:

$$\sum_{\substack{\text{occupation numbers} \\ \text{s.t. } \sum_{k=1}^C n_k = N}} 1 = \frac{(N + C - 1)!}{N!(C - 1)!}.$$

That you should not do, thinking classically, if the volume is what matters, and you are going to identify phase space points related by permutations. For in that case the volumes of arrangements along the diagonals of the reduced phase space (with occupation numbers greater than one) should be weighted *less* than all the rest. Since the relative weights are all that matter to the statistics, using the factor $N!/n_1!...n_C!$, or $1/n_1!...n_C!$ (dividing by an overall factor of $N!$, as one should), makes no difference. In comparison to this, quantum mechanics, *weighting them equally*, increases their weights in comparison to their classical values. This explains the comment, often made, that particles obeying Bose-Einstein statistics have the tendency ‘to condense into groups’ (Pauli 1973 p.99).

Quantum mechanically there is no volume measure, and no reason to weight one set of occupation numbers differently from any other. Classically, one might think *both* options are on the table: the (integral) count of arrangements, given by Planck’s expression, of the (non-integral) volume of reduced phase space, as given by the corrected Boltzmann expression. But the former, unlike the latter, depends crucially on the size of the elementary volume τ ; if there is to be a departure from classical statistics on this basis, it will require the existence of a fundamental unit of phase space volume, with the dimensions of action.¹⁸ The very discrepancy between Planck’s expression and the reduced phase space volume (the quantity $C^N/N!$) disappears, as it must, as $\tau \rightarrow 0$, as inspection of Fig.2 makes clear (imagine the triangle partitioned into much *smaller* squares; then the number of states - the number of dots - *is* approximately proportional to the area). Planck’s combinatorial count, multiplied by τ^N , is an increasingly good approximation to the volume as τ (or equivalently $1/C^N$) become small; that is just the condition $C \gg N$ considered previously, under which quantum and classical statistics agree (where on average no particle has the same 1-particle energy as any other, and the arrangements along the diagonal are on average unoccupied).

For the same reason Fermi-Dirac statistics are undefinable in classical terms, for the condition that no two particles are in the same 1-particle state, or in the same cell of the 1-particle phase space, is only physically meaningful if these cells have a definite size.¹⁹

¹⁸A consideration that applies to those, like Costantini (1987), who have claimed to explain quantum statistics in classical terms.

¹⁹Note added in proof: a condition that can, admittedly, be formulated independent of quantum mechanics (e.g. in terms of a lattice theory); as explained by Gottesman (2005).

5 Addenda

What explains the difference between classical and quantum statistics? The structure of their state spaces: in the quantum case the measure is discrete, the sum over states, but in the classical case it is continuous.²⁰ This makes a difference when one passes to the quotient space under permutations, as we should for particles intrinsically alike.

Our purely logical theorem, that shows all objects (of any finite collective) to be permutable (in a language without names) is evidently much broader in scope, for there is no restriction to objects intrinsically the same. There is also another long-standing tradition (due to Feynman 1965), that explains the distinction between classical and quantum statistics in terms of the possibility (or lack of it) of reidentification of particles over time. Both raise questions over the adequacy of the explanation so far given.

In fact the two are connected. For let us suppose that distinct particles may be labelled by distinct properties that are constant in time. Take again the example of two coins ($N = C = 2$), but suppose now that one coin is red (r) and the other green (g). Taking ‘red’, ‘green’ as proper names, one has distinguishable coins that may take on one of two phases (H or T), and correspondingly one has the unreduced phase space, similar to Fig.1, but now a 2 by 2 grid (with ‘ r ’ and ‘ g ’ in place of ‘ a ’ and ‘ b ’). But assimilating colours to the phases instead, we pass to the case $C = 4$ (the four phases $\{H, r\}$, $\{H, g\}$, $\{T, r\}$ and $\{T, g\}$), and the coins are again indistinguishable (with the reduced phase space of Fig.3). However, supposing the colours are stable under each toss of the coins, certain cells of the 2 particle phase space are inaccessible (those in which the two coins have the same colour, the regions shaded); the *effective* phase space for the 2 indistinguishable coins, consisting of the unshaded boxes in Fig.3, gives back the original unreduced phase space (where the colours function as names).

Fig.3: Recovering distinguishable particles

This explains why differences in intrinsic particle properties (such as mass, spin and charge), stable in time, are grounds for treating them as distinguishable (with no need to (anti)symmetrize in Hilbert space). The point about identification over time also falls into place; whatever the criterion for each particle, it is *ex hypothesis* stable in time and shared with no other. In the classical case, where there are definite trajectories, one can construct such properties by reference to a location in phase space at a given time (so that a particle has that property if and only if its trajectory passes through that location at that time). Quantum mechanically, for a state of non-interacting particles of definite occupation numbers at a given time (all of them 0’s and 1’s), the procedure

²⁰I do not suppose quantum interference phenomena more generally, traceable to (anti)symmetrization of the state, are similarly explained (I am grateful to Lee Smolin and Rafael Sorkin for pressing this point upon me).

is the same (the orbits of 1-particle states replace trajectories); or, alternatively, in the solid state, taking particles as lattice-sites, identifying particles by their position over time. In any of these ways one can pass to a phase space or Hilbert space description in terms of distinguishable particles, subject to Maxwell-Boltzmann statistics, and a non-extensive entropy. The utility of any such description, however, will depend on the ingenuity of the experimenter, to define operational conditions for the reidentification of such particles over time. In the solid state such conditions are plain; identification in terms of states is also possible in the high-temperature limit (occupation numbers all 0's and 1's), where talk of 1-particle states amounts to talk of modes of the quantum field (with excitation numbers all 0's or 1's) - this goes some way to explaining Schrödinger's result, that one can treat Bose-Einstein particles in terms of waves obeying Maxwell-Boltzmann statistics.²¹ For the classical example where one reidentifies particles over time by their trajectories, one needs more fanciful conditions, say a Maxwell's demon able to keep track of the individual molecules of a gas over time; that makes clear why one ought to have an entropy of mixing, and hence a non-extensive entropy function, in such circumstances.

A final comment. It may be objected that the treatment of indistinguishability is different in quantum mechanics than classically, and different from other classical symmetries like general covariance, precisely because one does not, in quantum mechanics, and parastatistics to one side, take an equivalence class of states as representing the physical situation; one takes instead the symmetrized state, itself a vector in the unreduced state space. Nothing comparable is available classically.

It is true that classical and quantum mechanics differ in this respect: classically only very special states in the reduced state space are also to be found in the unreduced space (and none at all if, following Bach, the diagonals are omitted). But the more general point, that classically one works not with a single invariant state, but with an equivalence class of states in the unreduced state space, I take to be a reflection of something still more fundamental: it is that whilst in both cases one can pass to the quotient space, only in quantum mechanics is the topology preserved unchanged (the space of symmetrized vectors is topologically closed, so it itself a subspace). The topology of the classical quotient space, under permutations, is in contrast enormously more complex than that of the unreduced space (and, omitting the diagonals, is not even topologically closed). Easier, then, classically, to work in the unreduced state space, taking the equivalence class of points as representative of the physical situation.²²

²¹And suggests a corresponding account of fermions.

²²As observed by Gibbs: 'For the analytical description of a specific phase is more simple than that of a generic phase. And it is a more simple matter to make a multiple integral extend over all possible specific phases than to make one extend without repetition over all possible generic phases.' (Gibbs 1902 p.188).

Appendix

Proof. Let \mathcal{L}^+ differ from \mathcal{L} only in the addition of countably many names a_1, a_2, \dots . The proof proceeds as follows: we construct a sequence of sentences A, T_1, T_2, T_S , where the last is of the desired form, where $T_1, T_2 \in \mathcal{L}^+$, and A is

the \mathcal{L}^+ -sentence $\bigwedge_{i,j=1, i \neq j}^N a_i \neq a_j \wedge \forall x \bigvee_{k=1}^N x = a_k$, satisfying:

- i) $T \wedge A \models T_1$
- (ii) $T_S \wedge A \models T_2$
- (iii) $\models T_2 \leftrightarrow T_1$
- (iv) $T_1 \models T$
- (v) $T_2 \models T_S$.

Since any sentence S in any first-order language has the same truth value in models that differ only in their interpretations of non-logical symbols not contained in S , the truth of T, T_S (which contain no names) in a model of \mathcal{L}^+ with universe V is independent of the assignment of names in \mathcal{L}^+ to elements of V . It then follows from (i) through (v) that $\models T \leftrightarrow T_S$. For suppose T_S is true in V ; if A is also true, then by (ii),(iii),(iv), T is true in V . Suppose T_S is true and A is false in V ; choose a model W identical to V save in the interpretation of symbols not in T_S, T , in which A is true (it will be clear from the construction of T_S that it only has models of cardinality N , so such a model can always be found). Then as before, T is true in W ; hence also in V . Thus $T_S \models T$. The proof that $T \models T_S$ uses (i), (iii), (v), but is otherwise the same.

It remains to prove (i) through (v). Define T_1 as $T \wedge A$ (so (i), (iv) follow immediately). Without loss of generality, let T be given in prenex normal form, i.e. as a formula $Q_n \dots Q_1 F x_1 \dots x_n$, $n \geq 1$, where each Q_i is either $\forall x_i$ or $\exists x_i$. Define a sequence of sentences $T^{(1)}, \dots, T^{(n)}$ by:

$$\begin{aligned} T^{(k)} &\stackrel{def}{=} Q_1 \dots Q_{n-k} G^{(k)} x_1 \dots x_{n-k} a_1 \dots a_N, \text{ for } 1 \leq k < n. \\ T^{(n)} &\stackrel{def}{=} G^{(n)} a_1 \dots a_N. \end{aligned}$$

The predicates $G^{(1)}, \dots, G^{(n)}$ are defined as follows. Let $[k]$ be \bigwedge if Q_k is $\forall x_k$, and otherwise \bigvee ; for any predicate $P x_1 \dots x_j \dots$, let $P x_1 \dots a_k \dots$ denote the result of replacing every occurrence of x_j in P by a_k . Then:

$$\begin{aligned} G^{(1)} x_1 \dots x_{n-1} a_1 \dots a_N &\stackrel{def}{=} [n]_{i=1}^N F x_1 \dots x_{n-1} a_i \\ G^{(k+1)} x_1 \dots x_{n-(k+1)} a_1 \dots a_N &\stackrel{def}{=} [n-k]_{i=1}^N G^{(k)} x_1 \dots x_{n-(k+1)} a_i a_1 \dots a_N, \text{ for } k+1 < n \\ G^{(n)} a_1 \dots a_N &\stackrel{def}{=} [1]_{i=1}^N G^{(n-1)} a_i a_1 \dots a_N. \end{aligned}$$

Evidently $G^{(1)}$ is totally symmetric in the a_k 's, and if $G^{(k)}$ is, so is $G^{(k+1)}$; hence, by induction on k , so is $G^{(n-1)}$; whereupon so also is $G^{(n)}$. The logical equivalences $A \models T \leftrightarrow T^{(1)}$, $A \models T^{(k)} \leftrightarrow T^{(k+1)}$ are obvious, hence, again by induction on k , $A \models T \leftrightarrow T^n$. Defining T_2 as $T^{(n)} \wedge A$, (iii) follows.

Now define T_S as the \mathcal{L} -sentence obtained by replacing every occurrence of a_k in T_2 (i.e. in $T^{(n)} \wedge A$) by x_k , $k = 1, \dots, N$, and preface the expression that results by N existential quantifiers. Obtain in this way:

$$\exists x_1 \dots \exists x_N \left(G^{(n)} x_1 \dots x_N \wedge \bigwedge_{i,j=1, i \neq j}^N x_i \neq x_j \wedge \forall x \bigvee_{k=1}^N x = x_k \right) \stackrel{\text{def}}{=} \exists x_1 \dots \exists x_N G x_1 \dots x_N$$

Then (v) is immediate. Since $G^{(n)}$ is totally symmetric, so is G , as required. It only remains to prove (ii). But in any model in which A is true and T_S is true, $G a_{\pi(1)} \dots a_{\pi(N)}$ is true for some choice of permutation π . Since G is totally symmetric, T_2 is true as well ■

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