Referee: 1

Comments to Author(s)
I'm afraid I still cannot recommend acceptance of this paper. To be honest I feel that only lip service has been paid to certain of my comments with no serious attempt to engage with them.

Actually, this Referee seems afraid to read the paper. His refusal to even acknowledge relevant new material in the revision reveals as ironic his complaint that the present author has paid only 'lip service' to his comments. Specific references to text in the revision, provided below, demonstrate that in fact all of his relevant comments were engaged and addressed. He may not like those refutations of his objections to the current work, but it cannot be fairly claimed that those objections were not engaged.

So, I still do not have a decent grasp on what the au means by a 'weakening' of haecceity - how can a form of 'primitive thinness' be weakened?!

The referee here offers up a visceral negative reaction, in the form of an incredulous rhetorical question, to terminology. But his aversion to the terminology is not a criticism of it, nor does it mean that the terminology hasn't been adequately defined in the revision. And in fact, it has been defined:

4. Quantum Haecceity

The above considerations remind us that the labels attributed to the quanta in product states such as $|A\rangle_1|B\rangle_2$ can be understood as having distinct referents, since exchanging the labels can be seen as changing the physical situation. This leads us to the conclusion that it is their haecceities, not their identical essences, that are being exchanged when the labels are permuted. However, clearly the quanta do not qualify as independent individuals in the usual classical sense, since we have exchange effects that result from a "blending" of these putative haecceities (quantitatively described by H_{Heis}, which demands symmetrization). Thus, we cannot be dealing with a classical sort of haecceity implying full-fledged individuality. Instead, we have what I would like to call *quantum haecceity* (QH), which involves a form of potentiality. While the latter notion is obviously a matter for further study, as a starting point we could note that a composite state in the form of a wavefunction $\Psi(x_1, x_2)$ such as

$$\Psi(x_1, x_2) = \frac{1}{\sqrt{2}}[\psi(x_1)\phi(x_2) + \psi(x_2)\phi(x_1)] \tag{7}$$

suggests that the indices in parameters x_1 and x_2 represent the potential for two distinct outcomes upon performing a measurement of position; whereas if there were only one quantum, we would have only one outcome. The labels or indices in (7) signify potentialities rather than actualities because there is no fact of the matter about any actual possession of a position-property by either of the quanta described by this state. Thus, in more quantitative terms, if the cardinality of the entanglement is N (i.e., we have N entangled systems), then we must have indices $i \in \{1, N\}$ to represent the set of possible outcomes ${o^j}_i, i \in \{1, N\}, j \leq N$ corresponding to observable(s) O^j measured locally on each of the quanta.

Now, the Referee may not like this definition. And/or he may not be comfortable with introducing the concept of potentiality here. But he cannot fairly claim that “QH” and its attendant weakening of classical haecceity has not been defined. Of course this would be a concept for further study and elaboration, and I make no claim that all possible questions about the nature of QH have been answered in this first paper on the subject. But it is simply not tenable for the Referee to pretend that the paper has not explained what is meant by a weakening of the usual classical notion. Moreover, this quantitative material is followed by a qualitative description of QH:

> Another, more qualitative way to get a sense of this notion of QH is in terms of organic systems. It is well known that trees of the same type, originating from distinct seeds, can merge into effectively a single entity (see Figure 2).
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> 
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> Figure 2. The organic quality of quantum haecceity.
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> The trees shown in Figure 2 have been deliberately coaxed into the forms shown, but trees in nature also naturally merge and separate given the appropriate conditions. The analogy with our current topic is that the saplings share identical essences but differing haecceities corresponding only to their cardinality: specifically, there are N saplings. On the left, two saplings have been planted and coaxed into joining to form a collective entity of the same essence, with the potential to diverge and reunite again, which has occurred. Without the haecceities corresponding to their number, they would have no potential to converge and to diverge as shown, since there would be

In the response the au states that 'it refers only to cardinality rather than a full, persistent form of transcendent identity'. However Krause and his collaborators have developed a framework for this issue that also retains a form of cardinality but in which quantum entities are not individuals at all. So quasi-haecceity can't *just* be about cardinality. I really do think further explication is required here.

Neither the original paper nor the revision say that QH is “just about cardinality”; to arrive at such a mistaken impression would require neglecting specific material such as the discussion around equation (7) pasted in above.

The Referee does not provide here any reference to Krause and his unnamed collaborators. I was able to find Krause and French (2007), whose findings do not conflict with the present paper. I have added at footnote to that effects, as follows:

"{Krause and French (2007) discuss cardinality in connection with quantum systems and conclude that cardinality alone does not confer individuality. The present study does not contradict this finding, since quantum systems under conditions involving symmetrization have only a quasi-individuality, in keeping with the weakening of haecceity accompanying QH. It is also important to notice, however, that cardinality alone is not the only feature that enters into QH; also needed is the possibility of distinguishing measurement, as discussed in relation to (7).} "

In any case, the paper makes quite clear that I am addressing the choice, formulated by Bigaj, between EE and EH. Clearly, EE doesn't survive the arguments in the paper–a point which has not been disputed by the Referee. If the Referee doesn't concur that the choice is between EE and EH, and doesn't like the terms "Haecceity" or "weakening of individuality" for the situation described qualitatively and quantitative above, he is welcome to suggest different terminology. But he can't reasonably claim that these terms have not been defined in the paper.

I did try to suggest some possible routes to that via the work of Howard and Toraldo di Francia but I fear the latter has misunderstood that work. So, TdF is cited (and only that) in the context of the transition from potentiality to actuality but that is not what I intended, nor is that what he is about: he maintains that quantum entities have a form of 'pseudo individuality' insofar as they appear to be individuals upon measurement (when we observe flashes on a scintillation screen say).

No, nothing was misunderstood, since the highlighted statement is very close to what is in fact described in the present manuscript. I admit to being incredulous that this point has somehow been missed by the Referee. Relevant material from the revision is copied below:

> distinguished by their detected properties, even if not by their indices. The indices, which index quantum haecceties (QH), reflected only the cardinality of the entanglement, which represents the potential for the number of outcomes upon measurement.[11] Upon measurement, the QH are transformed into specific distinguishable, contingent properties. This again reflects their status as indexing potentialities, not actualities.

Likewise Howard entertains a view according to which entanglement yields one 'global' quantum system that resolves into something akin to individual systems upon measurement. My intention in mentioning these was to encourage the au to reflect on them as possible foils or complements to this notion of quasi-haecceity. I do not think that has been done, explicitly or otherwise.

And again I admit to being mystified by this statement, since indeed **exactly that** was done, as demonstrated in the excerpt above. Additional material on this point is here:

Thus, in order to allow for the concept of QH, one must allow that quantum theory describes potentialities and their elevation into actualities through the process of measurement. The present author recognizes that this is perhaps an unfamiliar and even radical notion, although it certainly has longstanding precedent. Examples in the literature are: Heisenberg (1955, 12); Shimony (1997); Toraldo di Francia (1976); Kastner, Kauffman, and Epperson (2018); Jaeger (2017). Howard (1989) also notes that providing a measurement context allows for this sort of actualization, although he doesn't explore the matter in detail. However, his suggestion may be considered a precursor, at least in spirit, to the idea presented here.

Apparently the Referee missed all this as well.

With regard to the symmetrisation postulate, I'm afraid I do not understand what the au means by taking it to apply to polyhedra only.

I wouldn't understand that either had it happened. But since it did not, there is no real problem (except for the Referee's unfounded assertion that it did). For one thing, the term 'polyhedra' never appears in the revision and thus one wonders whether the Referee even read the actual submission, or simply objected based on the direct author response.
This statement misconstrues what is in the paper and, at best, evinces profound confusion about the difference between (1) the quantum symmetrization postulate (SP) and (2) general (valid) symmetries related to invariances. This distinction is quite clear in the revision with an explicit example of rotation of a symmetric body as a **counterexample** to the situation pertaining to application of the SP. We don't need a symmetrization postulate for a case of invariance under rotation, and I did not invoke it. Moreover, the only reason material related to justified cases of transformation symmetries and invariance was included is because the Referee spuriously charged the present author with "throwing out the grand tradition of relating symmetries to group theory," or words to that effect—a grotesque caricature of the original paper. Now that the revision has refuted his charge with a specific counterexample, he misconstrues the nature of counterexample, apparently based only on (a misreading of) the author response and without taking into account what was actually in the revision.

The Referee also seems to have ignored the specific arguments in the manuscript showing that the usual justification for the SP based on 'empirical indistinguishability' are untenable since the individual product states are claimed to be empirically unavailable. His unwillingness to even acknowledge those arguments, and then to misconstrue the paper as 'applying the SP to polyhedra' again makes ironic his claim that I have not engaged with his criticisms. Neither did the Referee acknowledge the revision's refutation of Bigaj's treatment of the scattering counterexample, or the material relating symmetrization and exchange forces to a perturbation, which appears to have been missing all along in the Received View and thereby serves as another reason to question it.

How is that relevant? My point was that the standard basis for the distinction between B-E and F-D statistics is given in terms of symmetrised and anti-symmetrised wave-functions and to deny the significance of the latter seems a radical step too far.

No. The paper does not in any way deny the significance of these states, and claiming that it does is again a caricature. Section 5(b) shows that symmetrization is always in force in cases resulting in the usual statistical differences between fermions and bosons (since these would correspond to free quanta or quanta sharing a bound state). In particular:

> More generally, it can be argued that symmetrization of identical quanta applies only to (i) free quanta and (ii) quanta subject to a common bound state.[15] An example of (i) is a pair of correlated electrons prepared in a spin-singlet state allowed to diverge, as in a typical Bell-type experiment; an example of (ii) is a pair of electrons in an ortho- or parahelium state.

Perhaps what offends the Referee is that the paper declines to take symmetrization as applying universally as a matter of convention or faith, but his apparent offense at the paper's criticisms of the Received View and its SP is not an argument (nor does it license a claim that the paper denies the significance of the symmetrized states).

At the very least, this still requires further articulation (and in this regard I don't think its is enough to simply refer to Jabs' work on the spin-statistics theorem which is, as far as I know, not widely accepted).

And again the paper does not "simply refer to Jabs' work", but raises many independent arguments, which the Referee either ignores or misrepresents in his second review. In particular, the paper specifically notes that the findings of the paper do not depend on Jabs' model:

> observable permutation invariance at the level of the individual states, and there is at the very least mathematical non-invariance of product state eigenfunctions under permutation). Nevertheless, it should be noted that the Jabs model is not required for the points raised in the current work. In particular, it may be noted that H_{Heis} (3) has rotational invariance. This, along with the non-invariance of product wavefunctions under permutation, also suggests that it is rotational invariance, rather than permutation invariance, that is physically applicable to the exchange.

In conclusion, the Referee's portrayal of the submission is a regrettable caricature, in which the Referee mischaracterizes many of the author's arguments, ignores others, and portrays the author's differences from the Received View as much more extreme than they are. His Styrofoam Man criticisms have been refuted above with reference to specific passages in the already-submitted revision. I have added a footnote in reference to Krause and French (2007) noting that QH goes beyond 'just cardinality' (which would already be evident in a fair and impartial reading of the paper).