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Particles and the Perversely Philosophical Schoolchild: Rigid Designation, Haecceitism and Statistics

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RESUMEN

En este artículo quiero llamar la atención sobre la conexión entre la designación rígida, con la consequencia de que nos permite estipular mundos y haecceitismo, y la doctrina de que hay mundos posibles iguales en todos sus rasgos cualitativos pero diferentes metafisicamente, esto es, dos individuos pueden tener todos sus rasgos intercambiados mientras siguen siendo los mismos individuos. Argumentaré que la estipulación conduce al haecceitismo, el cual, a su vez está comprometido con la haecceidad ("esteidad primitiva"). Sostengo que el haecceitismo es una doctrina poco atractiva. Aún así, hay un argumento poderoso a su favor, motivado por el deseo de dar sentido a las frecuencias observadas en sucesos probabilísticos en términos de configuraciones posibles de estados de fases, los cuáles pueden ser considerados como otra forma de mundos posibles. Sin embargo, el argumento falla una vez prestamos estrecha atención tanto a la estadística clásica como a la cuántica. La falta de atractivo del haecceitismo es, por tanto, la razón para rechazar una teoría de los nombres que considere que éstos funcionan automáticamente como designadores rígidos en contextos modales. En su lugar, nos deberíamos concentrar en la idea de descripciones cualitativas, en cuanto éstas proporcionan la base para identificar individuos. Algo se puede salvar en el programa de Kripke: lo que Adams llama "esteidad cualitativa" en vez de "esteidad primitiva". Kripke está en lo cierto en sus comentarios críticos a la teoría de las descripciones definidas cuando considera que los nombres funcionan rígidamente. No obstante, éstos sólo puenden funcionar como tal cuando los inividuos que nombran poseen esteidad cualitativa. La esteidad cualitativa fundamenta la posibilidad de encontrar una descripción definida que funcione rígidamente.

ABSTRACT

In this paper, I want to draw attention to a connection between rigid designation with its consequence that we are able to stipulate worlds and haecceitism, the doctrine that we have possible worlds alike in all qualitative features which nonetheless are metaphysically different, in that two individuals can have all their qualitative features swapped while remaining the same individuals. I shall argue that stipulation leads to haecceitism, which in turn depends upon commitment to haecceity ("primitive thisness"). Haecceitism is, I claim, an unattractive doctrine, but there is one powerful argument for it, drawn from the desire to make sense of observed frequencies of probabilistic events in terms of possible configurations of phase spaces which may be taken to be a form of possible world. However, by paying close attention to classical

and quantum statistics, we see that this argument fails. The unattractiveness of haecceitism is then reason for rejecting an account of names which sees them as automatically functioning as rigid designators in modal contexts, and instead we should focus on the idea of qualitative descriptions as providing the basis for picking out individuals. Something can be salvaged from Kripke's programme: attention to what Adams calls "qualitative thisness" rather than "primitive thisness"; Kripke is right in his critical comments on the theory of definite descriptions to see names as functioning rigidly, but they can only do so where the individuals they name possess qualitative thisness. Qualitative thisness grounds the possibility of finding a definite description which will function rigidly.

I. Rigid Designation and Haecceitism

In a passage near the beginning of N*aming and Necessity* Kripke discusses how we can set about finding the probability of obtaining a certain score, a "six" and a "five", on a pair of dice. He draws attention to the way we list all the possible outcomes: each one of the 6 on the first die has in turn 6 possibilities for the outcomes on the second die, giving 36 in all, of which two fit our required outcome. He writes:

> [No] school pupil [would] receive high marks for the question "How do we know, in the state where die *A* is six and die *B* is five, whether it is die *A* or die *B* which is six? Don't we need a "criterion of transstate identity" to identify the die with a six — not the die with a five — as our die *A*?" The answer is, of course, that the state (die *A*, 6; die *B*, 5) is *given* as such (and distinguished from the state (die *B*, 6; die *A*, 5)). The demand for some further "criterion of transstate identity" is so confused that no competent school child could be so perversely philosophical as to make it. The "possibilities" simply are not given purely qualitatively (as in: one die 6, the other 5). If they had been, there would have been just twenty-one distinct possibilities, not thirty-six. And the states are not phantom dice-pairs, viewed from afar, about which we can raise epistemically meaningful questions of the form, "which die is that?" Nor, when we regard such qualitatively identical states as (A, 6, B, 5) and (A, 5; B, 6) as distinct, need we suppose that *A* and *B* are qualitatively distinguishable in some other respect, say, color. On the contrary, for the purposes of probability problems, the numerical face shown is thought of as if it were the only property of each die [Kripke (1980, p. 17].

There are two points one can make in connection with this quotation. First, on a cursory glance, it seems to commit us to what Lewis calls haecceitism, namely the claim that there can be possible worlds which are the same in all qualitative features, yet differ in that the individuals possessing those qualitative features vary from one world to another (by permutation of qualitative properties). The step is then made from haecceitism to an account of why names function as rigid designators. Second, it looks as though implicit in the story is an argument from the statistics and long run frequencies we observe in support of haecceitism. Briefly, the argument is by inference to the best explanation (which one may have independent grounds for doubting; see for example, Van Fraassen (1989). Without haecceitism, two of the qualitatively indistinguishable yet distinct possible worlds would collapse into one, which would give us no grounds for saying that an outcome of (5, 6) was twice as likely as one of (6, 6). Thus we should be haecceitists.

Both these claims have problems. Lewis (1986), who also quotes the passage above, claims that Kripke is not committed to haecceitism, but is instead agnostic about it, and that we can make sense of rigidity without haecceitism in any case. It also turns out that the second claim, although seemingly supported by a consideration of the differences between quantum statistics and classical statistics, is also rather more complicated. I shall look first at Lewis's arguments, and claim that classical statistical mechanics gives reasons for thinking of situations like the pair of dice as possible worlds, rather than what Lewis calls "mini worlds". Then I shall look at the issue of quantum statistics, arguing (following Dalla Chiara (1985)) that quantum particles do force us to reconsider the issue of rigid designation and the relation between names and descriptions in terms of qualitative features.

II. Haecceitism and Classical Statistical Mechanics

Lewis gives the following account of what he calls haecceitism (where the term haecceity has its origins in the writings of Duns Scotus, who argued that in order for particulars to instatiate universals, particulars had to possess haecceities or "primitive thisnesses"[1]):

> If two worlds differ in what they represent *de re* concerning some individual, but do not differ qualitatively in any way, I shall call that a *haecceitistic difference. Haecceitism*, as I propose to use the word, is the doctrine that there are at least some cases of haecceitistic difference between worlds [Lewis (1986), p. 221].

Lewis goes on to say that Kripkean stipulation does not presuppose haecceitism, though he adds that anti-haecceitism does imply that, in principle, if not in practise, any Kripkean specification could be replaced by a purely qualitative one. But even if we accept anti-haecceitism, this does not rule out stipulations like the one in our opening quotation from Kripke. Lewis says that what we have here are not genuine possible worlds, but "less-than-maximally-specific 'miniworlds'". For these he says that, in effect, we ignore qualitative differences, and specify the 36 mini-worlds *de re*. Presumably we could, if we chose, give some sort of account where qualitative difference (spatial positions and trajectories, for instance) did the work of stipulation, but we don't need to. He goes on to say:

> Representation *de re* may supervene on qualitative character, in fact I am sure that it does, yet there is no reason why we may not attend to the former, ignore the latter, conflate worlds that agree in representation *de re* but differ qualitatively, and specify these conflations in the appropriate non-qualitative terms. We may; and we do [Lewis (1986), pp. 226-7].

Thus Lewis claims that though it is easy to think of Kripke as a haecceitist in contrast to his own anti-haecceitism, in fact Kripke is neutral as to whether there might be haecceitistic differences between worlds. He quotes a second passage from Kripke, a page later than the one I started from, in which Kripke goes on to say that he is not claiming that there could be qualitatively indistinguishable but distinct possible worlds; there could be, and an argument against them cannot hinge simply on the claim that possible worlds must be given purely qualitatively, for this would be to beg the question. Kripke concludes:

> What I defend is the *propriety* of giving possible worlds in terms of certain particulars as well as qualitatively, whether or not there are in fact qualitatively identical worlds [Kripke (1972), p. 18].

Thus, on Lewis's reading, Kripke seems agnostic as to whether one should be a haecceitist or not. It is at this point that the second of the two claims, about the explanatory value of haecceitism, enters the story.

III. From Classical Statistics to Quantum Statistics

Lewis's reading centres on his claim that the dice story involves what he calls miniworlds rather than possible worlds. However, if one thinks of the atomism of Boyle and his contemporaries in the 17th century, and the later developments of kinetic theory with Bernoulli's early attempts in the 18th century and Maxwell's and Boltzmann's work in the 19th, it is not to implausible to suggest that there is a sense in which classical physics, together with a reductionist or physicalist interpretation, envisages possible worlds precisely as sets of configurations of particles. We then appear to have an argument for haecceitism which runs thus: as can be seen from long run frequencies, an outcome of (5, 6) on our dice, or of (H, T) on a pair of coins, is twice as likely as one of (6, 6) or of (H, H). If we conceive of possible worlds as these arrangements of particles, and if we think of all possible worlds as equally likely, then only the haecceitist, for whom the worlds of (5, 6) and (6, 5) or (H, T) and (T, H) can be distinguished non-qualitatively, has a ready connection between frequencies and theory — there are twice as many possible worlds which could give rise to the results where the pair of outcomes differ, hence the events of the pair differing are twice as likely.

This reading seems borne out at first glance by a cursory look at statistical mechanics textbooks (though we shall take a less cursory look shortly). Thus, for instance, Huang (1987) notes that what we are interested in are macroscopic phenomena of temperature and pressure, which can be brought about by a range of arrangements of the particles (just as the gambler, once the dice roll, is interested only in the score of 11 or 12 on the pair of dice, not the way in which it was brought about). But to account for these phenomena, and things like the tendency towards equilibrium, we need to think about the arrangements (just as the gambler, in calculating the odds, needed to think about arrangements). We then find something which looks very like the connection between number of worlds, their equiprobability, and the likelihood of a given outcome:

> Therefore we think not of a single system, but of an infinite number of mental copies of the same system, existing in all possible states satisfying the given conditions. Any one of these system (*sic*) can be the system we are dealing with [...].
>
> POSTULATE OF EQUAL *A PRIORI* PROBABILITY When a macroscopic system is in thermodynamic equilibrium, its state is equally likely to be any state satisfying the macroscopic conditions of the system [Huang (1987), pp. 128-9].

The set of all such copies is called a phase space, and relative likelihoods of certain macroscopic states obtaining is tied to the volume of phase space occupied by the set of arrangements corresponding to the given macroscopic states.

One story which could then be used to counter this haecceitist vision would then involve paying attention to quantum mechanics. Concentrate on the two coin example. Each coin has a 50% chance of each outcome, and by counting possible arrangements and applying probability calculus, we get probabilities of 1/4, 1/2, 1/4 for the outcomes (H, H), (H, T), (T, T), respectively. We can find a quantum mechanical analogue of landing heads or tails; various elementary particles have intrinsic angular momenta, or spin, which we can picture (albeit inaccurately) as being like a child's top. Depending on whether the top spins anti-clockwise or clockwise, we can assign a vector pointing up or down (imagine rotating the fingers of your right hand in the direction of the spin; your thumb will give the orientation of the vector). Whether the spin is up or down is revealed by a deflection in the path of the particle in a magnetic field. If we feed a stream of identical particles through the field, there is a 50% chance of them being deflected up, and a 50% chance of them being deflected down. Now consider pairs of particles produced from some source. For some sorts of particles, bosons (of which photons are an example), the probabilities of obtaining (up, up), (up, down) and (down, down) are 1/3, 1/3, 1/3 respective, while for others, fermions (of which electrons are an example), there is a probability of 1 of obtaining the outcome (up, down). These probabilities are in stark contrast to the classical case.

It is tempting to read from this story a quick response to haecceitism, and a worry about rigid designation. The classical cases are distinguished haecceitistically. But in the case of bosons, the two cases which classically would count as distinct are in fact re-descriptions of the same world (hence the equal probabilities). Worlds are given qualitatively (one up, one down), not haecceitistically (the first up, the second down). We then turn this into an attack on rigidity (in a manoeuvre reminiscent of Leibniz's attack's on absolute space, where one shows that, were there to be absolute space, a certain distinction between situations would make sense; the fact that the distinction does not shows that the initial assumption was at fault). If we could name the bosons using rigid designators, we would then be able to stipulate our way to the description of two possible worlds (the first up, the second down, versus the first down, the second up) which we know from observation of long run frequencies are in fact the same world. Thus by a Leibnizean move we see that whatever led to our erroneous conclusion (that we have two distinct worlds rather than just one), namely the assumption that names functioned rigidly and could be used to capture haecceitistic differences, was in fact a mistake.

One could perhaps try to press the conclusion still further. If one is a reductionist, then ultimately everything is made of such quantum particles, particles for which any attempt at attaching names is doomed (as Dalla Chiara puts it, "the microuniverse represents in a sense a "land of anonymity"[Dalla Chiara (1985), p. 189]). Thus while classical physics leaves us thinking that names can be attached to particulars by baptism, and thereafter function rigidly in modal contexts, quantum mechanics teaches us that insofar as names can be attached at all, it is only in virtue of qualitative difference between particulars which emerge at the macroscopic level.

However, as we shall see, this rather quick answer does justice neither to classical statistical mechanics, nor to quantum statistical mechanics. But all is not well for haecceitism; it turns out that neither story leaves any need for haecceitism, and the quantum mechanical case leaves us with a rather weakened account of rigidity.

IV. Classical Statistical Mechanics and Haecceitism

The first point to be made is that haecceitism is not really what is at issue even in the explanation supposedly provided by classical statistical mechanics for the long run frequencies observed for classical systems. For instance, Nick Huggett points out that the two coins:

> provide a very misleading model of the statistics. For our classical coins we explain that heads-tails is twice as likely as heads-heads, because there are two states corresponding to heads-tails; permutations are distinct. But even countenancing the direct connection between frequencies and numbers of states, in realistic systems (such as a gas) one obtains the same statistics whether one counts permutations as distinct or not. There is thus no support for haecceitism from classical physics [Huggett (1997), pp. 120-1].

As I noted earlier, to think otherwise is based on a very superficial look at statistical mechanics textbooks. Thus, Huang, in setting up the Gibbs phase space [Huang (1987), ch. 3], notes that we are interested in the distribution function which assigns to each small volume element of the phase space (itself characterised by the 6N position and momentum coordinates, three components of each for every one of the N molecules, together with time) a number of molecules with position and momenta lying within the small element in question. We are interested only in occupation number, not which particular named individuals lie within the volume element, nor the order in which they are placed within it.

We see, then, that the supposed disanalogy between quantum mechanics and classical mechanics is not as great as we might have supposed. But things if anything look even worse for haecceitism. Remember that the story about counting possible worlds which could only be distinguished haecceitistically was meant to provide an explanation for frequencies which we would otherwise lack, and thus provide grounds (by appeal to inference to the best explanation) for haecceitism itself. Yet we find we can explain these differences without haecceitism. So we didn't need haecceitism even in the classical case, nor did we at any stage need to name the gas molecules. Thus, the preliminary conclusion on the basis of a consideration of classical mechanics is that we can hold to Lewis's position of rejecting haecceitism and also of assuming that anything which can be stipulated in terms of names of rigidly designated individuals could instead be described qualitatively.

V. DO QUANTUM STATISTICS TELL US ANYTHING ABOUT RIGID DESIGNATORS?

Let us return to the quantum mechanical situation. In our original version of the argument, it was suggested that maybe the quantum particles cannot be the bearers of names. However, it might be argued that what this really shows us is that the "particles" are not really particulars at all. Let me begin by introducing some notation which will help us to capture what is going on. As I noted earlier, the spin of the boson or fermion can be represented by a mathematical object called a vector. These vectors represent the state of an individual. Let us express a particle *a* (labelled by a name for the time being) as being in a state of spin up by the expression **U**(*a*) (with **D**(*b*) representing a particle *b* with spin down). Until we started to worry about the long run frequencies, we seemed to have 4 states:

(1) **U**(*a*) **D**(*b*)
(2) **U**(*b*) **D**(*a*)
(3) **U**(*a*) **U**(*b*)
(4) **D**(*a*) **D**(*b*)

But, as French and Redhead (1988) note, these are not the way we would write the states in quantum mechanics. Instead they are written as superpositions.

To see how this works, consider a vector quantity like direction. Suppose we were in an American city set out on a grid pattern and wanted to go diagonally. We could not proceed in a straight line "as the crow flies" but would instead have to go horizontally and then vertically. In this sense, we decompose our chosen path into components orthogonal to one another. Our vector states, **U** and **D**, are, analogously, orthogonal one to another[2]. Consider how we might describe the behaviour of a collection of single particles (rather than pairs of particles produced simultaneously by a source) whose spin we want to measure. We can now think of two ways of representing what happens when we measure their spin. We can think of the set as consisting of a mixture of particles, half in state **U** and half in state **D**, or we can think of the set as consisting entirely of particles each of whose state is represented by a vector sum of components in the two orthogonal directions, *C*(**U** + **D**) (where *C* is a constant, in this case, $1/\sqrt{2}$), together with a rule which says that on measurement, the probability of obtaining "up" is the square of the absolute value of *C*. It turns out that for reasons of empirical adequacy (Bell's theorem) and algebraic consistency (the Kochen-Specker theorem) we are forced to choose the second of these representations[3]. Extending this to the two particle state, we have 4 states represented thus:

(5) **U**(*a*) **U**(*b*)
(6) **D**(*a*) **D**(*b*)
(7) *C*(**U**(*a*) **D**(*b*) + **U**(*b*) **D**(*a*))
(8) *C*(**U**(*a*) **D**(*b*) - **U**(*b*) **D**(*a*))

Note that states (5), (6) and (7) are symmetric under exchange of the labels *a* and *b* (swapping the labels leaves us with the state we started with) while state (8) is antisymmetric (swapping the labels gives us –1 times the state we started with).

To see how far we are from the familiar classical ground on which we started, consider Redhead and Teller's (1991) summary of the classical view of how particles are individuated and named:

> They [classical particles] can be counted. The properties of several individuals can be rearranged: That is there is a difference between a first individual having property *A* and a second individual having property *B* as opposed to the first being *B* and the second *A*. And individuals can be thought of as being tagged, named or labelled [Redhead and Teller (1991), p. 44].

They separate this view into two strands: the idea that classical particulars can be the bearers of properties, and the idea that they can be the bearers of names. Redhead and Teller then turn to the interpretation of the classical states (1) to (4) and the quantum states (5) to (8). They connect the observed features of the states and their statistics (1/4, 1/2, 1/4; 1/3, 1/3, 1/3; 1) with the question of whether the particles can be the bearers of names. The two non-symmetric states (1) and (2) do not occur. Now if quantum particles could bear names, we could stipulate possible worlds represented by (1) and (2), but would be forced to say that, although they had a physical interpretation, they never occurred. On the other hand, if we held that the states had no physical interpretation, we would be left only with states (5) to (8) and the conclusion that quantum particles could not be the bearers of names. They argue for accepting the latter conclusion, a conclusion which can be supported by an argument due to Dalla Chiara.

Here is the way in which names come to be problematic. Return once more to consideration of a single particle rather than a pair of particles. If we have a particle prepared in a state represented by the vector **U** there is a probability of 1 that it will be found to have spin up on measurement. We can ascribe to the particle the state **U**(*a*). It seems natural to read this as a description cast in subject-predicate form which ascribes to the named individual *a* the property captured by the predicate **U** of having spin up. In general, for a language adequate to describe quantum mechanical phenomena, we can try to introduce a semantics in the standard way which will assign denotata to names and extensional interpretations to predicates. This is the strategy Dalla Chiara (1985) investigates for quantum logics. But such a semantic model must satisfy compositionality (or as Dalla Chiara calls it, the Frege Principle):

> The truth-value (extension) of an atomic sentence of the form "*a* is *P*" (*Pa*) is a function of the extensions of its parts: the name *a* and the predicate *P*. As a consequence, in order to ascertain whether *Pa* is true or false, one has first to identify the *denotatum* of the name *a* [Dalla Chiara (1985), p.189].

Yet quantum states of the sort we have been discussing fail to satisfy this principle; as Dalla Chiara points out, for a state like $C(\mathbf{U}(a)\ \mathbf{D}(b) - \mathbf{U}(b)\ \mathbf{D}(a))$, we can make the assertion "there is one *x* which is *F*" ("there is one electron which has spin up") but not form the definite description "the *x* which is *F*". And this is not just a problem for a Russellian definite description view of names; one cannot see how any process could pick out the individual bearing the name *a* rather than the name *b* either. In a paper pursuing the philosophical implications of the attempt to construct a semantics for quantum logics, Dalla Chiara and Toraldo di Francia write:

> There is no *trans-world* identity. In this situation the meaning of "rigid designator" becomes very fuzzy. Anyway, the term seems useless [Dalla Chiara and Toraldo di Francia (1993), p. 267].

The worry in accepting the conclusion that quantum particles cannot be rigidly designated is that we may be throwing the baby out with the bathwater: to admit this much seems to be to admit that quantum "particles" are in fact not individuals at all.

Two lines of approach have been suggested at this point [Redhead and Teller (1992]. The first is to accept that electrons, photons and the like are not individuals. The second is to accept that quantum particles are individuals, and the non-classical counting of states is to do with restrictions on the sort of states which bosons and fermions can occupy. On this view, the labels have a denotational significance but epistemically we cannot tell which particle to attach them to [French (1989)]. The argument for this second view as we have seen is that we seem able to write our states in a labelled formalism and it is natural to take the labels as names, and that once we have grasped the appropriate restrictions on accessible states, we can use the same sort of counting argument for explaining frequencies that we used in the two coin case. However, as we have seen, Dalla Chiara's discussion of semantics casts doubt on the identification of labels in the mathematics with names in the sense more familiar in formal semantics and philosophical logic, and in any case, the argument from counting worlds turned out to be based on an analogy with the classical case which was unsatisfactory. So we are forced to reconsider the first option.

What could be meant by denying that quantum particles are "individuals"? French has coined the term non-individual to capture the notion that, though not precise and definite like the macroscopic objects we normally think of as particulars, nonetheless, such particles should form part of our ontology. Perhaps the term quasi-individual would be better[4]. They are quasi-individuals in the sense that they can be counted ("there is one electron with spin up"[5]), but they cannot be denoted by proper names. One can ask whether things so strange could coherently be claimed to be part of our ontology. My suggestion is that they could, and that an investigation of the differences between them and more familiar particulars sheds light on the way in which names function.

In brief, the claim I want to conclude with is that insofar as particulars can be named, it is in virtue of their possessing qualitative thisness. Adams (1979) sets this position out. Thisness is that which makes something this individual and no other, that which underpins self identity[6]. Adams then distinguishes on a metaphysical level between primitive thisness and qualitative thisness. Primitive thisness (Duns Scotus's notion) is that things possess their identity and distinctness from other things as an irreducible feature; qualitative thisness claims that thisness depends on properties and relations. Adams notes that it is tempting to see Kripke's theory in which names function as rigid designators as showing that thisness must be primitive. However, this is not so. In an argument which adumbrates Lewis's approach which I mentioned at the beginning, Adams points out that Kripke's arguments against descriptive theories of names establish that thisness is semantically primitive:

> we can express [thisnesses] (and know that we express them) without understanding each thisness (the property of being this or that individual) in terms of some other property or properties, better known to us, into which it can be analyzed or with which it is equivalent [Adams (1979), p.10].

But this does not establish that thisnesses are metaphysically primitive. It could be, as Lewis claims, that ultimately thisnesses on a metaphysical level are qualitative. I think that quantum particles and their entangled states enable us to go further than Lewis's promissory note. They show us that where entities lack both distinguishing properties and relations and the possibility[7] of having such properties and relations, i.e. they lack qualitative thisness, they cannot be denoted either.

Quantum particles cannot have qualitative thisnesses in this sense, because the entangled states which describe them, giving seemingly maximal descriptions, do not provide us with either differences of intrinsic property (total spin, rest mass and the like are the same for both) nor in the relations they bear one to another. ([Teller (1986], has argued convincingly that these relations are strongly non-supervenient in that they are not reducible to intrinsic properties of the relata). Thus the quasi-individuals of the microscopic world lack qualitative thisness. If we accept that absence of qualitative thisness explains the problems we see with failure to be able to attach names to these particles in any way which enables us to pick out the objects denoted, we have a powerful argument for Lewis's claim that representation *de re* supervenes on qualitative characteristics. Names may function rigidly, but this is in virtue of features of the world which metaphysics and physics can help us to examine (and biology too, in the case of natural kind terms). It is not that a consideration of the workings of names within our language alone suffices to enable us to draw metaphysical conclusions.

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NOTES

[1] See Bettoni (1961), for an account of Duns Scotus, and Adams (1979), for a discussion of its place in modern philosophy.

[2] Orthogonal to one another in an abstract mathematical "spin space", not in physical space, where clearly the particles with spin up move in the opposite direction to the ones with spin down, not at right angles to them.

[3] See Albert (1992) for an excellent account of why we have to use vectors in this way, and Redhead (1987) for an account of Bell's theorem and the Kochen-Specker theorem.

[4] I am grateful to Peter Simons for suggesting this term to me.

[5] Though even this becomes problematic in quantum field theory, prompting the suggestion that any sort of particle ontology is inappropriate in this case.

[6] It is this notion of self identity, or the lack thereof in the case of quantum particles, which underpins claims that quantum particles are vague objects. See Maidens (forthcoming).

[7] I add "the possibility" to deal with classical cases like Max Black's world which contains only two globes, identical in all properties and (in the absence of any question begging assumptions about absolute space) all relations one to another. But we could conceive of one being a different colour to the other; such extra variables cannot be added into the quantum case (*pace* Bohmian interpretations which come with a heavy metaphysical price) without running foul of no-hidden-variable results like Bell's theorem.

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