Symmetry & Possibility: To Reduce or not Reduce?*

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Abstract

In this paper I examine the connection between symmetry and modality from the perspective of 'reduction' methods in geometric mechanics. I begin by setting the problem up as a choice between two opposing views: reduction and non-reduction. I then discern four views on the matter in the literature; they are distinguished by their advocation of distinct geometric spaces as representing 'reality'. I come down in favour of non-reductive methods.

1 The Geometry of Modality.

Hacking (1975) famously argued that spatially symmetric worlds could never constitute a counterexample to the principle of identity of indiscernibles [PII[1]]: there is always a way to redescribe the situation so that the symmetry is absent and so that there are fewer

[1]That is, the principle that any individuals, a and b, sharing all of their properties, $F \in \mathfrak{F}$, are really one and the same individual; formally, in terms of 2nd-order logic with identity: $[\forall F \, \forall ab : (Fa \equiv Fb) \to (a = b)]$. Different 'strengths' of PII can be formulated by restricting the range of the property variable F in various ways. By having F range over *all* properties and relations—including the property of self-identity, $a = a$—PII is rendered *trivial*, giving a *weak* form of PII (a theorem of the 2nd-order predicate calculus with equality); outlawing self-identity and excluding spatiotemporal properties and relations (absolute position, for example), but including all other relations and properties, gives us a stronger form; restricting F to just intrinsic, monadic qualitative properties gives the strongest form.

objects within the world. Belot has recently argued that the same holds for the principle of sufficient reason [PSR[2]] with respect to those counterexamples that utilize "a multiplicity of qualitatively identical worlds related by spatiotemporal or other symmetries" (2001: 2).[3] The claim is that such objects ought to be identified, and, moreover, we ought to identify as a "matter of policy" (*ibid*.). Thus, Belot claims that PII is enforced by PSR; this is just what I will deny: PSR is certainly compatible with PII, but it is also compatible with non-reductive (i.e. non-eliminative) options too. I argue that, quite to the contrary, as a matter of policy we should *not* reduce!

Belot explicitly connects his discussion to phase space descriptions of theories: the enlarged phase space[4] corresponds to a theory with symmetries and the reduced phase space to an empirically identical theory without the symmetries. Note that this equivalence is a purely classical affair; we can associate to any constrained classical system two phase spaces: an extended space with constraints and a reduced space obtained by solving the constraints. These spaces correspond to possibility spaces for physical systems, and the reduced space contains less possibilities than the enlarged space; it does not contain any indistinguishable worlds. This difference in the number of possibilities is (empirically) inert at the classical level[5], but it can manifest itself physically in the beha-

[2]That is, the principle that there must always be a reason why something is 'thus' rather than 'so'. In the context of this paper it is symmetry and indiscernible possibilities and worlds that put pressure on the principle; Belot's paper demonstrates how a simple application of PII can ease the pressure at a stroke by factoring out the symmetry and eliminating the indiscernible possibilities and worlds.

[3]The worlds are to be understood as inhabiting a possibility space, generally represented by points or paths in a phase space. (If you don't endorse PSR then simply redirect any reference to PSR to the possibility of indistinguishable worlds, perhaps differing solely with respect to which individuals are assigned to which properties – in other words, worlds that differ merely haecceitistically (see Melia (1995) for this definition of haecceitism; Lewis (1983 and 1986) offers a similar definition)). Both Hacking and Belot understand PII as operating on possibility space itself, rather than on its elements (i.e. it is about sets of worlds rather than the 'contents' of worlds). Hacking seeks to preserve PII by arguing that any symmetrical world put forward as a counterexample can be faithfully represented by a (qualitatively indistinguishable, empirically equivalent) world that isn't a counterexample; while the counterexample worlds can be imagined and described, they do not constitute *genuine* possibilities (see French (1995) for details and a critique of Hacking's proposal). Belot connects this notion up to the 'formal' possibility spaces provided by phase spaces and argues that PSR can always be protected from similar counterexamples simply by invoking PII.

[4]A phase space is used to represent physical systems in the Hamiltonian formulation. According to this formulation, each system is represented by a triple $\langle \Gamma, \omega, \mathrm{H} \rangle$ consisting of a manifold Γ (the cotangent bundle $\mathrm{T}^*\mathcal{Q}$, where $\mathcal{Q}$ is the configuration space of a system), a tensor ω (a symplectic, closed, non-degenerate 2-form), and a function H (the Hamiltonian $\mathrm{H} : \Gamma \rightarrow \mathbb{R}$). These elements interact to give the kinematical and dynamical structure of a classical theory. The manifold inherits its structure from the tensor, making it into a phase space (a symplectic geometry). The points of this space are taken to represent physically possible states of some classical system (i.e., set of particles, a system of fields, a fluid, etc...). Finally, the Hamiltonian function selects a class of curves from the phase space that are taken to represent physically possible histories of the system (given the symplectic structure of the space). In this paper I am concerned with a subclass of Hamiltonian systems; namely, those with constraints (I describe the formalities in §2). When I speak of the "enlarged phase space" I do not distinguish between constrained and unconstrained spaces: I simply mean an unreduced space of whatever kind. Generally speaking, however, I will be talking about kinematical spaces where the constraints have not yet been imposed (i.e. solved).

[5]Huggett (1999) draws on this inertness to defend the view that classical statistical mechanics is as permutation invariant as quantum statistical mechanics, and is compatible with both the reduced and un-

viour of the respective quantum systems associated to quantizations of the two types of space. The intuitive reason is that degrees of freedom that are absent from the reduced phase space description will be present and undergo fluctuations in the extended phase space quantization.

Now, as a matter of metaphysical practice, I agree with Belot that we should prefer the formulation of a theory without indistinguishable possibilities if those possibilities are redundant. But this preference is not a matter to be decided on the basis of the theory one is talking about if that theory treats the formulations with and without such possibilities as *equivalent*.[6] Put as simply as this, the point I am making may seem rather obvious; yet many authors implicitly contradict this simple point. Moreover, we can work with the enlarged phase space and get the large or small set of possibilities from it by choosing either a direct (one-to-one correspondence between phase points and possible worlds), indirect (many-to-one correspondence between phase points and possible worlds), or selective (only one phase point from an equivalence class represents a possible world) interpretation respectively. We do not have this elbow room if we automatically opt for the reduced space. Clearly, however, quantum theory may give us a reason to reduce or not reduce depending upon which formulation of the classical theory yields an empirically successful quantum theory. However, whether we are forced to reduce or not makes no difference to interpretive matters as they stand in the classical theory: one can still be committed to either the things represented by the symmetry operands or one can be committed to the equivalence classes of those things. The choice is purely metaphysical until a quantum theory comes along and tells us otherwise. Even if the reduced phase space is, for whatever reason, deemed to be the correct representational tool for our theories, we cannot simply see this as thereby underwriting relationalist stances, for anti-haecceitism is not a necessary part of the reductive form of relationalism; it is something that needs arguing for independently. Moreover, the substantivalist can occupy the reduced space

reduced phase spaces (or "anti-haecceitistic" and "haecceitistic" phase spaces, as he calls them). I agree with Huggett on this point, and it can be seen as applying my conclusions in the spacetime case to the case of particles - Saunders (2003: 302) endorses Huggett's line. (Note that Saunders (loc. cit.) claims that in §2 of our (2003), we (that is, myself and Steven French) are "clearly sympathetic to the view that Leibniz Equivalence, as applied to permutations, is incompatible with classical physics (equivalently, that classically one is committed to the use of haecceitistic phase space)". This simply isn't the case (I cannot speak for French here): there is underdetermination at both the classical and quantum levels, though the *type* of underdetermination is different in these two cases. In the classical case, the underdetermination is secured by the empirical equivalence of the reduced and unreduced spaces; in the quantum case, it is secured by imposing a symmetrization (initial) condition on the quantum states on the one hand and permutation invariance (reduced along the lines of Leibniz Equivalence) on the other. The view of symmetries I have been defending fully respects this conclusion; I have been at pains to uncover such underdetermination in all cases of invariance symmetries.)

[6]Clearly, in terms of the *conceptual* structure of the respective formalisms they will not be equivalent in general. The formulation without indistinguishables will not be able to accommodate counterfactual switchings of properties between individuals, for the fact that this is a symmetry (for maximal property swaps) will result in such possibilities being removed (of course, we might avail ourselves of Lewis' 'cheapskate' haecceitism (1983), so that one and the same world accommodates many possibilities). Indeed, the symmetry arguments I have examined have followed this form: properties are redistributed over individuals in such a way as to preserve observable relations. (See Teller (1993) for a nice discussion of a related problem concerning haecceities facing constructive empiricists who are wedded to semantic universalism.)

too, by endorsing anti-haecceitism (*cf.* Pooley (forthcoming))

2 Geometric Mechanics and Possibility Spaces.

Let us begin by focusing our attention on the nature of the geometric spaces under consideration. Let (Γ, ω) be the classical *unconstrained* phase space of some constrained physical system or structure $\mathfrak{S}$. This is, of course, a symplectic manifold of dimension $2n$. A *constrained* phase space is then constructed by imposing a set of conditions $\phi_i : \Gamma \to \mathbb{R}$ $(i = 1, ..., n)$, known as first-class constraints. This determines a submanifold $\mathcal{C} = \{x \in \Gamma \mid \forall_i : \phi_i(x) = 0\}$ called the *constraint surface* (of dimension $m \leq n$). These conditions allow us to view $\mathcal{C}$ as embedded in Γ, and in so doing we note that the restriction of symplectic form to the constraint surface, $\omega \mid_{\mathcal{C}}$, giving a geometrically weaker *presymplectic* form σ, determines a foliation $F_{\omega|_{\mathcal{C}}}$ of $\mathcal{C}$ whose leaves correspond to gauge orbits and, therefore, to phase points that represent the same physical state of $\mathfrak{S}$. A *reduced* phase space can then be constructed by forming the quotient space $\Gamma_{red} = \mathcal{C}/F_{\omega|_{\mathcal{C}}}$, resulting in a space of leaves or orbits. Crucially, Γ_{red} is a manifold[7] (with $\mathrm{Dim}(\Gamma_{red}) = 2n - 2m$) and, given a submersion map $\pi : \mathcal{C} \to \Gamma_{red}$, there is defined a symplectic form ω_{red} on Γ_{red}, such that $\pi^*\omega_{red} = \omega \mid_{\mathcal{C}}$. The resulting symplectic geometry $(\Gamma_{red}, \omega_{red})$ is the phase space of the system $\mathfrak{S}$ characterized by the constraints ϕ_i *when the constraints have been solved*.

Given a simple physical system described by a theory with constraints, the above constructions have the following meaning: (1) the unconstrained phase space contains points that do not represent physically possible states for the system; that is to say, not all points of the unconstrained space are (dynamically) accessible to the system[8]; (2) the constraint surface phase space ignores these 'unphysical' points so that only points representing physically possible states remain, although these points form equivalence classes representing the same physical state; (3) the reduced phase space identifies any equivalent points on the constraint surface, so that each point represents a physically possible and physically (i.e. qualitatively) distinct state of the system.

Distinct definitions of observables are associated with each type of space, and it is here that most of the philosophical problems we've considered have sprung from. In the unconstrained framework, an observable is simply a real-valued function on the unconstrained

[7] That the reduced phase space is a manifold does not hold generally; however, in the case of general relativity it is a disjoint union of manifolds, and so is in fact a manifold.

[8] One option here is to claim the inaccessible states as *metaphysical possibilities* but *physical impossibilities*. A similar move is suggested by French (1989) as a response to the differences between classical and quantum statistics. One considers the principle of permutation invariance as imposing an initial condition on particle states so that state vectors are constrained to remain in one or another subspace representing particle type (boson or fermion) - see French & Rickles (2003: 222-223) for further discussion. This point fits in quite nicely with one of the main claims of this paper that one can 'access' the reduced possibility set from the unreduced space by tweaking certain other aspects of an interpretation.

phase space, i.e. a map of the form $\mathcal{O}_\Gamma : \Gamma \to \mathbb{R}$. Of course, since some of the states of Γ are inaccessible, this characterization of the observables will produce too many; there will be observables that are in principle unmeasurable. To overcome this problem one can restrict the observables to the constraint surface, $\mathcal{O}_\mathcal{C} : \mathcal{C} \to \mathbb{R}$. However, the constraint surface is partitioned into gauge orbits by the foliation, where the usual interpretation is that the elements of such orbits are equivalent in the sense that they represent physically indistinguishable possibilities (the same physical state). In this case, with no further restriction on the form of the observables, there will be underdetermination: there will be distinct physical states that no observable can distinguish between. Of course, this is the source of the indeterminism that plagued direct interpretations of gauge theories; it is also the source of Newton's difficulties with the shift situations. To avoid this problem, the further restriction we then impose on the observables is that they be constant along gauge orbits (i.e. on the leaves of $F_{\omega|_\mathcal{C}}$), so that observables must satisfy $\mathcal{O}_{[x]}(x) = \mathcal{O}_{[x]}(x')$ whenever $x, x' \in [x]$ (e.g., when x and x' are connected by a gauge transformation). This definition is equivalent to requiring that the observables commute (weakly) with the first class constraints, $\forall_i, \{\phi_i, \mathcal{O}\} \approx 0$, where the constraints are understood as generators of gauge symmetries. Of course, this latest restriction simply amounts to a gauge-invariant definition of the observables; there is no underdetermination or indeterminism because the observables are now only sensitive to differences between entire gauge orbits. Gauge-invariant observables naturally induce a function $\mathcal{O}_{[x]} : \Gamma_{\text{red}} \to \mathbb{R}$ (under the submersion map π^*), which is just to say that such functions $\mathcal{O}_{\Gamma_{\text{red}}}$ on the reduced phase space are automatically gauge-invariant, corresponding as they do to gauge-invariant functions on the constraint surface.

We can see two levels of surplus structure at work in the preceding descriptions: (1) the surplus associated with the *inaccessible* states of the unconstrained phase space (i.e. $\{x \mid x \in (\Gamma - \mathcal{C})\}$); and (2) the surplus associated with the gauge orbits of the constraint surface (i.e. $\{x \mid x \in [x] \subset \mathcal{C}\}$). I think that it is important to distinguish between these two types of surplus structure: the first type is not nearly so problematic as the second, for the latter can be taken to represent physical possibilities but the former cannot. Thus, we can write off the former 'inaccessible' type as *merely unphysical*, an artifact of representation that can be resolved by introducing a set of constraints on to the space or focusing on the space of physically accessible states represented by $\mathcal{C}$. I don't think anyone would question this. The problems concern the latter type then, and writing it off isn't as simple as it is with the unphysical surplus structure for the points of $\mathcal{C}$ are physically possible.[9] Classically, each space leads to the same physics; but quantum theory messes up this nice tidy setup. There are distinct types of quantization method associated to each of

[9] I am ignoring the kinds of selective response given by Butterfield and Maudlin here. Clearly, that idea would amount to imposing some condition on $\mathcal{C}$ such that exactly one $x \in [x] \subset \mathcal{C}$ is physically possible for each orbit of the symmetry group. Since neither party offers such a condition we can put their views aside here. Recall also that most physically interesting systems will face a Gribov obstruction. Moreover, technically (i.e. ignoring aspects to do with how the geometric spaces represent), such moves simply match up to the construction of a reduced phase space. Given this match, we see a possible explanation of the relationship between PII (sophisticated) and non-PII (selectivist) endorsing substantivalist options: the basic representational spaces they call upon are isomorphic.

these structures and that lead to distinct (inequivalent) quantum theories.[10]

Prima facie, the reduced space looks to be the clear winner in the choice of representational space: it represents only the gauge-invariant information of the system. However, in taking the quotient of a constraint surface by the gauge orbits, one loses out on certain features that are, at the very least, technically useful: manifest Lorentz invariance and locality in space, for example. It is also often hard to find a set of coordinates for the reduced space – though, on the plus side, the coordinates (and the observables) will be immediately gauge-invariant if they can be found.[11] More serious are the points mentioned above, that the reduced phase space will not, in general, be a cotangent bundle of some configuration space $\mathcal{Q}_{red}$ such that $\Gamma_{red} \cong T^*\mathcal{Q}_{red}$. Reduced phase space is not the same as the phase space of an unconstrained phase space, and this makes quantization very difficult. Thus, non-reductive methods that quantize first and then single out the physically relevant structure from the surplus came about. Indeed, far from reducing, one often sees the opposite move being made: expansion! This idea forms the basis of BRST theory (see Henneaux & Teitelboim (1992)). Let me quickly outline the main ideas of this approach, for they may be unfamiliar to many readers.

The basic idea in BRST theory is similar in many respects to the reduced phase space methods: one wants to construct a symplectic manifold (without constraints) to function as the phase space of the gauge system one is interested in. In the BRST case, one *enlarges* the phase space of a constrained phase space (Γ, ω, ϕ_i) by adding auxiliary variables, giving the extended system $(\Gamma_{ext}, \omega_{ext})$. These auxiliary variables consist of fermi degrees of freedom, (θ, π), called *ghosts* and their conjugate momenta (*anti-ghosts*) and they are chosen in such a way as to ease quantization.[12] The reason behind their introduction is to construct an operator D (the classical BRST operator) whose cohomology yields the gauge-invariant functions of the theory. Quantization is carried out on *all* degrees of freedom (physical, unphysical, and ghost), and the resulting quantum system is then reduced using D. Note, however, that the BRST formalism is by no means restricted to quantum theory. It has quite respectable credentials in the classical context too: the original gauge symmetry of the classical theory is replaced by a (fermionic) rigid symmetry that acts on the expanded phase space in such a way as to encode the gauge symmetry within a simpler theory.

We thus have four available spaces (ordered according to 'size'): $(\Gamma_{ext}, \omega_{ext})$, (Γ, ω), $(\mathcal{C}, \sigma)$, and $(\Gamma_{red}, \omega_{red})$. Each of these spaces has a simple interpretation in terms of the possib-

[10]Which structure we choose to quantize on has become part of the debate between substantivalists and relationalists (see, for example, Belot & Earman (1999 and 2001) and Rickles (forthcoming)).

[11]This is one of Belot and Earman's main objections to gauge-invariant interpretations of general relativity (*cf.* (1999: 177)). The other one is the problem of time.

[12]The subject of the ontological status of ghostly variables is in need of investigation. Physicists are often ambiguous on the matter of their physical status, alternating between viewing them as a heuristic crutch and having direct physical significance (see, for example, Henneaux & Teitelboim (1992: 166 and Ch.11)). Unfortunately, this issue is too complicated to tackle here - see Weingard (1988) for a detailed analysis based on connections between the interpretation of ghost fields and virtual particles.

ility spaces I introduced earlier: the expanded and unconstrained phase spaces include impossible *unphysical* states (ghosts and inaccessible states in the former; inaccessible states in the latter) that, if they represent anything, represent physically impossible worlds; the constraint surface contains physically possible states (though some worlds may be multiply represented, or else there are worlds that differ haecceitistically); points of the reduced space can be put in one-to-one correspondence with physically possible worlds with no haecceitistic differences and no multiple representation. Naturally, each of these spaces has quite specific problems, and each generally produces a distinct quantum theory. However, what I am interested in in this paper is the question of whether any of these spaces is to be preferred on the basis of symmetries, and the symmetry arguments considered in this thesis. A secondary question I wish to consider is whether or not these spaces underwrite particular philosophical positions concerning spacetime ontology. Let me now deal with these issues together by isolating four views that can be seen as defending each space as a response to symmetry arguments (i.e. potential violations of PSR).

3 Four Views on Reduction.

Belot believes that the existence of spacetime points is bound up with possibility counting. For example, he claims that

> [s]ubstantivalists count each possible embedding of a set of N particles into $\mathbb{R}^3$ as (being capable of) representing a distinct possibility — which is just to say that they will work with the standard $6N$ dimensional phase space when constructing mechanical theories. Relationalists about space will deny that embedding related by rigid motions can represent distinct possibilities; so they will identify points in the standard configuration space so related; thus they will employ that $3N - 6$ dimensional configuration space (parameterized by the relative distances) and the $6N - 12$ dimensional phase space (parameterized by ... relative distances and velocities). [2000: 580]

Thus, according to Belot, substantivalism is bound to the unreduced phase space (high possibility count) and relationalism is bound to the reduced phase space (low possibility count). And his argument is that if one moves to the reduced space - as he believes one generally should - then one is committed to the non-existence of spacetime points. In my (2004) I argued that the proposed connection between possibility counting and spacetime ontology was based upon a hidden assumption about modality: the substantivalist-unreduced space connection requires haecceitism and the relationalism-reduced space connection requires anti-haecceitism. Thus, possibility counting has got nothing to do with spacetime ontology; it is the intrusion of modality that underwrites the supposed connection to possibility counting and particular representation spaces.

We need to concede, however, that the distinction between the reduced and unreduced spaces *is* connected to differences in possibility counting: the latter contains points that simply are not contained in the former. But the latter can, with suitable contortions, incorporate anti-haecceitistic possibility counting; so too can the reduced space incorporate the haecceitistic possibility counting of the unreduced space. This is simply a result of the formal and empirical equivalence of the formulations. Given this equivalence, what are the reasons for choosing one over the other? Belot argues for reduction along the following lines:

> The trick is to allow the absolutist to specify a large space of possibilities which fall into equivalence classes ... The advocate of PSR can then claim that the true space of possibilities arises by identifying equivalent absolutist possibilities, so that there is exactly one possibility corresponding to each of the absolutist's equivalence classes. ... we can always use this trick to protect PSR against refutation by Clarke's sort of examples, where indifferent possibilities are generated by the application of symmetries. [2001: 4]

Thus, as Belot notes, a direct interpretation of a theory with symmetries (of the relevant kind) will risk violating PSR and his answer is to shift to a reductive interpretation that puts orbits in to a direct correspondence with possibilities: "by always choosing interpretations ... which "factor out" symmetries ... we can ensure that our interpretations will always respect PSR" (*ibid*.: 7).[13] With this I don't disagree: we know - given the formal and empirical equivalence of the reduced and unreduced spaces - that we will always have the option of shifting to the reduced space (at least in principle) and we know that this space will have any points related by symmetries removed; since these points were responsible for the potential violations of PSR, we will indeed have resolved the difficulty. The *technical* foundations of Belot's proposal are impeccable, as one would expect. However, the question is whether this approach is necessary and, if not as I have been arguing, whether it is worth the various technical pitfalls that such approaches inevitably must face - i.e. the difficulties with construction mentioned above.[14] This is not to mention the chunk of possibility space that we will have jettisoned without any good physical reasons! In other words, the decision to reduce in the manner suggested by Belot is a purely *metaphysical* decision that, quite literally, makes worlds of difference.

[13]Belot claims that "the techniques and results of this literature [on symmetry in geometrical mechanics] promise to offer a unifying perspective on a number of classic problems in philosophy of physics (the relation between the nature of space and the nature of motion in Newtonian physics, identical particles, the nature and significance of gauge freedom and general covariance)". I agree with this statement as it stands, but Belot takes the claim too far and attempts to create alignments between philosophical stances regarding the nature of individuals and the treatment of symmetries in the areas of physics he mentions. The equivalences and underdetermination I have shown to hold in such contexts outlaws such alignments.

[14]There are also the problems - mentioned by Belot (2003: 407) - concerning the ad hoc removal of certain points (those representing symmetrical configurations and those representing collision points) from the unreduced phase space in order that the reduced phase space can be constructed. Technical details involving differences between discrete and continuous symmetries are crucial here - see Belot (2003b) for more details.

There are two further ‘technical’ problems with Belot’s proposal: (1) rarely do we construct ‘intrinsic’ reduced phase spaces for theories, generally beginning with the enlarged space with symmetries and then factoring them out; (2) although the enlarged and reduced spaces are classically equivalent, they in fact lead to distinct quantizations, and so physics *might* be decisive in choosing one over the other; which this is will surely vary. The first point is simply that if Belot’s PSR wielding theorist is to hold his head up high, he should be able to construct the reduced space form of a theory directly; as he points out himself, “the reduced theory knows where it came from” (2001: 14).[15] The second point is more complicated and arises out of studies at the intersection of geometric mechanics and quantization (see Gotay (1984) or Plyushchay & Razumov (1995) for details). The upshot, however, is simply that the choice between enlarged and reduced spaces cannot simply be a matter of policy. As to Belot’s underlying desire to show how PSR can always be protected by imposing PII on symmetrical worlds, there is another option that always works too: one simply views the symmetries as expressing an *indifference* concerning the states and observables of physical systems entering into them. Thus, there is always a sufficient reason for the world’s being where it is in a universe with a homogenous spacetime: the world is indifferent to where it placed; one position is as good as any other! Given this rather obvious possibility, Belot’s account seems to be a little under motivated. Further, his account faces a serious problem when one considers quantization, for certain states factored out via Belot’s method might be required to fluctuate in the quantum theory.[16] Saunders (2002, 2003) offers an alternative defense of the PSR based on his idea that the individuals entering into symmetrical relations of the kind we are interested in will be weakly discernible (and absolutely indiscernible) but referentially indeterminate; the symmetries fail to get their teeth into the PSR. However, I argue that the end result, as regards the question of reduction, is the same as with Belot’s proposal: there are no indiscernible possibilities; anti-haecceitism is enforced.

Saunders’ views on the question of reduction can best be appreciated from the perspective of the hole argument. The hole argument can be viewed as showing that particle coordinates at a given time are underdetermined, they are arbitrary functions of time, in other words: gauge. Likewise for the values of fields. Shifting from a coordinate dependent approach, we can couch the argument in terms of the points of the manifold so that the values of *local* fields or particle position are underdetermined. According to the hole argument, general relativity cannot predict such quantities uniquely. The natural solution is to shift focus away from absolute quantities, which are not invariant under the transformations of general relativity, to those that are invariant. These happen to be relational

[15]Compare this with Earman’s point that the relationalist should be able to construct his theories in relationally pure vocabulary, rather than hitching a ride on the substantivalists formulations (1989: 135).

[16]Belot is clearly well aware of this, of course (*cf.* (2003: 221)); indeed, it informs his and Earman’s taxonomy of interpretations of general relativity. Note that Belot seems to shift to the view that one must await an answer from quantum theory to the question of how best to deal with symmetry (see 2003b). However, if quantum statistical mechanics is anything to go by, even quantum theory cannot determine the correct geometric space of the classical theory: as Huggett and French & Redhead have demonstrated, the reduced and unreduced formalisms are compatible with both classical and quantum theories. (A lot can hang on the nature of the symmetries in question - discrete versus continuous - what goes for one type will not necessarily hold for the other).

quantities; that is, the invariant quantities of general relativity are those that are not defined relative to the manifold, but with respect to physical fields or objects. Now, Saunders argues that the sort of relationalism underwritten by such a response to these symmetry arguments “has nothing to do with a *reductionist* doctrine of space or spacetime” (2002: 2)—i.e. with what is standardly labeled “relationalism” and what Saunders calls “eliminative relationalism”. He uses a ‘modernized’ version of PII informed by modern logic from which he derives a position that he calls “non-reductive relationalism”. However, as Saunders points out himself, the view that emerges can be applied to “any exact symmetry in physics” (*ibid.*), not just spacetime symmetries.

Saunders argues that Leibniz was led to his eliminative relationalism because of the logic of his time, based as it was on the notion that propositions were of subject-predicate form. When relations are considered, the proposition is still taken to be of subject-predicate form, and applies to a single subject. The relations had to be reduced to monadic properties of their relata. This view of relations naturally underwrites what Saunders calls “Leibniz’s independence thesis”, the claim that a description of a thing should be intrinsic, containing no reference to other things or relations (*ibid*: 13). Now Saunders points out that when we consider Frege’s logic there is no such privileging of predicates, or “1-place concepts” in the terminology of Frege’s *Begriffschrift*, with its distinction between ‘object’ and ‘concept’. Relations are free standing and propositions aren’t restricted to subject-predicate form. Saunders then examines how this shift in logic affects PII. Firstly, he notes that if one deals solely in 1-place predicates then PII says that objects with exactly the same properties are (numerically) identical. Adding higher-order predicates into one’s language weakens the principle since then PII says that objects with exactly the same properties and relations are identical - there is another level of ‘similarity’ the objects have to satisfy. This gives us the strong and weak forms of PII respectively; clearly, Leibniz’s logic forced him to endorse the strong form, and it is this overly stringent form that lends itself to easy counterexamples. Working with relations and adding identity to our language, Saunders presents an axiom schema formalising the indiscernibility of identicals as follows (*ibid.*: 18):

$$\mathsf{x}=\mathsf{x};\ \mathsf{x}=\mathsf{y}\rightarrow(\mathsf{Fx}\rightarrow\mathsf{Fy}) \tag{1}$$

This schema implies that terms with the same reference can be substituted *salva veritae*. Now Saunders (*ibid.*: 19) proceeds to give a definition of identity[17] using only terms ‘x’ and ‘y’, and unary predicates A (i.e. properties), binary predicates B (i.e. relations), up to n-ary predicates P (i.e. higher order relations), such that $\mathsf{x}=\mathsf{y}$ iff:

$$\begin{gathered}\mathsf{A(x)}\longleftrightarrow\mathsf{A(y)}\\ \mathsf{B(x,u_1)}\leftrightarrow\mathsf{B(y,u_1)},\ \mathsf{B(u_1,x)}\leftrightarrow\mathsf{B(u_1,y)}\\ \vdots\qquad\vdots\end{gathered}$$

[17]He credits the definition to Hilbert and Bernays, and notes that it has been defended by Quine.

$$P(x, u_1, ..., u_{n-1}) \leftrightarrow P(y, u_1, ..., u_{n-1}), \text{ and permutations.}$$

The definition simply says that two things are identical if they match up on properties and relations. The relation conditions are defined so that, whatever relations x stands in (for some free variable: u_1 in the binary case u_1 to u_{n-1} in the n-ary case) y stands in too. I mentioned that for languages with only 1-place predicates one gets a strong principle of identity, such that: $[\exists F, (Fx \wedge \neg Fy) \rightarrow (x \neq y)]$. For more general languages, admitting higher order predicates, Saunders distinguishes three ways to get $x \neq y$, i.e. non-identity (*ibid.*: 19-20). Firstly, he says that two objects are "absolutely discernible" if there is a formula (e.g. $P(z, u_1, ..., u_{n-1})$) with some free variable u_i that applies to one, x say, but not the other, y. In which case $x \neq y$. Secondly, two objects are "relatively discernible" if there is a formula in two free variables (e.g. $P(z, u_1, ..., u_2)$) that applies to x and y only in one order.[18] Thirdly, two objects are "weakly discernible" if $B(x, y)$ is true, B is a symmetric predicate (i.e. $B(x, y)$ iff $B(y, x)$), and B is irreflexive, so that $B(x, x)$ is always false - this counts as non-identity according to the definition because it implies that there is a u_1 such that $B(u_1, x)$ is true and $B(u_1, y)$ is false, namely for $u_1 = y$.[19]

This is Saunders' modernised version of PII: "objects are numerically distinct only if absolutely, relatively, or weakly distinct" (*ibid.*: 20). With this definition of identity, and with his ways of getting non-identity, Saunders is able to show that the standard counterexamples to PII are in fact examples of weak discernibles, and so do not violate his PII. Black's two qualitatively identical iron spheres in empty space are weakly discernible according to Saunders' account because there exists an irreflexive (distance) relation. Symmetry and qualitative identity are not sufficient to secure indistinguishability, though they are clearly necessary. He notes that there is a counterexample to it in the form of two or more bosons in exactly the same state; so PII is neither necessary nor contingently true, it still faces difficulties in QM. Though it can accommodate fermions, for even in the most symmetrical scenario ("where the spatial part has exact spherical symmetry, and the spin state is spherically symmetric too" (*ibid.*)) the fermions will satisfy the relation of having opposite component of spin to one another but not to themselves. This is clearly irreflexive and so any two fermions will be weakly discernible.[20]

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[18] An obvious example is the 'taller than' relation: Joe is taller than Dean, but Dean isn't taller than Joe, hence, Joe and Dean bear a different relation to one another. Clearly, asymmetry is at the root of this case of non-identity.

[19] An obvious example here is to choose B as a distance relation between two objects: Steve is 10 meters away from Dean, and Dean is 10 meters away from Steve (so they are not relatively discernible), but Dean is not 10 meters away from himself, and neither is Steve.

[20] Saunders draws metaphysical conclusions from the violation of his PII by bosons. He advocates a non-individualistic view according to which bosons are modes of a gauge field (with the exception of the Higgs boson). Note that given his PII, it is not possible to advocate the 'state restriction' view whereby bosons are individuals whose wave-functions are subject to symmetrization as an initial condition. Now, I am quite sympathetic to this view for it makes a principled distinction between 'matter' and 'force', a difference that seems to occur in nature, but which is conflated on most other conceptions of quantum particles (*cf.* Saunders (2003: 294-5)). However, Saunders continues to refer to them as "objects", even though he claims that "one cannot *refer* to any one of them singly", and suggests that they be called "*referentially indeterminate*" (*ibid.*). I think as far as bosons go, he'd do better to drop all talk of objects at a fundamental

To return to the issue of reduction, let us recall Leibniz's shift argument. This was supposed to cause problems for PSR: space's being homogeneous, there was no reason why a system should be located at one part of space rather than some other. That does not mean PII is ad hoc, simply that Leibniz thought that commitment to PII was part and parcel of being committed to PSR. However, we cannot forget Leibniz's notion of object as given intrinsically, and its description as giving a 'complete concept'. Saunders calls this aspect of Leibniz's philosophy the "independence thesis": roughly, an objects identity is independent of anything else 'external' to that object. Saunders' claim is that Leibniz understood PII as entangled with the independence thesis: without the independence thesis, PII might allow external reasons to come into play in its protection of PSR and, in particular, in the individuation of the homogeneous parts of space, and thus bring the shift argument to a halt *without recourse to reductive* (i.e. eliminative) *measures*. With external reasons not playing a role, and internal reasons absent, the symmetry arguments are in clear violation of PSR. But the 'PII + independence thesis' package can be divided, and in so doing Saunders argues that a version of relationalism follows that is non-reductive precisely because it denies the independence thesis, and thus allows external factors to enter into the definition and individuation of an object. However, it remains committed to PII. The connection to the 'reduction/non-reduction' issue is clear: "relations, for Leibniz, had to be *reducible* - derivable from the monadic properties of their relata" (*ibid*. 17); when these monadic properties are equivalent so is the relational structure - the corresponding possibility space is represented by Γ_{red}.

In brief, we have the following chain of reasoning leading to Saunders' view. Leibniz's relationalism involves three components: PSR, the independence thesis, and PII. The independence thesis filters into PII, and restricts the latter principle to *internal* factors, so that relations to other things are not to be included in the description of an object. PSR faces trouble from the symmetry arguments, since it seems that objects related by certain symmetries count as identical in all *internal* respects, i.e. in all respects that matter in this case. PII, informed by the independence thesis, enters the analysis and is used to identify any such objects (points, worlds, etc...). This means that only internal (i.e. intrinsic) qualitative differences count towards numerical differences so that the differences generated by symmetries do not imply genuine physical differences. Saunders denies the independence thesis thus allowing any physical relations to individuate and, though absolute quantities - represented by e.g. gauge dependent variables - are eliminated in favour of relations between objects, his analysis still allows for spacetime points (and any weak discernibles) to be distinct and individuated. The upshot of this vis-à-vis the PSR is that the problems posed by the symmetry arguments dissolve; one uses relations to matter and events to specify points of space:

level entirely, possibly in favour of pure structure. The latter might seem vaguer, but at least one can refer to it singly and determinately! As pointed out in French & Rickles (2003: 228), the non-reductive nature of this form of relationalism sits well with the structuralist notion of individuation of relata by relations, according to which the relata do not have ontological primacy over relations but are understood in terms of "intersections of relations".

> Absolute positions disappear; under the PII points in space, considered independent of their relations with other point and with material particles, all disappear. But points in space considered independent of matter, but in relation to other points in space, are perfectly discernible (albeit only weakly), for they bear non-reflexive metrical relationships with each other. There is no problem for the PSR in consequence; there is no further question as to which spatial point underlies which pattern-position, for they are only weakly discernible. [2002: 23]

Saunders sees this as motivating an "even-handed approach to matter and space": things from either category can serve to individuate other members from their own and from the other category (*ibid*.: 25). Now, my claim is that reductive relationalism follows from the symmetry arguments involved here (e.g., the hole and Leibniz shift arguments) only if it is coupled to PII (construed as an anti-haecceitist principle). Saunders, however, claims that PII is not necessarily anti-haecceitist, nor is it necessarily reductive and, therefore, that the symmetry arguments do not imply reductive relationalism. His path was to deny the independence thesis and retain PII, whereas I prefer to say both that PII isn't a necessary part of the relationalist's position and nor was it *not* a part of the substantivalist's position. This latter point leads naturally into the sophisticated substantivalist positions (on which, see Pooley (2002)); and, indeed, Saunders mentions the similarities between his own self-styled relationalist approach and these other self-styled *non-relationalist* approaches. Both approaches accept PII but the latter see reduction (i.e. identification of equivalent worlds) as concomitant with this, whereas Saunders does not; rather, he claims that spacetime points can have well defined identities in the absence of matter, and can be uniquely referred to in the presence of matter. One might think that this is even more substantivalist than sophisticated substantivalist positions! But Saunders agrees with me that it is ontological priority that counts when it comes to the definition of these positions, and his approach is neutral on this: each category of 'stuff' (spacetime and matter) can be used to individuate the other. In this sense I would say that Saunders' position is more naturally understood as a structuralist one; indeed, he ends up in more or less the place I wish to end up, but he gets there by a different route and for different reasons.

Thus, Saunders protects PSR by implementing but modifying PII. The result looks non-reductive, but on closer inspection the non-reductive aspect concerns objects within worlds and not worlds themselves. Since the geometric spaces correspond most closely to possibility spaces, rather than singular possible worlds and their contents, we have to inquire as to what Saunders' version of PII says about possibility space. The first thing to note is that Saunders restricts the application to worlds that have the same physical laws as our own: for different laws there may be different PIIs (2003: 297). Then, since there are no physical relations that hold between distinct possible worlds[21], Saunders' PII reduces to Leibniz's PII and we are left with what is essentially a reductive version: "Given that possible worlds bear no physical relations to one another, it follows from the PII

[21] I think Saunders' reasoning is sound on this point. He notes that a world "is a system that is physically closed" (*ibid*.), and that simply means that any physical relations that hold at that world are contained in it too.

that numerically distinct worlds will be absolutely (and in fact strongly) discernible" (*ibid*.: 298). Furthermore, his discussion on the relationship between symmetry and observables shows that he in fact endorses a rather extreme reductive view. Although the PII itself does not appear anti-haecceitist or reductive at first sight, countenancing as it does physical relations, it is both of these things when applied in the context of possible worlds. Thus, although Saunders can retain spacetime points (and any weak discernibles) with his PII, he is lead to back the connection between relationalism and possibility counting that I denied in (Rickles 2004); according to this account, the relevant geometric space for physical theories is Γ_{red}. Hence, in the final analysis, PSR is preserved *á la* Leibniz-Belot, simply by implementing reductive PII. However, Saunders has shown us that reduction at the level of worlds does not imply eliminativism of indiscernible entities within worlds.[22] As I mentioned earlier, this is simply yet another flavour of sophisticated substantivalism, albeit one clothed in relationalist garments.

Rovelli has recently outlined an interpretation based on the full, unconstrained (extended) configuration space (along with its associated extended phase space). Rovelli's claim is that a number of thorny problems from general relativity and quantum gravity can be cleaned up or resolved by utilizing his distinction between 'partial' and 'complete' observables: a *partial* observable is a physical quantity to which we can associate a measurement leading to a number and a *complete* observable is defined as a quantity whose value (or probability distribution) can be predicted by the relevant theory. Partial observables are taken to coordinatize extended configuration space $\mathcal{Q}$ and complete observables coordinatize reduced phase space Γ_{red}; the "predictive content" of some dynamical theory is then given by the kernel of the map $f : \mathcal{Q} \times \Gamma_{red} \to \mathbb{R}^n$. The relevant aspect from this program for this section is captured by his claim that "the *extended* configuration space has a direct physical interpretation, as the space of the partial observables" (2002: 124013-1). This space gives the *kinematics* of a theory and the *dynamics* is given by the constraints, $\phi(q^a, p_a) = 0$, on the associated extended phase space $T^*\mathcal{Q}$. *Both* are invested with physicality by Rovelli. Thus, whereas, for example, Stachel (1993) argues that the kinematical state space of a background independent theory like general relativity has no physical meaning prior to a solution (so that only the dynamical state space is invested with the power to represent; kinematics being derivative), Rovelli appears to take both kinematic and dynamical spaces as equally capable.

The view Rovelli defends has some immediate philosophical interest since it is non-reductive and yet Rovelli is a self-proclaimed relationalist. Thus, prima facie, we seem to have an instance of a break between possibility counting/geometric spaces and spacetime ontology. However, it quickly becomes evident that there is a conflict between his relationalism and his choice of representational space. As regards the former, I think that a rather naive verificationism is responsible for Rovelli's views: only measurable things are real and since spacetime location is not measurable but relations between objects are

[22] Belot, on the other hand, sticks to the original PII to protect PSR. He doesn't consider Saunders' version of PII, and goes along with the idea that both absolute quantities *and* spacetime points (more generally: the things with respect to which absolute quantities are defined) are eliminated.

measurable, space and time are not real but are instead defined by correlations between objects. We can agree with Rovelli that the physically measurable quantities are those that are invariant under the symmetry group of a theory, i.e. the gauge-invariant quantities. It is quite another matter to then say that these are the only physically real things, that they *exhaust* physical reality. Clearly, in Rovelli's view, however, there are plenty of physically real objects; namely, those things entering in to relations that are not themselves measurable. Any relationalism will require a definite set of material objects to generate the required relations (particles, fields, etc...). Rovelli's view is not that there are no objects per se, but that there are no objects corresponding to those that ground absolute (non-measurable, gauge-dependent) quantities. Thus, he moves from the fact that we never measure position in spacetime to the non-existence of spacetime points. However, his work on partial observables suggests something very different to this rather crude verificationism. Let me develop some more of the details of this latter approach.

Rovelli distinguishes between two extremes of interpretation with respect to the formal variables of a theory for a system with constraints (I have changed the notation to suit my own):

> It is sometimes claimed that the theory can only be interpreted if one finds a way to "deparameterize" the theory, namely, to select the independent variable among the variables q^a. In the opposite camp, the statement is sometimes made that only variables on the physical phase space Γ_{red} have a physical interpretation, and no interpretation should be associated with the variables of the extended configuration space Γ. [2002: 124013-7]

By contrast, Rovelli invests elements of Γ and $\mathcal{Q}$ (including gauge-dependent quantities) with physical reality; indeed, elements of the latter are taken to be "the quantities with the most direct physical interpretation" (*ibid*.). Complete observables - i.e. the quantities we actually measure and are able to predict uniquely (i.e. Bergmann/Dirac observables: *cf.* Earman (2003)) - are dynamically determined *á la* Stachel (*op. cit.*):

> Such a quantity can be seen as a function on the space of solutions modulo all gauges. This space is the physical phase space of the theory Γ_{red}. ... Any complete observable can thus be expressed as a function on Γ_{red}. [*ibid*.: 124013-3]

Crucially, Rovelli notes that there is an equivalent description of any complete observable "as a function on the *extended* phase space having vanishing Poisson brackets with all first class constraints" (loc. cit.; my emphasis). Thus, we see again the formal equivalence between reduced and unreduced spaces even at the level of observables. In this approach, then, Rovelli distinguishes between what is observable and what there is (i.e. ontology), whereas elsewhere (1997 and 2001), in arguing for his relationalism, he assumed a direct connection between the two.

However, I think it is clear that Rovelli does not want to imbue what are physically *impossible* states with physical reality — that is, Γ isn't Rovelli's space of choice. That would clearly be crazy. Though he often speaks as if he means to endorse this 'crazy' metaphysic, we can best understand his view, I think, as being based upon the constraint surface $\mathcal{C}$. Thus, he speaks of a mechanical system as being completely determined by the unreduced, unconstrained phase space *plus* a set of constraints (if necessary). We should, therefore, view the constraints as physical 'reality conditions' and only those satisfy them as invested with reality. Nonetheless, Rovelli is still avowedly realist about non-gauge-invariant quantities, quantities that do not commute with the constraints (partial observables). However, he avoids any 'hole-type' problems by defining (complete) observables as functions on the reduced space, quantities that are constant along gauge orbits of the unreduced space. Though nowhere near as crazy as the above metaphysics involving impossible states, the position Rovelli presents is a metaphysics nonetheless. In imbuing the gauge-dependent quantities with physical reality he is putting in his metaphysics by hand, for it is not being read off the physics. Clearly, too, one could, as Rovelli did previously, adopt the view that only the complete observables are physically real; one doesn't require the reduced space for this sort of position.

Finally, we have the expansionist option. Redhead outlines such a *liberal* view of symmetries: "forget all about gauge symmetry in the original Yang-Mills sense, and impose BRST symmetry directly as the fundamental symmetry principle" (2003: 137).[23] The idea, as Redhead describes it, is to "allow non-gauge-invariant quantities to enter the theory via surplus structure ... [a]nd then develop the theory by introducing still more surplus structure, such as ghost fields, antifields and so on" (*ibid*.: 138). He claims that this is the method that is most in line with the practice of physics. He also notes that, given the mathematical nature of the surplus structure, "this [approach] leaves us with a mysterious, even mystical, Platonist-Pythagorean role for purely mathematical considerations in theoretical physics" (*ibid*.). However, though it may be of value in the quantum gauge field theories of the electromagnetic, electroweak, and strong forces, I don't see that it is at all applicable in the context of classical and quantum general relativity in which the gauge symmetries are directly connected to the dynamics. Even if it could be shown that the BRST method is applicable, the suggested enlargement of phase space is a purely *classical* affair: one reduces by the BRST operator once the quantum level has been reached. Thus, the device of BRST appears to be a purely heuristic one, and cannot be seen as underwriting any *unique* interpretive stance. Indeed, in the final (quantum) analysis, the resulting picture matches, more or less, the Dirac quantization methods in that reduction is carried out at the quantum level. Classically, of course (as Redhead points out), the problem is to make sense of the auxiliary variables that are employed, and this would require considerable work. In particular, I think analysis is needed on the differences between the various senses of 'surplus structure' that come into play here: ghosts, impossible states, and indiscernible states. If it can be shown that ghosts and impossible

[23]He may not actually wish to be associated with this view, it isn't fully clear from the text which of the methods he endorses. However, the fact remains that this is a possible interpretive option to take with regard to gauge symmetries. If he doesn't in fact endorse this view then let us say Redhead refers to some 'other-worldly' philosopher of physics who *does* endorse it.

states are of the same kind then I think this would give us grounds to reject the BRST expansionist approach.

Thus, we have four diverging views on the question of whether to reduce or not: Belot argues that we should, as a matter of general practice, reduce and get rid of the symmetries[24]; Saunders says that 'all out' reduction (i.e. elimination) is not necessary to get the kind of deflationary conclusion Belot wants, but nonetheless implements reduction at the level of worlds[25]; Rovelli says that we should utilize the constraint surface (or, rather, the unreduced space plus a set of constraints); and Redhead argues that we should expand rather than reduce or constrain. We saw above that Rovelli advocates a view whereby no reduction or gauge-fixing is carried out: the extended space and the set of constraints is sufficient to determine a sensible interpretation. With this I agree, but the interpretation I give differs from both Rovelli's and Saunders' 'non-reductive' methods. Let me quickly sketch this view by sounding a warning note for hasty reduction proposals, followed by a brief summary of what I hoped to have shown thus far.

It is clear that any choice of space must come about as a result of experimental confirmation; and this can only come about at the level of quantum theory. Even then, whether or not this will choice will be possible - i.e. whether it could ever be shown that a certain way of counting possibilities is the correct one - is far from obvious. On a purely conceptual level I suggest that the unreduced space is to be preferred over the reduced space. The unreduced is ontologically neutral in that it allows for large and small possibility counting in a fairly unproblematic way. It leaves intact (and manifest) properties to do with symmetries, such as covariance and locality. It makes no prior assumptions about what degrees of freedom should be quantized and allowed to fluctuate (*cf*. Plyushchay & Razumov (1995: 248-9)). The reduced phase space, of course, takes a stance on what is physically relevant, and this choice is carried over to the quantum theory. Thus, there will be elements of the unreduced space that will not be subject to quantum fluctuations, but will be eliminated instead. Though I think the reduced phase space can be given a well-motivated structuralist defense (encoding, as it does, the supposedly physical (invariant) structure), I think that it should be a part of the *honest* structuralists manifesto that stances taken regarding to the individual elements entering into gauge-type symmetries should be avoided, for there is a radical underdetermination between eliminative 'raw structuralist' ontologies and non-eliminativist object-laden ontologies.[26] This, of course,

[24]Castellani (2003) has recently defended a similar view in her analysis of Dirac's theory of gauge systems and constrained Hamiltonian systems. However, the reductive answer she gives to our opening question is not really defended at all.

[25]Essentially, Saunders' argument is that the fact that PII amounts to a reductive principle when imposed at the level of the worlds themselves does not, as if often believed (by Belot, for one), imply that PII involves elimination *within* worlds. Denying the independence thesis allows one to individuate what would have otherwise been indiscernible entities by using the relations they bear.

[26]Thus, I diverge quite radically from French & Ladyman's 'ontic' version of structural realism (see, for example, their (2003)). The reason: they see the underdetermination as applying to the 'individualistic' and 'non-individualist' packages only, and not as involving the eliminativist views; for this reason the drop the former package entirely and opt for an ontology of pure structure (not involving objects). These latter views are most naturally expressed in the reduced space, and it is that space that the structural realist

includes Rovelli's partial observables realism.

I hope to have shown (or at least made plausible) the following in this paper:

- Theories are not bound to either reduced or unreduced spaces (they admit PII and non-PII-type formulations while still respecting PSR). Reduced and unreduced formulations are empirically equivalent.
- The reduced and unreduced spaces *are* bound up (in a certain sense) with possibility counting: they contain different cardinalities of possibilities. But haecceitism and anti-haecceitism are nonetheless compatible with theories formulated in both spaces.
- If we choose the reduced space (without pressure from quantization) then we are cutting out possibilities in a way not dictated by the physics itself – i.e., the metaphysics of possibility counting that results is not 'read off' the physics in this case.
- Since unreduced spaces allow 'all the options' (conceptual elbow room, as it were) we would be better off choosing such a space as the neutral base. We should, more properly, view the constraint surface as our base, for in this case the metaphysics of outlawing physically impossible states *is* easily read off the physics.

Whether or not pressures from quantum gravity squeeze out the elbow room offered by non-reductive accounts is something that remains to be seen.

4 References.

Adams, R. M. (1979) "Primitive Thisness and Primitive Identity." *The Journal of Philosophy.* **Vol. LXXVI**, No.1.

Alexander, H. G. (1956) *The Leibniz-Clarke Correspondence.* Manchester University Press.

Baez, J. C. & Munian, X. (1994) *Gauge Fields, Knots, and Gravity.* World Scientific Press.

(of the ontic brand) will wish to be aligned with: it encodes all and only the *invariant* and, they will say, physical structure. Incidentally, I think the fact the the reduced space does encode this structure, and can be associated with an elimination of objects – though this is underdetermined, of course; hence my desire to stick to the unreduced space –, offers a quick and easy answer to the question: 'What is structure?' It is given precisely by the variables that separate the points of this space; I suggested that these should be understood as structural (i.e. non-reducible) correlations. Moreover, the factorization (by the gauge group) that leads to the reduced space also offers a response to Cao's objection concerning the distinction between mathematical and physical structure (on which, see French & Ladyman (2003: 45-6)): it is that which is invariant under this group of transformations.

Belot, G. (2000) “Geometry and Motion”. *Brit. J. Phil. Sci.*, **51**: 561-595.

Belot, G. (2001) “The Principle of Sufficient Reason.” *J. Phil.* **98**: 55-74.

Belot, G. (2003) “Notes on Symmetries.” In K. Brading and E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press: 393-412.

Belot, G. (2003) “Symmetry and Gauge Freedom.” *Stud. Hist. Phil. Mod. Phys.*, **34**: 189-225.

Black, M. (1952) “The Identity of Indiscernibles.” *Mind*, **61**: 153-164.

Butterfield, J. (1993) “Identity and Interpretation in Quantum Theory”. *Stud. Hist. Phil. Mod. Phys.*, **24**, 3: 443-476.

Castellani, E. (2003) “Dirac on Gauges and Constraints.” Forthcoming in *International Journal of Theoretical Physics*.

Cover, J. A. & O’Leary-Hawthorne, J. (1996) “*Haecceitism* and Anti-*Haecceitism* in Leibniz’s Philosophy”. *Noûs*, **30**, 1: 1-30.

Dirac, P. A. M. (1964) *Lectures on Quantum Mechanics.* Yeshiva University, Belfer Graduate School of Science Monographs Series, No.2.

Earman, J. (1989) *World Enough and Space-Time. Absolute versus Relational Theories of Space and Time.* MIT Press.

Earman, J. (2003) “Ode to the Constrained Hamiltonian Formalism.” In Brading, K. & Castellani, E. (eds.), *Symmetries in Physics: Philosophical Reflections.* Cambridge University Press, 2003: 140-162.

French, S. (1989) “Quantum Physics and the Identity of Indiscernibles.” *Australasian Journal of Philosophy,* **67**: 432-446.

French, S. (1995) “Hacking Away at the Identity of Indiscernibles: Possible Worlds and Einstein’s Principle of Equivalence.” *Journal of Philosophy*, **92**, Issue 9: 455-466.

French, S. (1998) “On the Withering Away of Physical Objects.” In E. Castellani (ed.), *Interpreting Bodies.* Princeton University Press: 93-113.

French, S. (1999) “Models and Mathematics in Physics: The Role of Group Theory.” In J. Butterfield & C. Pagonis (eds.), *From Physics to Philosophy.* Cambridge: Cambridge University Press: 187-207.

French, S. (2001) “Getting Out of A Hole: Identity, Individuality and Structuralism in Space-Time Physics.” *Philosophica*, **67**: 11-29.

French, S. & Rickles, D. (2003) “Understanding Permutation Symmetry.” In K. Brading and E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press: 212-238.

Gambini, R. & Pullin, J. (1996) *Loops, Knots, Gauge Theories, and Quantum Gravity.* Cambridge University Press.

Guilini, D. (2003) “That Strange Procedure Called Quantisation.” In D. Giulini, C. Kiefer, and C. Lammerzahl (eds), *Quantum Gravity: From Theory to Experimental Search.* Springer-Verlag, 2003: 17-40.

Gotay, M. (1984) “Constraints, Reduction, and Quantization.” *J. Math. Phys.*, textbf27(8): 2051-2066.

Govaerts, J. (2001) “The Quantum Geometer’s Universe: Particles, Interactions and Topology.” arXiv:hep-th/0207276.

Hacking, I. (1975) “The Identity of Indiscernibles.” *Journal of Philosophy,* **72**: 249-256.

Henneaux, M. & Teitelboim, C. (1992) *Quantization of Gauge Systems.* Princeton University Press.

Huggett, N. (1994) “What are Quanta, and Why does it Matter?” *PSA, 1994*, **Volume 2**: 69-76.

Huggett, N. (1999) “Atomic Metaphysics.” *Journal of Philosophy*, **96**: 5-24.

Ismael, J. & van Fraassen, B. (2003) “Symmetry as a Guide to Superfluous Theoretical Structure.” In K. Brading & E. Castellani (*eds.*), *Symmetries in Physics: Philosophical Reflections*. Cambridge University Press: 371-392.

Kaplan, D. (1975) “How to Russell a Frege-Church”. *J. Phil.* **72**: 716-729.

Kripke, S. (1980) *Naming and Necessity*. Oxford: Basil Blackwell.

Lewis, D. K. (1975) *Counterfactuals.* Oxford: Basil Blackwell.

Lewis, D. K. (1983) “New Work for a Theory of Universals.” *Australasian Journal of Philosophy.* **61**: 343-377.

Lewis, D. K. (1983b) “Individuation by Acquaintance and by Stipulation.” *The Philosophical Review.* **92**: 3-32.

Lewis, D. K. (1986) *On The Plurality of Worlds.* Blackwells.

Marsden, J. (1992) *Lectures on Mechanics.* Cambridge University Press.

Marsden, J. & Weinstein, A. (1974) “Reduction of Symplectic Manifolds with Symmetry.” *Reports in Mathematical Physics*, **5**.

Matschull, H-J. (1996) “Dirac’s Canonical Quantization Programme.” arXiv:quant-ph/9606031.

Melia, J. (1999) “Holes, Haecceitism and two Conceptions of Determinism.” *Brit. J. Phil. Sci.*, **50**, 4: 639-638.

Plyuschchay, M. S. & Razumov, A. V. (1995) “Dirac versus Reduced Phase Space Quantization.” In J. M. Charap (ed.), *Geometry of Constrained Dynamical Systems.* Publications of the Newton Institute, Cambridge University Press: 239-250.

Pooley, O. (2002) *The Reality of Spacetime.* Unpublished PhD Thesis. Oxford University.

Pooley, O. (forthcoming) “Points, Particles, and Structural Realism.” To appear in S. French, D. Rickles, and J. Saatsi (*eds.*), *Structural Foundations of Quantum Gravity.* Oxford University Press.

Quine, W. V. O. (1976) “Whither Physical Objects?” In R. S. Cohen, P. K. Feyerabend, and M. W. Wartofsky (eds.), *Essays in Memory of Imre Lakatos*. Dordrecht: Reidel.

Redhead, M. L. G.. (1975) “Symmetry in Intertheory Relations”. *Synthese*, 32: 77-112.

Redhead, M. L. G. (2003) “The Interpretation of Gauge Symmetry”. In K. Brading & E. Castellani (eds.), *Symmetry in Physics: Philosophical Reflections.* Cambridge University Press: 124-139.

Redhead, M. & Teller, P. (1992) “Particle Labels and the Theory of Indistinguishable Particles in Quantum Mechanics.” *Brit. J. Phil. Sci.*, **43**: 201-218.

Rickles, D. (2004) “Relationalists can be Inflationists Just Like Substantivalists.” Unpublished Manuscript.

Rickles, D. (forthcoming) “Time and Structure in Canonical Gravity.” To appear in S. French, D. Rickles, and J. Saatsi (*eds.*), *Structural Foundations of Quantum Gravity.* Oxford University Press.

Rovelli, C. (1997) “Halfway Through the Woods”. In J. Earman & J. Norton (eds.), *The Cosmos of Science*. University of Pittsburgh Press: 180-223.

Rovelli, C. (2001) “Quantum Spacetime: What Do We Know?”. In C. Callender & N.

Huggett (*eds.*), *Physics Meets Philosophy at the Planck Scale*. Cambridge University Press: 101-122.

Rovelli, C. (2002) “Partial Observables.” *Phys. Rev. D*, **65**: 124013-124013-8.

S. Saunders. (2002) “Indiscernibles, General Covariance, and Other Symmetries”. http://philsci-archive.pitt.edu/archive/00000416/.

S. Saunders. (2003) “Physics and Leibniz’s Principles.” In K. Brading and E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press: 289-307.

Stachel, J. (1993) “The Meaning of General Covariance: The Hole Story.” In J. Earman, A. Janis, and G. Massey (eds.), *Philosophical Problems of the Internal and External Worlds: Essays on the Philosophy of Adolf Grünbaum.* Pittsburgh: University of Pittsburgh Press: 129-160.

Teller, P. (1993) “Quantum Mechanics and Haecceities.” In E. Castellani & P. Mittelstaedt (eds.), *Interpreting Bodies.* Princeton University Press.

Woodhouse, N. M. J. (1980) *Geometric Quantization*. Oxford: Clarendon Press.