# Metaphysical Underdetermination: Why Worry?

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Abstract
Various forms of underdetermination that might threaten the realist stance are examined. That which holds between different 'formulations' of a theory (such as the Hamiltonian and Lagrangian formulations of classical mechanics) is considered in some detail, as is the 'metaphysical' underdetermination invoked to support 'ontic structural realism'. The problematic roles of heuristic fruitfulness and surplus structure in attempts to break these forms of underdetermination are discussed and an approach emphasizing the relevant structural commonalities is defended.

## 1. Introduction: Varieties of Underdetermination

> 'The phenomena underdetermine the theory. There are in principle alternative developments of science, branching off from ours at every point in history with equal adequacy as models of the phenomena. Only angels could know these alternative sciences, though sometimes we dimly perceive their possibility. The theory in turn underdetermines the interpretation. Each scientific theory, caught in the amber at one definite historical stage of development and formalization, admits many different tenable interpretations. What is the world depicted by science? That is exactly the question we answer with an interpretation and the answer is not unique.' (van Fraassen 1989)

Within this passage we can identify different varieties of underdetermination. The first is the (comparatively) straightforward, well-known kind, whereby the 'phenomena' underdetermine theory. Adopting the representational framework of the so-called 'semantic' approach, we can present this form of underdeterminaiton in terms of the possibility that a given set of empirical sub-structures may be embedded in more than one set of theoretical structures. A well-known objection is that, within scientific practice, it is typically difficult to find one such theoretical structure, much less come up with others, but the modal element of the above claim should not be overlooked: even if we cannot now identify more than one theory that can 'save the phenomena', such theories *could* exist. Obviously this raises the question of the grounds for such a modal claim.

When it comes to the issue of how we should handle such cases, the structural realist urges ontological commitment to the underlying structure that is taken to be common to both theories (Ladyman and Ross 2007). Of course, the objection will run that there may not be any such common structure but then the matter has to be determined on a case by case basis. Rather than consider this form of underdetermination any further, I would like to explore the other varieties that can be 'extracted' from the above passage and consider, in particular, how the structural realist might respond to them.

2. Modal Underdetermination

This arises from van Fraassen's further claim that '[e]ach scientific theory, caught in the amber at one definite historical stage of development and formalization, admits many different tenable interpretations.' If the distinction between theory and interpretation becomes blurred, one could understand these different tenable interpretations as morphing into different theories, particularly if, as suggested, the theory is at a stage where further development and formalization is possible (surely almost always the case, given that the typical block on such is empirical). One could also obviously (trivially?) generalise the above claim and state that each set of empirical sub-structures, 'caught in the amber etc. etc.', admits (that is, can be embedded into), many different tenable theories. This amounts to little more than a re-statement of the standard underdetermination claim, but with the added emphasis on the historical dimension. This, in turn, can be related to recent claims of 'alternative' histories of science, which have also been put forward as a means of undermining realism.

The claim here is that one can identify certain points in the history of science at which it was possible for science to take an alternative course, as it were, and for alternative theoretical programmes to be developed (for a discussion of this claim and the associated subtleties, see French 2008 and the associated papers in a special *Isis* symposium)[i]. There is an obvious concern with regard to the basis for such identification and I suspect that the grounds for the above claim are entwined with the so-called 'romantic' view of discovery which typically ignores the relevant heuristic factors and surrounding (internalist) historical context. Relatedly, one can raise concerns with regard to the evidence for such alternative possibilities. This is to revisit the worry expressed above: how do we *know* that a further theoretical structure into which a given empirical sub-structure can be embedded is even possible? Here one might bring to bear general philosophical concerns regarding the epistemology of modality (see French *op. cit.*). However, a dilemma arises in the case of the history of science: on the one hand, mere conceivability cannot serve as evidence for genuine scientific possibilities, since what is typically conceived in no way amounts to a full-blown theory; on the other, if we had such a full-blown theory, it would no longer count as a possibility – it would, in a sense, be actual and subject to the impact of empirical data (although of course, we might still be left with the 'standard' form of underdetermination).

A possible and prima facie plausible example is that of Weldon's non-Mendelian genetics (see Radick 2005), where there is sufficient textual evidence in the form of notebooks etc. for someone with the relevant biological expertise to form a clear conception of the relevant theory and can begin to construct an alternative history. If this is the case and there is no evidence that falsifies the Weldonian theory, this would give us a genuine, and standard, case of underdetermination. In that case, the structural realist will urge commitment to the common underlying biological structure. Furthermore, from a reductionist perspective, the Weldon example is perhaps not so worrisome – the realist could restrict her ontological commitments to physical entities that are more fundamental than 'genes', for which it might be argued that the notion of objecthood is vague anyway.

However, Stanford has offered an approach that yields a vastly greater number of potential examples, without having to confront issues of evidential support (Stanford 2006). His argument runs as follows: we now know that at the time Newton's theory, for example, was proposed, developed and accepted, there was an alternative possible theory, namely Einstein's, that could have been proposed, developed and accepted but was not; we also now know this for many other theories throughout the history of science and we can (meta-)inductively infer that the same will be so for our current scientific theories (namely, someone at some future vantage point will be able to claim that they then know that there are now alternative possible theories). Hence, we cannot conclude that our current accepted theories are most likely to be true, or approximately true and realism is undermined.

This gets around the kinds of evidential concerns laid out above, since we obviously do have well developed alternatives in these cases – they are the very theories we now claim to accept! However, one must ask: in what sense could these alternatives have been developed and accepted at the time? Obviously, it is only if the relevant alternative could actually have been developed, or conceived of at the time, that we can claim that an alternative history was possible and realism undermined. Although I cannot comment on the biological case studies he presents, it seems clear that we can answer negatively in the case of Newton, or in that of Maxwell with regard to quantum electrodynamics: in such cases it can surely be claimed that it was not possible (in the relevant sense) for anyone at the time to even begin to conceive of, much less develop the alternatives, because the relevant supporting theoretical developments had not taken place, or even, in some cases, the relevant mathematical tools had not been produced. Hence, the force of the 'could' that underpins our thoughts about alternative histories is hugely diminished[ii].

There is more to be said about such modal considerations but let me move on to the other forms of underdetermination and the structuralist responses to them.

## 3. 'Jones' Underdetermination

In his critique of realism Jones puts forward a whole series of examples which mesh well with van Fraassen's suggestion of theories with multiple interpretations (Jones 1991). According to Jones, realism '… envisions mature science as populating the world with a clearly defined and described set of objects, properties, and processes, and progressing by steady refinement of the descriptions and consequent clarification of the referential taxonomy to a full-blown correspondence with the natural order.' (p. 186) But since what we have in these examples are different empirically equivalent interpretations and since these interpretations offer different sets of objects, properties and processes, this realist vision cannot be achieved.

Now, there are a number of things the realist can say about the examples Jones presents. For example, when it comes to different 'interpretations' of General Relativity, such as that underpinned by the Friedmann-Lemaître-Robertson-Walker solution, these can be taken by the realist as simply different models of GR which are known to be at best only partially true. A more problematic example, perhaps, is that of the Hamiltonian and Lagrangian formulations of classical mechanics.

## 4. Hamiltonian vs Lagrangian Mechanics

Both formulations are straightforwardly related to Newton's equations. The Hamiltonian equations are given by:

$$\dot{q} = \partial H/\partial p$$

$$\dot{p} = -\partial H/\partial q$$

where p represents the generalized momentum, q the generalized coordinates and H (H(*p*, *q*, *t*)), the Hamiltonian, represents the total energy of the system and effectively encodes the dynamical content. And the Lagrangian equations are:

$$d/dt\ (\partial L/\partial \dot{q}) = \partial L/\partial q$$

where L represents the difference between the kinetic and potential energies.

In brief, we can say that the content of Newton's equations is encoded in the structures defined over certain spaces:

> Hamiltonian: the relevant space is the space of initial data for the equations; that is, the space of possible instantaneous allowable states. The underlying structure is that of the relevant cotangent bundle.
>
> Lagrangian: the relevant space is the space of solutions to the equations; that is the space of allowable possible worlds. The underlying structure is that of the tangent bundle. (See Belot 2006)

As is well known, under an appropriate transformation, the Lagrangian yields the Hamiltonian and indeed, this forms the basis of the claim that the two formulations are inter-translatable. This can be appealed to by the structural realist in support of the suggestion that the underlying structure of these formulations is essentially the same and it is to this that we should be committed.

However, Pooley has raised two concerns about this kind of move (Pooley 2006). First, he notes that on most straightforward characterisations of structure (such as, and in particular, the set-theoretic characterisation favoured by most structuralists), different formulations can be understood as giving rise to different structures. Hence, in terms of the structuralists' own framework, the underdetermination would remain. Secondly, and perhaps more problematically, he insists that establishing an inter-relation between formulations is not enough. What is needed is a 'single, unifying framework' that can be interpreted as corresponding more faithfully to reality than the alternatives. In the absence of such a framework, the structural realist has no grounds for resolving the underdetermination as indicated above.

I shall try to respond to these concerns below, but first let me outline various ways in which one might try to break the underdetermination.

## 5. Breaking the Underdetermination$_1$: Appeal to Metaphysics

Musgrave's response to Jones' critique was to appeal to metaphysics, where the latter is not 'mere philosophical whim and prejudice' but is seen to be continuous with physics (Musgrave 1992). Thus '… physics has to look to metaphysics to help decide (fallibly, of course) between experimentally undecidable alternatives.' (p. 696)

However, there is the obvious concern regarding the justification for the invoked metaphysical principles themselves. We shall touch on this when we consider 'metaphysical underdetermination', but it is not entirely clear what principles could be invoked to decide between the Hamiltonian and Lagrangian formulations and one's view of such principles will obviously determine whether one thinks the underdetermination can be resolved in this way or not.

More significantly, perhaps, there is the concern that much of modern metaphysics appears to have distanced itself from any grounding in modern physics and hence one might worry – perhaps in normative fashion – that appealing to principles drawn from this 'physics-free metaphysics' in order to break the underdetermination between different formulations or interpretations of theories could lead to some potentially disastrous choices being made. It is this concern which underlies, in part, Ladyman and Ross's critique of current metaphysics (Ladyman and Ross *op. cit.)* Something like their view may be what Musgrave had in mind when he suggested that metaphysics should be seen as continuous with physics, but it doesn't really help break the underdetermination since an obvious circularity could arise. Appealing to metaphysics seems to leave us with a dilemma: either the metaphysics floats free of the physics and requires justification itself; or it is continuous with the physics but then it can't actually break the underdetermination.

Let us consider an alternative appeal – to the heuristic fruitfulness of one formulation over the other.

## 6. Breaking the Underdetermination$_2$: Appeal to Heuristic Fruitfulness

The idea is that we should prefer that formulation which is more heuristically fruitful, in some sense, where that sense can be broadly characterized, strongly, as leading to, or, weakly, as indicating (again in some sense) an empirically successful theory (see Pooley *op. cit.*). Now, one might immediately wonder whether it is even possible for a *formulation*, as opposed to a theory per se, to give rise to a new theory. Of course this raises again the issue of the distinction, if any, between theories and formulations, but the thought is two-fold: generally, there is the question whether formulations are the kinds of things which can enter, with theories, into the sorts of inter-relations that come to be established following certain heuristic moves; more particularly, there is the question whether the well-known kinds of moves that one can discern as leading from one theory to its successor, also hold between a formulation and a future theory (where it is not yet clear whether 'successor' is the appropriate term here).

Now, one could simply retreat at this point and defend a notion of heuristic fruitfulness in the still broad sense of leading to a better, deeper or whatever, *understanding* of the given theory – that is, a new formulation – but that seems a less than conclusive way of breaking the underdetermination. Here what one wants is some set of criteria for what counts as underdetermination breaking, conclusive or not, in this case. There is the (realist) intuition (carried over from the standard form of theory-theory underdetermination) that establishing that one 'horn' of the underdetermination leads to an empirically successful theory, whereas the other does not, certainly counts. However, establishing that one formulation rather than the other yields a new formulation and consequently, better *understanding*, appears not so decisive, since we don't have that crucial factor of empirical success in this case.

Refusing to retreat would mean insisting that, first of all, the relevant inter-relations can hold between formulations and theories, however characterised, and secondly, that appropriate heuristic moves can be made leading from one to the other. One can at least make a first pass at the latter and note that, for example, the Lagrangian formulation is typically regarded as the 'natural' way to extend Newtonian particle dynamics to fluids[iii] and the extension to quantum field theories is well known, with, for example, the Lagrangian density being straightforwardly related to Feynman diagrams. A quick scan of the relevant physics literature will show Lagrangians all over the place, in quantum chromodynamics, quantum black holes, etc. Nor is their ubiquity a mere matter of pragmatics: Wallace has argued that although much foundational analysis in quantum field theory has focussed on algebraic QFT, with its clear set of axioms, 'naïve' Lagrangian QFT is sufficiently well delineated as a theory that it too can serve as a focus for foundational analysis (Wallace 2006).

Of course, in the quantisation of a classical field the Hamiltonian plays a crucial role. And the central importance of the Hamiltonian for quantum mechanics hardly needs emphasising. What does deserve more careful attention are the moves that led to this central role. Here one can at least point to the 'bridge' provided by the Poisson bracket, which allows for a convenient phase-space representation of the Hamilton-Jacobi equations of motion. As is well known, this is strictly inapplicable in the quantum context and must be replaced by the appropriate commutator but formally we can see a relevant connection via the deformation of the underlying Poisson algebra to yield 'Moyal' brackets (the phase-space isomorphs of the commutators in Hilbert space); here the role of the underlying Lie algebra would require further explication. Historically of course, it was the apparent similarity between the Poisson bracket and the commutator that lead Dirac to his formulation of quantum mechanics and the heuristic role of the correspondence principle here is well known[iv]. Saunders has coined the useful phrase 'heuristic plasticity' to describe this feature of certain mathematico-physical entities which allows for their generalisation into new forms, or extension into new domains (Saunders 1993).

Clearly both formulations can claim some degree of heuristic fruitfulness. What one would then have to do would be to evaluate and compare the 'heuristic plasticity' of the relevant entities in the two formulations, in an attempt to weigh the one against the other. But even before we embark upon such an enterprise, further doubts might creep in as to whether heuristic fruitfulness is really sufficient to break the underdetermination. Consider: suppose we were evaluating the promise of the Lagrangian and Hamiltonian formulations at some point prior to development of quantum mechanics, in the late 1890s say. At that time, any determination of the fruitfulness of one approach over the other, or the plasticity of certain elements as compared with others, can only act as a kind of 'promissory note', since it could be that the plasticity leads to a dead end and the fruitfulness withers away to nothing. Of course, looking back, we can take a realist stance and say these developments were in some sense inevitable, because that's how the world is (so, for example, the structural realist might insist that the structure of the world correspond to, and is hence best represented by, some form of Lie algebra), but at the time we have no such guarantee. Is such a promissory note, presented in modal terms as it has to be, sufficient to cause us to select one formulation over the other? Surely not; at best, any such selection must itself be tentative.

But now consider this: suppose we were to evaluate these formulations from a perspective reached after the relevant developments have taken place. Looking back, of course the promise of one over the other may become clear but, equally, the relevant developments will also be clear, as will the new theory led to by these heuristic moves. In this situation, there will no longer be any underdetermination, because theoretical developments have effectively made the choice for us. Of course, in the case of the Lagrangian and Hamiltonian formulations, one can justifiably claim that each demonstrated a degree of fruitfulness, and the relevant elements an associated degree of plasticity, so in this case one can't even make a retrospective determination. But the point is that even if one could, even if it were clear which formulation turned out to be more fruitful than the other, such considerations are really no help in breaking the underdetermination at all: either they are mere promissory notes, or there is no underdetermination to break!

7. Breaking the Underdetermination$_3$: Appeal to Less Structure

In a recent work, North has argued that the Hamiltonian formulation should be preferred over the Lagrangian on the grounds that the former involves less structure than the latter (North forthcoming). Essentially she reminds us that whereas the underlying framework of the Lagrangian formulation is configuration space with a (Riemannian) metric structure and associated distance measure, that of the Hamiltonian is phase space with a symplectic structure and associated volume element. The symplectic structure, she claims, is sufficient for the relevant physics, so the choice is less structure (Hamiltonian) over more (Lagrangian). The idea, then, is that since metric structure determines, or presupposes, a volume structure, but not vice versa, the former adds another level of structure to what is needed to express the Hamiltonian equations of motion. Furthermore, the metric structure appears to be essential for the Lagrangian formulation, given the way the generalized coordinates feed into the Lagrangian[v].

North writes:

'I think modern physics suggests that realism about scientific theories is just structural realism: realism about structure. Modern geometric formulations of the physics suggest that there is such a thing as *the* fundamental structure of the world, represented by the structure of its fundamental physics. There is an objective fact about what structure exists, there is a privileged carving of natures at its joints, along the lines of its fundamental physical structure.' (North, pp. 27-28)[vi]

And the way we arrive at the structure of the world is straightforward:

'Take the mathematical formulation of a given theory. Figure out what structure is required by that formulation. This will be given by the dynamical laws and their invariant quantities (and perhaps other geometric or topological constraints). Make sure there is no other formulation getting away with less structure. Infer that this is the fundamental structure of the theory. Go on to infer that this is the fundamental structure of the world, according to the theory.' (North, p. 24)

Following this procedure in this case, North concludes that the fundamental structure of the word is symplectic.

Now, the crucial step in this inferential procedure is to reject any formulation that can do the same job but with surplus, or in some way, superfluous, structure. This may seem straightforwardly plausible from a realist point of view – and one could obviously underpin such a move through considerations based on simplicity – but it is in tension with the previous suggestion regarding heuristic fruitfulness, since it may well be this very surplus structure that confers such fruitfulness. This was a point made by Redhead when he noted that a number of significant developments in theoretical physics were achieved through the appropriate interpretation of mathematical structures that are related to those in terms of which empirically grounded theories are couched (Redhead 1975; 2001). Redhead himself presented this idea in terms of a 'function space' formulation of what has come to be called the 'model-theoretic' approach, but it can also be understood, more or less straightforwardly, in terms of the standard set-theoretic formulation. Within this framework, one has empirical sub-structures embedded in theoretical structures, the latter related via partial homomorphisms to the relevant mathematical structures. These are related in turn to further structures which are then open to physical interpretation and hence being related to an extension of the theory, or a new theory entirely (see Bueno, French and Ladyman 2003).

There are numerous examples of the fruitful role of such surplus structure, such as Dirac's 'negative energy' solutions of his famous equations, and Redhead himself considered the significance of gauge symmetries within field theory in this context: understanding gauge transformations as acting non-trivially only on the surplus structure, he suggested that non-gauge-invariant properties can enter the theory via this structure leading to further developments via the introduction of yet more surplus structure such as ghost fields etc. Other examples can be given but what is important is the positive role played by such structure in these cases. In general, rejecting formulations that involve surplus structure may mean rejecting precisely that which could prove heuristically fruitful. This introduces an element of restraint when it comes to North's structuralist programme. Indeed, one might say that appealing to the formulation that has less structure not only carries with it all the standard problems that appeals to simplicity face, but in addition risks constraining heuristic fruitfulness[vii]. As we'll see shortly, an alternative option for the structural realist is to look for some common structure underlying these formulations. Before we examine this approach, and the associated issue of 'which structure?' let us consider the final form of underdetermination that I have called 'metaphysical'.

## 8. Metaphysical Underdetermination

The (now) standard example of metaphysical underdetermination arises from quantum statistics. Philosophical reflection on the 'new' quantum mechanics was entwined with the development of the physics itself, with Born and Heisenberg, for example, suggesting that quantum statistics – both the Bose-Einstein and Fermi-Dirac varieties – implied that particles could no longer be regarded as individuals (see French and Krause 2006, pp. 94-115). For many years this was effectively the 'received' view of the matter, until it was argued that such particles could be regarded as individuals, subject to certain constraints (French 1989; van Fraassen 1989; French and Krause 2006). With the development of 'non-standard' logico-mathematical frameworks suitable for accommodating the 'Received' view's 'non-individuals' and a detailed understanding of the afore-mentioned constraints, two distinct metaphysical packages can be elaborated, consistent with the physics: particles-as-non-individuals (described via quasi-set theory) and particles-as-individuals (subject to certain state accessibility constraints).

Structural realists have presented this as a challenge to the object-oriented realism of, for example, Psillos and others, following van Fraassen's invocation of this form of underdetermination for anti-realist purposes[viii] (French 2006; van Fraassen 1989). The fundamental flaw in the latter is the combination of a form of 'minimal' naturalism that states that we should believe our best current theories, and hence take the world to be as these theories say it is, with a 'classical' metaphysics of individual objects. The existence of this kind of underdetermination implies that physics cannot, in fact, tell us what the world is like when it comes to the most fundamental aspect of the nature of its objects – it simply cannot tell us whether they are individuals or not. Analogously to the stance taken by the anti-realist when it comes to the standard form of theoretical underdetermination, the structural realist urges us to retract our metaphysical commitments, away from objects to the underlying common structures.

Not everyone is convinced, of course. Chakravartty points to a form of metaphysical underdetermination that one can associate with our characterisation of 'everyday' objects and argues that if the realist is not expected to be concerned whether 'everyday' objects should be described as substances-plus-properties or bundles of properties, or whether the properties themselves should be described as instantiated universals or tropes , so she should not be at all concerned whether quantum particles should be described as individuals or non-individuals (Chakravartty, 2003). In response, the structural realist can point to the differences between these two situations. In the case of everyday objects the issue is not whether they are *objects* or not, but rather, having already established that, how their objecthood should be conceived. Here the matter of access loom large: we have sensory mediated access to 'everyday' entities in terms of which we can separate out those that count as distinguishable objects, by means of the relevant properties, or location in space-time and so on. Once we've established distinguishability, at least in principle, we can then go on to speculate as to the 'ground' of individuality, whether via properties within the scope of an appropriate form of the Principle of Identity of Indiscernibles (PII), or in terms of some form of 'primitive thisness', or whatever. Here, as elsewhere, I am following Gracia (1983) who suggests, in Scholastic vein, that epistemically it is via distinguishability that we become aware of an object as an individual in a 'negative' fashion, but that ontologically, and 'positively', this individuality (or 'individual unity' as Suarez put it) is grounded in some underlying 'principle', broadly construed, such as haecceity, or substance or whatever. And for certain entities, of course, appropriate distinguishability cannot be established – entities such as (famously) euros in a bank account, which are then not regarded as individual objects.

When it comes to quantum particles, we lose that form of access and the danger of simply reading off the metaphysics from the physics is that the latter, or rather what we take to be the latter, may be infected, as it were, with the metaphysics of the everyday. Indeed, the very foundations of the mathematics we use to frame our theories is already so infected, requiring the genius of Weyl and his understanding of both those foundations and group theory to effectively 'twist' that everyday metaphysics to accommodate the new physics (French and Krause 2006, pp. 261-263). Here we cannot establish distinguishability to begin with, and the choice the realist faces is not the apparently innocuous one of deciding between different metaphysical accounts of the individuality of objects, but that of deciding whether they should even be regarded as individual objects to begin with. In other words, the choice is the much more fundamental one of deciding whether quantum particles are like euros in your pocket or euros in your bank account (see also French and Ladyman 2003; French 2006).

9. Underdetermination breaking$_4$: Weak Individuality

We recall that one option is to try to break the underdetermination by appealing to certain metaphysical principles. Thus, one could try to rule out the particles-as-nonindividuals package by appealing to Quine's famous dictum, 'no entity without identity' and insisting that since particles-as-non-individuals have no identity, they cannot actually be entities in the first place. But as straightforward as this may seem, Quine's slogan has been subject to criticism, the most focussed and penetrating (to my mind) such criticism having been proposed by Barcan-Marcus, who responded with her alternative, 'no identity without entity'[ix]. At the heart of this disagreement lies a fundamental issue to do with the status of identity (is it a relation that can only be said to hold once we have the relata (or relatum in this case), or is it constitutive of the entity?) and one's stance on that will effectively determine whether one thinks the above underdetermination can be resolved in this way or not. Furthermore, the development of quasi-set theory and 'Schödinger logics' goes some way to allaying the concerns of those who might wonder how we can formally accommodate the notion of particles whose identity is not well-defined.

Alternatively, we might 'break' the underdetermination by considering how the particles-as-individuals package might be further supported. Typically, those who wish to restrain their metaphysical commitments when it comes to individuality have appealed to some form of the Identity of Indiscernibles in order to ground this individuality on some property of the objects concerned. Well-known concerns in the quantum realm have been taken to block this approach (again see French and Krause *op. cit.* for a detailed account of this discussion), leaving – it would seem – Lockean substance, haecceity or some form of primitive thisness as the only options if we are to regard quantum particles as individuals. As a way of breaking the above metaphysical underdetermination this is seen as particularly costly, in ontological terms, and as leaving the realist wide open to anti-metaphysical criticism.

However, an alternative approach has recently been put forward that draws on a Quinean reformulation of the Identity of Indiscernibles and claims that a relevant sense of individuality can be grounded in a notion of 'weak' discernibility applicable to quantum particles (Saunders 2006). The central idea is to admit relations within the scope of PII and then to note that fermions in, for example, a singlet state, can be weakly discerned via irreflexive relations such as 'has opposite spin to'. This weak discernibility can then ground a form of 'thin' individuality that does not apply to bosons which, hence, cannot be regarded as objects (see also Muller and Saunders 2008).

Now, it is not clear whether this is sufficient to 'break' the metaphysical underdetermination, particularly given the considerations of the alternative, below. Indeed, the above approach can be seen as simply reinforcing it by offering a more plausible metaphysical alternative to heacceities and the like. Of course, the 'force' behind any such break is metaphysical, again, and this is not unproblematic. Admitting relations into the scope of PII has long been seen as controversial on the grounds that since relations conceptually require relata, and since the latter must be appropriately discerned prior to consideration of what relations they enter into, this approach begs the relevant question as to the discernibility and individuality of the relata. One way to avoid this accusation is to situate this approach within a structuralist framework and insist that what it provides is an appropriately structuralist or contextual' notion of individuality (Ladyman 2007; French and Krause *op. cit.* p. 172). In effect, then, this offers an alternative stance that the structuralist can take with regard to metaphysical underdetermination: rather than pulling back her ontological commitments in the face of the underdetermination, she can 'break' the latter via appealing to weak discerniblity and thin individuality and still appropriately restrict her commitments. The difference feeds into current discussions over the various forms of structural realism currently on the table and in particular relates to the (possibly wafer thin) distinction between 'eliminativist' forms which attempt to remove the notion of object entirely from the metaphysical pantheon and those that accept an appropriately 'thin' characterisation.

10. Underdetermination breaking$_5$: Non-Individuality and QFT

The most well-known way of handling the metaphysical underdetermination is to urge adoption of the particles-as-non-individuals package on the grounds that it meshes better with quantum field theory, where particle labels are simply not assigned right from the start (Redhead and Teller 1991 and 1992). In effect, and from the perspective of quantum mechanics, this is another appeal to the heuristic fruitfulness of one 'horn' of the underdetermination over the other. It is also a retrospective move, insofar as, having QFT to hand, we know now that there is such meshing, so it is not a mere promissory note. Still, the concern has been raised: why should appeal to a successor theory count in breaking the underdetermination associated with an earlier theory? Underlying this is the kind of modal issue alluded to above and captured in the question: if we were faced with this underdetermination in the quantum context only, without the benefit of having QFT to hand, what weight would we give to such a promissory appeal?

In pursuing this approach, advocates have attacked the other 'horn', arguing that the particles-as-individuals package is problematic insofar as it must posit the existence of certain 'inaccessible' states in order to appropriately accommodate quantum statistics (Redhead and Teller *op. cit.*)[x]. These states represent unwanted 'surplus structure' and hence, again on what amount to grounds of simplicity, this package should be rejected in favour of the other. Again, however, this is a problematic move for the same reasons as before (once again see French and Krause *op. cit.*, pp. 189-197 this surplus structure may prove to be heuristically fruitful in various ways, and indeed it has in the case of para-particle states (consideration of which played a crucial role in the early history of quantum chromodynamics) and anyon states. Attempting to draw a line between such 'useful' surplus structure and the clearly redundant is notoriously problematic and adopting 'reject surplus structure' as a general methodological rule is crude at best, foolhardy at worst.

11. Don't Break It: Seek the Commonalities

The relevant 'common' structure underlying the above alternatives has been characterised as group-theoretical (French 1999). Again putting things rather crudely, the idea is that instead of conceiving our ontology in terms of objects, and then having to face the dilemma of whether to regard them as individuals or not, we focus on the relevant group-theoretical structures underpinning quantum statistics and effectively re-conceptualise (or eliminate) our putative objects in terms of these structures[xi]. This of course follows a well-known historical tradition[xii] but there are two sets of concerns that have been raised.

The first has to do with the distinction between the presentation of the putative objects in structural terms, at the level of scientific practice, and the appropriate representation of the relevant structure, at the level of the philosophy of science. Brading and Landry have recently advocated a form of 'minimal' structuralism, according to which putative objects are to be taken as presented via the 'shared structure' of a theory's models but this structure should not be assumed to be set-theoretic (Brading and Landry 2006). They insist that,

'… to account for the fact that two models share structure we do not have to specify what models, *qua* types of set-structures, are. It is enough to say that, in the context under consideration, there is a morphism between the two systems, qua mathematical or physical models, that makes precise the claim that they share the appropriate kind of structure.' (p. 2; see also Landry 2007)

There is much that I agree with in this approach (see French forthcoming), particularly with regard to the contextual determination of appropriate object-level structure (and hence appropriate structural ontology) but where I disagree is with the claim that those of us who are advocates of the model-theoretic approach effectively assume the relevant structures, *at the level of the scientific practice*, are set-theoretic. A clear distinction needs to be drawn between the structural presentation of putative objects at the level of practice, and the meta-level representation of that structure. For me, the latter is most effectively accommodated via set theory but that is not, of course, to say that the 'structure of the world' is set-theoretical.

This then allows us to respond to Pooley's concern that if the structure we are interested in is straightforwardly characterised set-theoretically, say, then different formulations will give rise to different 'structures', understood in those terms. The concern arises from a conflation of the characterisation of structure with its meta-level representation. We may choose to *represent* the relevant structure set-theoretically, or via category theory, or however, but such meta-level representation does not *characterise* - in the sense of ontologically constituting - the structure. Of course, there remain the issues of how we can be sure there is such a common underlying structure, and, relatedly, of how we access it and characterise it. But the point is, that having concluded there is such a common structure, and noted its presentation in mathematical and physical terms (e.g. via group theory), our different meta-level set-theoretic *representations* of the associated different formulations should not be accorded inappropriate ontological import. There are not different structures in this case, just different representations of the underlying structure.

Secondly, the issue has been raised whether group-theoretic structures are enough, in some sense, to fully capture our structuralist commitments. Let us return to the underdetermination between the Hamiltonian and Lagrangian formulations. If we are to find structural commonalities here, and hence respond to Pooley's second concern above, what we need to do is show how a 'single, unifying framework' is revealed by moving to some underlying structure. Belot, for example, has noted that,

'It is a fact of primary importance that for well behaved theories the space of initial data and the space of solutions share a common geometric structure—these spaces are isomorphic as symplectic manifolds.' (Belot, p. 17)[xiii]

That is, the Lagrangian solutions can be mapped to the Hamiltonian initial data and in effect the actions of the groups implementing time translation (Lagrangian) and time evolution (Hamiltonian) can be considered as intertwined (Belot, op. cit.)[xiv]. Now, there is more to say, of course[xv], but this gives some indication of the way to proceed. In terms of representing this common structure, what one would also have to do is something akin to the work Muller has done in showing how the common structures of the Heisenberg and Schrödinger formulations of quantum mechanics can be appropriately represented via the semantic approach (Muller 1997).

However, as far as the structural realist is concerned, of greater importance is the issue of what this implies for her claims about the structure of the world. Bain, in particular, has drawn attention to the significance of 'dynamical' structures which must supplement the group-theoretic representation of putative objects above. Thus, with regard to the electron, for example, he has argued that the relevant structure is given by either the Hamiltonian or Lagrangian formulation of electron theory, with the evidence for this structure given via the well-known 'historically stable properties' of the electron (Bain, and Norton 2001). This dynamical structure, however, is not strictly group structure, since it is encoded not just in the invariants of the relevant groups, but also in the spaces that carry the representations of these groups. Thus, to give another example, the dynamics of Yang-Mills theories is encoded not just in the relevant invariants (twistors) but in the geometric structures defined over the projective carrying space (Bain forthcoming.) Hence, the structuralist still has some work to do in supplementing the 'object structure' with the relevant dynamical structure and fleshing out her picture of the 'world structure' as multi-featured (French 2006). In effect what we have is an appropriately complex ontology that includes both the group-theoretically characterised structure underlying the particles-as-individuals and particles-as-non-individuals packages, and the common symplectic structure underlying the Hamiltonian and Lagrangian formulations. In this way both instances of underdetermination can be accommodated and resolved[xvi].

## 12. Conclusion

There are two broad sets of conclusions I would like to draw from the above sketch.

First, realist attempts to 'break' cases of underdetermination by appealing to heuristic fruitfulness and surplus structure are problematic. If we're in a situation where the successor theory has yet to appear, appeals to heuristic fruitfulness are little more than promissory notes and as such as are hardly likely to trouble the anti-realist. If, on the other hand, we already have the successor to hand, such appeals are redundant. In these cases, heuristic fruitfulness cannot play the role attributed to it. Having said that, heuristic fruitfulness can play a positive role in a different arena, by casting doubts on attempts to resolve cases of underdetermination by appealing to the excess structure one formulation may have over the other. In addition to the concern about the justification for the purported link between less structure and (approximate) truth, rejecting surplus structure may be an unfortunate methodological policy to adopt. The alternative I prefer is to adopt the structuralist approach of looking for underlying commonalities and focussing on 'essential' structure.

Secondly, however, the lesson for the structuralist is that this essential structure must be expanded beyond the group-theoretical 'object' structure to include the dynamical. Following Bain (who in turn was motivated by Ruetsche 2002), we can informally represent this essential structure in the following terms:

{state space, dynamics, symmetries} (Bain, p. 24)

The structural realist will insist *this* is what we should be realists about, where the issues remain as to how we are to both appropriately represent it at the meta-level (in terms of set theory, category theory or whatever) and how we are to understand it at the metaphysical level.

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[i] There is a connection here with van Fraassen's 'bad lot' argument to the effect that science may have deviated from the path of truth and our 'best' theories are nothing more than the best of a bad lot.

[ii] Of course, alternative theories are eventually developed and if their ontologies are sufficiently discontinuous from their predecessors, this may also cast doubt on realism.

But this is a different objection, associated with the Pessimistic Meta-Induction and, of course, the structural realist has a response to this! My concern here is only with the conceivability argument.

[iii] Interestingly for what comes later, 'conservation of particle identity' is fundamental to this approach, where fluid 'particle' identifiers – such as position at time *t*, or relevant thermodynamic properties – are treated as independent variables, although a form of indistinguishability also holds since the dynamics remains unchanged through permutation of 'particles' of the same mass, momentum and energy.)

[iv] Dirac himself represented the fundamental underling discovery as occurring in a 'flash of insight' while out walking but he also had an excellent understanding of the Hamiltonian formulation, particularly as it had been applied by Sommerfeld to atomic systems.

[v] This difference in the structures has implications for the claim that we can straightforwardly transform from one formulation to the other, as North notes. In essence it implies that such transformations are only possible within certain constraints. There are also associated costs, as she notes that within the Hamiltonian approach, *momentum* must be regarded as a fundamental property.

[vi] Although she acknowledges Ladyman's earlier form of structural realism, she insists that this account is different. Clearly further work is required to explore these differences.

[vii] Note that we are talking about *formulations* here. Of course, in the case of underdetermined *theories* one might be reluctant to discard even quite extensive surplus structure without running the appropriate empirical tests first (this is just an expression of the usual dominance of empirical success over simplicity, however characterised).

[viii] The relevant passage is at the very end of his book on quantum mechanics, under the section heading 'Goodbye to Metaphysics'. One reading of this section is that van Fraassen sees this underdetermination as deriving from the unnecessary metaphysical commitments of the realist, although not everyone agrees with this interpretation.

[ix] The difference here has to do with the differences for things and objects; for Barcan Marcus object-reference is taken to be a wider notion than thing-reference, where the latter involves well-defined identity conditions, as well as other restrictions, such as spatiotemporal location.

[x] These are states that are inaccessible to particles of a certain symmetry type; e.g. as far as bosons are concerned only symmetric states are accessible, and all others – anti-symmetric, para-symmetric and non-symmetric – are inaccessible.

[xi] The two most relevant such structures are those associated with anti-symmetric (fermionic) and symmetric (bosonic) states, although others are of course possible and as already indicated, may be fruitful. 'Weak' discernibility is, in effect, a manifestation of anti-symmetric structures.

[xii] Eddington, for example, famously propounded and defended this approach; see French 2003.

[xiii] North agrees that 'if and when' both statespace structures are vector fibre bundles, they will be isomorphic *as vector spaces*. Nevertheless she insists that the two formulations differ in *relevant* structure, not least because the Hamiltonian statespace need not be a vector bundle, whereas the Lagrangian statespace must. Hence, she

maintains, the Hamiltonian formulation is still to be preferred. There is obviously more to discuss here.

[xiv] Belot further suggests that a symplectic structure is the *sine qua non* of quantization.

[xv] Again, North argues that the Hamiltonian statespace need not be a vector fibre bundle, unlike the Lagrangian, and hence the two formulations may differ in relevant structure. Clearly further work is needed on this point.

[xvi] In effect this is to acknowledge the breadth and complexity of the relevant structures, something that Falkenburg, for example, has recently highlighted (Falkenburg 2007).