Are Elementary Particles Individuals?
A Critical Appreciation of Steven French and Décio Krause's *Identity in Physics: A Historical, Philosophical, and Formal Analysis*

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Steven French and Décio Krause have written what bids fair to be, for years to come, the definitive philosophical treatment of the problem of the individuality of elementary particles in quantum mechanics and quantum field theory. The book begins with a long and dense argument for the view that elementary particles are most helpfully regarded as non-individuals, and it concludes with an earnest attempt to develop a formal apparatus for describing such non-individual entities better suited to the task than our customary set theory. Along the way one is treated to a compendious philosophical history of quantum statistics and a well-nigh exhaustive (I'm tempted to say, "exhausting") analytical history of philosophical responses to the quantum theory's prima facie challenge to classical notions of particle individuality. The book is also a salvo from the headquarters artillery company of the "pro" side in the contemporary structuralism wars, and an essay in metaphysical naturalism. Whew!

There are too many places where the friendly critic wants to engage the argument, and few where the authors have not already anticipated such engagement. I take this as my excuse, then, for offering not any systematic response to the whole project, but just some questions and observations about several points that caught my attention.

1. *Are Elementary Particles Individuals?*

Are elementary particles individuals? I don't know. It depends on what one means by "individuals." This much is certain – elementary particles as described by quantum mechanics and quantum field theory are not individuals *in the same sense* in which classical mechanical systems, the molecules constituting a Boltzmann gas, or Daltonian atoms are individuals. The elementary particles of quantum mechanics obey either bosonic or fermionic statistics. The molecules of a Boltzmann gas do not. An individual elementary particle can find itself in a state that is a superposition of eigenstates of some observable. Dalton's atoms cannot. On the standard interpretation of quantum mechanics, Interacting quantum systems are described by an entangled joint state that is not fixed completely by any possible separate states of those systems. Not so the interacting systems described by Newtonian mechanics and gravitation theory. In quantum field theory, there might be states of indefinite or indeterminate particle number. In classical physics, a given region of space during a given time always contains a determinate number of atoms. In quantum field theory, particle number is frame dependent, mutually accelerated observers feeling or seeing different numbers of photons in one and the same region of space. In classical physics, particle number is frame independent. You and I must detect the same number of atoms or molecules regardless of our relative states of motion.

So, are elementary particles individuals? Again, I don't know. It depends on what one means by "individuals." The philosopher's customary way of approaching the question is via the Principle of the Identity of Indiscernibles (PII), which asserts the identity – hence the lack of individuality – of two things that share all of the same properties. The strength of the Principle of the Identity of Indiscernibles depends upon what one takes to be the relevant individuating properties. French and

Krause distinguish three versions, in order of increasing strength. The weakest, PII(1), includes all properties. Slightly stronger is PII(2), which excludes spatio-temporal properties. Stronger still is PII(3), which excludes all relational properties.

Some version of the Principle of the Identity of Indiscernibles *seems* to be relevant in the case of at least some kinds of elementary particles. Consider the more straightforward case of bosons. Photons are massless, spin-1 systems obeying Bose-Einstein statistics. Hence, on the standard interpretation, quantum mechanics does not regard as physically different two configurations that one might think describable as involving merely the switch of physical location of two otherwise identical photons. Not even a difference in spatial situation suffices to endow two such photons with discernible or distinct identities of a kind sufficient to mark the two configurations as physically different. Two such photons being, thus, indiscernible, in spite of a difference in spatial situation, one is tempted to conclude by way of PII(2), that they are identical, and, for that reason, not individuals.

This metaphysical state of affairs has definite, testable, physical consequences. There being only one way of arraying two bosons in two different locations, that configuration receives a statistical weight equal to that attaching to each of the two different ways of arraying the two bosons in one or another of the same locations. When, then, we ask what is the probability of the two bosons being together in the same place, the answer is 2/3, significantly higher than the "classical" value of 1/2, which was computed by applying equiprobability to what were, classically, four different configurations. As noted, this difference between quantum and classical statistics has testable consequences, most famously in the form of Einstein's 1925 prediction of the phenomenon of Bose-Einstein condensation, wherein, at sufficiently low temperatures the atoms of a bosonic gas all condense into the same lowest quantum state, an effect first demonstrated in the laboratory in 1995 by Cornell and Wieman using rubidium atoms cooled to 1.7 nanokelvin.

So, again, one is tempted to conclude that two (or more) indiscernible bosons are not individuals. This conclusion can be resisted in various ways. Thus, one might choose a non-standard interpretation of quantum mechanics, such as the Bohmian interpretation, which ascribes determinate trajectories to all elementary particles, endowing those particles with in-principle distinguishing identities, and locating all of the quantum weirdness elsewhere, mainly in the quantum potential. Or one can assume that, in spite of their seeming indiscernibility, two such indiscernible bosons are nevertheless rendered metaphysically distinct by virtue of their each possessing some form of transcendental individuality, primitive "thisness," or, in the language of the Scholastics, *haecceitas*. One recovers the standard quantum statistics by applying the appropriate probabilities – 1/6 – to what are, on this view, distinct configurations differing only by particle exchange.

A price must be paid for one's resisting the conclusion that bosons are not individuals. Going the Bohmian route requires lots of extra apparatus, including the quantum potential, quantum ergodicity, and relativistic non-localities at the level of the hidden variables. Choose the route of *haecceitas* and one requires a seemingly ad hoc weighting of configurations, a weighting for which there is no plausible physical explanation beyond the ex post facto argument that those are the weightings needed to get the predictions to turn out right. But if one is willing to pay some such price, then neither logic nor empirical evidence can debar one's taking one of these routes.

None of what I've just said is not already said by French and Krause, except for my last remark about logic and empirical evidence not blocking the way to retaining the view of elementary particles as individuals. They attach a higher price to individualism about elementary particles, a price they think unreasonable to pay. To this point of difference between them and me I will return.

Here and now yet another point. While it is clear, as I said above, that elementary particles as described by quantum mechanics are not individuals *in the same sense* as the molecules of a Boltzmann gas, what it means *positively* to say that they are non-individuals is less clear. The second major goal of French and Krause's book is to develop formal tools in the form of quasi-set theory to make easier our speaking and thinking clearly about non-individuals. To this, too, I will return. Instead here, like French and Krause – but for slightly different reasons – I want to suggest that some of the preliminary unclarity about the sense in which elementary particles are non-individuals derives from our having chosen to approach the question in the first instance via the Principle of the Identity of Indiscernibles. That PII is the philosophers' favorite tool does not imply that it is the most helpful tool for the physicist to employ.

Two considerations suggest that there is something odd about deploying the Principle of the Identity of Indiscernibles in assaying the individuality of bosons. Firstly, even if one accepts the conclusion that, owing to their indiscernibility, two bosons are not individuals, in spite of their difference in spatial situation, this lack of individuality does not come in the form of the bosons' being numerically identical. That is to say that this variety of indiscernibility does not imply that we have just one boson, rather than two. No. It is still an objective fact of the matter that there are two bosons, not one, as evidenced by such circumstances as the total energy. In this setting, indiscernibility implies identity in the form of indistinguishability, but not, again, in the form of numerical identity. One cannot tell which boson is which, perhaps not just because of a lack of epistemic access to that which might constitute the "whichness" of the bosons, but because they lack such "whichness" from the start. But that there are two bosons, not one, is a fact. This is all the more obviously so in situations like Bose-Einstein condensation, where one might have several score or more massive bosonic atoms in the same condensed quantum state.

Secondly, in thinking about Bose-Einstein statistics, what are more helpfully regarded as being indiscernible in the sense relevant for the applicability of the Principle of the Identity of Indiscernibles, are not the bosons, themselves, but rather the configurations classically distinguished by particle exchange. This is because the indiscernibility of the configurations does, as noted, imply the strict numerical identity of those configurations. In bosonic quantum statistics there is, literally, only *one* configuration in which there is exactly one boson in each of two cells of phase space. There is a strong analogy here to the manner in which Leibniz himself deployed the Principle of the Identity of Indiscernibles in what is known as the "Leibniz shift" argument against a Newtonian absolutist (or, in one sense, substantivalist) conception of space. Imagine a set of material bodies shifted so many meters in some one direction in otherwise empty space. The original and shifted configurations are indiscernible in spite of the seemingly different spatial situations of the bodies. Hence there are not two configurations, but only one. Hence, spatial situation alone does not individuate such configurations.

Never mind the fact that the Leibniz shift argument simply begs the question against Newton, just as the argument in the Bose-Einstein case begs the question against the proponent of "primitive thisness." That's beside the point. For the point toward which I mean now to be tending in so querying the helpfulness of the Principle of the Identity of Indiscernibles is that, in making all of this out to be an argument about the individuality of elementary particles, we might be asking the wrong question or a question that is not well posed.

Many meanings attach to the notion of an "individual" in the philosophical tradition, and we have had many different reasons for worrying about whether this or that entity is an individual. We might be worried about the contrast between the group and the individual in law or social philosophy. We might be anxious to understand the individuality of Christ as part of the Trinity. Philosophers of mind who like science fiction might puzzle over what manner of individuality attaches to the members of the Borg Collective. In the philosophy of language we might be thinking of individuals as subjects of predication. In metaphysics we might be thinking of individuals as bearers of properties. That any *one* analysis of the nature of individuality should be serviceable in so many different settings is probably too much to expect. That *any* one such analysis of individuality appropriate to one or more such settings should be helpfully exportable to the very different setting of elementary particles in quantum mechanics is also, probably, too much to expect.

French and Krause agree in querying the helpfulness of the Principle of the Identity of Indiscernibles in assessing the individuality of elementary particles. Indeed, they go so far as to assert the contingent falsity of the principle in this context. However, their alternative is not to drop the question about the individuality of elementary particles but to recast the question of individuality in other terms.

French and Krause have a reason for wanting the question about elementary particles to be a question about individuality. It is because arguments for the non-individuality of elementary particles are supposed to be arguments for some version of structuralism, the idea being that one wants a structuralist characterization of elementary particles as being not, themselves, ontologically primitive, but ontologically derivative from relations of some kind. And yet, no version of structuralism known to me stands or falls on a claim about the non-individuality of elementary particles. Structuralism would not be refuted if bosons were distinguishable particles, and the very same physics that encourages a view of elementary particles as non-individuals entails other problems for structuralism, in the form of the necessary existence of non-unitarily equivalent representations of algebraic quantum field theory, an issue to which I will also return below.

If the Principle of the Identity of Indiscernibles proves unhelpful in assaying the individuality of elementary particles, then in what other way are we to pose the individuality question? As best I understand the argument in French and Krause, the answer is that they concur in what they term the "Received View of particle (non-) individuality." On this view indistinguishability, alone, establishes the non-individuality of elementary particles. Of course one must still also reject alternatives like "primitive thisness," but let's not tarry any longer over that point.

So what kind of individuality or non-individuality is intended here? French and Krause stress the importance of noting a distinction between individuality and distinguishability, so what is assumed cannot be a *definition* of individuality as distinguishability. Instead, what seems to be assumed is something that one might dub the "Principle of the Non-Individuality of Indistinguishables." Well and good. We have here a criterion. But, again, what kind of individuality or non-individuality is at issue here? Two bosons are indistinguishable. Should that suffice to make them non-individuals? After all, there are still two of them, not one, even if one cannot tell which is which, or even if there is no "whichness" in the first place. So if determinate cardinality is one manifestation of the individuality of the members of a set, as surely it is, then isn't there at least *something* "individual"-like about bosons? Think about it this way. Are two identical, hence, indistinguishable human twins to be regarded as non-individuals simply because they are indistinguishable? Assume for the sake of argument that human twins could also not be distinguished even by their spatio-temporal situations, that continuity of trajectory, as it were, would also not suffice to tell us which is which. Would one think them, therefore, devoid of the kinds of legal rights and responsibilities that attach to individuals? No. Indeed, our legal tradition has a concept ready-made for such situations, for in certain circumstances we regard the members of a group as "jointly and severally responsible" for this or that, and for the purpose of deploying this notion of responsibility the "whichnesses" of the members of the group are irrelevant. In such cases the law does not care who is who, but we do not, therefore, regard the members of the group as non-individuals. So where's the harm in regarding two indistinguishable bosons as, nonetheless, individuals that "jointly and severally" enjoy all the rights and responsibilities accorded to systems? That's surely not the kind of individuality evinced by classical physical systems, but perhaps the lesson is that progress in science has forced us to revise our notion of individuality rather than denying its applicability to elementary particles.

My own view is that all of the complications now accumulating around the notion of individuality – and I've spared you many more such complications that are presented in detail by French and Krause – suggest that, in making the question one about individuality or non-individuality, we've been asking an unhelpful and perhaps ill-posed question.

But if individuality – whatever it might mean – is not the most helpful issue to be raising about elementary particles, what is? I try to look at this from the point of view of the physicist, not the philosopher. From that point of view, the relevant question seems obvious. It is simply this: Does difference of spatial (or spatio-temporal) situation suffice to endow physical systems always and everywhere with separate, real, physical states of such a kind that determine, univocally, the real physical states of any aggregation of such systems?

In classical particle mechanics, the answer to this question is, clearly, "yes." Likewise in classical electrodynamics, as long as we understand that, by "system," we mean points in space or collections of points in space, to which attach determinate values of electric and magnetic field strengths. And so, too, in Einstein general relativity, where space-time events or sets of space-time events play the role of "systems," each such space-time event having attached to it a determinate value of the metric tensor.

In quantum mechanics and quantum field theory, on the standard interpretation, the answer is, clearly, no, because of entanglement and such special cases of entanglement as those represented by bosonic and fermionic statistics. In general, interacting quantum systems are represented by joint states that do not supervene on any separate states of the interacting systems. That quantum and classical physics differ in this way is – as recognized by Einstein, Bohr, Schrödinger, and many others – the single most important novelty in the quantum domain, virtually all of the other salient quantum novelties being derivative therefrom.

One might think that I am suggesting here just a different way of approaching the individuality question, that I'm asking whether, in the quantum domain, what French and Krause term the Principle of Space-time Individuation (STI) is true. One might ask that question, but I'm not. I'm asking not about the individuation of the systems occupying different regions of space-time, at least not in the first instance. I'm asking, instead, whether those systems possess states of a certain kind, namely separate, real states that fix, univocally, all joint states. It's a question about the states, not the systems. Impressed by the ubiquity of quantum entanglement, one might then *also* ask what kinds of things are the systems that can be the bearers of entangled joint states, and that might lead one back into the thicket of questions about individuality. But that's not the question I asked. The question I asked is simply whether quantum mechanical states are always separable. And that is a clean, well-posed question, the answer to which is, in general, no. Non-separability or entanglement is everywhere.

Does quantum non-separability have any bearing upon questions of particle individuality? Again, it depends on what one means by "individuality," but some connections are obvious. Thus, were there no entanglement, were the physical universe, therefore, in this sense, "classical," were difference of spatial situation always and everywhere sufficient to endow physical systems with separate, real, physical states that fix, univocally, all joint states, then physical systems would be individuals in a very robust sense of the word. But that's not surprising, since our way of thinking about individuals in many domains – one thinks especially of the borrowings in social and political philosophy – is modeled upon the ontology of classical atomism.

We live, however, or so we think at the moment, in a quantum universe. Does the ubiquity of entanglement entail that there are no individual elementary particles? Yet again, it depends in large measure on what one means by "individuality." But in no case does entanglement in any form entail that two indistinguishable particles become, by virtue of that indistinguishability, just one particle. Nor does it mean, in many cases, that two entangled particles lose their trackable identities, their "whichness." They do if they are "identical" particles, such as two photons in a laser beam. They do not if they are not "identical," in the sense of sharing all intrinsic properties, such as the members of an entangled particle-antiparticle pair resulting from a pair-creation event, or better still, an elementary particle of any kind and the detector apparatus with which it becomes entangled in a measurement interaction.

If quantum entanglement is the important novelty in the quantum realm, then asking whether entangled quantum systems are individuals is not likely to help us much in understanding how an entangled quantum world differs from a classical one. As has happened before with progress in the sciences, we find science giving us here not new answers to old metaphysical questions, but, instead, new questions.

Answering those new questions might well be helped by the development of new kinds of formal apparatus, as has so often been the case in the history of physics, from Newton and Leibniz's invention of the calculus to Einstein and Grossmann's having prompted the development of modern differential geometry. And such might well prove to be the case with French and Krause's elaboration of quasi-set theory. But I'm not sure that I see how we are helped by trying to assimilate the new question about entanglement to old and confused questions about individuality.

So if, now, one asks again, "Are elementary particles individuals?", I answer, as before, "I don't know." It depends on what one means by "individuals." But now, speaking from a physicist's point of view, I'm tempted to add, "And I don't care."

2. *Metaphysical Underdetermination*

One prominent theme in the foregoing paragraphs and in the first half, roughly, of French and Krause's book is the difficulty of extracting from the physics definitive answers to our metaphysical questions. They, themselves, pose the meta-question whether a kind of metaphysical underdetermination is inevitable, in the sense that scientific theory cannot, by its nature, or does not, in the typical case, entail a unique metaphysics. Quantum mechanics might strongly suggest that bosons do not possess trackable identities, but one can choose a non-standard interpretation of quantum mechanics, such as the Bohmian interpretation, or simply assume "primitive thisness," and then adjust the statistical weights on now different configurations so as to get the right predictions. Perhaps there are limits to how tightly one's physics can constrain one's metaphysics. As mentioned, French and Krause prefer to think of our metaphysical options as more tightly constrained by our physics, since they want the quantum physics to yield or strongly suggest the conclusion that elementary particles are non-individuals. But, as I've just argued, if the question is about the individuality of elementary particles, I'm not sure that the physics gives us anything close to a definitive answer, partly because, as I also just argued, I'm not sure that that's the best question to ask. As regards the metaphysical question of the individuality or non-individuality of elementary particles, the quantum theory does underdetermine the metaphysics, in my opinion.

On the other hand, as a student of Abner Shimony's, I've grown up with a deep sympathy for what he termed "experimental metaphysics," which is the idea that one lets one's physics (or biology or whatever) be one's guide to the deep structure of reality. One takes as important and legitimate even those questions about deep physical reality that carry one far beyond a domain accessible to unmediated observation or direct experiment, but one takes only those answers that are proffered by one's physics – however incomplete they might be – trusting neither a priori analysis nor metaphysical intuitions to complete or correct the answers that physics gives. Bell's theorem and its experimental tests are Shimony's favorite example. Thanks to the work of Bell, Aspect, Zeilinger, and others, we now know a lot more about the deep structure of physical reality than we did before. We know that quantum mechanical reality is non-local in some sense of the word. Just more physics, some might say, but the Shimony who's a full-blooded realist and also a fan of Whitehead asks why should we deny it the name "metaphysics," if it's engaging nature at as deep a level as Aristotle ever did. Of course it's not "beyond" physics; that's just the point. But it's physics taking us into domains where Aristotle and Carnap (one of Shimony's teachers) never imagined it could. There's no obvious philosophical gain in withholding the name "metaphysics" from such investigations.

It is noteworthy, to be sure, that such metaphysical insight as is provided by Bell's theorem and its experimental tests is more negative than positive. We now know that the quantum world is "non-local"; we know what it is not. And while we have hints of a more positive characterization of the quantum weirdness in the form of violations of what Shimony terms "outcome independence" – the statistical distribution of measurement outcomes in one wing of a Bell experiment is typically not statistically independent of the measurement outcomes in the other wing – we don't yet really know for sure what that means, beyond noting that it's a consequence of quantum entanglement. We might see, here, a generic feature of experimental metaphysics. It might be mainly a *via negativa.* It might work mainly by foreclosing metaphysical options. Still, as in theology, so too in fundamental physics: No-go theorems are a form of metaphysical progress.

French and Krause are perhaps right to distinguish metaphysical underdetermination from the more familiar underdetermination of theory choice by empirical evidence. But if, after the manner of Shimony, one sees the enterprise of metaphysics as being continuous with or a continuation of physics, then a distinction between the two forms of underdetermination might not be all that sharp. In both cases it is empirical evidence that serves as the ultimate constraint on our musings.

In any case, I've long thought it more helpful to explicate such a looseness of constraint on our metaphysics with the analogy not to underdetermination but with either of two other analogies. One such more helpful analogy is to what, in formal semantics, we term categoricity or monomorphism. A formal language or formal system is categorical if and only if all of its models are isomorphic. Since this notion first began to find a clear formulation in the late-nineteenth century, structuralists have thought categoricity a desirable property of theories, for the obvious reason that, if theory is supposed to concern structure, then one would like it to yield a univocal structural characterization of its intended domain. Whether, after Gödel, this is any longer a reasonable expectation is vexed question. At the end of this talk I'll take up the categoricity question specifically with respect to what quantum field theory suggests regarding the fate of structuralism.

The other such more helpful analogy is the one made famous by Quine in his discussion of ontological relativity. Recall the basic features of Quine's argument. To be is to be the value of a bound variable. Saying what are the values of a theory's bound variables is always nothing other than a matter of translating the theory into some background language. In formal semantics we are explicit and careful about this being the nature of the enterprise. Specifying a theory's ontology in other ways differs from exercises in formal semantics only in point of the clarity and specificity in the choice of a background language, the degree of formalization of that background language, and the degree of rigor in our "translations," as when we define the natural numbers by translating Peano arithmetic into, say, Zermelo-Fraenkel set theory. Finally, says, Quine, the translation of a theory into a background language is "doubly relative." It is relative to the choice of the background language and it is relative to the choice of a "manual of translation," a way of carrying through the translation. What is the "natural number '2'"? It depends on the background language one chooses, and even after specifying some one version of set theory, say, it depends on how one defines "2." Is it the set of all pair-sets or the pair set which has as its members the null set and the unit set of the null set?

There is no one right background language and no one right manual of translation. The alternatives differ only with respect to pragmatic, heuristic, or pedagogical advantages.

Back to elementary particles. Are they individuals? I don't know. Tell me first what formalized version of quantum mechanics or quantum field theory is in question. Specify next the background language into which I am to translate. Is it set theory or quasi-set theory? Whichever the choice, specify next the manual of translation. Do I start with a state space that assumes distinguishability on which I define a permutation operator that is stipulated to commute with all observables? Do I construct, instead, a reduced state space that has built into it all the needed permutation symmetry? Do I do neither and simply impose superselection rules that prohibit all systems' occupying any states in which the supposedly indistinguishable would be distinguishable? Quite honestly, I don't know.

What I do know is that, knowing about all of these many alternatives, I understand quantum mechanics better than I would if I had before me only one of the alternatives. For example, I'm not especially fond of Bohmian mechanics. Quite apart from the unresolved technical challenges it faces, as with the question whether one can construct a viable Bohmian quantum field theory or the question whether a truly compelling reason can be given for why one should assume quantum ergodicity, the Bohmian program has always bothered me because of its nostalgic, backward-looking character. Other things being equal, I prefer interpretations that challenge rather than flatter old intuitions as being, on balance, more fecund, more likely to suggest new questions and new lines of research. And, yet, I respect the view promoted vigorously by Gary Bowman and others that Bohmian mechanics enjoys a decided pedagogical advantage in training beginning physicists.

Quine's thesis of ontological relativity is widely read as a form of skepticism. If ontology is, thus, doubly relative, if there are ten different equally viable explications of the notion of "natural number," (and if, to boot, Peano arithmetic in first-order formulation is non-categorical), then how can we ever pretend to know the truth about the natural numbers? That response to ontological relativity is surely too quick. For one thing, Quine reminds us that if, by "truth," we mean truth as Tarski taught us to think about it, then "truth," too, is relative, in the sense that "truth" is defined not once for all languages and theories, but separately for each different language, and in each such case a Tarski truth definition involves the same double relativity to background language and manual of translation at the point where it is dependent upon a definition of the "satisfaction" relation between the target language and the background language. And a word of caution for the unwary: A Tarski truth definition is possible only for languages in which sentencehood is recursively specifiable, hence not for natural languages. The restriction to formal languages is not an artificial limitation. If to speak clearly about truth is to employ a Tarski truth definition, then the restriction is essential for clarity.

Do ontological relativity and the kindred relativity of truth a la Tarski imply skepticism? Only if one approaches the issue with the contrary assumption that there must be a single truth about the world and that such truth must be cognitively accessible. But whence such an assumption? Our experience of the world in the refined form it takes in modern science does not warrant such a view, the elusiveness of a definitive answer to the question about the individuality of elementary particles being but a reminder of that general fact (and, yes, I mean by this to dispute the structuralist response to the pessimistic meta-induction, but that's a large question best left to another occasion). The strongest form of inference to the best explanation at the meta-level, which goes by the name of the "ultimate" or "no-miracles" argument for scientific realism, has been famously and ably disputed by one of our number. What's left is some form of transcendental argument, wherein the existence and accessibility of a univocal truth about nature is held to be a necessary condition for the possibility of some 'X' (objectivity?, blocking the slide into radical relativism?). But with all arguments of the form "A is a necessary condition for the possibility of X" one must query both the 'X' and the claimed necessity of 'A' for 'X.' As goes the fate of Newtonian particles mechanics and gravitation theory, so goes a Kantian argument for the transcendental necessity of Euclidean metrical structure. And is the principle of equivalence a necessary condition for there being empirical content in Einstein's gravitation theory? No. Such queries usually reveal transcendental arguments to be little more than a philosopher's version of wish fulfillment.

Does ontological relativity amount to a form of skepticism? I choose to think not. I choose to regard our having a variety of ways of explicating a theory's ontology to be a resource, not a failing. I choose to regard such variety as a way of expanding our knowledge of nature, not limiting it. Thanks to the progress of science, we now know many ways the world almost surely is not, and in a few cases we even know ways the world cannot be, consistent with the empirical evidence. Beyond that, the world is rich with possibility, and such a panoply of possibility might be the way the world is.

So, then, if elementary particles can be regarded either as individuals or non-individuals, or perhaps as neither, is that a problem or an asset? My view is that there being such an array of possible metaphysical interpretations enriches our understanding of quantum mechanics.

3. *W(h)ither Metaphysical Naturalism?*

If metaphysics is, thus, only weakly constrained by current physical theory, does it make sense to continue to take our best current physical theory as our guide to metaphysical truth? Does not the weakness of theory's constraint on metaphysics mean that physical theory is a rather unhelpful guide to the metaphysician? Does not the weakness of theory's constraint on metaphysics mean that there is still an important role for a priori metaphysics? Or, to push the argument in a very different direction, does not the weakness of theory's constraint on metaphysics suggest as the most reasonable attitude a metaphysical agnosticism?

One would be more comfortable urging a continuing role for a priori metaphysics were there good reason to think that there were a way to limn the ultimate nature of physical reality better than that indicated by the motto, "trust theory." One would be more comfortable counseling agnosticism were there not reason to worry that such agnosticism would hinder the progress of theory, itself.

Consider these two alternatives in turn. (And forgive the breezy way in which I now proceed, time being at a premium.) First, what could possibly be the alternative to the motto, "trust theory"? Transcendental arguments? No. Unless one thinks that projecting one's wishes upon nature is a legitimate form of philosophical argumentation. Intuition? Hardly. Intuition has a wretched track record. And that's no surprise, because, unless those are right who argue that intuition speaks for a Divinely endowed capacity for getting it right (and where's the evidence for that claim, if it's not just another form of wish fulfillment, or dogma?), then intuition is just another word for unexamined custom, habituation, prejudice, group-think, indoctrination, or a really bad education. Inference to the best explanation? No. For even if I do have a metric of comparative explanatory quality in the sciences, I surely don't have one in metaphysics, beyond such platitudes as "don't multiply entities beyond necessity, and so I don't know what "better" and "best" even mean in this domain. And even if I did know what "best" meant, all such judgments are comparative, and so, unless I know that I have surveyed every logically possible metaphysical explanation – and how can I know *that*? – then how am I justified in proclaiming as "best" some one among the metaphysical accounts that just happen to be on offer?

Second, what about the counsel of agnosticism? There is something right about this advice. If the human epistemic condition is one of metaphysical underdetermination, or some otherwise analogized weakness of constraint upon metaphysics, then belief – full stop – in some one of the underdetermined alternatives is surely not warranted. But perhaps the real lesson is that *belief* was never the appropriate epistemic attitude in the first place, in which case agnosticism, as the suspension of belief, is not even a relevant option. What might be the appropriate epistemic attitude in a universe thick with metaphysical possibility and thin on certainty? Well, Peirce's old idea of pursuitworthiness bids fair as a more sensible epistemic attitude, one that makes more sense than does belief in appreciating how science, and a scientifically driven metaphysics, really works. Pursuitworthiness has the double advantage of being forward looking and prizing fertility or fecundity as among the pre-eminent epistemic virtues. It has the further advantage of being not exclusionary, but commodious. More than one alternative can enjoy equal favor from the point of view of pursuitworthiness.

But now, if fertility in the form of the generation of pursuitworthy physical and metaphysical possibilities is a paramount virtue in inquiry, a very important role for naturalistic metaphysics is indicated. For there is no stronger induction on the history of science than that which records how, again, and again, and again, the metaphysical imagination has been enriched by the progress of science. Those who claim that everything is a footnote to Plato are wrong. And it is not true that all of physics and metaphysics is a variation on Parmenidean and Heraclitean themes. The history of physics is a history of previously unimagined possibilities. Newtonian inertia might bear a genetic relationship to medieval impetus, and its introduction might not mean a revolutionary rupture between incommensurable paradigms, but I know of no ancient, medieval, or Renaissance thinker who anticipated this seventeenth-century conceptual innovation, in the form of the idea that it was deviations from, not the preservation of straight-line, inertial motions that required an explanation. The Stoics had their pneuma, but Maxwell's electromagnetic field, with its orthogonal electric and magnetic field strengths, the helicity required to explain circular polarization, and Lorentzian symmetries was something genuinely new and different. Examples multiply. Entropy. Darwinian evolution. Minkowski spacetime. Spin. Quantum entanglement. Kin selection. Connectionist models of mind. Genetic algorithms. Dark matter. Dark energy.

Physics is not the royal road to truth in metaphysics. But it is a portal to a world of radical new metaphysical possibilities. Physics forecloses some metaphysical possibilities – our universe cannot be as Newton described it – but it opens so many more. And nothing can show more conclusively the bankruptcy of the view that metaphysics concerns itself with those claims that are true in all possible worlds than the lesson that array of possibilia is, every day, further enlarged. Metaphysical naturalism is here to stay.

4. *Structuralism and Quantum Field Theory*

Let's now bring ourselves back down to earth. Enough with meta-philosophy. Let's do a little in-the-trenches metaphysics. Let's return to a point twice hinted at before, where the implications of quantum field theory for structuralism and the issue of categoricity were mentioned. Let me make, in conclusion, just a small point, but one that I think important and still not well addressed in the literature.

If quantum mechanics and quantum field theory problematize the view of elementary particles as individuals, they lend support to a structuralist way of characterizing elementary particles, not as individuals that are bearers of relations but as nodes implicitly defined by a presumed structure of relations. The problem first appears in non-relativistic quantum mechanics, with bosonic and fermionic quantum statistics. It becomes only more acute in relativistic quantum mechanics and quantum field theory, where one encounters states of indeterminate particle number, and where particle number becomes a frame-dependent quantity. So the quantum theory appears to afford a sustained and compelling argument for a structuralist quantum ontology.

But not so fast. Let's pause an think about the ontology of quantum field theory. As always, we have to ask what formalism is in question. The structuralist likes algebraic formulations of theory. In non-relativistic quantum mechanics, in the quantum mechanics of systems with finite numbers of degrees of freedom, it's the algebraic point of view developed by Wigner, Weyl, von Neuamann, and others that made clear the way in which the seeming difference between such radically different ontologies as those assumed by Schrödinger's wave mechanics and Heisenberg's matrix mechanics represented no relevant difference at all. For, in the context of algebraic quantum mechanics, the Stone-von Neumann theorem tells us that wave mechanics and matrix mechanics are just different representations of one and the same algebraic structure, and that, as well all representations of that algebra, they were, of necessity, unitarily equivalent. Well and good. A triumph of the structuralist point of view.

But the non-relativisitic quantum mechanics of systems with a finite number of degrees of freedom is not our best current theory. Our best current theory is quantum field theory. It is a relativistic theory (in the sense of special, not general relativity) and it is a theory of systems with an infinite number of degrees of freedom. As such, in its most natural algebraic form, it can be shown to possess representations that are, of necessity, unitarily inequivalent. This is the algebraist's way of saying that the theory is not categorical, that it does not constrain the class of its models up to the point of isomorphism. In other words, if the algebraic point of view that elsewhere serves the structuralist so well is here, too, the right point of view, then there is a prima facie problem for the structuralist, inasmuch as the theory does not and, so it would seem, cannot privilege a single structural representation of quantum reality.

The work-a-day particle physicist replies that the framework of algebraic quantum field theory represents nothing but a philosopher's fetishizing of axiomatic simplicity, and that one can only calculate in a specific representation. But absent an argument that picks out a single correct representation, such as a Fock-space or occupation-number representation – and I know of no such argument – it's hard to dismiss the worry that, here again, there is a lack of univocalness in the way in which theory constrains ontology. Only now the lack of univocalness is more severe. For it's not just that there *happen* to be a variety of alternative ontological pictures among which theory is impotent to choose. No, in this case there exist, of necessity, a variety of structurally inequivalent representations of physical reality.

If the structuralist is committed to the view that theory affords a unique structural representation of nature, and if the algebraic point of view is the right point of view in quantum field theory, then the structuralist has a problem. My own view is that structuralism should never have committed itself to a uniqueness claim in the first place. But then, I am not a structuralist. So I have no right to an opinion.