Structural Realism and the Problem of Inequivalent Representations in Quantum Field Theory

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Introduction. Structural realism (SR) is an important contemporary view about science. It is important mainly for its claim that it can accommodate both realist and anti-realist intuitions about scientific theories and put thereby an end to the debate around scientific realism. But it is important also because it proposes, in one of its versions, challenging metaphysical theses, that one ought, for example, to give up an object-oriented ontology and take structure as bearing the most fundamental ontological significance.

My objective in this paper is to develop a criticism of SR. In short, I aim to show that the algebraic turn taken by some proponents of this view leads to a backbreaking difficulty raised by the existence of inequivalent representations of abstract algebraic structures in quantum field theory.

The outline of the paper is as follows. In the first section, I present SR. We will see that John Worrall, among others, defended a view according to which the structure of the world has epistemic priority over the nature of physical objects, nature that will remain forever inaccessible to our eyes (Worall 1989). I refer to this approach as non-eliminative structural realism (NESR). Then, I explain Bertrand Russell's related views on structure (Russell 1927) and their revival via a Ramsified conception of scientific theories by Grover Maxwell (1970a). Further, I present Max Newman's criticism of Russell (Newman 1928) and its revival by William Demopoulos and Michael Friedman (1985). Briefly, the criticism says that the structure defined over a domain of objects underdetermines the structure-giving relation (or system of relations) between those objects. Therefore, cognitive access to the mere structure does not guarantee cognitive access to the individual objects in the domain. In blatant contradiction to what Russell claimed, our structural knowledge of the world is either trivial or false. This criticism is usually understood as raising a fatal problem to NESR. Indeed, on the one hand, NESR is shown to hardly differ from instrumentalism (Ketland 2004). Instrumentalists themselves argue that the NESR attempt to steer away from an empiricist position takes 'an air of schizophrenia' (van Fraassen 2006). On the other hand, scientific realists claim that NESR does not add basically anything new to a traditional realist view, which already conceives of unobservable entities via the theoretical structure expressed by our scientific laws (Psillos 1995).

In the second section, I review the proposal (inspired by quantum mechanics) to switch from a notion of logical structure to an algebraic one. This move underlies a view, advocated today by Steven French and James Ladyman, which I refer to as eliminative structural realism (ESR). The main idea is that there are no objects in the world, except insofar as they are defined via an abstract, group-theoretical structure, which is all there is out there. Group theory was introduced in quantum mechanics by Eugene Wigner and Hermann Weyl. I analyze its role in the debate over structuralism between Arthur Eddington and Richard Braithwaite, in order to provide historical background for the subsequent discussion of ESR. Then, I look at French and Ladyman's motivation for ESR. As they assert, this comes mainly from non-classical indistinguishability phenomena in quantum mechanics that are taken to suggest the thesis that physics underdetermines metaphysical identity. Further support for this thesis is allegedly given by philosophical implications of quantum field theory. Here, too, elementary particles do not respect, we are told, classical criteria for individuality. However, this thesis has been questioned, and the debate over its significance for ESR is, as of yet, inconclusive.

In the last section, I raise a new challenge to ESR. I contend that the view according to which the physical content of a theory is fully captured by the mathematical structure is bound to fail. I support my contention with an analysis of the problem of inequivalent Hilbert space representations of a C^*-algebra in quantum field theory. I show that no mathematically available types of equivalence can be taken as a criterion for the physical equivalence of Hilbert space representations without loss for the ESR position. I close with an example from quantum statistical mechanics, regarding the explanation of thermodynamic phase transitions, which makes quite intuitive the kind of conundrum the ESR account of scientific theories has to meet.

1. Structural Realism and its Challengers. In his case for scientific realism, Ernan McMullin charts and attempts to dispel some sources of scientific antirealism: the classical concept of force, quantum indeterminism, the pessimistic meta-induction over the history of past theories, and so on (McMullin 1984). The development of science seems to suggest, or so the meta-induction goes, that since in the past every theory was replaced by a new theory, those presently accepted will in their turn be replaced by other theories. The latter normally accommodate the empirical success of the former, but introduce different (revolutionary) theoretical commitments. Therefore, there is no good ground for the claim that our present theories give a true or even an approximately true description of the physical world. However, McMullin argues, rather than looking at global explanatory theories, like mechanics, it would be more effective for the rebuttal of the pessimistic meta-induction to look at some other sciences, like geology and cell biology. Here, one realizes that scientists aim at progressively discovering the structure of the world, and that their theories are approximations of this structure that are (at least partially) preserved across theoretical change.

McMullin's argument for SR assumes a full-blooded endorsement of retroduction (or inference to the best explanation). In science, this leads to belief in the existence of the tectonic structure of the earth and of species in Devonian from the success of particular geological hypotheses. But, despite its widespread use in science, it is arguable whether philosophers can legitimately avail themselves of retroduction in arguing for scientific realism. For, to say the least, one must admit that this might never change the mind of someone already inclined towards anti-realism.

Other philosophers considered, therefore, that it was necessary for the goal of defending SR to take a look at those 'global explanatory theories'. Worrall's main motivation for defending SR as a halfway position between scientific realism and instrumentalism, one that could retain 'the best of both worlds', as it were, is to provide a historically accurate account of theoretical change, as a defense against the pessimistic meta-induction. He argues likewise that scientific knowledge is cumulative over theoretical change: successive theories display continuity of mathematical structure. This is the case, for example, in the development from Fresnel's theory of light (in terms of periodic disturbances in an elastic solid ether) to Maxwell's electromagnetic explanation of light (in terms of fields). Worrall takes the general applicability of Bohr's correspondence principle (which says, roughly, that the mathematical equations of an old theory correspond to a limiting case of the mathematical equations of the new theory) as evidence for SR (Worall 1989, 161). Thus, he talks about an 'approximate' continuity of structure from classical to relativistic mechanics. At the same time, he endorses the 'no miracle' argument for realism, which explains the success of science by maintaining that this pervading mathematical structure gives the true or approximately true picture of the world.

One distinction to which SR seems to be committed is that between the *structure* of the phenomena described by a theory and the *nature* of the entities underlying those phenomena. To repeat, NESR is the view that maintains that our epistemic abilities cannot access the nature, but only the structure; whereas ESR denies that there is nature above or beyond structure. Another distinction to keep in mind is that between *logical* SR and *algebraic* SR. The former makes use of a notion of structure informed by formal logic (second-order logic and set theory eventually included). The latter employs a group-theoretical notion of structure. Also, I will draw attention to the distinction between *concrete* and *abstract* structure (Haag and Kastler 1964, Redhead 2001), where the former is given by first-order relations of whatever relata make up the domain of the structure, whereas the latter by their higher-order properties and relations. More on this below.

Now, here is one important episode in the history of SR.[1] In *The Analysis of Matter*, Russell grants phenomenalism (i.e., the view that only percepts, objects of direct acquaintance, exist) 'as a method of separating perceptual from non-perceptual elements of physics, and of showing how much can be achieved by the former alone' (Russell 1927, 215). He rejects, however, the phenomenalist tenet that non-perceptual elements are 'unreal'. Russell develops (without being able to give a 'demonstration,' as he frankly admits on page 198) a view called 'the causal theory of perception', which claims that our percepts have real non-perceptual causes. This was a major move in Russell's thinking, away from the project of *Our Knowledge of the External World* (1914), where he had recommended that logical constructions from percepts be substituted for inferred entities.

In 1927, Russell is inclined to assume that there are unperceived events in the physical world, causally connected with our percepts. He believes that such events are essential to the affirmation of causal laws and that their being only logically constructed from percepts could not account for this role they play (op. cit., 214). But he also emphasizes that our knowledge of the unperceived events is rather limited: 'we can only infer the logical (or mathematical) properties of physical space, and must not suppose that it is strictly identical with the space of our perceptions' (op. cit., 252 sq.). In line with his causal theory of perception, he also assumes that 'any difference

[1] For a detailed historical account of SR, see Gower 2000 and van Fraassen 2006.

between two simultaneous percepts implies a correlative difference in their stimuli' (op. cit., 252), thereby aiming to establish a similarity relation[2] between the structure of percepts and the structure of their stimuli. So, he concludes: 'Thus it would seem that, wherever we infer from perceptions, it is only structure that we can validly infer; and structure is what can be expressed by mathematical logic, which includes mathematics' (op. cit., 254). Unlike structure, the nature of unperceived events in the physical world is, to us, inaccessible.

The *Principia Matematica* definition of the notion of structure is rehearsed in section XXIV of *The Analysis of Matter*. 'Two relations P, Q are said to be "similar" if there is a one-one relation between the terms of their fields, which is such that, whenever two terms have the relation P, their correlates have the relation Q, and vice versa.' (Russell 1927, 249) More explicitly, taking U and V as two domains, the structure (U, P) is said to be 'similar' to (V, Q) if but only if there is a bijective (i.e., one-to-one and onto) mapping $f : U \rightarrow V$, such that for every two terms u_i, u_j in U, $Pu_iu_j \leftrightarrow Qf(u_i)f(u_j)$. Of course, this definition can be generalized. Let us call (U, $P_1, P_2, ..., P_n$) a concrete structure. Also, let us call the class of all concrete structures similar to (U, $P_1, P_2, ..., P_n$) an abstract structure (Russell's isomorphism type, or relation number). For Russell, knowledge of the latter is all we can hope for in our theoretical exploration of the world.

In the 1970's, Grover Maxwell took up Russell's position and wedded it to Ramsey's conception of theories. Let me explain why. Maxwell was driven by the need to give an account of the meaning of theoretical terms concocted in our physical theories, and he found Russell's principle of acquaintance ready to serve (albeit in a slightly modified form, which takes observation as coextensive with acquaintance): 'All the descriptive (non-logical) terms in any meaningful sentence refer to items with which we are acquainted.' (Maxwell 1970a, 181) Thus, all descriptive terms are observation terms. Now, what about theoretical terms? They are usually taken as referring to unobservable entities, but given the above constraint principle it seems this can't be the case. Nevertheless, Maxwell contends that there are unobservable entities, but you don't need theoretical terms to refer to them. What you need is the Ramsey sentence for a theory.

[2]That is, a one-to-one correspondence. Russell also considers a semi-similar relation, i.e., a many-to-one correspondence. He is aware of theoretical underdetermination as a source of 'uncertainty, which remains even when we assume all the canons of scientific inference' (op. cit., 256), but believes that even then one may still obtain something 'useful'.

The Ramsey sentence $\mathcal{R}(T)$ of a scientific theory T is standardly thought of as a reformulation of that theory that eliminates theoretical terms by replacing them with existentially quantified, bound predicate variables. So, if what T says is represented as $T(o_1, o_2, ..., o_n, t_1, t_2, ..., t_m)$, where o's and t's are observation terms and theoretical terms, respectively, its Ramsey sentence $\mathcal{R}(T)$ says $\exists x_1 \exists x_2 ... \exists x_m T(o_1, o_2, ..., o_n, x_1, x_2, ..., x_m)$. Most importantly, as Maxwell notes, $\mathcal{R}(T)$ has the same observable consequences as does T and captures the physical content of T, that is, it provides epistemic access (via description) to the structural characteristics of the unobservable entities. More exactly, what we can have knowledge of by means of a Ramsified theory are not the intrinsic (or first-order) properties of unobservables, but their extrinsic ones (second or higher-order) described by $\mathcal{R}(T)$ (op. cit., 188). Thus, like Russell, Maxwell asserts the knowability of the abstract structure of the world. However, as we'll see presently, this rather Arcadian picture of our cognitive abilities collapsed under Newman's criticism.

In his review of *The Analysis of Matter*, read before the Cambridge Moral Science Club in December 1927, Newman notes Russell's anti-Kantian drive in the latter's remark that the unperceived cause of a percept is not a mere *Ding an sich*, at least not so if one accepts 'the usual canons of scientific inference' (Newman 1928). Chief among these canons is the postulate of structural similarity between cause and effect (when both are complex). As we have just seen, this postulate helped Russell claim that the abstract logical structure of the world is cognitively accessible to us. It is exactly this claim that Newman finds defective. He argues that knowledge of only this structure of the physical world is either trivial or false. Russell's position is summarized and criticized as follows:

> The world consists of objects, forming an aggregate whose structure with regard to a certain relation R is known, say W; but of the relation R nothing is known (or nothing need be assumed to be known) but its existence; that is, all we can say is, "*There is* a relation R such that the structure of the external world with reference to R is W". Now I have already pointed out that such a statement expresses only a trivial property of the world. Any collection of things can be organised so as to have the structure W, provided there are the right number of them. Hence the doctrine that *only* structure is known involves the doctrine that *nothing* can be known that is not logically deducible from the mere fact of existence, except ("theoretically") the number of constituting objects. (op. cit., 144)

What does Newman mean here? As above, let U be the domain of the objects in the world and let R refer to a system of (first-order) relations $(R_1, R_2, ..., R_n)$. Thus, Newman considers correctly that, for Russell, the concrete structure (U, $R_1, R_2, ..., R_n$) of the world is unknowable. What can be inferentially known is the abstract structure W. However, provided the cardinality of U is determined, the knowledge of W is a priori, and so it cannot be the sort of knowledge given by our physical sciences. It is a priori because it is a logical consequence of (what has come to be known as) Newman's theorem: 'For given any aggregate A, a system of relations between its members can be found having any assigned structure compatible with the cardinal number of A.' (op. cit., 140)[3] This is the reason why Newman considers Russell's structural realist claim trivial, meaning that the mere knowledge of W is not sufficient to specify the intended concrete structure (U, $R_1, R_2, ..., R_n$). Any additional criterion to the effect that the concrete structure of the world gets specified goes beyond Russell's view, and consequently, renders it false.[4]

I want to pursue next more closely the question about the consequences of the Newman problem for SR, a question first asked by William Demopoulos and Michael Friedman. They contend that due to the Newman problem Russell's structuralism collapses into phenomenalism (Demopoulos and Friedman 1985, 631).[5]

Since, as Newman showed, knowledge of the abstract structure W is, contrary to Russell's claim, merely a matter of logic (plus the empirical determination of the cardinality of the domain U), then it follows, Demopoulos

[3]For a proof of this theorem, see Ketland 2004, 294sq.

[4]Russell's reaction to Newman's criticism is to completely abandon the structuralist position, without even trying to provide the slightest amendment. In his letter to Newman, he admits that 'you make it entirely obvious that my statements to the effect that nothing is known about the physical world except its structure are either false or trivial, and I am somewhat ashamed at not having noticed the point for myself.' (quoted in Demopoulos and Friedman 1985, 631) Indeed, Russell was well aware of the point, since he himself had raised it (in a quite similar guise) against Dedekind's construction of the number system. In his *Principles of Mathematics*, Russell writes: 'It is impossible that the ordinals should be, as Dedekind suggests, nothing but the terms of such relations as constitute progressions. If they are to be anything at all, they must be intrinsically something; they must differ from other entities as points from instants, or colors from sounds . . . Dedekind does not show us what it is that all progressions have in common, nor give any reason for supposing it to be the ordinal numbers, except that all progressions obey the same laws as ordinals do, which would prove equally that any assigned progression is what all progressions have in common.' (Russell 1903, 242)

[5]On the same line, Jeffrey Ketland argues that a Ramsey-sentence version of SR collapses into instrumentalism (Ketland 2004, 298).

and Friedman note, that a structural statement about the world is implied by any other statement, statements of perception included. But this is exactly what phenomenalism maintains, that is, that our science can be logically reconstructed on the basis of our perceptions only. One other thing that follows is that physical science is trivialized, i.e., it is reduced to determining the cardinality of the domain U. The combination, à la Maxwell, between Russell's structuralism and the Ramsification procedure accentuates this undesirable consequence:

> The problem is that this procedure trivializes physics: it threatens to turn the *empirical* claims of science into mere *mathematical* truths. More precisely, if our theory is consistent, and if all its purely observational consequences are true, then the truth of the Ramsey-sentence *follows* as a theorem of set theory or second-order logic, provided our initial domain has the right cardinality - if it doesn't, then the consistency of our theory again implies the existence of a domain that does. Hence, the truth of physical theory reduces to the truth of its observational consequences; [...] Russell's realism collapses into a version of phenomenalism or strict empiricism after all: *all* theories with the same observational consequences will be equally true. (Demopoulos and Friedman 1985, 635) [6,7]

Needless to say, empiricists rejoice over this conclusion. Bas van Fraassen points to Worrall's endorsement of the 'no miracle' argument as the only move that attempts to keep SR away from sheer empiricism (van Fraassen 2006). But he believes that this move 'is, frankly, schizophrenic'. He agrees, of course, that scientific knowledge is cumulative and that accumulation is at the level of structure. But he finds no argument from science that this structure represents some reality behind the phenomena. For van Fraassen, the 'no miracle' argument does not raise a metaphysical question, but an epistemological one: 'The success of science is *not a miracle*, because in any theoretical change both the past empirical success retained and new empirical successes *were needed as credentials* for acceptance.' (op. cit.,)

[6] For a formal proof of Demopoulos and Friedman's claim that the truth of the Ramsey-sentence $\mathcal{R}(T)$ follows logically from its empirical adequacy, see Ketland 2004.

[7] There are various ways open to the structural realist to resist this conclusion. See, e.g., Zahar and Worall 2001. Ioannis Votsis argues, too, that a structural realist can very well (indeed has no choice but) bite Newman's bullet and live with that (Votsis 2003). Also, Psillos argues that Ramsification, at least as it was understood by Ramsey himself, has no close affinity to structuralism, and further, that Ramsey's approach to theories could avoid the Newman problem (Psillos 2004).

However, the realist still has the option of asking the metaphysical question behind the 'no miracle' argument and remaining sane. Thus, to avoid the trivialization problem and the collapse into some version of anti-realism, she could give up the SR claim about our restricted cognitive access to the mere abstract structure of phenomena and, in order to find a rejoinder to the pessimistic meta-induction, focus instead on their concrete structure (Chakravartty 2004). Relatedly, she could advocate epistemic access to the nature of the unobservable entities in the world, but give up the distinction between concrete structure and nature, and define the latter in terms of the former (Psillos 1995). This option takes a somewhat more traditional route to realism, and I will not discuss it here. Another possibility, the one I will focus on, is to reconceptualize nature in terms of abstract structure, i.e., to think of unobservable entities as mere instantiations of places in the structure. As we will see in the next section, this move trades the notion of a logical structure for that of an algebraic one.

It is remarkable that both these alternatives seem to be suggested by Newman, in his paper. After explaining why Russell's structuralism does not work, Newman attempts to come to its rescue. One of his proposals is to consider a further criterion in order to distinguish between isomorphic instantiations of W, i.e., to specify the concrete structure of the real world. For example, the intended system of relations could be identified as the 'important' one. But, as Newman notes, there is no justification for considering one system of relations as the important one, since nothing more is known about such systems 'save their incidence (the same for all of them) in a certain aggregate' (of determined cardinality). To take 'importance' as 'among the prime unanalysable qualities of the constituents of the world' is, Newman thinks, 'absurd'.[8] He concludes, therefore, that 'it seems necessary to give up the "structure - quality" division of knowledge in its strict form.' (Newman 1928, 147)

Another attempt to rescue SR, albeit merely hinted at by Newman, proposes that one deny the truth of Newman's theorem, which as we have seen above is the heart of the problem for structuralism. But, says he, 'this involves abandoning or restricting Mr. Russell's own definition of a relation, namely, the class of all sets $(x_1, x_2, ..., x_n)$ satisfying a given propositional function $\phi(x_1, x_2, ..., x_n)$.' (op. cit., 145) Newman does not give any further detail and I don't want to claim that he envisaged here something drasti-

[8]Facing a similar problem, Carnap introduced the notion of 'foundedness' as an undefinable, fundamental concept of logic, without considering, though, the problem solved (Carnap 1928, §154). For an analysis, see Demopoulos and Friedman 1985.

cally different from Russell's notion of logical structure, since he wrote in the logicist tradition of Whitehead and Russell and probably believed that all mathematics is logic. But it is worth noticing that a group-theoretical notion of structure was being introduced at the time in quantum mechanics, through the work of Wigner and Weyl, in the late 1920's. As we'll see below, Eddington makes use of this notion in upholding his version of structuralism against Braithwaite.

2. Algebraic Eliminative Structural Realism. In 1963, Thomas Kuhn asked Wigner about his introduction to group theory, early in the 1920's, and received the following answer: 'I knew about groups because of Weissenberg and possibly a little because of John von Neumann. Weissenberg told me: "Here is Weber's Algebra: read that and then you will prove to me that stable positions in crystals are symmetry points".' (quoted in Chayut 2001, 57) Wigner worked in chemistry and crystallography, being fascinated by crystal symmetries. He found that group theory immensely facilitated his calculations and helped him construct the proof suggested by Weissenberg. Later on, he increasingly realized its significance for quantum mechanics and applied group-theoretical symmetry principles to the study of elementary particles.

Prior to 1925, quantum mechanics was, in von Neumann's words, 'a conglomeration of essentially different, independent, heterogeneous and partially contradictory fragments' (von Neumann 1932, 4). This motivated him 'to present the new quantum mechanics in a unified representation which, so far as it is possible and useful, is mathematically rigorous' (op. cit., viii). He considered other such attempts (Schrödinger's proof of the mathematical equivalence of the two theories, Dirac and Jordan's transformation theory) as insufficiently rigorous, and proposed other mathematical instruments for achieving the desired unity (i.e., Hilbert spaces, later replaced by his W^*-algebras; see next section).

Sensitive to the importance of algebra for quantum mechanics, Weyl, too, emphasized that a common algebraic structure underlies both the wave and the matrix mechanics:

> This newer mathematics, including the modern theory of groups and 'abstract algebra', is clearly motivated by a spirit different from that of 'classical mathematics', which found its highest expression in the theory of functions of a complex variable. The continuum of real numbers has retained its ancient prerogative in physics for the expression of physical measurements, but it can justly be maintained that the

> essence of the new Heisenberg-Schrödinger-Dirac quantum mechanics is to be found in the fact that there is associated with each physical system a set of quantities, constituting a non-commutative algebra in the technical mathematical sense, the elements of which are the physical quantities themselves (Weyl 1931, viii).

As one proponent of ESR put it, 'Weyl recognised that the mathematical status of the two rival theories of quantum mechanics as alternative [concrete] *representations* of the same [abstract] mathematical structure makes preference for either eliminable once a unified framework is available' (Ladyman 1998, 421). This structuralist position receives its clearest expression in the work of Eddington. This is cited by another advocate of ESR: 'The investigation of the external world in physics is a quest for *structure* rather than *substance*.' (quoted in French 2003, 231) And further, 'We cannot describe substance; we can only give a name to it. Any attempt to do more than give a name leads at once to an attribution of structure. But structure can be described to some extent; and when reduced to ultimate terms it appears to resolve itself into a complex of relations.' (op. cit., 232) For Eddington, the most appropriate formal means to capture this complex of relations is group theory. It is interesting to follow his debate with Braithwaite over the epistemological claims of structuralism, because it is here that the role played by group theory is more clearly explained.

Braithwaite wrote a review of Eddington's *The Philosophy of Physical Science* (1939), where he criticized the latter's 'epistemological derivation of the hypercomplex algebra of physics' and launched an argument that recalled Newman's comments on Russell's structuralism (Braithwaite 1940). He pointed out that one essential characteristic of a group is that a certain rule of composition for its elements has to be given. If no such rule is specified and if one treats it, as it were, as a variable, then one has in fact no criterion for choosing the 'substantial' relation, i.e., the one that gives the structure of substance.

Eddington's reply is his most elaborate analysis of the role of group theory in physics. He suggests that Braithwaite missed the point of an *abstract* algebraic structure:

> In this abstraction the group is specified solely by the pattern [of an interweaving of elements]; and it is essential that the combining relation C should not be 'given', since the group would then cease to be abstract. This is the feature which makes the group concept useful in the philosophy of physics; for our structural knowledge of the

> external world gives no hint of the nature of C. What is more, it does not recognize the distinction (implied by Braithwaite) between the nature of an element and the nature of the combining relation which makes it an element of a group. *The element is what it is because of its relation to the group structure*. [...] I think the experts would dismiss the whole matter by saying that Eddington is talking about an abstract group and Braithwaite about realizations of a group. Be that as it may, I must insist that I am rescuing out of the mathematical formalism what is for physical purposes the most essential feature of the group conception of structure, namely, that primarily the elements of a group (or ring or algebra) are defined solely by their role in that group (or ring or algebra). (Eddington 1941, 268sq)

Eddington clearly endorses here a structuralist conception of the theoretical entities as elements defined exclusively in terms of a purely abstract algebraic structure. On this conception, he thinks that Newman's predicament is avoided. Indeed, he says so explicitly:

> Russell, in his pioneer development of structuralism, did not get so far as the concept of group-structure. He had glimpsed the idea of a purely abstract structure; but, since he did not concern himself with the technical problem of describing it, he had no defence against Newman's criticisms. Russell's vague conception of structure was a pattern of entities, or at most a pattern of relations; but the elements of group theory make it clear that pure structure is only reached by considering a pattern of interweaving, i.e., a pattern of interrelatedness of relations. [...] Newman rightly pointed out that the earlier descriptions of structure provided only trivial information, unless they were supplemented by knowledge which was not structural; but I can see no foundation for Braithwaite's contention that this objection applies to structure described by a group-multiplication table. (op. cit., 278)

Now, this is obviously not entirely correct. Russell did more than merely glimpsed the idea of abstract structure. As we have seen above, he conceived of it as a similarity class of concrete structures (an isomorphism type of relational systems). What Russell did not do is specifically define the objects in the domain of a relational system by their role in the corresponding similarity class, that is, he did not surrender the distinction between nature and abstract structure, as did Eddington. And this is why, unlike Eddington, he was a target to Newman's criticism.

Eddington concedes, though, that Braithwaite's understanding of a group structure, as a concrete realization of an abstract structure, can be indeed relevant if one is aware that it does not describe the external world, but a particular conception of the external world, or as he put it, 'not the abstract group-structure which is all we can know of the external world, but a particular conceptual representation of the group-structure' (op. cit., 271). A particular representation is chosen in virtue of a 'necessity of thought' - the mind's own contribution to the scaffolding of theoretical physics. 'Thus, in construing philosophically the assertions of physics, we have to allow for the fact that its language refers to a particular representation of the structure, and is more particularized than our empirical knowledge of the external world warrants.' (op. cit., 271)

It is also worth stressing that Eddington does not turn 'necessity of thought' into a criterion for specifying the concrete structure of the world, a criterion that would replace ad hoc criteria like Newman's 'importance' or Carnap's 'foundedness' (see footnote 7 above). We have seen that, for Eddington and Russell, our knowledge of the external world is limited to its abstract structure. In Russell's case, this knowledge follows logically from Newman's theorem and is therefore not the kind of knowledge one expects from physics. In contrast, Eddington seems to reject at the outset the idea that the real (abstract) structure of the external world can be completely revealed by our physical theories. Like Weyl, he believes that a theory gives merely a particular representation of the structure of the world.

On this background, let's discuss now French and Ladyman's ESR. In addition to the need, shared with NESR, to overcome the 'pessimistic meta-induction' and accommodate the 'no miracle' argument, their position is strongly motivated by the desire to come to terms with the metaphysical implications of modern physics. Inspired by Weyl and Eddington, ESR proposes a radical reconceptualization of the notion of a physical object in terms of abstract group-theoretical structure. Talk of physical objects in science is considered metaphysically ambiguous, since, it is argued, quantum mechanics has shown that elementary particles resist classical criteria of individuality (Leibniz's principle of the identity of indiscernibles), and so they can be interpreted either as non-classical individuals (i.e., individuals that are different but indistinguishable) or as non-individuals (French and Redhead 1988). More exactly, the difference between classical and quantum notions of objecthood is reflected by the fact that classical statistical mechanics counts different permutations of objects as distinct physical arrangements, while in quantum statistical mechanics permutation of objects does not imply physical changes. This supports the idea that physics underdetermines metaphysics, and motivates the replacement of objects by structure as the fundamental ontological constituent of the world:

> Given the above metaphysical underdetermination, a form of realism adequate to the physics needs to be constructed on the basis of an alternative ontology which replaces the notion of object-as-individual/non-individual with that of structure in some form. (French and Ladyman 2003, 37)

However, the usual object-oriented discourse in contemporary physics has heuristic value. 'The elements themselves, regarded as individuals, have only a heuristic role in allowing for the introduction of the structures which then carry the ontological weight.' (French 1999, 204) So, if one still talks of quantum particles as individuals, this is due to a sort of 'metaphysical transference' that attaches to particles 'a legend of individuality' (French 2003, 228).[9]

The ontological dissolution of elementary particles as particular objects, suggested by the metaphysical underdetermination thesis, finds support, according to French and Ladyman (op. cit., 46), also in our most fundamental theory about the physical world, quantum field theory. This is seen as rejecting elementary particles in favor of fields. What is a field? '[T]he field is the structure, the whole structure and nothing but the structure.' (op. cit., 48)[10]

However, the underdetermination thesis has been recently questioned. Simon Saunders argues that, in a sense, the individuality/non-individuality underdetermination of elementary particles is not idiosyncratic to quantum mechanics, but can be similarly found in classical mechanics (Saunders 2003, 131). Still, nobody rejected the object-oriented ontology on classical statistical grounds. Also, Anjan Chakravartty argues that it would be inconsistent

[9]This way of talking about individuals is traced back to Poincaré's approach to geometry, which stressed that in order to construct a group structure one has to start with the 'gross matter' of our sensations and use that as a 'crutch' (op. cit., 254).

[10]This challenging view is shared by Redhead: 'Realism has been often attacked on the grounds that there is a significant lack of convergence in the history of theoretical physics which, so the argument runs, is characterized by discontinuity rather than any continuous cumulative progression. But I believe that detailed historical analysis often reveals more continuity than one suspects, at any rate at the level of structure rather than ontology. To see the distinction in a general sort of way, compare asking the question What *is* a field? with the question What are the mathematical equations governing its behaviour?' (Redhead 1995, 18). Likewise, by Saunders: 'I believe that objects are structures; I see no reason to suppose that there are ultimate constituents of the world, which are not themselves to be understood in structural terms. So far as I am concerned, it is turtles all the way down.' (Saunders 2003, 129).

to consider the individuality/non-individuality underdetermination as depending on whether an entity is observable or unobservable (Chakravartty 2004, 159). Still, not everybody rejected the object-oriented ontological views about macroscopic everyday objects. Both these attacks are themselves questionable, but I don't want to pursue this here. Instead, I want to offer my own criticism of ESR.

3. A New Challenge to Eliminative Structural Realism. Let's start by noting that the structuralist view defended by French and Ladyman has at least one undesirable consequence: it blurs the distinction between mathematical and physical structures, pulling one toward a quite dubious physicalism about abstract mathematical constructs. But this leads, I believe, to an even more serious problem. For ESR considers that the physical content of a quantum theory is *fully captured* by its abstract algebraic structure. No non-structural ontological residue is part of that content. It follows that representations of the abstract algebra can add no new physical content. In other words, these representations must be all physically equivalent.

However, in quantum field theory, as we will see presently, an abstract C^*-algebra has an infinity of unitarily inequivalent Hilbert space representations. If unitary equivalence is taken as a criterion for physical equivalence, then the abstract algebra will obviously be unable to exhaust the physical content of the theory. This fact renders it difficult for ESR to claim that a physical theory provides an adequate representation of the world. More on this in section 3.2. below. If some mathematically less stringent notion of equivalence is chosen as a criterion for physical equivalence, then a proponent of ESR would have to either give up the abstract algebraic point of view or switch to an operationalist conception of science.

In order to reach a good understanding of the problem raised by inequivalent Hilbert space representations for ESR, I will review in section 3.1. some mathematical notions essential for the narrative of the historical development of quantum theory from the Hilbert space formalism proposed by von Neumann (1932) up to the W^*-algebras and the C^*-algebras.[11]

3.1. Quantum Field Theory and the Operator Algebras. Consider a classical system with a finite number of degrees of freedom. Properties

[11] C^*-algebras entered the scene of the quantum first in 1947 with Segal and then, following their characterization in 1943 by Gelfand and Naimark, with Haag and Kastler (1964). The W^*-algebras had been introduced already in 1934 by Murray and von Neumann. For further technical details the reader is referred to the excellent introduction to operator algebras by Bratteli and Robinson (1987).

of the system, like momentum and position coordinates, are associated (in Heisenberg's formalism) matrix operators p_m and q_n. To quantize the system means to have these operators obey canonical commutation relations (CCRs):

$$p_m p_n - p_n p_m = 0 = q_m q_n - q_n q_m,\ p_m q_n - q_n p_m = -i\hbar\delta_{mn}.$$

The dynamics of the system relative to a canonical operator A is then given by

$$\frac{\partial A}{\partial t} = \frac{i(HA_t - A_t H)}{\hbar} \tag{1}$$

where H is the Hamiltonian operator, and $\hbar$ is Planck's constant.
In Schrödinger's formalism, the dynamics is represented by Schrödinger's equation:

$$i\hbar\frac{\partial\psi_t}{\partial t}(x_1, x_2, ..., x_n) = H\psi_t(x_1, x_2, ..., x_n). \tag{2}$$

where ψ is a wavefunction in the space $L^2(\mathbb{R}^n)$ associated to the system's state space.

With von Neumann (1932), the state space is associated a Hilbert space $\mathcal{H}$, thereby resulting a unifying mathematical framework: $\mathcal{H} = L^2(\mathbb{R}^n)$. $\mathcal{H}$ is a metrically complete, normed, vector space over the complex numbers, with a Hermitian inner product. Physical quantities (observables) in the system are associated self-adjoint, linear operators on $\mathcal{H}$, which obey corresponding CCRs (in Poisson bracket notation):

$$[\hat{p}_m, \hat{p}_n] = 0 = [\hat{q}_m, \hat{q}_n],\ [\hat{p}_m, \hat{q}_n] = -i\hbar\delta_{mn}\hat{I}.$$

The expectation value of an observable O, described by the self-adjoint operator $\hat{O}$ in a given state $|\phi\rangle$ of the system, is $\langle\phi|\hat{O}|\phi\rangle$. And the equivalence between (1) and (2) above is given by (Bratteli and Robinson, *op. cit.*, 5):

$$\langle\phi_{t_0}|\hat{A}_t|\phi_{t_0}\rangle = \langle\phi_t|\hat{A}_{t_0}|\phi_t\rangle.$$

Since $\mathcal{H}$ is an infinite dimensional space, one normally associates to canonical operators $\hat{p}_m$ and $\hat{q}_n$ bounded unitary operators on $\mathcal{H}$, $\hat{U}_m(t) = e^{i\hat{p}_m t}$, $\hat{V}_n(t) = e^{i\hat{q}_n t}$, the so-called Weyl operators, which obey the *Weyl form* of the CCRs:

$$\hat{U}_m(t)\hat{U}_n(s) - \hat{U}_n(s)\hat{U}_m(t) = 0 = \hat{V}_m(t)\hat{V}_n(s) - \hat{V}_n(s)\hat{V}_m(t)$$

$$\hat{U}_m(t)\hat{V}_n(s) = e^{ist\delta_{mn}}\hat{V}_n(s)\hat{U}_m(t).$$

The algebraic structure of these relations is captured by a *Weyl algebra*, i.e. a C^*-algebra generated by the Weyl operators on any representation of the Weyl CCRs. A C^*-algebra is a metrically complete, normed *-algebra, satisfying the following six conditions:

$$\|A\| \geq 0 \text{ and } \|A\| = 0 \text{ just in case } A = 0,\ \|\alpha A\| = |\alpha|\|A\|,$$

$$\|A + B\| \leq \|A\| + \|B\|\ ,\ \|AB\| \leq \|A\|\|B\|,\ \|A\| = \|A^*\|,\ \|A^*A\| = \|A\|^2$$

for all $A \in \mathfrak{A}$, where $\|A\|$ is the norm of A.

If $\mathcal{H}$ is a Hilbert space and $\mathfrak{L}(\mathcal{H})$ is the set of bounded linear operators on $\mathcal{H}$, given $\pi : \mathfrak{A} \to \mathfrak{L}(\mathcal{H})$ a *-morphism on a C^*-algebra, then the pair $(\mathcal{H}, \pi)$ is a *representation* of $\mathfrak{A}$. This is an *irreducible* representation if no (nontrivial) subspace of $\mathcal{H}$ is invariant under the operators in $\pi(\mathfrak{A}.)$ If π is a *-isomorphism, then, and only then, $(\mathcal{H}, \pi)$ is a *faithful* representation. The relevant result here is the **Stone-von Neumann** theorem: Any irreducible, faithful representation $(\mathcal{H}, \pi)$ of $\mathfrak{A}$ is univocally determined up to a unitary transformation (*bis auf eine unitäre Transformation eindeutig festgelegt*). (See von Neumann 1931, 577.)

The theorem holds only for a physical system with finitely many degrees of freedom, and guarantees that this has an unique quantization (up to unitary equivalence). The dynamics of such a system is represented as in (1) or (2) above. But for a system with continuously many degrees of freedom, in quantum field theory, the theorem fails and the dynamics is represented by (a group of) *-automorphisms of the associated Weyl algebra, $\tau : \mathfrak{A} \to \mathfrak{A}$.

Another essential result is the **Gelfand-Naimark-Segal** theorem: Let $\mathfrak{A}$ be a C^*-algebra and let ω be a state (i.e., a positive normalized linear functional) over $\mathfrak{A}$, $\omega : \mathfrak{A} \to \mathbb{C}$. Then, for every $A \in \mathfrak{A}$, there exists a representation $(\mathcal{H}_\omega, \pi_\omega)$ such that $\omega(A) = \langle\Omega_\omega|\pi_\omega(A)|\Omega_\omega\rangle$, where $|\Omega_\omega\rangle$ is a cyclic vector in the Hilbert space $\mathcal{H}_\omega$.[12] The cyclic representation $(\mathcal{H}_\omega, \pi_\omega, |\Omega_\omega\rangle)$ is univocally determined up to a unitary transformation.

Now, we are in a position to define three types of equivalence. Let $(\mathcal{H}_1, \pi_1)$ and $(\mathcal{H}_2, \pi_2)$ be representations of a C^*-algebra $\mathfrak{A}$. They are said to be

1. *unitarily equivalent*, if there is a unitary operator $\hat{U} : \mathcal{H}_1 \to \mathcal{H}_2$ such that $\pi_1(A) = \hat{U}\pi_2(A)\hat{U}^*$ for all $A \in \mathfrak{A}$,

[12] A vector $|\Omega\rangle$ is *cyclic* for π, in $\mathcal{H}$, if the subset $\{\pi(A)|\Omega\rangle : A \in \mathfrak{A}\} \subset \mathcal{H}$ is dense in $\mathcal{H}$, that is, if $\mathcal{H}$ is a closure for that subset.

2. *quasi-equivalent*, if the von Neumann algebras[13] $\pi_1(\mathfrak{A})''$ and $\pi_2(\mathfrak{A})''$ are *-isomorphic, i.e., there is a *-isomorphism α such that $\alpha(\pi_1(A)) = \pi_2(A)$ for all $A \in \mathfrak{A}$.

3. *weakly equivalent*, if $Ker(\pi_1) = Ker(\pi_2)$, where $Ker(\pi) = \{A \in \mathfrak{A} : \pi(A) = 0\}$.

Each of these three types of equivalence may be taken as the criterion for physical equivalence of Hilbert space representations. If we choose 1, we have to deal with the failure of the Stone-von Neumann theorem for systems with an infinite number of degrees of freedom. There are various ways to do this, none of them completely satisfactory. One can reject, for example, those representations in which the vacuum state is not a positive normalized linear functional on the algebra, and so obtain a natural class of equivalence; or, one can invoke the principle of locality and consider as physical only those representations whose algebraic structures coincide locally (Haag and Kastler, *op. cit.*). But one can also retreat to mathematically less stringent notions like 2 or 3 above. Here, however, one encounters again not insignificant problems. 2 makes use of von Neumann algebras and therefore represents a step back from the abstract algebraic point of view endorsed by ESR.[14] 3 leads straight to adopting an operationalist view, which is of course hell for a proponent of ESR (since it equates the truth conditions for the theory with conditions under which physicists fail to falsify the theory. See Arageorgis *et al., op. cit.*, 158).

3.2. Unitary equivalence as the criterion for physical equivalence. I want to discuss a bit more the choice of unitary equivalence as our criterion for physical equivalence. In this case, we have to check and see if there are any unitarily equivalent but physically different representations. To get an idea about the kind of physical differences between the unitarily inequivalent representations of an abstract algebra, let's take a look at an example. Let's consider the quantum statistical explanation of thermodynamic phase transitions, i.e., the explanation of the existence, at certain temperatures and pressures, of multiple thermodynamic phases at equilibrium.[15] For a

[13] A von Neumann algebra on $\mathcal{H}$ is a *-subalgebra $\mathfrak{N}$ of $\mathfrak{L}(\mathcal{H})$ such that $\mathfrak{N} = \mathfrak{N}''$, where $\mathfrak{N}' = \{B \in \mathfrak{L}(\mathcal{H}) : [B, A] = 0, \text{ for all } A \in \mathfrak{N}\}$ is the commutant of $\mathfrak{N}$.

[14] A von Neumann or W^*-algebra is a *concrete* algebra, that is, its elements are defined in terms of bounded linear operators on $\mathcal{H}$.

[15] So, for example, at a temperature of -38.83°C and a pressure of 0.2 mPa, mercury exists at equilibrium in three phases, solid, liquid, and gas. Water, ice and steam coexist at 0.01°C and 611Pa.

finite dimensional quantum system, at a certain temperature and pressure, the equilibrium state is uniquely described by a Hilbert space representation, up to unitary equivalence. But for an explanation of the coexistence of multiple thermodynamic phases, we need correspondingly multiple distinct equilibrium states, and so, a more general notion of equilibrium. This is provided in the thermodynamic limit of quantum statistical mechanics (Ruetsche 2003, 1335).

In the thermodynamic limit, we consider an infinite dimensional quantum system. For such a system, there are different temperatures and pressures at which the system is in multiple thermodynamic phases at equilibrium. Therefore, there is no unique equivalence class of Hilbert space representations to describe its equilibrium states, at different temperatures and pressures, but many unitarily inequivalent representations. Now, to reject the physical significance of those representations that do not belong to a selected equivalence class means to deny the possible existence of some equilibrium states, i.e. to state that there is only one temperature and one pressure at which the system is in equilibrium. But this rejection has no justification and, as Ruetsche put it, it 'offends' our modal intuitions (op. cit., 1337). Physical differences between the unitarily inequivalent representations are manifest as differences of temperatures and pressures. Moreover, in this framework, phase transitions are explained by describing the multiple equilibrium states, at a certain temperature and pressure, via a plurality of unitarily inequivalent representations. So, physical differences between these representations are manifest also as differences of phase.

This example shows clearly, I believe, that if one espouses algebraic ESR, if one maintains that the physical content of a theory is entirely captured by an abstract algebra and that the latter's concrete representations add no physical content, then one rejects differences of temperatures, pressures, or phases. But surely no one would want to deny that water is different from ice, and ice from steam! Therefore, it is reasonable to ask the structural realist to consider the inequivalence of Hilbert space representations as a problem for her account. As far as I can see, she has two options. First, she could frankly admit that the notion of abstract algebraic structure is unable to entirely capture the physical content of a theory and should be further refined in order to accommodate the difficulty raised by unitary inequivalence. Or else, non-structural ontological aspects of scientific theories have to be consented to. Secondly, she could invoke a notion of 'partial structure' and claim, more modestly, that science is not able to account for more than merely a rough and incomplete picture of the world. And that would be just fine, if it did not actually mean, as we have just seen in the case of quantum statistical mechanics, to erroneously deny physical differences between certain states of a quantum system. For the realist, this denial comes at a high price, since one has to admit then that our theories are unable to provide adequate representations of physical systems. And if that is the case, then the success of science cannot be explained any more by the (approximate) truth of our scientific theories.

Conclusion. I have presented above SR about science, a view advocated today by philosophers like McMullin, Worrall, and Maxwell, among others, and earlier in the 20th Century by Russell, Weyl, and Eddington. I have also explained the challenge raised by Newman, and those charged by Demopoulos and Friedman against a Ramsified version of SR. I have then argued that algebraic ESR, as is today defended by French and Ladyman, meets with an insuperable difficulty raised by the existence of inequivalent representations of abstract C^*-algebra.

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