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Structural Realism: a neo-Kantian perspective

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1. *Structural realism: the status quo*

Structural realism was notoriously born in the attempt to reach a compromise between a realist's argument and an antirealist's one, namely the 'no miracle' argument and the 'pessimistic meta-induction', respectively. According to the 'no miracle' argument, scientific realism is the only philosophy that does not make the success of science a miracle. The only way of explaining why science is so successful in making predictions that most of times turn out to be verified, is to believe that theoretical terms refer, that theories in mature science are true or at least approximately true, and that the same term refers to the same thing even if it occurs in different theories. It is the referential nature of scientific theories that explains the success of science.

This realist's argument clashes nonetheless with a compelling antirealist's argument whose aim is precisely to break the link between reference and success: reference does not imply success, nor does success warrant a presumption of reference. History of science provides us with plenty of examples of theories that were genuinely referential and yet were neither strictly true nor necessarily successful (e.g. from Bohr's atomic theory to Mendel's genetic theory, Prout's chemical theory). On the other hand, success cannot be taken as the gold standard of reference either: from caloric to phlogiston, from the epicycles to the ether, history of science provides us with an embarrassment of riches when it comes to theories that enjoyed a relative empirical success and that nevertheless turned out to be non-referential. Hence the 'pessimistic meta-induction': as entities postulated by past successful theories turned out to be not existent, what can guarantee us that the entities currently postulated by our most successful scientific theories will not similarly turn out to be not existent in the future? Success cannot be taken as warranting a presumption of reference, *pace* the no miracle argument.

Moreover, pessimistic meta-induction has negative consequences also for another crucial realist's claim: the claim that there exist inter-theoretic links among subsequent theories, and that theories in mature science embed earlier theories as limiting cases, and are able to explain why their predecessors were successful (insofar as they were) by preserving the references of their central terms. But if the central terms of the earlier theories were not referential (as in the case of ether, phlogiston, among others), how is it possible to retain inter-theoretic links? Warranting reference continuity across theory-change is all the more relevant to a defence of scientific realism, and this is precisely what pessimistic meta-induction challenges.

Structural realism is meant to provide a solution to this problem: what warrants continuity across theory-change are not the entities theoretical terms refer (or may refer) to. In other words, it is not the ontology of a scientific theory, but rather the mathematical structure of the theory that warrants continuity across theory-change. In recent years, John Worrall has drawn attention to this epistemological version of structural realism, which he traces back to Henry Poincaré, although much of the following discussion has actually been influenced by Bertrand Russell more than Poincare.[1] Focussing on the historical case study of Fresnel's ether theory and Maxwell's electromagnetic theory, Worrall has famously argued that structural realism licenses an optimistic induction about theory-change, concerning not scientific entities themselves but mathematical structures. Although there is no continuity between the ether and the electromagnetic field, continuity can however be found between the mathematical structures of the two theories. Fresnel misidentified the nature of light; nonetheless his theory described the structural properties of light accurately and using mathematical equations that were in fact formally similar to those later employed by Maxwell to describe the properties of the electromagnetic field. Thus, what is carried over in the passage from Fresnel's to Maxwell's theory is not ontology but mathematical structure.

Structural realism claims then to do justice to the realist's 'no miracle argument' by identifying in the mathematical structure the element that warrants continuity across theory-change and hence safeguards inter-theoretic links. Once again, reference explains success and success warrants a presumption of reference,

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[1] Worrall (1994).

where however reference is no longer identified with the ontology, i.e. with the unobservable entities that may (or may not) be the referents of theoretical terms, but with the mathematical structure of the theory. Worrall's structuralism is mainly an epistemological thesis about what we can know and be realist about. In so doing, epistemological structural realism vindicates, rather than revises the ontological commitments of scientific realism. On this view, the objective world is composed of unobservable and unperceivable objects between which certain properties and relations obtain; but we can only *know* the properties and relations of these properties and relations, that is the *structure* of the objective world.[2] However, precisely because of this vindication of traditional ontology, structural realism stands condemned together with scientific realism of leaving unsolved the problem of ontological discontinuity across theory-change.

With an eye towards amending this problem, French and Ladyman have urged a metaphysical or ontic structural realism, which offers a 'reconceptualisation of ontology, at the most basic metaphysical level, which effects a shift from objects to structures'.[3] Modern physics itself seems to prompt this reconceptualisation, the necessity of rethinking from scratch our ontology in terms of 'structures', rather than in terms of 'objects'. French and Ladyman latch their metaphysical structural realism onto Ernst Cassirer's structuralism. But while Cassirer's structuralism was inherently related to neo-Kantian epistemology, French and Ladyman want to maintain the distance from neo-Kantianism and detach metaphysical structural realism from neo-Kantian epistemology so as to do justice to the realist's demand for mind-independence. This manoeuvre rises however some difficulties that have been at the centre of a recent ongoing debate: can we really 'dissolve' entities into mathematical structures? How can we even conceive of structural relations without relata?[4]

In this paper, it is not my intention to go all over again this well-known debate on structural realism, but rather to ask where it leaves us and attempt a philosophical diagnosis. In the following section I shall try to offer a diagnosis of the current stand-off within structural realism between the epistemological and the metaphysical variant, by drawing attention to some important assumptions

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[2] Ladyman (1998), p. 412.
[3] French and Ladyman (2003a), p. 37.
[4] See Cao (2003a), (2003b). For a response see French and Ladyman (2003b).

underlying the structural realist programme, and to their philosophical sources. It is the heterogeneity of these sources—I suggest—that is mainly responsible for the current stand-off within structural realism.

2. Structural realism: an overview of the philosophical sources

The variety of alleged forefathers of structural realism is symptomatic of the mixture of philosophical sources and traditions underlying the structural realist programme. Worrall traces it back to Poincaré; Grover Maxwell built it up on Russell, and tied it to the Ramsification of scientific theories. French and Ladyman appeal to Cassirer. It is beyond the purpose of this paper to give a historical reconstruction of the philosophical origins of structural realism. But I do want to point out the difficulty of reconciling Poincaré's and Cassirer's structuralist views on the one side, with Russell's structuralism on the other side. The following brief historical overview helps us unveil some of the hidden assumptions of structural realism, which I think are responsible for the tension and divergence of views that characterise the current debate.

2.1 Poincaré's structural realism and the physics of the principles

Henri Poincaré's structural realism is strictly connected with the so-called physics of the principles.[5] According to Poincaré, the structural continuity between Fresnel's ether theory—no matter how ontologically false the hypothesis of ether was—and Maxwell's electromagnetic theory was warranted by some fundamental scientific principles such as the principles of conservation of energy and the principle of least action.[6] This much celebrated historical episode notoriously prompted Worrall's epistemological structural realism and the discussion that

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[5] See "The Physics of the Principles" in Poincaré (1905), Engl. transl. (1982), pp. 299-301.

[6] The principle of least action, in Maupertuis's original formulation, says that "in Nature, the action necessary for change is the smallest possible" where action was defined as the product of the mass of the body times its velocity times the distance it moves. If we consider particles as light rays, the principle of least action says that the integral of the velocity of light over distance is an extremum for the path traversed. This principle is the converse of Fermat's principle of least time, which says that the trajectories of light rays traversing different media follow the path the minimizes time (i.e., the integral of the inverse of velocity over distance is an extremum).

followed Worrall's fortunate article. This discussion had however the effect of shifting the focus from Poincaré's original motivations to the realist's demand for referential continuity across theory-change. As a result, the relations encoded by Fresnel's equations came to be regarded as possible candidates for bearing the referential burden that ––for obvious reasons ––could not be borne by the ether. Metaphysical structural realists have subsequently latched onto this reading of the Fresnel–Maxwell story by stressing that these relations are all what there *is* from an ontological point of view, and not just from an epistemological one. As anticipated in the introduction, what is common to both these approaches is the idea that structural relations expressed by mathematical equations bear the *referential burden* required by the realist's no miracle argument. They must warrant referential continuity, regardless of whether we believe that these relations apply to objects which qua referents we are in no position of ever knowing, *or* we believe instead that these relations are themselves the referents and there is no other referent to look for. But if we take a closer look at Poincaré, we can see that the very same idea of structural relations bearing a referential burden is alien to his view. In Poincaré's words,

> the aim of Fresnel was not to find out whether there is really an ether, whether it is or is not formed of atoms, whether the atoms really move in this or that sense; *his object was to foresee optical phenomena*. Now, Fresnel's theory always permits of this, today as well as before Maxwell. The differential equations are always true; they can always be integrated by the same procedures and the results of this integration always retain their value. (…) That some periodic phenomena (an electric oscillation, for instance) is really due to the vibration of some atom which, acting like a pendulum, really moves in this or that sense is neither certain nor interesting. But that between electric oscillation, the motion of pendulum and all periodic phenomena there exists a close relationship which corresponds to a profound reality; (…) that this is a consequence of more general principles, that of energy and that of least action; *this is what we can affirm*; *this is the truth which will always remain the same* under all the costumes in which we may deem it useful to deck it out.[7]

According to Poincaré, both Fresnel's and Maxwell's theories "express true relations and the contradiction is only in the images wherewith we have clothed the

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[7] Poincaré (1902), Engl. transl. (1982), pp. 140-1. Emphasis added.

reality".[8] The contradiction between Fresnel and Maxwell is solved by giving a conventionalist twist to scientific theories: two theories may well both be 'true' if we give up a realist reading of their languages and regard them as different ways of describing the same "true relations" encoded by scientific principles. The structural continuity that Poincaré envisaged is grounded then on the fact that Fresnel's wave optics was founded on the very same basic principles (the principle of least action and conservation of energy), on which Maxwell's theory too was founded.[9] And Poincaré deemed these scientific principles certain and almost permanent across scientific developments, because they are useful conventions that cannot be confirmed or refuted by experiments.

Thus, in the end, the continuity between Fresnel and Maxwell is not grounded on any alleged referential role played by structural relations, but rather on the *conventional* nature of the scientific principles that encode these structural relations. It is the conventional nature of scientific principles that warrants their certainty and (almost) permanence across scientific theories, and hence (indirectly) warrants also continuity across theory-change. On the other hand, precisely because they are conventional, scientific principles give us enough leeway to speculate about the physical nature of things: they do not single out a unique description as the 'true' one (i.e., the one that corresponds to the way things are), but are instead compatible with alternative and apparently contradictory images.[10]

Without entering into a discussion of conventionalism, it suffices here to say that the answer that Poincaré gave to what we now call pessimistic meta-induction and the problem of referential discontinuity across theory-change consisted in playing down semantic realism as the view that we must construe the language of our scientific theories literally, i.e. that we must understand theoretical terms such as "ether", "electromagnetic field", "electron", and so forth, as *referring* to objects in the external world, and that we must understand fundamental laws of nature as *singling out* the 'real' (and unique) order of things in nature. Poincaré defended

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[8] Poincaré (1902), Engl. transl. (1982), p. 142.

[9] For a detailed discussion of this point see Ch. XII in Poincaré (1902), Engl. transl. (1982), pp.174-83.

[10] For instance, both Fresnel's wave theory of light and Laplace's corpuscular theory of light were founded on the very same principle of least action. Because of the aforementioned (see footnote 6) interconvertibility of the principle of least action with the principle of least time (when we replace velocity of light with its inverse), this very same basic principle grounded both Laplacian corpuscular optics (least action) and Huygens/Fresnel wave optics (least time).

instead a conventional construal of the language of science: his structural realism undercut pessimistic meta-induction by playing down the very same concept of reference (and the related notion of truth as correspondence) on which the problem hinges.

2.2 Cassirer's structural realism and the architectonic of scientific knowledge

The aim of Ernst Cassirer's neo-Kantian position, programmatically expressed in *Substance and Function*,[11] was to replace the deeply instilled 'substantialistic' conception of science with a 'functional' conception. According to the 'substantialistic conception', the world is a world of substances, of physical entities bearing certain properties and entering into definite relations with other entities. Laws of nature are read off the entities, their properties and relations. From Cassirer's 'functional' viewpoint, on the other hand, entities constitute no longer the self-evident starting point, but the final point of scientific inquiry. The starting point is instead the concept of 'function' as it emerges in mathematical physics. The world is a world of functional relations encoded by laws of nature, through which only we have epistemic access to scientific entities. In his later book *Determinism and Indeterminism in Modern Physics*[12] Cassirer portrayed scientific knowledge as a three-layer architectonic consisting of (1) results of measurements, (2) laws, and (3) principles. Cassirer made it clear that this distinction should not be read hierarchically, or as implying some sort of reductionism. It is rather a purely 'architectonic' distinction, so to speak.[13] Results of measurement and scientific principles occupy the two complementary poles of this architectonic. The former provide the empirical basis. The latter fulfil the regulative task of systematizing and conferring an order on this empirical basis, as an integral and indispensable part of empirical knowledge. As a result of this systematisation, lower-level phenomenological laws could be derived. Cassirer clearly distinguished between laws and principles: scientific principles are "the birthplace of natural laws, a matrix

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[11] Cassirer (1910), Eng. trans. (1953).

[12] For a more comprehensive analysis of Cassirer's neo-Kantian view, see Massimi (2005), Section 1.4.2, on which I draw here.

[13] Cassirer (1936), Engl. trans. (1956), p. 36.

as it were, out of which new natural laws may be born again and again".[14] This architectonic of scientific knowledge in turn fixes and delimits the boundaries of 'objective reality'. According to Cassirer, 'objective reality is attained only because and insofar as there is conformity to law, not vice versa'. Beyond those boundaries, there is no other reality for us to investigate or seek after: the boundaries of what we can know are the very same boundaries of reality, or at least of the reality that is meaningful for us, i.e. the reality we can have scientific knowledge of. By building up on Kant's epistemological lesson, rather than on conventionalism, Cassirer's structuralism too played down the notion of reference. Or more precisely, he redefined such a notion from a neo-Kantian *internalist* perspective, according to which what objects the world consists of is a question that makes sense only within a scientific description of reality and that we can answer only in the light of some fundamental mathematical functions encoded by laws and principles.

Despite the differences, conventionalism and neo-Kantianism agree about giving less of a role to the notion of reference. Yet there is a third important philosophical source for structural realism, which does not square well with Poincaré's and Cassirer's structural realism and which nevertheless has represented perhaps the most influential expression of this movement: Bertrand Russell's structuralism.

2.3 Russell's structural realism and the legacy of reference

Among the philosophical sources of structural realism, Bertrand Russell occupies a special position. No-one else has exerted a greater influence on this movement than him. In *The Analysis of Matter* Russell[15] anticipated most of the theses of epistemological structural realism. He argued that we can and do have knowledge of the external world, i.e. of unperceived events, but this knowledge is purely structural. Whereas of percepts we can know both their qualities (i.e. properties and relations) and the properties of their qualities (i.e. structure), of unperceived events we can know only the properties of their properties and relations: we know only the structure of the external world, not its intrinsic (first order) properties and relations. Despite the Kantian flavour of some sentences about

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[14] *Ibid.*, p. 52.

[15] Russell (1927).

the things in themselves of the external world being unknowable noumena, the Kantian echoes are here filtered through Russell's theory of reference and truth.[16]

As is well known, Russell's distinction between knowledge by acquaintance and knowledge by description run parallel to a distinction between terms that refer to things we know by acquaintance (i.e. names of sense data), and terms that refer to things we can only know by a description of the type 'The one and only entity which…'. In this respect, Russell's theory of descriptions anticipated the Ramsification of scientific theories that Grover Maxwell[17] has advocated as a method allowing indirect reference to unperceivable entities by replacing theoretical terms such as 'ether', 'electron', and so forth, with Ramsey sentences of the form $\exists t_1 \ldots \exists t_m(O_1 \ldots O_n;\ t_1 \ldots t_m)$ correlating observational data O about the putative entity with theoretical content $t_{1 \ldots} t_m$. Having so defined the reference of terms, an assertion can be held true—according to Russell—if the corresponding state of affairs obtains, false otherwise.

Russell's structural realism hinges on scientific realist's intuitions about reference and truth. This scientific realist's intuitions have proved persistent and dominant in the following philosophical literature. Structural realism was born precisely from an inner conflict between the scientific realist's demand for reference and truth (expressed in the 'no miracle' argument) and the awareness that this demand cannot be satisfied (given pessimistic meta-induction).

3. The Newman problem as a problem about reference

Russell's structural realism faces a major problem that Newman originally spotted.[18] Saying that we know only the structure of the external world is to say nothing at all, because it follows from set theory or second order logic that given a collection of objects, there will always be a relation *R* holding among them and obeying a certain structure *W*, as long as *W* is compatible with the number of objects. To put the problem in a more pointed way, once the domain is fixed, there is no way of distinguishing a relation *R* from another relation *S* on the same domain

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[16] Russell (1912), (1914).
[17] G. Maxwell (1970a), (1970b).
[18] Newman (1928).

having both structure *W,* i.e. there is no way of distinguishing between important and unimportant relations.

Newman's problem is a problem about reference. Russell's structural realism is in the end a theory about how we can fix the reference of theoretical terms and be sure that they are genuinely referential, even if the objects at issue are unperceived and unperceivable. But, as Newman pointed out, Russell's structuralist solution was actually unable to single out reference, and hence unable to deliver on the original promise.

Apropos of this, Demopoulos and Friedman have rightly noticed an analogy between the Newman problem and Hilary Putnam's problem about reference.[19] The problem, famously analysed in chapter 2 of Putnam's *Reason, Truth, and History*, amounts to the following: given a language L and given an admissible interpretation of L, i.e. given a set of operational and theoretical constraints like those that rational inquirers would accept and that determine which sentences in the language are true, there is no way of determining what our terms *refer* to. Putnam shows in particular how a given sentence such as 'A cat is on a mat', where on the standard interpretation 'cat' refers to cats and 'mat' refers to mats, can be reinterpreted so that in the actual world 'cat' refers to *cherries* and 'mat' refers to *trees* without affecting the truth-value of the sentence in any possible world. Putnam's argument is meant to be a criticism of standard scientific realism, and to prompt an alternative realist view, an internalist one, according to which '*what objects does the world consists of?* is a question that it only makes sense to ask *within* a theory or description.'[20] Putnam identifies in Kant the forefather of internal realism, as a view opposed to what he calls the externalist perspective (the God's eye point of view) typical of scientific realism, or metaphysical realism as Putnam calls it.

The problem about reference that Newman raised against Russell's structuralism is somehow complementary to the problem about reference that Putnam raises against metaphysical realism. Indeed they are just two sides of one and the same problem about reference: (i) either the reference of theoretical terms is fixed by objects in the external world, or (ii) the reference of terms is fixed by the description of the relevant structural properties of these objects. The problem with (i), as Putnam pointed out, is that it is not clear how reference can be singled out

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[19] Demopoulos and Friedman (1985), p. 633.

[20] Putnam (1981), p. 49. Emphasis in the original.

uniquely and unequivocally on any given admissible interpretation of a language. Nor does (ii) fare any better on this score: as Newman showed, we cannot unequivocally single out reference given the description of structural properties either.

I think that the main lesson we should draw from the Newman problem concerns the persistence of some deeply instilled *metaphysical realist* assumptions in the current debate on structural realism, and the problems that they inevitably bring along with them. Russell's structuralism crucially retained an *externalist* perspective about reference. This externalist perspective persists in the current debate on structural realism, and constitutes the common denominator of all the different variants. In the end, epistemological structural realists and metaphysical structural realists agree on one point: namely, that the primary aim of structural realism is to do justice to the (metaphysical realist) view about reference as expressed by the 'no miracle' argument. This externalist perspective about reference, which is the residue of Russell's highly-influential philosophical agenda, faces nonetheless some inescapable problems. By contrast, there are other philosophical traditions, to which current debates seem to have paid only lip-service, and that may be worth exploring since they avoid the problems affecting Russell's structuralism. Poincaré's structuralism and Cassirer's structuralism are possible candidates. In what follows I advocate a neo-Kantian twist on structural realism along the lines of Cassirer. It is far from the scope and purpose of this paper to offer a full-blown neo-Kantian view on structural realism. The best I can do is to raise some questions and foreshadow possible answers. Much work needs to be done to spell out the implications of a neo-Kantian perspective. What follows must then be read with an eye towards improving on a still largely unexplored area.

4. A neo-Kantian perspective

In the light of the Newman problem discussed above, I want to suggest that structural realism should not be understood as a form of *semantic realism*, as a way of retaining a literal construal of the language of science in the face of the challenge posed by referential discontinuity across theory-change. This way of understanding the aim and programmatic intent of structural realism is only the residue of Russell's influential agenda, and most of the recent discussions seem to have been going along Russell's conceptual path. Epistemological structural realism follows Russell in identifying structure with what remains fairly stable across theory-change and hence as a candidate to bear the referential burden required by the no miracle argument. Nor does metaphysical structural realism represent a real change with respect to this philosophical agenda: in the end, also in this case the aim is to give an ontological gloss on structure so that it can better bear the referential burden *by itself*, i.e. without the need of assuming an ontology of objects as the relata of structural relations.

Structural realism should instead be understood as a form of *epistemic realism*: it helps us to cash out truth, not reference. Namely, it helps us to make sense of what it means for an assertion like 'the electron has momentum p_1' to be true, where 'to be true' must here be understood as 'to be justified'. Of course, the identification of truth with justification has a distinguished philosophical pedigree in the Kantian tradition, to which in recent times Hilary Putnam has drawn new attention.[21] As is well-known, after his Kantian turn, Putnam identified truth with idealised rational acceptability:[22] a sentence is true if we are justified to assert it under *sufficiently good epistemic conditions*, such as the ones that rational beings with our nature can have.[23] But what are the *sufficiently good epistemic* conditions that rational beings with our nature can have? Putnam answered this question with rather mundane examples of macroscopic observable objects such as a chair being in my study and me being able to see it without anything wrong in my eyesight, etc. But, surely, these examples cannot address or shed light on the question that really matters here, namely what the sufficiently good epistemic conditions are for us to be

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[21] For the relationship between Putnam's view and Michael Dummett's similar view about truth as justification, see Putnam (1983), xvi–xviii.

[22] 'What then is a true judgement? Kant does believe that we have *objective* knowledge: we know laws of mathematics, laws of geometry, laws of physics (…). The use of the term "knowledge" and the use of the term "objective" amount to the assertion that *there is still a notion of truth.* But what is truth if it is not correspondence to the way things are in themselves? (…) The only answer one can extract from Kant's writing is this: a piece of knowledge (i.e. a "true statement") is a statement that a rational being would accept on sufficient experience of the kind that it is actually possible for beings with our nature to have.' Putnam (1981), p. 64.

[23] As Putnam later clarified 'ideal' epistemic conditions should not be confused with Peirce's view of truth as intersubjective agreement of a community at the ideal limit of inquiry: 'I do not by any means *ever* mean to use the notion of an 'ideal epistemic situation' in this fantastic (or utopian) Peircean sense. By an ideal epistemic situation I mean something like this: If I say 'There is a chair in my study', an ideal epistemic situation would be to be in my study with the lights on or with daylight streaming through the window, with nothing wrong with my eyesight, with an unconfused mind, (…). Or, to drop the notion of 'ideal' altogether, since that is only a metaphor, I think there are *better and worse* epistemic situations *with respect to particular statements'* Putnam (1990), viii.

justified in asserting things about *microscopic* and / or *unobservable* objects (electrons, quarks, ether, phlogiston, etc.), i.e. the vast majority of objects postulated by our scientific theories and primarily responsible for the referential discontinuity across theory-change. If truth as justification is to do any job at all, we'd better fill the lacuna about what the sufficiently good epistemic conditions are under which we can make assertions about unobservable objects *in a reasonable and justifiable (albeit fallible) way*. Putnam explicitly denied the possibility of even sketching 'a theory of actual warrant (a theory of the "nature" of warrant), let alone a theory of idealised warrant'[24] and simply offered what he called a 'picture'. It is not my intention or aim to provide a theory of actual warrant; needless to say, a theory of idealised warrant. Nevertheless I do want to sketch some possible guidelines for a future would-be theory of the 'nature' of warrant. I think that structural realism can help us sketch such a theory; namely, it can help us cash out what the good epistemic conditions are under which we may be justified in making assertions about unobservable objects. In other words, I want to suggest that mathematical structures should not regarded as bearing the referential burden, but rather as fixing the epistemic conditions under which we can reasonably and justifiably (albeit fallibly) make assertions about physical entities. If structuralism has to play a role in physics at all, it should play it with respect to the epistemic conditions of justified assertibility rather than with respect to reference. This move of course implies a radical re-thinking of the aim and purpose of structural realism as it has been advocated and championed so far in the literature. Paraphrasing the title of a famous article of Worrall, if we can remain reasonably optimistic despite pessimistic meta-induction, it is not because mathematical structures can warrant the referential nature of scientific theories that we feared was lost. Rather, we can remain reasonably optimistic because—*problem of reference notwithstanding*—mathematical structures fix the good epistemic conditions under which we are warranted in making assertions about certain physical entities but not about certain others (within the fallible and empirically revisable limits of human knowledge, of course). It is in this specific respect that structural realism should be regarded more as a form of epistemic realism than as a form of semantic realism: it cashes out truth, not reference. Let me try to flesh out the slogan.

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[24] Putnam (1990), p. 42.

From a neo-Kantian perspective as the one I want to advocate here, the *good epistemic conditions,* under which we are warranted to assert some sentences about unobservable physical entities, are given by a particular combination of experimental evidence and mathematical structures. More precisely, they are given by the particular way in which available experimental evidence gets built into a theoretico-mathematical structure. Along the lines of Cassirer's architectonic of scientific knowledge, I am suggesting that the good epistemic conditions that justify us to assert some sentences about unobservable entities such as electrons, positrons, quarks, and so forth, are those conditions in which the experimental data (Cassirer's 'results of measurement') are built into first order *relations* among measured physical quantities as displayed by laws of nature, and then into second order *structural relations* (relations of relations) as displayed by scientific principles (the higher layers of Cassirer's architectonic). Let me give a couple of examples to illustrate this point.

4.1 ***Pauli's exclusion principle between fermions and parafermions***

In my book,[25] I have analysed how spectroscopic evidence accumulated in the old quantum theory led Wolfgang Pauli to introduce in 1925 the exclusion principle as a simple phenomenological rule for the closure of electronic groups. Only in 1926, with the independent contribution of Dirac and Fermi, did it become clear that Pauli's veto could be re-expressed as veto on the class of mathematical states allowed for electrons: it excluded all states different from the antisymmetric ones, where antisymmetric states are those states that change sign under permutation of the space and spin coordinates of two electrons. Electrons turned out to obey the Fermi–Dirac statistics: they were fermions. In 1940, with the proof of the spin-statistics theorem, Pauli's veto was extended to any half-integral spin particle. When in the 1960s the quark model for hadrons was introduced, quarks as half-integral spin particles were assumed to obey Pauli's principle. But some negative evidence was found: the baryons' spectra revealed that quark space and spin wave function was actually symmetric, rather than antisymmetric as required by Pauli's principle.

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[25] Massimi (2005).

A possible way of reconciling this negative evidence with the quark theory consisted in postulating that quarks did not follow strictly the Pauli principle, and they obeyed instead a quantum statistics intermediate between Fermi–Dirac and Bose–Einstein (so-called 'parastatistics)': quarks may be 'parafermions'. The possibility of parafermions, and more generally of paraparticles, followed from permutation invariance: as Greenberg and Messiah proved in 1964, in quantum mechanics given $|\psi\rangle$ the vector representing the state ψ of a composite system of n indistinguishable particles, it is not possible by measuring the expectation value of any observable B to distinguish $|\psi\rangle$ from any permutation $\mathrm{P}|\psi\rangle$. This permutation invariance is satisfied not only in the case in which $|\psi\rangle$ is either symmetric or antisymmetric, but also in the case of some subspaces of the Hilbert space of dimension greater than 1, called generalised rays, which are invariant under all permutations. Thus, permutation invariance allows for symmetric, antisymmetric, and higher symmetry states too. Pauli's veto turned out to be only one among other possible symmetry types. However, the experimental search for paraparticles did not give the expected results (although it is still ongoing). In the 1990s important experiments were run to test eventual Pauli-violating (parafermion) copper and helium atoms: they gave negative results, and in so doing reduced the limit on possible violations of the exclusion principle. In the meantime another research programme had been developed in the 1960s that reconciled the negative evidence about Pauli's principle with the quark theory by introducing a new degree of freedom for quarks, the 'colour'. Hence, the development of quantum chromodynamics and the experimental search for coloured quarks that has led to amazingly fruitful results in the past forty years (from the discovery of scaling violations and charmonia, to the renormalization of the electroweak theory).

I have reconstructed all this historical evolution of the Pauli principle in detail in my book, where I defend a Kantian view about the origin and role of the exclusion principle. Here I want to draw attention instead to the role that the *structural relations* expressed by Pauli's principle play for the above discussion about structural realism as a form of epistemic realism. We can distinguish three stages in the history of the exclusion principle:

(i) Pauli's original 'exclusion rule' was, as I mentioned, a simple phenomenological rule saying that there cannot be in an atom two electrons in the same dynamic state (where the dynamic state was expressed by a set of four quantum numbers). If there is already an electron in that state, the state should be considered as occupied. This phenomenological rule expresses a simple *first order relation* about an electron, say, electron 1 being in the state say *nkm*, and another electron, say electron 2 not being in that same *nkm* state.

(ii) Reformulated as an antisymmetrization prescription with the Fermi–Dirac statistics, Pauli's principle comes to express a *second order or structural relation* (a relation of relation) concerning no longer the dynamic state in which two electrons can be, but rather the classes of mathematically allowed states for an assembly of indistinguishable half-integral spin particles (electrons, but also protons, neutrons, muons, quarks, etc.). Given say an assembly of only two electrons, and given the two mathematically possible states (symmetric S and antisymmetric A) resulting from the permutation of the space and spin coordinates of the two electrons (i.e. given the two possible first order relations e_1Se_2 and e_1Ae_2), the principle excludes the class of symmetric states and selects the class of antisymmetric states as the only mathematically allowed one. Hence it expresses a second order structural relation between an assembly of indistinguishable half-integral spin particles *and* the class of mathematical states (antisymmetric) that applies to it among all the mathematically possible ones. Or, to put it in a slightly different way, it expresses the structural relation between the kind of spin (half-integral) an assembly of indistinguishable particles has and the kind of quantum statistics (Fermi–Dirac) the particles follow.

(iii) Finally, permutation invariance allows not only for symmetric or antisymmetric states but also for higher symmetry types. Accordingly, in the 1960s physicists tried to relax the ban imposed

> by Pauli’s principle on fermions, and allowed half-integral spin particles (e.g. quarks) to obey para-Fermi statistics. In other words, it follows from invariance under the permutation group that we can embed the structural relation expressed by Pauli’s principle into some sort of *disjunctive structural relation* that says: given an assembly of indistinguishable half-integral spin particles, they can be with a certain probability either in the usual antisymmetric state, or with another probability in an anomalous (Pauli-violating) state. This is what the parastatistics programme claimed and tried to prove.

Going then back to my aforementioned suggestion about structural realism as a form of epistemic realism, we can regard experimental evidence *plus* the structural relation encoded by Pauli’s principle as displaying some of the good epistemic conditions under which we are justified to make assertions about electrons, and more generally about fermions (protons, positrons, neutrons, quarks,…). On this view, we are (or are not) justified in asserting a sentence like “The omega minus particle consists of three equivalent *s* quarks” or “Copper atoms emit anomalous (Pauli-violating) K-shell X-rays” in the light of the particular way in which the available experimental evidence fits (or does not fit, respectively) into a system of first order, and second order structural relations expressed by phenomenological laws and scientific principles, respectively. Depending on this fit, we are (or are not) justified in asserting these sentences, and hence they are true (or false). Should the near future give us any positive experimental result about parafermions that would fit the disjunctive structural relation allowed by permutation invariance; or, should we modify our system of knowledge by introducing new higher-level symmetry principles that modify the structural relations currently known, we would accordingly modify and revise the conditions of assertibility of sentences about physical entities.

4.2 *Bohr vs. Einstein on physical reality*

This internalist perspective about physical reality as not independent of the particular experimental and theoretical circumstances we can avail ourselves of, is perhaps the most important philosophical lesson emerging from quantum mechanics, in particular from the orthodox Copenhagen interpretation. As Niels Bohr repeatedly stressed against Einstein, what kind of physical properties we can meaningfully ascribe to quantum objects depends ultimately on the quantum mechanical formalism, on the one side, and on the empirical evidence available, on the other side. The Bohr–Einstein debate on the completeness of quantum mechanics is illuminating in this respect. The real divergence between Einstein and Bohr and the reason why this is such an important episode in philosophy of physics resides precisely in the different conceptions of physical reality endorsed by Einstein and Bohr.

As is well-known, in 1935 Einstein published a joint paper with Podolsky and Rosen where they argued that the quantum mechanical description of physical reality was incomplete. Einstein was presupposing—along the lines of classical physics—the existence of an external, mind-independent reality that was correlated with a physical theory so that a theory gives a complete description of reality if and only if *every element of physical reality has a counterpart in the physical theory.*[26] Einstein–Podolsky–Rosen then fixed a criterion of physical reality, which said that if, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity. The criterion was presented as a sufficient condition for physical reality and was said to be in agreement with quantum-mechanical as well as with classical ideas of reality. Given then the completeness condition and the criterion of physical reality, Einstein, Podolsky and Rosen proceeded to present a thought-experiment that showed how the description of physical reality given by the quantum mechanical formalism was incomplete, i.e. it could not capture all the physical properties a particle has. In particular, given a composite system of two particles that have interacted in the past but are no longer interacting, it was possible by measuring say the position of the first particle to predict with certainty the position of the second particle, and similarly for the property momentum. So, the second particle seemed to have (in the light of the criterion of physical reality) both a real position and a real momentum, which however were not both captured by the state function of the

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[26] Einstein, Podolsky, Rosen (1935), p. 777.

composite system. Hence a dilemma: either the quantum mechanical description of physical reality is incomplete (as EPR argued for), or we can save quantum mechanics completeness at the cost of saying that the properties of the second particle are causally influenced by the measurement of properties on the first, separate and non-interacting particle (which implies a violation of locality and separability).

As Bohr stressed in his response to EPR,[27] the argument was based on an essential ambiguity concerning the criterion of physical reality: that criterion was inadequate for the physical reality we encounter in quantum theory. As Bohr pointed out, although a measurement on the first particle could not physically disturb the second particle (locality is not violated), however the experimental arrangement required for the measurement determines the epistemic conditions for meaningfully ascribing the physical property at issue both to the first and to the second particle. Hence Einstein was mistaken in assuming that the second particle must have both an exact—yet unknown— position and momentum. An object cannot meaningfully be said to have certain properties in the absence of the experimental and theoretical conditions which make such talk meaningful. Quantum mechanical formalism *and* experimental set-up *jointly* provide the conditions under which we can justifiably ascribe properties to particles. Ascribing properties to particles *regardless of* the mathematical formalism *and* of the available experimental set-up amounts to an unwarranted metaphysical claim about physical reality, according to Bohr. When we run an experiment to measure the position of the first particle, the quantum mechanical formalism allows us to predict the position also of the second particle. But that very same experimental set-up that allows us to make assertions about the positions of the two particles, does *not* allow us to make assertions about the momentum of either particle 1 or particle 2. To do that, we need a different experimental set-up, incompatible with the other, such that when we measure momentum, we cannot in turn make any assertion about the position of either particle.

Bohr's reply to EPR implied a radical revision of the classical notion of physical reality that Einstein was not willing to endorse. The more recent scientific developments after Bell's inequalities and Aspect's experiments seem to favour

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[27] Bohr (1935).

Bohr: ironically enough, the hidden variable programme, which was prompted by Einstein's desire to retain a classical picture of physical reality, can retrieve the quantum mechanical predictions only at the cost of giving up the important locality condition that EPR weaved originally against Bohr to claim that the theory was incomplete.

Mathematical formalism and results of measurement are all what we have: the former display the mathematically allowed structural relations among the physical quantities of unobservable entities; the latter tell us something about the values of these quantities. Jointly, they give us the conditions of assertibility of sentences about physical entities. Or better, they jointly provide us with the (reasonably) good epistemic conditions under which we are justified in making certain assertions about unobservable entities. As such, from a neo-Kantian perspective, they are the truth-makers of these sentences.

A crucial question arises at this point. For the neo-Kantian perspective I have sketched above to be entertainable, we must show that the epistemic conditions displayed by mathematical structures plus results of measurement are not a too large meshed net to capture truth. In other words, we want to make sure that the very same epistemic conditions do not license falsehood as well as truth, e.g. that they do not equally justify us to make assertions about the ether as well as about the electromagnetic field, for instance. One may object to the account sketched above that if the mathematical structure of Fresnel's theory does not differ much from the mathematical structure of Maxwell's theory, and if this mathematical structure has to fix the conditions of assertibility and hence the truth-conditions of sentences—as I am suggesting—, we are left with the problem of explaining why *under very similar epistemic conditions* assertions about the ether come out false whereas assertions about the electromagnetic field come out right. Is there any way of distinguishing between truth and falsehood *from a neo-Kantian internalist perspective*, i.e. without falling back once again on the notion of reference and saying that sentences about the ether are false simply because there is no such a thing as ether? This is an important challenge for a neo-Kantian internalist account. I shall foreshadow a possible answer to it by revisiting the much celebrated Fresnel–Maxwell episode.

5. *How mathematical structures cash out truth: revisiting the Fresnel–Maxwell case*

As highlighted in Section 2, there are philosophical traditions within the structural realism programme that do not primarily aim at preserving reference or referential continuity. Poincaré's conventionalism and Cassirer's neo-Kantianism are two different examples of how one may play down reference, and nevertheless have an answer to the threat posed by pessimistic meta-induction. Poincaré's solution—as I have suggested—relies on the conventional character of scientific principles, on their being unassailable by experiments, which warrants their certainty and (almost) permanence across scientific theories. Cassirer's solution, on the other hand, hinges on a particular architectonic of scientific knowledge, where scientific principles play a crucial role as providing systematization and unification on the empirical basis given by results of measurement. On Poincaré's view, Fresnel's theory is as good as Maxwell's insofar as both express the same "true relations" encoded by the same (conventional) principles. Either goes, once we give up any realist construal of their respective languages. But this is not similarly the case from a neo-Kantian point of view: we want to retain a notion of truth (albeit an internal one) and show that Fresnel was less justified in making certain assertions about the ether than Maxwell was in making assertions about the electromagnetic field, despite similarities in their mathematical equations and despite the fact that both resorted to ether models in some way. Can we make sense of this distinction from a neo-Kantian internalist perspective?

I think we can if we start looking more closely at what Fresnel could *justifiably assert* about optical phenomena. There is a kernel of truth in Fresnel's theory that remains after Maxwell: Fresnel *was justified in asserting* certain things about optical phenomena, for instance about refraction and diffraction, but not about polarization. And he was justified in asserting them precisely because his equations provided the long-sought mathematics for diffraction (confirmed by the unexpected result of Poisson's experiment in 1818) as well as yielding Snell's law of refraction and Huygens's law of double refraction. On the other hand, Fresnel was not justified in his claims about polarization because for that he did not have any mathematical tool (such as the differential equations of motion later introduced by Cauchy) and had to rely instead on a questionable molecular hypothesis about the ether.

The polarization of light (discovered by Etienne Louis Malus in 1808) implied asymmetric properties that could easily be accounted for in a corpuscular theory of light (because corpuscles do have a shape and hence a directionality), but not in Fresnel's theory as in any other wave theory of light (because waves are perfectly symmetrical about their axes). Fresnel had realised already in 1817 that if an unpolarised ray consisted of two vibratory components, one along the ray (longitudinal) and one at right angle to it (transverse), polarization could be explained if the longitudinal components were destroyed; but the main stumbling-block was to understand how this process of selective destruction could happen mechanically. The solution to this problem that Fresnel found in 1821 hinged on a particular hypothesis about the physical nature of the ether. Fresnel postulated that the ether consisted of molecules in the Laplacian sense between which forces acted. By assuming that two parallel lines of molecules can be readily separated laterally but strongly resist mutual approach, he could uncouple transverse and longitudinal vibrations, and since the former would travel much more slowly than the latter, the problem of selective destruction of longitudinal waves was avoided. Thus Fresnel's theory had to rely on a particular hypothesis about the molecular nature of the luminiferous ether in order to explain polarization.[28] Fresnel finally deduced the wave surface of a biaxial crystal from the properties of the ether, but the resultant ether model and ether dynamics was not easy to construct.

It was Augustin Louis Cauchy who in 1830 built up on Fresnel's programme of ether dynamics and realised that the propagation of the transverse vibrations of light could be obtained from the differential equations of motion of an elastic solid. Not only did he correct Fresnel's erroneous deduction of the wave surface of a biaxial crystal, but he introduced a new mathematical tool in wave optics, namely differential equations. By 1835 he developed a unified theory of double refraction and dispersion in which both phenomena were explained by assigning specific values to the constant coefficients of the differential equations.

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[28] For details, see Buchwald (1981).

But in 1839 James MacCullagh demonstrated that optical rotation[29] was incompatible with the molecular equations of motion. MacCullagh proposed then a new type of elastic solid whose potential energy depended only on the rotation of its elements, and in which transverse waves alone were transmitted (with a speed of propagation that depended on the density of the medium). These equations were very similar in form to those that Maxwell proposed later but they were not taken too seriously at the time because there was no mechanical model available for such an unusual medium (incompressible and resisting only rotations of its elements). On the other hand, Cauchy tried to accommodate the problem of optical rotation by introducing periodic (instead of constant) coefficients in the differential equations. This was a difficult task and the new mathematics required to solve it was extremely complicated and underdeveloped; the failure to explain optical rotation pointed at a deeper difficulty with ether dynamics.

In the meantime a major breakthrough occurred in the history of electricity and magnetism. Following up on the previous experimental researches of Oersted, Faraday, and Thomson, in 1865 Maxwell wrote *A dynamical theory of the electromagnetic field*: by contrast with an action-at-a-distance theory of the electric action (where forces operate between electrified bodies across finite distances of space), he argued that forces are mediated by the contiguous elements of an electromagnetic field existing in the space between separated electrified bodies. The propagation of force between contiguous infinitesimal elements of the electromagnetic field was mathematically expressed by partial differential equations. But already in this work, Maxwell presented only the mathematical equations describing the electromagnetic field and did not discuss anymore vortices and idle wheels as in his previous model of the electromagnetic ether:[30] the equations have proven to be correct and survived, while the mechanical models of the ether were all finally abandoned.

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[29] When a beam of linearly polarised light passes through a crystal of quartz in a certain direction it splits into two beams, one left circularly polarised, and the other right circularly polarised. A single resultant beam emerges, and it is again linearly polarised, but its plane of polarisation has been rotated.

[30] In 1861 Maxwell wrote *On physical lines of force*, where the magnetic field was represented as a fluid filled with rotating vortex tubes, whose geometrical arrangement corresponded to the lines of force, and the angular velocities of the vortices corresponded to the intensity of the field. The model was based on an analogy between a rotating vortex tube and a tube of magnetic flux. At the time it was common to assume the existence of an electromagnetic ether, as a medium responsible for electric and magnetic phenomena and distinct from the luminiferous ether allegedly responsible for optical phenomena.

In 1873 with the *Treatise on electricity and magnetism* Maxwell found that transverse elastic waves were transmitted with the same velocity as light waves: or better, light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena. Indeed, given Coulomb's law for the electric field **E** produced by a static point charge q

$$\mathbf{E} = k_1 \frac{q}{r^2} = \frac{1}{4\pi\varepsilon_0} \frac{q}{r^2}$$

and given the Biot–Savart law for the magnetic field **B** produced by a wire of directed length l carrying a current i

$$\mathbf{B} = k_2 \frac{il \times \mathbf{r}}{r^3} = \frac{\mu_0}{4\pi} \frac{il \times \mathbf{r}}{r^3}$$

where the two constant k_1 and k_2 were determined independently by experiment from various phenomena of electrostatics and magnetostatics, the ratio

$$\frac{k_1}{k_2} = \frac{1}{\mu_0 \varepsilon_0} = c^2$$

turned out to be equal to the velocity of light c squared, where the value of c had already been measured prior to Maxwell's work.[31] Since the value of c had already been measured, and so was also the value of $1/\mu_0\varepsilon_0$—independently measured from constants k_1 and k_2—their numerical agreement was the decisive proof that light was an electromagnetic wave.

This led to the serendipitous identification of electromagnetic and luminiferous media, and hence to the unification of optics and electromagnetism, subsequently confirmed by Hertz's experiments in 1887-8. Hertz showed that electromagnetic radiation had all the characteristics of light: reflection, refraction, interference and polarization. The direct determination of the velocity of this radiation was however beyond the instrumentation available to Hertz: experiments

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[31] In 1862 Foucault established an estimate of the speed of light of 298.000 km/sec which was 4% below the value 310.000km /sec. Maxwell himself tried to improve this estimate, and in 1868 he found a value of 288.000 km/sec. Soon afterwards M'Kichan found a better estimate of 293.000km/sec.

run after 1895 confirmed that the speed of electromagnetic waves was equal to the speed of light in free space. Hertz's experiments demonstrated then conclusively the validity of Maxwell equations.

Going then back to our original question, if we take mathematical structures and results of measurement as jointly fixing the conditions under which we are justified in making assertions about unobservable entities, and hence as the truth-makers of these sentences, we can start to appreciate the difference between Fresnel and Maxwell. More precisely, if we take Cassirer's architectonic of scientific knowledge as some sort of test concerning the conditions of justified assertibility of sentences, we can now see that Fresnel's claims about the ether do not pass the test, whereas Maxwell's claims about the electromagnetic field do pass the test.

On the one side, we have the insurmountable difficulties with Fresnel's wave theory and Cauchy's later work of ether dynamics. In order to explain polarization, some experimentally unwarranted hypotheses were introduced about the molecular nature of the ether. Nor did Cauchy's efforts to improve on Fresnel by introducing differential equations solve all problems: the problem of optical rotation remained unsolved and pointed at a deeper difficulty with ether dynamics.

On the other side, we have streams of different research traditions in electrostatics and magnetostatics that from Coulomb's law and Biot–Savart law, via the works of Faraday on magnetic induction (among others), arrives at Maxwell's great synthesis. Interestingly enough, this synthesis is the product of the predicted and experimentally confirmed agreement between the ratio of two constants (entering in Coulomb's and Biot-Savart's law, respectively) and the squared value of the velocity of light, independently measured as early as 1862. The experimental values of these three quantities k_1, k_2, c—independently found from a variety of electric, magnetic and optical phenomena—are the "results of measurement" that via Coulomb's and Biot-Savart laws lead to Maxwell's synthesis. In turn, Maxwell's synthesis predicted that electromagnetic waves should have all the observable characteristics of light (reflection, refraction, interference and polarization) as was later confirmed by Hertz's experiments. Maxwell's equations provide the long-sought synopsis of a wide-ranging array of electromagnetic and optical phenomena. Many already known phenomenological laws (from Faraday's law of induction to Ampère's law) could be deduced from them.

It is this serendipitous architectonic of results of measurement, laws and principles that from a neo-Kantian point of view justifies Maxwell's claims about the electromagnetic field. On the other hand, it is precisely the lack of a similar architectonic that explains why Fresnel was not similarly justified in his claims about the ether. Despite similarities between Fresnel's equations and Maxwell's, there is a crucial difference that justifies the latter but not the former: Maxwell's claims were built into (indeed, they were one of the highest expressions of) a system of scientific knowledge, which has an empirical basis constituted by experimental results, and mathematical structures at the higher level providing the necessary synopsis to this empirical basis. Experimental results and mathematical structures are all what we have. Only within their boundaries can we try to make reasonable guesses about what there is or there is not.

6. Conclusion

Where does all this discussion leave us? We saw that the original motivation behind structural realism was the attempt to reconcile two conflicting arguments: the realist's 'no miracle' argument, and the antirealist's 'pessimistic meta-induction'. Given the neo-Kantian perspective I have been urging, new light can be cast on these two arguments.

As we saw, the core of the 'no miracle' argument consists in showing that there is a crucial two-way relationship between reference and success: reference explains success, and success in turn warrants a presumption of reference. However, the main problem that the received view of structural realism faces concerns precisely reference. The Newman problem is a problem about reference. Hence the shift I have urged from structural realism intended as a form of semantic realism to structural realism as a form of epistemic realism, where the structural relations displayed by our mathematical formalism should not be understood as 'what remains fairly stable across theory-choice' and hence as warranting referential continuity across scientific revolutions, but rather as what fixes (together with experimental evidence) the conditions of justified assertibility, and hence the truth-conditions of sentences about unobservable entities.

Accordingly, the ‘no miracle’ argument needs to be reconsidered. On the received structural realist view, the argument runs as follows: there are some objective (mind-independent) structural features of the external world which are somehow isomorphic to the mathematical structures of our scientific theories, and this explains the empirical success of science. However, the Newman problem stands against this structuralist version of the no miracle argument. Since structure does not pick out a unique relation on a given domain, and in fact there may well be more than one relation on the same domain compatible with the same structure, the success of our scientific theories does not warrant any presumption of reference and on the contrary it risks being once again a miracle or a lucky coincidence.

I want to suggest a sort of post-Darwinian solution to the no miracle argument, echoing van Fraassen’s so-called ‘Darwinian’ solution to it. From a neo-Kantian perspective, we can do justice to the realist’s intuition behind the no miracle argument, albeit in quite different terms, namely without entrusting structure with any referential role. My ‘post-Darwinian’ account describes the survival of currently accepted theories in terms of a process of mutual adaptation between the mathematical structures of the theory on the one side, and the experimental evidence available on the other side. Adaptation is a two-way street: we fit our mathematical structures to the available experimental data, but we also modify and extend the experimental data to reach an increasingly better fit with the mathematical structure. It is the mutual fit of these two elements that provides the ‘environment’—so to speak—where scientific entities evolve and come to be selected, where note that they do not simply adapt to this (mathematical and experimental) ‘environment’ but they actively contribute to its modification and evolution by feeding it constantly with new pieces of experimental evidence. Taking inspiration from evolutionary biology, we can regard the relationship between our scientific theories and unobservable physical entities as analogous to the relationship between niches and creatures.[32] As creatures and niches evolve together and together come to be selected by developing suitable symbiotic strategies, similarly we can regard unobservable physical entities (e.g. protons, quarks, muons,….) as evolving together

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[32] I owe this metaphor to Thomas Kuhn (1991), who in his later years repeatedly used it to describe the role of a scientific lexicon to shape our scientific categories. Although I do not agree with Kuhn on scientific lexicons and incommensurability (see Massimi, 2005, chapter 3), I want to use this metaphor to describe a quite different relationship, namely that between our scientific theories and scientific entities.

and being selected together with certain specific mathematical structures plus experimental evidence that jointly provide the ideal ‘environment’ for the survival of those entities.

Thus, my ‘post-Darwinian’ account retains unobservable entities but regards them in a ‘dynamic way’: unobservable scientific entities are not mind-independent objects, given once and for all, that our scientific theories can at most try to represent, as an externalist (God’s eye) viewpoint would suggest. Rather, unobservable scientific entities evolve with time and with the evolution of our scientific knowledge. In the end, what electrons, quarks, muons are, is a question that can only make sense given a certain mathematical formalism and some available experimental evidence. Scientific entities are not prior to scientific theories. But they arise instead out of our scientific theories, or more precisely they evolve symbiotically with our scientific theories.

From this point of view the success of science is not miraculous, and it is not surprising either. The mathematical structures of our scientific theories allow us to make various types of predictions. For instance, from permutation invariance we can predict the existence of both Pauli-obeying quarks and of Pauli-violating paraparticles. It is experimental evidence, namely results of measurement that in the end have given the verdict to Pauli-obeying quarks rather than to Pauli-violating paraparticles. We now believe that there are coloured quarks, but not paraparticles, because we are justified in making some assertions about the former, but not about the latter. Mathematical structures disclose the spectrum of possible predictions we can make: some of them will turn out true, some others will turn out false. In the end, the verdict is given by the available experimental evidence: echoing Cassirer, results of measurement are the alpha and omega of our system of knowledge. This solution is not going to appeal scientific realists: from an internalist, neo-Kantian perspective, the no miracle argument looses some of its realist strength. But, on the other hand, if we cannot live up to the promise of the no miracle argument (given the aforementioned problem about reference), perhaps it is wise to reformulate the argument in a way that does not take any longer reference for granted.

The main advantage of this strategy is that it becomes easier to reconcile the no miracle argument with pessimistic meta-induction. Playing down with reference can help us mitigate the tension between the two arguments. More precisely, pessimistic meta-induction need no longer be as frightening as it has traditionally appeared. It may well turn out in the future that there are not really such things as coloured quarks, electrons, protons as there were not such things as caloric, ether, and phlogiston. But from an internalist perspective, this has not the devastating consequences that it has for an externalist perspective. From an internalist perspective, we may simply say that as we have discarded caloric, ether and phlogiston because they turned out to be obsolete and no longer functional with respect to the available theoretical knowledge and empirical evidence, similarly we may one day discard electrons, coloured quarks, and muons on similar grounds, i.e. because 'unfit' to the 'environment' displayed by our current mathematical structures and experimental evidence. And this is as it is to be expected on an empirical and revisable view of science, according to which our currently accepted scientific entities are those that have evolved together with our scientific theories, but nothing guarantees us that it will continue to be so.

To conclude, a neo-Kantian perspective has the advantage of demystifying some both realist and antirealist assumptions behind structural realism. It can explain the success of science without resorting to the God's eye point of view about reference. It can shed light on the reason why scientific entities get discarded across theory-change without dispensing with scientific entities altogether. It does not make the success of science a miracle, but it does not take it for granted either. It can do justice to scientific revolutions without leading us to conceptual relativism. A science within the boundaries of mathematical structures and empirical evidence is all what we have and can be realist about: there is for us no other reality to be investigated and sought after.

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