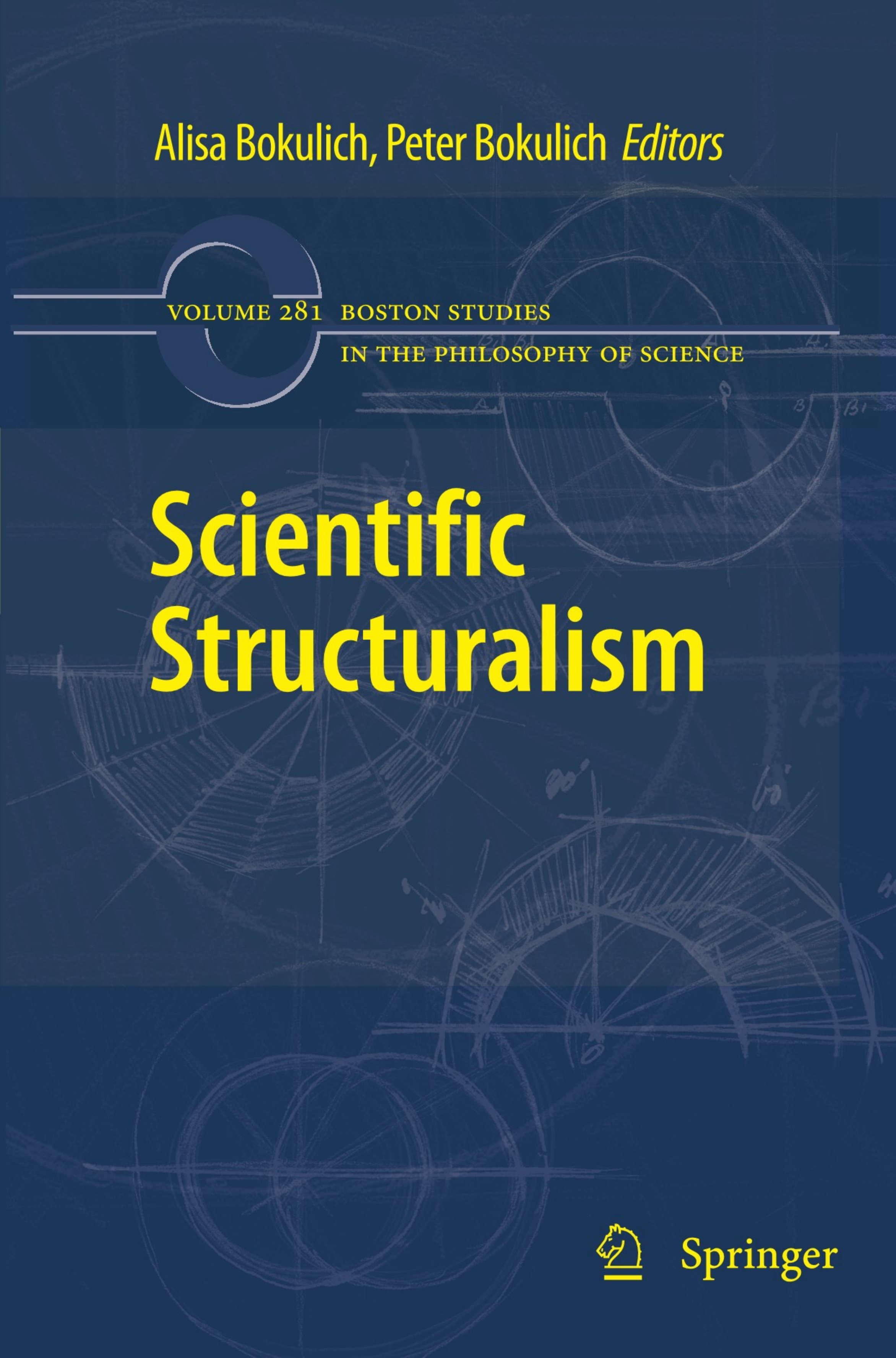

Alisa Bokulich, Peter Bokulich *Editors*

VOLUME 281 BOSTON STUDIES
IN THE PHILOSOPHY OF SCIENCE

Scientific Structuralism

Springer

Scientific Structuralism

BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE

VOLUME 281

For further volumes:
http://www.springer.com/series/5710

Peter Bokulich • Alisa Bokulich
Editors

Scientific Structuralism

Springer

Editors
Peter Bokulich
Boston University
Boston, MA
USA
pbokulic@bu.edu

Alisa Bokulich
Boston University
Boston, MA
USA
abokulic@bu.edu

ISBN 978-90-481-9596-1 e-ISBN 978-90-481-9597-8
DOI 10.1007/978-90-481-9597-8
Springer Dordrecht Heidelberg London New York

Library of Congress Control Number: PCN applied for

Printed on acid-free paper

Springer is part of Springer Science+Business Media (www.springer.com)

For Julian and Kai

Contents

Contributors

Katherine Brading
Department of Philosophy, University of Notre Dame, Notre Dame, USA,
kbrading@nd.edu

Otávio Bueno
Department of Philosophy, University of Miami, Coral Gables, FL 33124, USA,
otaviobueno@mac.com

Mauro Dorato
Dipartimento di Filosofia, Università degli Studi Roma Tre, Rome, Italy,
dorato@uniroma3.it

Michael Esfeld
University of Lausanne, Department of Philosophy,
Michael-Andreas.Esfeld@unil.ch

Laura Felline
Dipartimento di Scienze Pedagogiche e Filosofiche,
Università degli Studi di Cagliari, Cagliari, Italy

Steven French
Department of Philosophy, University of Leeds, Leeds, LS2 9JT, UK,
s.r.d.french@leeds.ac.uk

James Ladyman
Department of Philosophy, University of Bristol, Bristol, Avon BS8 1TH, UK

Michela Massimi
Department of Science and Technology Studies, University College London,
London WC1E 6BT, UK,
m.massimi@ud.ec.uk

Christopher Pincock
Department of Philosophy, Purdue University, West Lafayette, IN 47907, USA,
chrispincock@gmail.com

Martin Thomson-Jones
Department of Philosophy, Oberlin College, Oberlin, OH 44074

Ioannis Votsis
Philosophisches Institut, Heinrich-Heine-Universität Düsseldorf,
Universitütstrasse 1, Duesseldorf 40225, Germany
votsis@phil-fak.uni-duesseldorf.de

Vincent Lam
University of Queensland, School of mathematics and Physics and School of
History Philosophy, Religion and Classics,
v.lam@uq.edu.au

Introduction to *Scientific Structuralism*

Scientific structuralism is a label used to describe a broad family of approaches that emphasize the structural features of scientific theories as a way of addressing particular epistemological and ontological problems in the philosophy of science. Articulating what precisely these structural features are is one of the central projects of scientific structuralism, and different authors have defined this central notion in different ways. Intuitively, the structural features of a scientific theory are to be contrasted with its ontology, where structure is understood broadly as the relations between elements. In the mathematical sciences, structure is often described as the relations that are captured in the theory's equations. So, for example, rather than being committed to the existence of electrons (the ontology of the theory), the structural realist is committed to the reality of the relations between electronic phenomena that are described by Maxwell's equations, and arguably those relations are preserved in the move from classical electrodynamics to quantum electrodynamics. While some structuralists have borrowed the mathematical notion of structure from set theory, other structuralists have tried to define a more robust metaphysical notion of structure (for a defense of non-formal notions of structure see Chapter 7 by Thomson-Jones, and for a critique of realist approaches to defining structure see Chapter 5 by Bueno).

There are several historical antecedents to contemporary scientific structuralism. For example, Henri Poincaré (1854–1912) writes in his book *Science and Hypothesis*,

> Now, we daily see what science is doing for us. This could not be unless it taught us something about reality; the aim of science is not things themselves, as the dogmatists in their simplicity imagine, but the relations between things; outside those relations there is no reality knowable. (Poincaré [1905] 1952, xxiv)

We see Poincaré here emphasizing that what science gets right about the world is not its detailed ontology, but rather its structural features. Moreover, he argues that this is the best that science can do—our knowledge is limited to these structural or relational features.

Ernst Cassirer (1874–1945) has similarly argued for a reorientation of the goals of science, though he approaches his structuralism through a neo-Kantian, rather than a conventionalist perspective. Cassirer notes that there has been a trend in fields like physics towards an increasingly abstract and mathematical characterization of objects and substance. He notes,

> This progressive transformation must appear unintelligible, if we place the goal of natural science in gaining the most perfect possible *copy* of outer reality. It is only owing to the fact that science abandons the attempt to give a direct, sensuous copy of reality, that science is able to represent this reality as a necessary connection of grounds and consequents. . . . Instead of imagining behind the world of perceptions a new existence built up out of the materials of sensation, it traces the universal intellectual schemata, in which the relations and connections of perceptions can be perfectly represented. Atom and ether, mass and force are nothing but examples of such schemata, and fulfill their purpose so much the better, the less they contain of direct perceptual content. (Cassirer [1923] 1953, pp. 164–165)

In other words, the goal of science is simply to represent these "relations and connections," rather than reality as we experience it. Indeed Cassirer seems to go a step further in encouraging us to abandon the view of a reality behind our experience, and instead to reconceptualize objects such as atoms in relational terms as "schemata."

Yet a third important influence on contemporary scientific structuralism is Bertrand Russell (1872–1970), who arguably had the most fully developed account of structural realism of these early authors. In his book *The Problems of Philosophy*, Russell writes,

> Thus we find that, although the *relations* of physical objects have all sorts of knowable properties, derived from their correspondence with the relations of sense-data, the physical objects themselves remain unknown in their intrinsic nature, so far at least as can be discovered by means of the senses. (Russell [1912] 1959, p. 34)

Again in his later work *The Analysis of Matter* he emphasizes,

> 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. (Russell 1927, p. 254)

Like other scientific structuralists, Russell argues that our knowledge is limited to the structural features of the world, and that physical objects themselves remain unknowable. (The historical influence of these figures on the contemporary scientific structuralism literature is further explored in Chapter 1 by Massimi and Chapter 2 by French and Ladyman).

Much of the recent revival of interest in scientific structuralism can be attributed to John Worrall's seminal essay, "Structural Realism: The Best of Both Worlds," which breathed new life into an otherwise languishing realism debate at the end of the 1980s. On the one hand Worrall argues that structural realism can account for the realist's "no miracles" argument, according to which the remarkable successes of science would be an unexplained "miracle" if our current theories were not at least basically true. On the other hand structural realism can also make sense of the antirealist's so-called "pessimistic meta-induction", which points out that almost all of science's past ontological claims have turned out to be false (e.g., the ether, phlogiston, caloric, crystalline spheres, humors, etc.), so there is good inductive reason to believe that the ontologies of our current scientific theories will also turn out to be false someday. Insofar as structural realism is only committed to the structural content of our best current scientific theories—not those theories's ontologies—it seems to escape the brunt of this objection.

Most of the work in scientific structuralism to date has been focused on the issue of scientific realism. Far from being a unitary position, however, there have emerged several different competing versions of structural realism. One of the most marked divides is between what James Ladyman (1998) has called "epistemic structural realism" (ESR) and "ontic structural realism" (OSR). Very broadly, while the epistemic structural realist argues that all we can know about the world are its structural relations (the objects that support these relations are forever "hidden"), the ontological structural realist is an eliminativist about objects, arguing that all that there is to the world, at bottom, is structure (for a defense of OSR against a wide range of objections see Chapter 2 by French and Ladyman). Each of these views can themselves be further divided according to various interpretive options (see Chapter 6 by Votsis for a brief review).

In response to structural realism, an antirealist position, known as structural empiricism, has emerged. While the structural empiricists also emphasize the structural features of scientific theories and even recognize a considerable continuity of structure across revolutionary theory change, they deny that this structure should be interpreted realistically, as revealing the structure of the world. Instead, according to structural empiricism, what science succeeds in knowing is merely the structure of appearances, which answer only to the condition of empirical adequacy. Bas van Fraassen summarizes the structural empiricist position as follows:

> Science represents the empirical phenomena solely as embeddable in certain abstract structures . . . [which] are describable only up to a structural isomorphism. There is warrant for the assertion of an accumulation of empirical knowledge through theory change precisely if it can be demonstrated for phenomena counted among the empirical successes of earlier science that, if they are embeddable in the new models then they are 'approximately' embeddable in the old models. (van Fraassen 2006, p. 305)

This position of structural empiricism has been further elaborated and defended by Otávio Bueno (see Chapter 5 of this volume).

Although most of the work on scientific structuralism has been focused on the realism-antirealism debate, an increasing number of authors are adopting structuralist approaches to other key issues in the philosophy of science, such as intertheory relations (see, for example, Bokulich 2008) and scientific explanation (see, for example, Hughes 1989 and Dorato and Felline's contribution in Chapter 9 of this volume).

The nine articles collected in this volume, written by the leading scholars in scientific structuralism, represent some of the most important directions of research in this field. In the first chapter, "Structural Realism: A Neo-Kantian Perspective," Michela Massimi begins by tracing the historical influence of Poincaré, Cassirer, and Russell on contemporary structural realism, arguing that the current debate between the ontic and epistemic forms of structural realism can be traced back to the heterogeneity of these historical sources. She argues that the correct lesson to take from Maxwell Newman's critique of Russell's structuralism (known as the "Newman problem") is that structural realism should not be understood as a form of semantic realism; that is, it should not be thought of as a way of addressing referential discontinuity across theory change. Through the development of a neo-Kantian approach to structural realism, she argues that the proper function of mathematical structure is instead to fix the epistemic conditions under which one can make justified assertions about unobservable entities. She summarizes this new view by the slogan that what the structural relations expressed in the theory's mathematical formalism cash out is truth—not reference.

In the second chapter of the volume, Steven French and James Ladyman cogently defend the ontic version of structural realism (OSR) against a wide range of objections. OSR, recall, advocates a reconceptualization of physical objects in structuralist terms; that is, it rejects the existence of objects in the traditional sense. French and Ladyman draw their inspiration from Cassirer not only in adopting the ontic form of structuralism but also in following Cassirer's tying of structural realism to contemporary developments in theoretical physics. In particular, French and Ladyman argue that the permutation invariance of elementary particles in quantum mechanics and the diffeomorphism invariance in general relativity are sympathetic with OSR in undermining the haeccitism that goes along with the traditional notion of objects. After showing how these current developments in physics lend support to the OSR position, they turn to a defense of structural realism against the following three key objections. First, the history of science shows that there can—and have been—structural losses in theory change; mathematical structure is not always preserved simpliciter in the move from a predecessor theory to its successor. Second, critics have argued that OSR is incapable of making sense of 'first order' intrinsic properties, such as mass and charge, insofar as they eliminate the object in which these intrinsic properties are to inhere. And third, a number of authors have objected that OSR is unable to accommodate causality. French and Ladyman argue that there are a number of interpretative options open to the defender of OSR to respond to these three objections.

In Chapter 3, Katherine Brading challenges French and Ladyman's view that contemporary physics supports OSR's elimination of objects. She draws an important distinction between the issue of objecthood on the one hand and whether those objects are metaphysical individuals on the other. The developments in contemporary physics that French and Ladyman point to merely restrict the available notions of objecthood—they do not force its elimination in favor of pure structure. Brading notes that there are nonetheless two additional obstacles that need to be overcome in order to develop a structural realist account of the objects of physics, and the remainder of her paper is devoted to outlining what these obstacles are, and how they might be overcome within the framework of the semantic conception of theories. The first obstacle concerns what Brading calls the problem of the "proliferation of models." Recent work in modeling has shown that a variety of different—incompatible—models can mediate between a high-level theory (the theoretical models) and the data models. While the traditional scientific realist can countenance this proliferation of models by appealing directly to the theory's ontology, this sort of model-independent characterization of objects is not available to the structural realist. The structural realist, by contrast, is a realist about the structure of some theoretical model, claiming that it mirrors the structure of the world. Hence, a proliferation of models means that there is no longer a unique structure linking all these models in the hierarchy together that the structural realist can appeal to as being *the* structure of the world. The second remaining obstacle that Brading considers concerns modality. It has been argued that modal realism is essential to the kinds of explanations that realists seek to offer; in other words, what separates the scientific or structural realist from the scientific or structural empiricist is whether or not they represent the world as modal. Brading argues that the traditional structural realist view, that takes representation to be a relationship of shared structure between a *particular* model and a physical system, is unable to accommodate modal facts. Brading argues that a possible—even if unpalatable—way of overcoming this obstacle is to take modality to be a feature of a *collection* of models rather than a feature of any individual model.

In Chapter 4, Christopher Pincock turns to a consideration of what role mathematics plays in the articulation and defense of epistemic structural realism (ESR). Specifically, Pincock argues that while an appeal to mathematics can succeed in making the structural realists' notion of 'structure' more precise, it ultimately leads to problems with their realism thesis. More specifically, Pincock raises the concern that the use of mathematics to derive successful empirical predictions in no way guarantees that the mathematical structure matches the structure of the world. If the mathematical structure involves either more or less structure than the structure of the phenomenon in question, then there is no assurance that this structure will be preserved through theory change. He argues that even a partial preservation of structure, expressed for example by the equations of a successor theory being a limiting case of the equations of the predecessor theory, is insufficient for a defense of the realist claim. Pincock concludes by noting that this is a problem not only for the structural realist, but for any traditional form of realism that attempts to infer existence from those mathematical elements of the theory that are required for a successful prediction.

Building on many of the objections articulated in the earlier chapters, Otávio Bueno argues in Chapter 5 that we should reject structural realism and instead adopt the antirealist position of structural empiricism. Bueno focuses his critique of structural realism on the following four problems: first, the difficulties of defining a metaphysically robust notion of structure needed to support the realist claims; second, a challenge to French and Ladyman's approach to incorporating quantum mechanics into OSR; third, the existence of structural losses in revolutionary theory change; and finally, a retooling of Putnam's paradox that raises a problem of non-uniqueness of structure for the structural realist. While these four problems undermine a realist approach to scientific structuralism, they pose no difficulty for an empiricist approach. Bueno concludes by showing how the structural empiricist approach is also able to account for the "success of science" and is able to address some of the worries (raised by Pincock in the previous chapter) regarding how one can use mathematical theories without being to committed to the existence of those mathematical objects.

In Chapter 6, Ioannis Votsis takes up the challenge posed by Bueno and others, that the existence of structural losses in the history of science undermines structural realism. Votsis begins by noting that the continuity thesis component of structural realism is in need of a more precise formulation. He suggests two important modifications to the continuity claim: first, the structural realist is only committed to those structures that play an active role in the predictive and explanatory successes of the theory (what he calls "operative structures"); and second, those structures that are preserved need not be preserved intact—they may only be preserved as limiting cases. After appropriately restricting the continuity claim in these ways, Votsis turns to examine concrete examples of purported structural losses in the history of science. He concludes that while the preservation of structure is neither a necessary nor sufficient condition for (approximate) truth, it is nonetheless a reliable guide.

In Chapter 7, Martin Thomson-Jones explores a number of theses regarding scientific representation and what implications various notions of representation have for different versions of structural realism. Thomson-Jones begins by distinguishing several different notions of structure—three informal and two formal—and arguing that the informal notions of structure are preferable. He then goes on to distinguish two views regarding scientific representation: what he calls "vehicle structuralism" (all scientific representation is representation *by means* of structure) and "content structuralism (all scientific representation *is* representation of structure). He argues that the epistemic structural realists' evasion of the "pessimistic induction" relies on the idea that theories tell us *both* about the structure of the world and about the nature of objects in it. This move is incompatible with content structuralism insofar as content structuralism asserts that scientific theories *only* represent structure. Likewise he argues that a defender of epistemic structural realism (ESR) also has good reasons for rejecting vehicle structuralism; hence, structural realism and structuralism about scientific representation are in tension with one another. Moreover, Thomson-Jones concludes that insofar as the semantic view of theories tends to support vehicle structuralism, there is good reason for the epistemic structural realist to reject the semantic view of theories as well.

In Chapter 8, Michael Esfeld and Vincent Lam argue that there is no ontological distinction between objects and properties, but rather that this distinction is merely conceptual. From this insight, they build a new "moderate" form of ontic structural realism (OSR), which admits the existence of physical objects instantiating structural relations, while denying that those objects have any intrinsic properties (including identity) over and above the relations in which they stand. (This is to be contrasted with more radical forms of OSR, such as Ladyman's, which deny the existence of objects altogether.) Harkening back to what they see as a Spinozian metaphysics, they view properties, including relations, as modes or ways in which objects exist. This new version of moderate OSR marks a break from Esfeld and Lam's earlier (2008) account, and arguably allows them to avoid some of the difficulties that plagued their previous view. They conclude by indicating how this new form of moderate OSR can take objective modality into account.

In the final contribution to this volume, Mauro Dorato and Laura Felline explore the connections between structural realism and scientific explanation. In particular, they defend a structural account of scientific explanation that has largely been overlooked in the literature. They argue that structural explanation—rather than deductive-nomological or causal explanation—best characterizes the sort of explanation one typically finds in quantum mechanics. They illustrate this form of explanation with two case examples: the explanation of Heisenberg's uncertainty relations and the explanation of quantum non-locality. Although they defend the structuralist claim that structural properties are explanatorily more important than intrinsic properties, they are dubious that this can, by itself, lead one to endorse structural realism. They conclude that the explanatory primacy of structural relations entails neither that these structural relations are all we can know (*pace* ESR) nor that they are all that exist (*pace* OSR).

References

Bokulich, A. (2008), *Reexamining the Quantum-Classical Relation: Beyond Reductionism and Pluralism*. Cambridge: Cambridge University Press.

Cassirer, E. ([1923] 1953), *Substance and Function and Einstein's Theory of Relativity*. New York: Dover Publishing.

Esfeld, M. and V. Lam (2008), "Moderate Structural Realism about Space-Time", *Synthese* 160: 27–46.

Hughes, R.I.G. (1989), "Bell's Theorem, ideology, and structural explanation", *Philosophical Consequences of Quantum Theory: Reflections on Bell's Theorem*. Notre Dame, IN: University of Notre Dame Press.

Ladyman, J. (1998), "What is structural realism?", *Studies in History and Philosophy of Science* 29: 409–424.

Poincaré, H. ([1905] 1952), *Science and Hypothesis*. New York: Dover Publications.

Russell, B. ([1912] 1959), *The Problems of Philosophy*. New York: Oxford University Press.

Russell, B. (1927), *The Analysis of Matter*. New York: Harcourt, Brace & Co.

van Fraassen, B. (2006), "Structure: Its shadow and substance", *British Journal for the Philosophy of Science* 57: 275–307.

Worrall, J. (1989), "Structural realism: the best of both worlds?", *Dialectica* 43: 99–124.

Chapter 1
Structural Realism: A Neo-Kantian Perspective

Michela Massimi

1.1 Structural Realism: The Status Quo

Structural realism was born in the attempt to reach a compromise between a realist argument and an antirealist 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 the time 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 argument clashes nonetheless with a compelling antirealist 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.[1] 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, and 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.

---

[1] See Laudan (1981).

M. Massimi (✉)
Department of Science and Technology Studies, University College London,
London WC1E 6BT, UK
m.massimi@ud.ec.uk

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_1, © Springer Science+Business Media B.V. 2011

Moreover, the 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 is the case with ether and phlogiston, among others), how is it possible to retain inter-theoretic links? Warranting referential continuity across theory-change is all the more relevant to a defence of scientific realism, and this is precisely what the 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 Henri Poincaré, although much of the following discussion has actually been influenced by Bertrand Russell more than Poincaré.[2] 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 ontological 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 used mathematical equations that were in fact formally similar to those later employed by Maxwell to describe the properties of the electromagnetic field. Thus, what entities carried over in the passage from Fresnel's to Maxwell's theory are not entities but mathematical structures.

Structural realism claims then to do justice to the realist '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, where, however, reference is no longer identified 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 objects, that is the *structure* of the objective world.[3]

---

[2] Worrall (1994). As a disclaimer: five years have elapsed since I first wrote this paper in 2005 for this edited volume, and several papers on the topic have been published in the meantime, which this paper could not possibly have addressed. See in particular Worrall (2007) and Worrall and Zahar (forthcoming).

[3] Ladyman (1998), p. 412.

However, precisely because of this vindication of traditional ontology, epistemological structural realism stands condemned together with scientific realism for 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."[4] 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 raises, 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?[5]

In this paper, it is not my intention to go over again this well-known debate on structural realism, but rather to see where it leaves us and attempt a philosophical diagnosis. In the following section I 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 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.

1.2 Structural Realism: An Overview of the Philosophical Sources

1.2.1 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.[6] 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. This much celebrated historical episode notoriously

---

[4] French and Ladyman (2003a), p. 37.

[5] See Cao (2003a), (2003b). For a response see French and Ladyman (2003b).

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

prompted Worrall's epistemological structural realism and the discussion that followed Worrall's seminal article. The ensuing 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 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 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,

> [T]he 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 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 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.

---

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

[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.

Thus, in the end, the continuity between Fresnel and Maxwell is not grounded in 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 (quasi-) 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 instead a conventional construal of the language of science: his structural realism undercut the 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.

1.2.2 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 entities, their properties and relations. From Cassirer's 'functional' viewpoint, on the other hand, entities no longer constitute 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 we only have epistemic access to scientific entities. In his later book *Determinism and*

---

[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 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).

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

Indeterminism in Modern Physics[12] Cassirer portrayed scientific knowledge as a three-layer architectonic consisting of (1) results of measurement, (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 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 upon 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 asking "what objects does the world consist of?" makes sense only within a scientific description of reality; and we can answer this question 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 been perhaps the most influential expression of this movement: Bertrand Russell's structuralism.

1.2.3 Russell's Structural Realism and the Legacy of Reference

Among the philosophical sources of structural realism, 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.

---

[12] For a more detailed 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.

[14] Ibid., p. 52.

[15] Russell (1927).

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 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 runs 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 the pessimistic meta-induction).

1.3 The Newman Problem as a Problem About Reference

Russell's structural realism faces a major problem that M. H. A. 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 another way, once the domain is fixed, there is no way of distinguishing a relation *R* from another relation *S* on the same domain 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

---

[16] Russell (1912), (1914).

[17] G.Maxwell (1970a), (1970b).

[18] Newman (1928).

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 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 Kant as 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 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 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

---

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

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

some inescapable problems. By contrast, there are other philosophical traditions, to which current debates seem to have paid only lip-service, and which 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 account of structural realism. The best I can do is to raise some questions and foreshadow possible answers. Much work needs 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.

1.4 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 despite 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 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 be understood as a form of *epistemic realism*: it helps us cash out truth, not reference. Namely, it helps us make sense of what it means for an assertion like 'the electron has momentum *p*' 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 Hilary Putnam has drawn new attention.[21] As is well-known, after his Kantian turn, Putnam identified truth with idealised rational acceptability:[22]

---

[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.

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 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 be 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 the 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

---

[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.

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

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.

From a neo-Kantian perspective, as the one I am sketching 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.

1.4.1 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.

A possible way of reconciling this negative evidence with the quark theory consisted in postulating that quarks did not strictly follow the Pauli principle, and they obeyed instead a quantum statistics intermediate between Fermi–Dirac and Bose–Einstein statistics (so-called 'parastatistics'): quarks may be 'parafermions'. The possibility of parafermions, and more generally of paraparticles,

---

[25] Massimi (2005).

followed from permutation invariance: as Greenberg and Messiah proved in 1964, in quantum mechanics given $|\psi>$ the vector representing the state $|\psi>$ of a composite system of n indistinguishable particles, it is not possible by measuring the expectation value of any observable B to distinguish $|\psi>$ from any permutation $\overline{\mathrm{P}}$ $|\psi>$. This permutation invariance is satisfied not only in the case in which $|\psi>$ 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 possible 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 40 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 in making 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.

1.4.2 Bohr Versus Einstein on Physical Reality

This internalist perspective about physical reality as dependent on 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, and 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 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 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

---

[26] Einstein et al. (1935), p. 777.

[27] Bohr (1935).

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 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 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-largely 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 give a possible answer to it by revisiting the much celebrated Fresnel–Maxwell episode.

1.5 How Mathematical Structures Cash Out Truth: Revisiting the Fresnel–Maxwell Case

As highlighted in Section 1.1, 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 (quasi) 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 were assumed to 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 a 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, which 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) among which forces acted. By assuming that two parallel lines of molecules could be readily separated laterally, but strongly resisted 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 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.

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*

---

[28] For details of this story, see Buchwald (1981), on which I draw here.

[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.

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 proved to be correct and survived, while the mechanical models of the ether were all finally abandoned.

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

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

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 had also the value of $\frac{1}{\mu_0 \varepsilon_0}$ – independently measured from constants k_1 and k_2 – their numerical agreement was the decisive proof that light was an electromagnetic wave.

---

[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. 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.

[31] In 1862 Foucault established an estimate of the speed of light of 298,000 km/s which was 4% below the value 310,000 km/s. Maxwell himself tried to improve this estimate, and in 1868 he found a value of 288,000 km/s.

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 1887–1888. 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 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 on 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 (from Faraday's law of induction to Ampère's law).

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 equations 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 we have. Only within their boundaries can we try to make reasonable guesses about what there is or there is not.

1.6 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 'no miracle' argument, and the antirealist '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-change' 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 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, but 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 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 should at best 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 Pauli-violating quarks, 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 loses some of its realist strength. But, on the other hand, if we cannot live up to the promise of the no-miracle argument

---

[32] I owe this metaphor to 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.

(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 the pessimistic meta-induction. Playing down reference can help us mitigate the tension between the two arguments. More precisely, the 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 they are ‘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 they will continue to be so.

To conclude, a neo-Kantian perspective has the advantage of demystifying 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 we have and can be realist about: there is for us no other reality to be investigated and sought after.

References

Bohr, N. (1935) ‘Can quantum-mechanical description of physical reality be considered complete?’, *Physical Review* **48,** 696–702.

Buchwald, J. Z. (1981) ‘The quantitative ether in the first half of the nineteenth century’, in G. N. Cantor and M. J. S. Hodge (eds.) *Conceptions of Ether. Studies in the History of Ether Theories 1740–1900* (Cambridge: Cambridge University Press), pp. 215–37.

Cao, T. (2003a) ‘Structural realism and the interpretation of quantum field theory’, *Synthese* **136**, 3–24.

Cao, T. (2003b) ‘Can we dissolve physical entities into mathematical structures?’, *Synthese* **136**, 57–71.

Cassirer, E. (1910) *Substanzbegriff und Funktionsbegriff. Untersuchungen zu den Grundfragen der Erkenntniskritik* (Berlin: Bruno Cassirer). Engl. transl. (1953) *Substance and Function* and *Einstein’s Theory of Relativity*, by W. C. Swabey (New York: Dover Publications).

Cassirer, E. (1936) *Determinismus und Indeterminismus in der modernen Physik* (Göteborg: Högskolas Arsskrift 42). Engl. translation (1956) *Determinism and Indeterminism in Modern Physics*, by O. T. Benfey (New Haven: Yale University Press).

Demopoulos, W. and Friedman, M. (1985) 'Bertrand Russell's *The Analysis of Matter*: its historical context and contemporary interest', *Philosophy of Science* **52**, 621–639.

Einstein A., Podolsky B., and Rosen N. (1935) 'Can quantum-mechanical description of physical reality be considered complete?', *Physical Review* **47,** 777–780.

French, S. and Ladyman, J. (2003a) 'Remodelling structural realism: quantum physics and the metaphysics of structure', *Synthese* **136**, 31–56.

French, S. and Ladyman, J. (2003b) 'The dissolution of objects: between Platonism and Phenomenalism', *Synthese* **136**, 73–7.

Kuhn, T. S. (1991) 'The road since structure', in A. Fine, M. Forbes, and L. Wessels (eds.) *PSA 1990: Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association,* vol. 2 (East Lansing, MI: Philosophy of Science Association), 3–13. Reprinted in Kuhn (2000) *The Road since Structure. Philosophical Essays, 1970–1993, with an Autobiographical Interview* (Chicago: University of Chicago Press), 90–105.

Ladyman, J. (1998) 'What is structural realism?', *Studies in History and Philosophy of Science* **29**, 409–424.

Laudan, L. (1981) "A confutation of convergent realism", *Philosophy of Science* **48**, 19–49.

Massimi, M. (2005) *Pauli's Exclusion Principle. The Origin and Validation of a Scientific Principle* (Cambridge: Cambridge University Press).

Maxwell, G. (1970a) 'Structural realism and the meaning of theoretical terms', in S. Winokur and M. Radner (eds.) *Analyses of Theories, and Methods of Physics and Psychology: Minnesota Studies in the Philosophy of Science*, Vol. IV (Minneapolis: University of Minnesota Press), pp. 181–192.

Maxwell, G. (1970b) 'Theories, Perception, and Structural Realism', in R. Colodny (ed.) *Nature and Function of Scientific Theories*, University of Pittsburgh Series in the Philosophy of Science, vol. IV (Pittsburgh: University of Pittsburgh Press), pp. 3–34.

Newman, M. H. A. (1928) 'Mr Russell's "causal theory of perception"'. *Mind* **37**, 137–148.

Poincaré, H. (1902) *La science et l'hypothèse*. Engl. transl. (1982) *Science and Hypothesis*, in J. Royce (ed.) *The Foundations of Science* (Washington: University Press of America).

Poincaré, H. (1905) *La valeur de la science*. Engl. transl. (1982). *The Value of Science*, in J. Royce (ed.) *The Foundations of Science* (Washington: University Press of America).

Putnam, H. (1981) *Reason, Truth, and History* (Cambridge: Cambridge University Press).

Putnam, H. (1983) *Realism and Reason. Philosophical Papers*, Vol. 3 (Cambridge: Cambridge University Press).

Putnam, H. (1990) *Realism with a Human Face* (Cambridge, MA: Harvard University Press).

Russell, B. (1912) *The Problems of Philosophy* (London: George Allen & Unwin).

Russell, B. (1914) *Our Knowledge of the External World as a Field for Scientific Method in Philosophy* (London: George Allen & Unwin).

Russell, B. (1927) *The Analysis of Matter* (London: Allen & Unwin).

Worrall, J. (1994) "How to remain (reasonably) optimistic: scientific realism and the 'luminiferous ether". *PSA* **1**, 334–342.

Worrall, J. (2007) "Miracles and models; why reports of the death of structural realism may be exaggerated", *Royal Institute of Philosophy Supplements, Vol. 82, Supplement* **61**, 125–154.

Worrall, J. and Zahar, E. G. (forthcoming) "The Newman objection to structural realism—what objection?".

Chapter 2
In Defence of Ontic Structural Realism

Steven French and James Ladyman

2.1 Introduction

In this paper we clarify how 'epistemic' (ESR) and 'ontic' structural realism (OSR) should be understood and reply to some important criticisms of the latter. We shall begin with an outline of the historical origins of what might broadly be called the 'structuralist tendency' within philosophy of science. This has come to be identified with a form of 'structural realism' but it should be noted that it also includes those who adopt an anti-realist or empiricist stance.[1] Because of the width of its embrace and its complex history, defining what is meant by 'structure' and characterising the tendency in general, is problematic. However, we begin by pointing out that the structuralist tendency always involves a shift in focus away from objects – however they are metaphysically conceived – to the structures in which they are (supposedly) embedded (where the reason for the qualifier 'supposedly' will become clear shortly). This is vague but the tendency, both historically and in its current incarnation, is not monolithic but rather includes various overlapping subgroups of structuralists.

The resurgence of interest in the structuralist tendency was initiated to a large extent by Worrall's (1989) paper on structural realism in which he cites Poincaré as a major precursor. But although Poincaré does exhibit certain of the commitments associated with the tendency, most significantly perhaps with regard to group theory, he was no realist in the sense that Worrall is. In his later work, Worrall looks back to Russell (especially his 1927), as Maxwell (1962 1970a, b, 1972) did before him, and indeed, to a large extent, it is Russell's form of structuralism

---

[1] Van Fraassen (2006, 2007, 2008) is a contemporary empiricist structuralist. See also Ryckman (2005) for a transcendental idealist structuralism.

S. French (✉)
Department of Philosophy, University of Leeds, Leeds, LS2 9JT, UK
e-mail: s.r.d.french@leeds.ac.uk

J. Ladyman
Department of Philosophy, University of Bristol, Bristol, Avon BS8 1TH, UK

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_2, © Springer Science+Business Media B.V. 2011

which both defenders and critics have seized upon as an important representative of the tendency. In particular Newman's (1928) famous criticism of Russell, which caused him to abandon his structuralism, has been taken to apply across the tendency, despite the fact that other structuralists articulated views of structure very different from that of Russell. In this regard it is important to note the historical context of Russell's work: *The Analysis of Matter* was written in 1926 and published in 1927 and although it contains suggestive indications of the coming quantum revolution, it fails to engage with the metaphysical implications of the new quantum mechanics.

The metaphysics of physics is explored in the works of Cassirer and Eddington, two prominent members of the structuralist tendency whose positions have tended to be overshadowed by Russell's. In both cases their structuralism was significantly informed by their reflections on the foundations of General Relativity; in both cases they emphasised group theory as representing the sense of structure they had in mind (in a way that, as far as Eddington was concerned, allowed him to rebuff Newman's criticism); in both cases they can hardly be described as realists, again, at least not in the sense in which we understand that position today; and in both cases they extended their structuralism to the quantum context, and saw quantum physics as further supporting it. In particular, they argued that the nature of quantum objects is such that they could not be understood as individuals in any of the standard metaphysical senses. Indeed, Cassirer, a neo-Kantian, argued that it was this revision of our understanding of physical objects, rather than causality or determinism, that was most significant with respect to the new physics. The sense in which such objects could be understood as 'non-individuals' was in terms of a structuralist framework, in which they are conceptualised as 'nodes' in the structure or mere 'intersections of relations' (see French 2003; Cei and French 2009).

This concern with the implications of quantum physics re-emerges in our structuralism, but with a significant twist. As a number of commentators recognise, one can in fact maintain that quantum objects are individuals, within certain constraints, and this gives rise to a form of metaphysical underdetermination in which the physics supports two different ontological 'packages': particles-as-individuals and particles-as-non-individuals (see French and Krause 2006). As part of his own empiricist 'take' on quantum mechanics, van Fraassen (1991) adduces this underdetermination as one of the reasons to say 'goodbye to metaphysics' and if we're not careful, we find ourselves waving bye to realism too. Our response is to argue that the underdetermination can be side-stepped by rejecting the standard conception of physical objects and reconceptualising the quantum world in terms of modal structure (French and Ladyman 2003a, b).

We hope we've said enough to provide some context for the issues we want to focus on here. In the next section we clarify the distinctions between 'epistemic' and 'ontic' forms of OSR, before considering the reconceptualisation of the notion of 'object' within the latter, and the issue of appropriately capturing theory change. We then respond to our critics who have claimed that OSR cannot offer an account of intrinsic properties, and that OSR cannot accommodate a conception of causal powers.

2.2 ESR Versus OSR

In this paper we will concentrate on defending OSR rather than criticising ESR. However, it is worth noting that the two have much in common:

(i) ESR and OSR both involve commitment to the claim that science is progressive and cumulative and that the growth in our structural knowledge of the world goes beyond knowledge of empirical regularities.
(ii) ESR and OSR both depart from standard scientific realism in rejecting term by term reference of theories, and hence standard referential semantics, and any account of approximate truth based on it.
(iii) According to both OSR and ESR, scientific theories do not give us knowledge of the intrinsic natures of unobservable individual objects.

Two versions of ESR can be contemplated. According to ESR_1 there are such objects but we cannot know them, and according to ESR_2 there may or may not be such objects, but we cannot know either way, and if there are such objects we cannot know them. (It seems that Worrall now advocates ESR_2.)

One key thesis of OSR is eliminativism about objects over which science, according to either version of ESR, draws a veil. The defender of ESR_2 may argue that in being eliminativist OSR goes too far since a properly humble epistemic attitude ought to leave one agnostic about the existence of things that both sides agree are epistemically inaccessible. This raises general issues concerning the burden of proof between non-believers and agnostics that are familiar from debates about the existence of God. We take it there are defeasible but compelling grounds for the repudiation of individual objects with intrinsic natures as follows:

(a) Many physicists have concluded that quantum mechanics requires it. Subsequent developments in physics have done nothing to undermine their arguments.[2]
(b) Such objects arguably belong to the conceptions of common sense based on everyday experience of the macroscopic world that are part of our evolutionary and cultural endowment. Since the conditions under which these were selected for form a parochial corner of the world revealed to us by science then we have positive reason to regard them as likely to be inadequate for fundamental metaphysics based on the physical sciences.[3]
(c) A commitment to individual objects with intrinsic natures motivates haecceitism, namely the idea that worlds that agree about the qualitative properties of things and the relations among them may differ solely with respect to the

---

[2] Krause and French *op. cit.*, explain the history of the debate about identical particles in physical theories in detail. They defend the received view that quantum particles are not individuals, and develop a formal framework for non-individual objects. See Rickles et al (2006) for the context of quantum gravity. See also Ladyman and Ross (2007), Chapter 3.

[3] See Ladyman and Ross *op. cit.*, Chapter 1.

> permutation of individuals. Haecceitism is prima facie incompatible with permutation invariance in Quantum Mechanics and with diffeomorphism invariance in General Relativity.

The connection between individual objects and haecceitism is a complicated one.[4] It depends on how the idea of individual objects is understood. Recently, philosophers sympathetic to OSR have defended the claim that both quantum particles and spacetime points are individuals. However, they have very thin notions of individual objecthood in mind. In the next section we review this issue and show that the basic idea of OSR is not undermined.

2.3 Through Thick and Thin

The received view in the philosophy of physics is that quantum particles are not individuals. This is because they violate the Principle of the Identity of Indiscernibles (PII) as standardly understood. As French and Redhead (1988) pointed out, failure of PII does not alone entail that quantum particles are not individuals. It could be that they are individuals, but if so then their individuality would transcend their qualitative properties. Such 'transcendent individuality' is associated with the ideas of primitive thisness, haecceity and bare particulars and is regarded with suspicion by empiricist philosophers. Hence, the received view. However, Saunders (2003a, b, 2006) has contested both the latter and the claim that all quantum particles violate PII.

Saunders challenges the claim that PII is false for elementary fermions (and also it seems for elementary bosons in entangled states) by reintroducing Quine's distinction between three kinds of discernibility, namely absolute, relative and weak. We will not review the details here since they have now been much discussed (see Hawley 2006; Ladyman 2005, 2007; Müller and Saunders 2008) but the most important point is that two objects are 'weakly discernible' just in case there is two-place irreflexive relation that they satisfy. In virtue of their joint states always being anti-symmetrised, elementary fermions always satisfy weak discernibility. The weak notion of individuality advocated by Saunders (according to which weak discernibility is sufficient for individuality) seems coherent. It would be question-begging to deny the sufficiency of weak discernibility merely because stronger forms of discernibility are sometimes available. Note however that while Saunders's view vindicates an ontology of individuals in the context of Quantum Mechanics, it is a thoroughly structuralist one insofar as objects are not assumed to be individuated independently of the nexus of relations in which they stand.[5] Rather they are *contextually* individuated in the sense that their individuality does not obtain independently of their relations to each other.[6]

---

[4] See French and Krause *op. cit.* pp. 16–18, and Ladyman (2007).

[5] For further discussion see French and Krause 2006, pp. 167–173.

[6] The idea of contextual versus intrinsic individuality is due to Stachel 2005 and is discussed in Ladyman 2007.

As mentioned at the end of the last section, the main reason for revoking primitive identity facts in physics is that, by implying haecceitism, they seem to imply a violation of the permutation invariance that in different ways holds in quantum mechanics, with respect to particles, and in general relativity, with respect to spacetime points. Clearly Saunder's idea of contextual individuality grounded in relations is compatible with the denial of haecceitism. On the other hand, if individuation is intrinsic, and not grounded in qualitative properties, in other words is primitive, then the identity of an individual object may be regarded as determinate in other counterfactual situations, and permuting an object with another indiscernible object gives rise to another state of affairs. However, the question now arises as to whether primitive contextual identity is also compatible with the denial of haecceitism, and the answer is readily seen to be affirmative. If individuality is contextual then, whether it is primitive or grounded in relations, there is in general no reason to regard talk of the same object in another relational structure as intelligible. So what does the work in avoiding haecceitism is contextual individuation, not whether or not it is grounded in relations.

So it seems the options with respect to quantum particles and individuality are now numerous:

(i) No quantum particles are individuals (the received view).
(ii) Quantum particles are individuals in virtue of primitive intrinsic individuality (articulated in terms of haecceities, primitive thisness, etc.).
(iii) Elementary fermions are individuals in virtue of being weakly discernible (grounded contextual individuation).
(iv) Quantum particles are individuals in virtue of primitive contextual individuation.

It is at this point that forms of OSR diverge. Thus, one can take these options as feeding into the metaphysical underdetermination noted above and in earlier work. Options (iii) and (iv) reinforce the 'particles-as-individuals' horn of the metaphysical dilemma, allowing one to adopt this 'package' without having to invoke any kind of primitive intrinsic individuality, which may seem metaphysically unattractive. Advocates of the form of OSR that is motivated (in part) by this underdetermination can now insist that there is really little to choose between the two (at least in terms of metaphysical plausibility) and hence, if we are not to follow van Fraassen in saying goodbye to realism, the very notion of object should be given up.[7]

Alternatively, one may feel inclined to cleave to a 'thin' notion of object, at least (whether because one wishes to resist the force of the underdetermination, or because, despite the efforts of French and Krause and others, one does not see the 'particles-as-non-individuals' package as a genuine alternative). In particular, option (ii) raises concerns about a potential clash with physics over whether or not permutation gives rise to genuinely distinct worlds.[8] Hence, option (i) and some form of contextual individuality may then be the only

---

[7] This is the view taken by French, for example.

[8] See French and Krause 2006.

options for a robust naturalist. Note that none of these options are compatible with the 'thick' idea of individuals in opposition to which OSR was originally proposed. Options (i), (ii) and (iv) correspond to positions in the debate about spacetime ontology too. (i) is the response to the hole argument that denies that spacetime points are individual objects, (ii) is the standard substantivalist view that that they are and that Leibniz equivalence fails, and (iv) is the view of sophisticated substantivalists such as Pooley (2006) and Stachel (2002, 2006). The adoption of contextual individuation in support of a thin notion of individual objects is a vindication of OSR since it is a thoroughly structuralist conception of objecthood. An advocate of OSR can allow that quantum particles and spacetime points are individuals in a thin sense, while remaining committed to the abandonment of any notion of individual that entails haecceitism.[9] The core claim of OSR that relational structure is ontologically primary can be retained even if the existence of individuals in the weak sense is endorsed.

Hence we have two forms of OSR: one that retains a 'thin' notion of object, contextually individuated; and another that dispenses with objects entirely. Articulating the differences between the two is still a matter of on-going discussions, but certainly one could argue that the 'nodes' of the latter are nothing more than the 'thin' objects of the former.

Interestingly, similar issues also arise in mathematics. Permuting structurally similar individuals in a mathematical structure results in exactly the same structure. Hence, if primitive identity facts are posited in mathematics they must respect permutation invariance in mathematical structures such as edgeless graphs. The positing of a kind of primitive identity that allows for this, because it is contextual rather than intrinsic, seems to make for a consistent form of mathematical structuralism.[10] Given this, the question arises as to whether primitive contextual identity holds in the physical domain too, and hence whether elementary bosons are individuals after all. The weak indiscernibility of elementary bosons is taken by Saunders to be sufficient for denying them the status of individual objects. The existence of mathematical objects that are not even weakly discernible may be one important difference between mathematical and physical objects, or it may be taken as a reason for disputing the claim that elementary bosons are not objects. After all, bosons, like fermions and like nodes in an unlabelled edgeless graph, can be aggregated (although not enumerated). If there is nonetheless good reason for denying that elementary bosons are individuals, then this may point to a significant difference between identity in physics and mathematics.

---

[9] This is the option advocated by Ladyman.

[10] This is defended in Leitgeb and Ladyman (2008).

2.4 Theory Change and Representation

Several critics of structural realism have pointed out that structure in general, and mathematical structure in particular, is also often lost in theory change (see, for example, Chakravartty 2004, 164; Stanford 2003, 570–572). If this is right then what of the alleged motivation for SR as a response to the pessimistic meta-induction? It is of course correct to say that some of the structure of theories is lost in episodes of radical theory change. The structure of absolute space and time in Newtonian physics is not preserved in relativity theory, the structure of circular orbits in not preserved in Kepler's model of the solar system, the structure of continuous spatiotemporal trajectories of particles in classical mechanics is not preserved in quantum mechanics. However, of course, much of the empirical content of these theories has been retained. The structural empiricist (van Fraassen) also claims that theories represent the relations among, or structure of, the phenomena, and they agree that in general in science the empirical content of a well-confirmed old theory is recovered as a limiting case of the replacement theory. There are no 'Kuhn-losses', in the sense of successor theories losing all or part of the well confirmed empirical structures of their predecessors (Post 1971, 229). If this is all that the structural realist is saying then their position does not go further than structural empiricism.

In defending OSR we have always maintained that the difference between it and structural empiricism is that the latter, if it incorporates the empiricist disdain for objective modality that van Fraassen defends (see, for example, van Fraassen 1989), must regard the relations in question as extensional generalizations about concrete actual phenomena. On the other hand, to a realist about the modal structure of the world, the relations in question constitute the causal/nomological structure of the theory and are not reducible to empirical regularities. For example, it is part of the structure of Newtonian mechanics that the laws of physics are invariant under the Galilean transformations, and the latter are recovered in approximate form as part of the structure of relativistic physics. Similarly, the inverse-square law, and not merely the approximate truth of Kepler's kinematical laws of planetary motion, can be recovered from the more fine-grained structure of General Relativity. There are numerous other examples from quantum mechanics, from ray and wave optics, and even from economics (see Ross 2008), where the structure of theories that is preserved in theory change goes beyond the empirical regularities in observable phenomena implied by the theory. Hence, we claim that more than the empirical content of theories is preserved in theory change (if empirical content is understood in the narrow sense of van Fraassen).

This explains how OSR differs from structural empiricism but does not answer the charge with which we began this section. Clearly some of the modal structure of Newtonian mechanics is not retained by special relativity because, for example, the velocity addition law is completely wrong for frames between which there is relative motion at velocities anywhere near that of light. The advocate of OSR is not claiming that the structure of our current theories will be preserved simpliciter, but rather that the well-confirmed relations between the phenomena will be preserved in at least approximate form and that the modal structure of the theories that underlies them, and plays the appropriate explanatory role, will also be preserved in approximate form. Newtonian mechanics was never well-confirmed for situations involving very fast relative velocities or very small particles. The job of predicting what will be preserved and what abandoned by future science belongs to science itself not to philosophy, but our claim is that from a structuralist point of view it is possible to explicate the continuity in scientific theories that often does not hold at the level of objects and properties. Indeed this is how the continuity in theories is explicated in physics itself. Standard scientific realism is vulnerable to the argument from theory change because the latter shows that successful reference is not a necessary condition for novel empirical success. SR is not similarly vulnerable because it is not claimed that the structure of the theory being exactly right is necessary for novel empirical success. A counterexample to SR would be a case where the structure of a theory that had enjoyed novel empirical success was completely abandoned but we know of no such example.[11]

Our defence of OSR has always been articulated in the context of the semantic approach to scientific theories because we think that mathematics cannot be rivalled by first order logic as a language for representing the structure of theories (see Ladyman 1998; French and Ladyman 1999). Landry has argued that a category-theoretic framework offers a more appropriate set of representational resources (Landry 2007). However, it is contentious whether such a framework is sufficiently fine-grained to appropriately represent the kinds of inter-theory relations that must be considered in discussing theory change. Here, at least, the partial structures version of the semantic approach seems to have the necessary resources to hand (da Costa and French 2003).

More recently, Brading and Landry (2006) have suggested that a minimal notion of shared structure is sufficient for the structuralist's needs. The worry with this suggestion is that in the absence of further articulation as to the mode of representation, such an approach may collapse into little more than a meta-level positivistic recitation of the 'facts' pertaining to whatever case study is being considered (French forthcoming). Of course, there is the well-known concern that adopting a set-theoretic framework compromises the underlying metaphsyics of OSR, since it suggests a commitment to sets of objects.

One response is to insist on the 'thin' notion of object above, and argue that such commitment does not take us beyond the bounds of structuralism, since these objects are themselves individuated structurally. Alternatively, if one has adopted the other form of OSR, one could make the manoeuvre that goes back to Poincaré (1898), and insist that although we introduce the terminology, or perhaps better, symbology, of objects as part of our representation, these are mere devices that

---

[11] See Ladyman and Ross (2007), Chapter 2, and Ladyman (2007), Section 2, for a discussion of the problem of theory change for standard scientific realism.

allow us to appropriately represent the relevant structure and even if they have representational priority, they should not be taken as ontologically foundational. Consider, for example, the manner in which the permutation invariance that lies at the heart of quantum statistics is represented (French 1999): we apply group theory which, of course, assumes a domain of elements upon which certain transformations can be applied. As a result of this application we conclude that quantum entities should not be regarded as individual objects at all and hence their initial representation as group-theoretical elements was a kind of heuristic device, necessary to get the application of the relevant mathematics off the ground, as it were. More generally, having reconceptualised these entities in structural terms, in order to represent that structure we have little option but to draw on the resources of extant mathematics, or rather, set-theory but that should not be understood as a re-introduction of the very notion of object we have rejected.

On either form of OSR, we are basically saying the following: although the set-theoretic representation of structure, <A, R> (where A is a set of elements and R a family of relations) must be written down from left to right, from an ontological perspective it should be read semitically, that is, from right to left.

The appropriate representation of structure is a matter of on-going discussion and an obviously attractive option is to adopt some form of pluralist stance, according to which one should adopt the representational device appropriate for those features of scientific practice one is concerned with (French 2010). Whether such a stance is entirely compatible with OSR is something we shall leave for further consideration, but certainly we agree that the use of set theory as a foundation for the semantic approach is not an essential ingredient of this position.

2.5 OSR and Intrinsic Properties

A significant concern of the critic of OSR has to do with the structural realist's supposed indifference to the instantiation of 'first order' (intrinsic) properties such as mass and charge (Busch 2003). The underlying concern here is that 'second order' structural properties are relations among first order ones which in turn are possessed by objects, and if the latter are abandoned, then the whole structural edifice is put in jeopardy. Clearly, there is potential question-begging here. Obviously a structuralist will have to say something about intrinsic properties. And indeed they have. Cassirer explicitly includes a consideration of charge, for example, in his structuralist accommodation of quantum physics. And intrinsic properties can be accommodated within the context of the structuralist 'reconceptualisation' of particles, although there are various issues that need to be considered.

First of all, there is the well-known and complex issue of the characterisation of 'intrinsic' properties. A definition that might serve our purposes here is the following: if an object has a property intrinsically, then it has it independently of the way the rest of the world is. On this view, intrinsic properties are those that could be possessed by ‘lonely’ objects, where a lonely object is an object that could exist in a possible world in which there are no other objects.[12] Now, a structuralist could begin by insisting that such properties are not ‘possessed’ by objects at all, or at least not in the sense of objects existing ‘over and above’ the properties. This ties in with the early structuralists’ rejection of substance and the similar rejection of objects by both modern day and mathematical structuralists. Of course, it may be objected that this rejection alone is not enough to yield an interesting form of structuralism, since there is a sense in which those who metaphysically analyse individuality in terms of a bundle theory of qualitative properties will also reject objects in *this* sense. Structuralism is certainly closely related to various forms of the bundle theory. So the objection which might be directed at the structuralist that for there to be properties they must not only be ‘possessed by’ some object, but must ‘inhere in’, or ‘attach to’ or otherwise subsist in such an object, must also be faced by the defender of the bundle theory.

Leaving this to one side, the critic might insist that insofar as the intrinsic properties are not relational, removing any underlying object (especially one conceived of in substantival terms) does not yield an interesting form of structuralism, since we retain non-structural elements, namely the intrinsic properties themselves. Of course, there are issues to do with drawing a clear distinction between ‘intrinsic’ and ‘relational’ (see Weatherson 2002), as well as issues to do with characterising structuralism wholly in relational terms, but let us bite the bullet and consider whether the structuralist might plausibly deny the existence of intrinsic, in the sense of non-relational, properties.

One option might be to follow Kant, who maintained that among the relations that we know, some are ‘self-subsistent and permanent’, and that it is through these that we obtain a determinate object. Among such relations might be included the apparently intrinsic properties such as charge. As Cassirer then went on to note, following the anti-substantival trend noted above, ‘... the constancy of a certain relation is not at all sufficient for the inference of a constant carrier’ (reference). So, charge, for example, comes to be conceived of as a constant relation, with no constant object ‘carrying’ it. There is the problem, nicely expressed by Chakravartty (2003), that it is a notable feature of our experience that certain such ‘constancies’, come together, so we get charge associated with a certain rest mass and a certain spin, and one explanation for this association is that there is an underlying object which possesses all these properties. Obviously the structuralist will have to provide an alternative explanation (see French 2006). Note that the bundle theorist also has to - in, of course, non-substantival terms – invoke some sense of ‘tie-ing together the bundle’ (or, if she has a inclination to favour tropes, some notion of compresence). The structuralist could adopt something

---

[12] This notion of lonely objects plays a crucial role in the scholastic distinction between individuality and distinguishability drawn on by French in his interminable discussions of quantum individuality. See Langton and Lewis (1998) and Weatherson (2002) on intrinsic properties.

similar, suggesting that there is a certain 'compresence' among different relational properties in the overall world-structure, or she could simply take it as a primitive metaphysical 'fact' (French 2006).

Instead, one could argue for this stance on the basis of contemporary science. Thus one could maintain that the relevant theory tells us what *would* happen to a charged particle, say, if another charge were to be brought near. Of course, this brings in a modal element, but leaving that aside for now, one might try to argue in this way from a structuralist view of theories to a structuralist view of 'objects'. Busch notes the point that the only properties that matter in science are structural, but insists that this does not imply that all there is are structures. The problem is that if non-structural (intrinsic) properties are non-scientific then they are, like Worrall's natures, otiose.

This leaves the issue of the lonely object: the mere possibility of such a situation may be used by the critic to press the claim that intrinsic properties are not structural (Chakravartty, private correspondence). A quite radical response would be to follow Mach, and Leibniz before him, and insist that such a situation does not constitute a 'genuine' physical possibility and hence cannot undermine a metaphysics based on current physics.[13] A more moderate approach would be to allow the invocation of such possible worlds for certain purposes, but to note that they only reveal a certain aspect of the relevant properties and effectively miss out on their structural nature, which is revealed not through metaphysical imaginings but reflection on the relevant theory.

Hence the issue of whether, on either form of OSR, non-structural property elements remain, is not straightforward. Certainly, it is not obvious that intrinsic properties must be understood as non-structural in the relevant sense and there are metaphysical avenues open to the ontic structural realist.

2.6 OSR and Causation

Busch (2003), Psillos (2006) and Chakravartty (2003) have all argued that individual objects are central to productive rather than Humean conceptions of causation and hence to any genuine explanation of change. Objects, it is alleged, provide the 'active principle' of change and causation. French (2006) replies to this charge and we expand on that defence here (see also Ladyman 1998 and 2004, and French and Ladyman 2003).

As Psillos (2006) has noted, a prima facie promising way of understanding causation in a structuralist context would be to adopt some form of Russellian 'structure-persistence' view, according to which events form causal chains and the members of

---

[13] Of course, one has to be careful about what counts as 'genuine' here. Leibniz, for example, understood it as whatever conformed with his metaphysical principles, notably the Principles of Sufficient Reason and Identity of Indiscernibles, which would hardly be appropriate here.

such a chain are taken to be similar in structure. As he points put, however, this appears to fail as there are examples of causal changes which do not seem to involve the persistence of structure, such as the explosion of a bomb. Furthermore, in analysing a process in terms of that whose persistence renders the process causal, Psillos insist that the 'natural' account would involve objects and properties and hence persistence cannot be purely structural.

However, one might object that this is to approach the issue in entirely the wrong way. OSR does not advocate analysing all macroscopic causal processes in a structuralist fashion – we have to recall the motivation for this position in quantum physics. In essence, OSR piggybacks on the physicalist's reduction of such processes in terms of ultimately quantum processes and then insists that those have to be understood in structuralist terms. Imagine for example two particles of the same charge approaching one another and being mutually repelled. OSR would take the currently accepted theoretical description of that process – whether in terms of field theoretic interactions or the exchange of force particles or whatever – and would simply insist that rather than thinking of this description in terms of causally interacting physical objects, we think of it in terms of a system of relations some of which are causal. Of course there is more to say here about the metaphysics behind such a picture and the manner in which structure has to be in some sense dynamical, but the idea is not to analyse events into series of similar structures, but rather to view the interactions between particles in structuralist terms.

Persistence is problematic in the quantum context but leaving that to one side, and also the possibility that the insistence that the most 'natural' account of such persistence is in terms of objects and their properties might be question begging (and of course it is precisely thinking in terms of objects that makes persistence so problematic in the quantum context), it is not clear what is being asked for here. We recall the old adage that to talk of change presupposes something undergoing change but the structuralist would precisely resist the move to some account of what is underlying change in terms of either substance or individual objects; rather what we have is the world structure with a particular configuration, if you like, or family of relations at one time and a different configuration, or a different arrangement or family of relations at either earlier or later times. That is all that is needed (well, not quite because there are metaphysical issues as we shall see).

Psillos goes on to push the point by arguing that a structuralist approach (which views causation in terms of a relation between isomorphic systems) cannot distinguish between cause and effect. Now of course this is a major metaphysical issue that we cannot hope to address fully here. Still, as Psillos notes, an obvious response would be to say the cause is that which precedes the effect in time. Whilst acknowledging that this means the structure has to be taken '*in re*', he points out that 'being in space and time' are non-structural properties, hence this obvious response admits something deeply non-structural into the OSR picture. But in what sense is being in space or time not a structural property?

Setting aside quantum considerations for a moment, how would the structuralist analyse the statement that object a is 'at' position x at time t, or relativistically, 'in' space–time. One might approach this by suggesting that there is a relationship between *a* and point *x*, time *t*, or the relevant space–time point. Now, taking *a* to be reconceptualised in structural terms, it is not at all clear that there is any block on this relationship also being understood in such terms. Of course, on a substantivalist view of space, time and/or space–time, the relationship would be *with* something non-structural, so if that were deemed sufficient to render the apparent property 'being in space–time' non-structural, one could either go for the relationist option, in which case spatio-temporal relations are ultimately reduced to relations between material objects and if the latter are appropriately reconceptualised, again, it is hard to see what has entered the picture that is non-structural; or, if one felt the need to be a completely consistent structuralist about not only physical objects but space–time as well, then one could follow the lead of the earlier structuralists, such as Cassirer and Eddington who understood General Relativity in such terms, or Stein who argued more recently that space–time is an aspect of the structure of the world.[14]

There is a further concern as to how the structuralist would understand the *relata* of causal relations. As Psillos points out, typically these are taken as either events or facts, but in either case these are further decomposed to objects and their properties. Even adopting a Davidsonian view of events as particulars leads to problems, since, Psillos notes, they are *in re* and hence cannot be abstract structures. Hence, he concludes, '… the truth-makers of causal claims require objects and properties.'

Now of course, this touches on profound issues in metaphysics to do with the nature of events and facts, and, in particular, arguments to the effect that only 'immanent' objects – those that are situated in space–time – can causally interact. Hence if facts are true propositions, as usually understood, they cannot perform such a role. The typical response is to propose a substitute for facts and the obvious choice would be objects (ultimately, elementary particles and aggregates of them) that provide the immanent basis required. Again there is nothing here to trouble the ontic structuralist who could happily agree that, on first analysis, the relata of causal relations are objects – understood in the usual, non-reconceptualised sense (e.g. electrons, quarks etc.) but would then insist, of course, that such objects are then to be understood as mere nodes in the structure. It may be objected that this threatens to lead to circularity: causal relations hold between causal relata which are resolved into objects which are themselves nothing but 'nodes' of (whatever that means) sets of causal relations. The worry dissipates if we think of levels of metaphysical analysis, but ultimately as far as the structuralist is concerned, there is nothing but the structure – that is, the set of relations – and leaving aside the issue of relations without relata, there is the significant concern about how relations can have in whatever sense causal powers. But it is unclear why this should be more of a problem for the structuralist than the non-structuralist who, ultimately, must hold that objects, or more basically, bare substances must have causal powers.

Thus Psillos' conclusion seems unwarranted. First of all, it should be remembered that if the decomposition of events and facts is into either everyday or the

---

[14] Further consideration of structuralist approaches in the context of space-time theory can be found in Rickles et al. *op. cit.*

physicists' objects, then the structuralist is not denying that we can continue to refer to such objects as a *façon de parlez*, only that these must themselves be regarded as decomposable into or reconceptualised as nodes in a structure or whatever. Secondly, we would argue that the structure of OSR should not be considered as 'abstract' (since it is the structure of the world), so the point about Davidsonian events should slide off our backs. Thirdly, however, and more importantly, given the motivation for OSR, such metaphysical divisions should always be placed in a quantum context. As indeed they have been (see Ch. 9 of French and Krause 2006). Here it has been made explicit that if one were to regard events, say, as decomposable into objects, the metaphysical underdetermination still applies, with the choice of whether to understand those objects as individuals or non-individuals and nothing in the physics to guide you. Best then to eschew (non-structural) objects entirely; as we've said, one can still insist that an ' event', such as a light coming on, is to be taken as metaphysically decomposable into objects – photons, say – and their properties – energy levels – but these objects are themselves to be understood as structural. And of course the Davidsonian view of events as particulars does not appear to present any special problems. Of course, an event on this view is to be identified, not via its spatiotemporal location, but rather in terms of its 'causal location' within the causal net of the world. Well-known ways of accommodating this within the context of QFT (see French and Krause 2006) could easily be adapted to the structuralist cause, so that events are understood as instantiations of (field) properties at space–time points. We've already touched on the issue of accommodating space–time above and Auyang gives a nice structuralist account of QFT in which neither the field event structure nor the space–time structure are given ontological priority but both 'emerge together' as aspects of the world-structure (Auyang 1995; this is very similar to certain features of Eddington's more developed view).

Still, there are more overtly metaphysical problems with accommodating causality. Both Psillos and more extensively, Chakravartty have raised concerns about whether the structuralist framework can accommodate the inherent 'activity' of causality. Thus Chakravartty argues that if objects are reconceptualised away, and in particular, if structures are understood as 'relations without relata', then we lose the 'active principle that transforms [one] set of relations into another.' (Chakravartty 2003, p. 872). If we adopt a structuralist form of regularity view and reject such active principles, and simply accept an analysis of events in terms of brute successions of structures, then we appear to run into Psillos' objection regarding structure preservation. We don't think we need to do that – although we might if we were a structuralist but anti-realist about causality – because (again) we're not convinced that the requirement that causality have some 'active' component requires a metaphysics of objects and properties.

Consider (naively perhaps): where might this active principle be located? With the object or the properties? If the former then we obviously need to press a little further and ask for an account of objecthood which could accommodate such activity. Obviously the idea of objects as ultimately bare substrata can't do the job; and equally obviously the views of objects as bundles of either properties or tropes forces us to look closer at the latter as the source of this activity. There are then further options: either each property that is causally active has some causal principle particular to that property, or kind of property, that is involved in the conferral of causal power on the possessors of that property; or there is some generic causal activity which together with the other features of properties confers such powers.

This raises deep metaphysical issues, to do with the relationship between properties and their causal powers or capacities, whether different properties can have or bestow the same power, whether the same property can bestow different powers on its instances and so forth. But the important point is that among metaphysicians the connection between properties and their powers is not fully understood (see Swoyer 2000). Indeed, it has been suggested that to be a property is to bestow causal powers; that it is through such bestowal that properties are identified; that this bestowal involves some intimate relationship between the property and its powers and, of course, that, at least when it comes to the properties that we are concerned with here – that is, 'fundamental' properties like charge and spin and mass and so on – they are inter-related in ways that are appropriately described by physical laws and theories in general. A well-known option is to insist that a causal power is a further property over and above the property that has or bestows it, but that introduces obvious complications and the possibility of regress; if this option is rejected, however, it is not clear what the relationship is. We don't think the structuralist should have to sort these issues out and there certainly appears to be nothing here that could not be appropriated. She could similarly note that on the view of structures in relational terms we face a choice between particular relations or kinds of relations having, as features, causal aspects particular to those relations or kinds and some form of underlying causal activity that imbues the relevant relations with causal powers. Granted that the former seems more clearly structuralist, we can't see why the second couldn't be incorporated as well.

This brings us back once more to the metaphysical analysis of properties themselves. Again crudely speaking, we might think of at least two alternatives: first that properties are somehow metaphysically irreducible, so that all they are, are their causal powers and nothing more; or we might think of a property as composed, in some fashion, of various features, including some form of particular or generic causal activity, and perhaps something else, some quiddity that makes the property the particular property that it is. Adopting this latter view would commit one to a non-structural feature, and so, as Psillos has pointed out, structuralists might be naturally sympathetic to causal structuralism which holds that all there is to a property is its causal powers and no more; that is, it denies quiddities. We think sympathy is the right attitude, but we don't think it would be inconsistent for someone to be structuralist about physical objects, but accept that properties and relations have some quidditical aspect.

Psillos argues that following causal structuralism and denying quiddities entails a kind of 'hyperstructuralism', according to which the causal powers of a property are seen to be purely structural; and that we end up with 'nothing but a formal structure'. But again, one can imagine someone being a structuralist about objects and their properties, but drawing a line at a structuralist account of the causal powers of these properties. Indeed, if one thinks of structure in relational terms, it is hard to see precisely how such an account could proceed – would the causal powers of properties and relations themselves be properties and relations? Even if that could be spelled out, there appears to be nothing inconsistent in being a structuralist about the objects and their properties but not about the latter's causal powers. Indeed, perhaps the most intuitively plausible form of structuralism is precisely one according to which objects and their properties are metaphysically dissolved into a multi-layered network of relations, where certain of these relations are causally empowered and where this empowerment, for want of a better word, is inherent to the relation. Is that inherent empowerment non-structural? Yes, in the sense that it is not itself a structure or describable in structural terms (if it were so describable an obvious regress would threaten); no, in the sense that it is another aspect of the world structure. And again, even if one were to go the hyperstructuralist route, it is not clear why moving up a level, as it were, would render causal powers as nothing but formal structure.

2.7 Conclusion

What we have tried to do here is survey some of the current discussions surrounding structuralism, and also to indicate how the structuralist can avail herself of various metaphysical resources, should she feel so inclined, in order to respond to a range of concerns. The overall message is that there are a range of options available and various avenues down which OSR may proceed. Indeed, this should be expected, as structuralism in general, and OSR in particular, develops and matures.

References

Auyang, S. (1995). *How is Quantum Field Theory Possible?* Oxford: Oxford University Press.

Brading, K. and Landry, E. (2006). Scientific structuralism: Presentation and representation. *Philosophy of Science* 73: 571–581.

Busch, J. (2003). What structures could not be. *International Studies in the Philosophy of Science* 17: 211–225.

Cei, A. and French, S. (2009). 'On the Transposition of the Substantial into the Functional: Bringing Cassirer's philosophy of Quantum Mechanics into the 21st Century', in M. Bitbol, P. Kerszberg and J. Petitot (eds.), *Constituting Objectivity, Transcendental Perspectives on Modern Physics,* pp. 95–115. Western Ontario Series in Philosophy of Science; Dordrecht: Springer.

Chakravartty, A. (2003). The structuralist conception of objects. *Philosophy of Science* 70: 867–878.

Chakravartty, A. (2004). Structuralism as a form of scientific realism. *International Studies in Philosophy of Science* 18: 151–171.

da Costa, N.C.A. and French, S. (2003) *Science and Partial Truth.* Oxford University Press.

French, S. (1999). Models and mathematics in physics: The role of group theory. In J. Butterfield and C. Pagonis, eds., *From Physics to Philosophy*, pp. 187–207. Cambridge: Cambridge University Press.

French, S. (2003). Scribbling on the blank sheet: Eddington's structuralist conception of objects. *Studies in History and Philosophy of Modern Physics* 34: 227–259.

French, S. (2006). Structure as a weapon of the realist. *Proceedings of the Aristotelian Society* 106(2): 1–19.

French, S. (forthcoming). 'The Presentation of Objects and the Representation of Structure', forthcoming in E. Landry and D. Rickles (eds.). *Structure, Object, and Causality*, Western Ontario Series in Philosophy of Science; Dordrecht: Springer.

French, S. (2010). Keeping quiet on the ontology of models. *Synthese* 172:231–249.

French, S. and Krause, D. (2006). *Identity in Physics: A Historical, Philosophical and Formal Analysis*. Oxford: Oxford University Press.

French, S. and Ladyman, J. (1999). Reinflating the semantic approach. *International Studies in the Philosophy of Science* 13: 103–121.

French, S. and Ladyman, J. (2003a). Remodelling structural realism: Quantum physics and the metaphysics of structure. *Synthese* 136: 31–56.

French, S. and Ladyman, J. (2003b). Between platonism and phenomenalism: Reply to Cao. *Synthese* 136: 73–78.

French, S. and Redhead, M. (1988). Quantum physics and the identity of indiscernibles. *British Journal for the Philosophy of Science* 39: 233–246.

Hawley, K. (2006). Weak discernibility. *Analysis* 66: 300–303.

Ladyman, J. (1998). What is structural realism? *Studies in History and Philosophy of Science* 29: 409–424.

Ladyman, J. (2004). Modality and constructive empiricism: A reply to van Fraassen. *The British Journal for the Philosophy of Science* 55: 755–765.

Ladyman, J. (2005). Mathematical structuralism and the identity of indiscernibles. *Analysis* 65: 218–221.

Ladyman, J. (2007). On the identity and diversity of individuals. *The Proceedings of the Aristotelian Society* 81: 23–43.

Ladyman, J. and Ross, D. (2007). *Everything Must Go: Metaphysics Naturalized*. Oxford: Oxford University Press.

Landry, E. (2007). Shared structure need not be shared set-structure. *Synthese* 158: 1–17.

Langton, R. and Lewis, D. (1998). Defining 'intrinsic'. *Papers in Metaphysics and Epistemology*, pp. 116–132. Cambridge: Cambridge University Press.

Leitgeb, H. and Ladyman, J. (forthcoming). Criteria of identity and structuralist ontology. *Philosophica Mathematica* 16: 388–396.

Maxwell, G. (1962). The ontological status of theoretical entities. In H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science, Volume 3*, pp. 3–14. Minneapolis: University of Minnesota Press.

Maxwell, G. (1970a). Structural realism and the meaning of theoretical terms. In S. Winokur and M. Radner, eds., *Analyses of Theoreis and Methods of Physics and Psychology: Minnesota Studies in the Philosophy of Science Volume 4*, pp. 181–192. Minneapolis: University of Minnesota Press.

Maxwell, G. (1970b). Theories, perception and structural realism. In R. Colodny, ed., *The Nature and Function of Scientific Theories: University of Pittsburgh Series in the Philosophy of Science, Volume 4*, pp. 3–34. Pittsburgh: University of Pittsburgh Press.

Maxwell, G. (1972). Scientific methodology and the causal theory of perception. In H. Feigl, H. Sellars and K. Lehrer, eds., *New Readings in Philosophical Analysis*, pp.148–177. New York: Appleton-Century Crofts.

Müller, F. and Saunders, S. (2008). Discerning Fermions. *British Journal for the Philosophy of Science* 59: 499–548.

Newman, M.H.A. (1928). Mr Russell's causal theory of perception. *Mind* 37: 137–148.

Poincaré, H. (1898). On the foundations of geometry. *The Monist* 9: 1–43.

Pooley, O. (2006). Points, particles and structural realism. In D. Rickles, S. French and J. Saatsi, eds., *Structural Foundations **of** Quantum Gravity.* Oxford: Oxford University Press.

Post, H.R. (1971). 'Correspondence, Invariance and Heuristics: In Praise of Conservative Induction', *Studies in History and Philosophy of Science* 2: 213–255.

Psillos, S. (2006). *The* structure, the *whole* structure and nothing *but* the structure? *Philosophy of Science* 73: 560–570.

Rickles, D., French, S. and Saatsi, J. eds. (2006). *Structural Foundations of Quantum Gravity.* Oxford: Oxford University Press.

Ross, D. (2008). Ontic structural realism and economics. *Philosophy of Science* 75: 732–743.

Russell, B. (1927). *The Analysis of Matter*. London: Routledge Kegan Paul.

Ryckman, T. (2005). *The Reign of Relativity: Philosophy in Physics 1915–1925*. Oxford: Oxford University Press.

Saunders, S. (2003a). Indiscernibles, general covariance, and other symmetries. In A. Ashtekar, D. Howard, J. Renn, S. Sarkar and A. Shimony, eds., *Revisiting the Foundations of Relativistic Physics: Festschrift in Honour of John Stachel*. Dordrecht: Kluwer.

Saunders, S. (2003b). Physics and Leibniz's principles. In K. Brading and E. Castellani, eds., *Symmetries in Physics: Philosophical Reflections*. Cambridge: Cambridge University Press.

Saunders, S. (2003c). Critical notice: Cao's 'the conceptual development of 20th century field theories'. *Synthese* 136: 79–105.

Saunders, S. (2003d). Structural realism again. *Synthese* 136: 127–133.

Saunders, S. (2006). Are quantum particles objects? *Analysis* 66: 52–63.

Stachel, J. (2002). 'The relations between things' versus 'the things between relations': The deeper meaning of the hole argument. In D. Malament, ed., *Reading Natural Philosophy: Essays in the History and Philosophy of Science and Mathematics*. Chicago and LaSalle, IL: Open C*ourt.*

Stachel, J. (2005). Structural realism and contextual individuality. In Y. Ben-Menahem, ed., *Hilary Putnam*. Cambridge: Cambridge University Press.

Stachel, J. (2006). Structure, individuality and quantum gravity. In D. Rickles, S. French and J. Saatsi, eds., *Structural Foundations of Quantum Gravity*. Oxford: Oxford University Press.

Stanford, K. (2003) *Exceeding Our Grasp*. Oxford: Oxford University Press.

Swoyer, C. (2000). Properties. Stanford Encyclopedia of Philosophy; http://plato.stanford.edu/entries/properties/.

Van Fraassen, B. (1989). *Laws and Symmetry*. Oxford University Press.

Van Fraassen, B.C. (1991). *Quantum Mechanics: An Empiricist View*. Oxford: Oxford University Press.

Van Fraassen, B.C. (2006). Structure: Its shadow and substance. *The British Journal for the Philosophy of Science* 57: 275–307.

Van Fraassen, B.C. (2007). Structuralism and science: Some common problems. *Proceedings of the Aristotelian Society* 81: 45–61.

van Fraassen, B.C. (2008). *Scientific Representation: Paradoxes of Perspective*. Oxford: Oxford University Press.

Weatherson, B. (2002). Intrinsic vs. extrinsic properties. *Stanford Encyclopaedia of Philosophy.* http://plato.stanford.edu/entries/intrinsic-extrinsic/

Worrall, J. (1989). Structural realism: The best of both worlds? *Dialectica* 43: 99–124. Reprinted in D. Papineau, ed., *The Philosophy of Science*, pp. 139–165. Oxford: Oxford University Press.

Chapter 3
Structuralist Approaches to Physics: Objects, Models and Modality

Katherine Brading

3.1 Introduction

My goal is to develop a structuralist approach to the objects of physics that is realist – but there are obstacles in the way. This paper is about three of them. The first is familiar, having received a great deal of attention in the recent literature, and concerns the suggestion by structural realists French and Ladyman that we should give up talk of objects. This leaves me in the uncomfortable position of being pro-structuralist and pro-realist, but siding with some opponents of structural realism (at least in its ontic form, about which more below) when it comes to objects, so I had better have something to say. In fact I do (see Section 3.4), and I think this obstacle can be moved out of the way. The other two obstacles I have yet to overcome, and the purpose of this paper is to explain what they are, how they arise, and why they are a problem for the structural realist specifically. The resources open to the scientific realist in facing these obstacles are not available to the structural realist, and the reason is the same in both cases.

The latter two difficulties arise in attempting to develop structural realism within the context of the semantic view of theories, and they are challenges that the realist must address in convincing us that his structuralism is indeed a form of realism. I will begin, then, by outlining both a version of the semantic view of theories, modified from the original proposal in three important respects (Section 3.2), and my preferred version of structuralism in this context (Section 3.3). With this in place, I will discuss the first obstacle (concerning structuralism and objects, Section 3.4), and then turn my attention to developing the second and third. These concern the implications of further modifications of the semantic view of theories that are strongly suggested by current physics and by recent work on models (Section 3.5), and the place of modality in structuralist approaches to physics (Section 3.6). As I write, these obstacles stand in the way of my goal of developing a structuralist approach to the objects of physics that is realist. Many of the criticisms that current

K. Brading (✉)
Department of Philosophy, University of Notre Dame, Notre Dame, USA
e-mail: kbrading@nd.edu

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_3, © Springer Science+Business Media B.V. 2011

structural realists face are "global", in the sense that they offer reasons as to why the structural realist project taken as a whole is not viable (e.g. there cannot be relations without relata). My concerns in Sections 3.5 and 3.6 are, in contrast, internal to the project: given that such a project is prima facie viable, what challenges does it face along the way? The obstacles raised in Sections 3.5 and 3.6 fall into this category.

The structural realism of interest here is "ontic" structural realism, as opposed to "epistemic" structural realism, in the following sense. My goal is to develop a structural account of the objects of physics that is complete: no further unknown, or even unknowable, features of objects are part of the content of the theory, or are assumed to be properties of the objects in the world.

3.2 The Semantic View of Theories: Three Modifications

The semantic view of theories arose as an alternative to the syntactic view of theories. According to the syntactic view, a theory consists of two ingredients: an uninterpreted, or partially interpreted, axiom system and correspondence rules or coordinating definitions. These latter are invoked in connecting the theory to the phenomena. The semantic view of theories rejects correspondence rules or coordinating definitions, appealing to "models" instead. Retaining the view that one ingredient of a theory is an axiom system, these models can be understood in the Tarksian sense that they satisfy the axioms of the theory and so are truth-makers (i.e. in each model the sentences of the theory come out true). Moreover, the connection between the theory and the phenomena is made via the models: the models must function not just as Tarskian truth-makers, but also as *representations* of the phenomena.[1]

According to one of the fathers of the semantic view, Pat Suppes, scientific theorizing consists of "a hierarchy of theories and their models" (Suppes 1962, 255; see also Suppes 1960) that bridge the gap between the high level theory and the lower level phenomena (Fig. 3.1). The connection between each level of the hierarchy is made by a relation of *isomorphism* between the models at one level and the models at the next level. An isomorphism is a map that preserves "all the relevant structure".[2] The idea is that this relationship between the models found at the different levels produces a cascade effect, ensuring that the higher level theory applies to the lower level phenomena.

Recent work – by Steven French and his collaborators concerning the appropriate morphisms, and by Margaret Morrison and others concerning the nature and roles

---

[1] The term "model" in science is, of course, replete with connotations of representation, and the temptation might perhaps be for users of the semantic view of theories, with its Tarskian models, to slide between models as truth-makers and models as representations without offering a justification. This is not acceptable, but I will not pursue this issue here.

[2] Of course, this is somewhat vague until we say what is meant by "relevant structure", and that will depend on the case at hand. What can be said in general is that for any given physical theory we will have some handle on what the relevant elements and relations appearing in our models are; the requirement that we can map all these elements at one level of the hierarchy onto the elements at another level, preserving the relations between them, is not trivial.

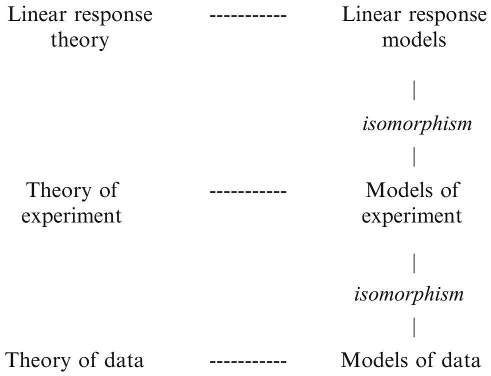

Fig. 3.1 Suppesian possible hierarchy of theories[3]

of models in physics – shows the need for two important modifications to the semantic view of theories. The first modification is the relaxation of the isomorphism requirement such that not *all* the "relevant structure" need be preserved in the mapping from models at one level to models at another level. While French and his collaborators have sought to replace isomorphism with another particular type of morphism (see, for example, Da Costa and French 1990), Elaine Landry and I have argued (2006) that the crucial *philosophical* claim is simply that the models at one level *share structure* with the models at the next level in the hierarchy – we can leave the particular type of morphism, and the type of structures between which we are mapping, open in our general philosophical analysis and investigate it on a case-by-case basis. Making precise the structures and morphisms doing the work may – and in our opinion likely will – depend on features peculiar to a given case. The notion of "shared structure" is weak enough to accommodate this variety, and strong enough to capture the philosophical import of what is common across all cases.

Margaret Morrison (1999), and Morrison and Morgan (1999, Chapter 2), argue persuasively for the partial *autonomy* of "mediating models" in physical theorizing, from both high level theory and from data models. By examining the practice of science they show that there are models which mediate between high level theory and data models, which are not themselves derivable from either the high level theory or the data models. These mediating models share features with both the data models and the high level theory, but are also partially independent or autonomous, this autonomy being crucial to the importance of mediating models in scientific theorizing. I will not rehearse the arguments for this view here: I think they are persuasive and I accept the need for partially autonomous mediating models in our account of scientific theorizing.[4]

---

[3] Based on Table 1 on p. 259 of Suppes (1962).

[4] One line of response might be to assert that mediating models occur only when the related theory is not mature. I side with those who don't think that this is right, but making my case would require detailed discussions of appropriate case studies. While such discussions lie outside the scope of this paper, I recognize this is an as-yet undischarged responsibility.

Morrison and Morgan (1999, Chapter 2) view the partial autonomy of mediating models as counting against the semantic view of theories and the Suppesian hierarchy, claiming that “models are not situated in the middle of a hierarchical structure between theory and world” (p. 17). However, the relaxation of the isomorphism requirement to one of shared structure allows for the partial autonomy of mediating models from high level theory and from data models whilst retaining mediating models within the hierarchy (see Fig. 3.2, below). Thus, I maintain that mediating models are to be included in the hierarchy, and treated within the (modified) semantic view of theories. (We can note in passing, at this point, that including mediating models turns out to require further modification of the hierarchy with important consequences for structural realism that are, in my opinion, problematic; this will be the subject of detailed discussion in Section 3.5, below.)

There is a further reason why mediating models might be thought to count against the semantic view of theories, and this will bring us to the second modification of the semantic view of theories that I support. According to Morrison and Morgan’s account, mediating models are not Tarskian models: there is no theory for which they are truth-makers. Suppes required that at each level of the hierarchy we have a theory, and that the models appearing at that level be Tarskian models of that theory. However, we can again modify the hierarchy to take account of the special character of mediating models by leaving a blank space in the left-hand column of the hierarchy – see Fig. 3.2.

Figure 3.2 shows the two modifications discussed so far. The first modification is that we replace the relationship of isomorphism between the models at the different levels with the more general relationship of shared structure. The second modification is that we insert a level in the hierarchy for mediating models in which

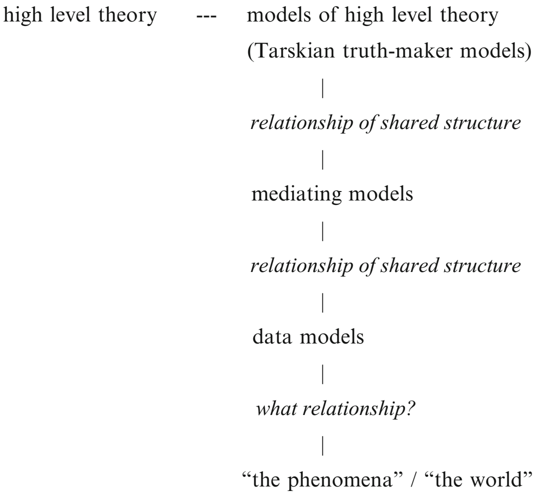

Fig. 3.2 Hierarchy including mediating models

there is a blank space to the left of mediating models – there is no theory for which they function as truth-makers.

Note that I have left open how we connect data models to “the phenomena” or “the world”. I am in search of a realist account, and therefore one according to which we succeed in representing features of the world. If our high level theory is to have a representational function, then this will be achieved in two steps. First, there is the link between the models of the high level theory and the data models, achieved through the hierarchy via the relationship of shared structure. Second, there is the link between the data models and the world, but in this paper I will not discuss how this crucial step in representation is to be achieved, or how and when we may be justified in asserting that it has been achieved.

The third modification that I advocate, in common with many adherents of the semantic view of theories, is dropping the requirement that a theory be axiomatized, or even axiomatizable in principle. Our concern is physics, in which our high level theories are highly mathematized, and presented in terms of equations of motion, or field equations. At the top of the hierarchy let’s put equations and solutions instead of axiomatization and models (see Fig. 3.3, below).[5] We will call the solutions (where generic solutions, in which initial and/or boundary conditions are not specified, are understood as standing for sets of particular solutions)

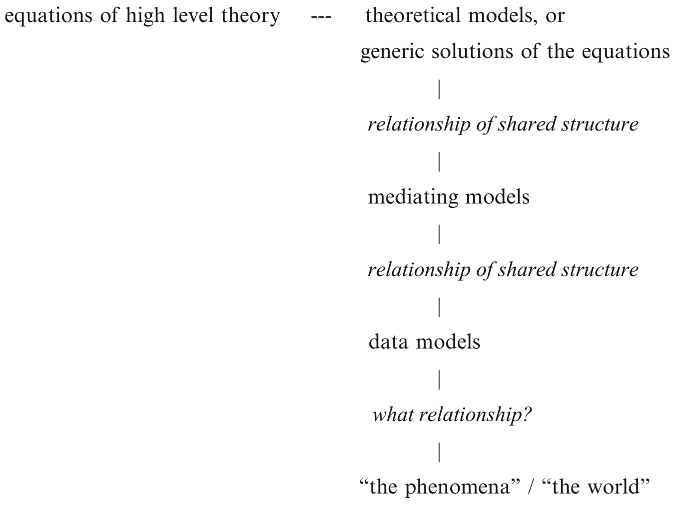

Fig. 3.3 Hierarchy in which the axiomatization requirement is dropped

---

[5] Our theoretical models satisfy our equations, and in this sense might be thought analogous to Tarskian truth-makers. However, we should not take this analogy too seriously, and indeed it has recently been argued that the models of the semantic view are in any case *not* best understood along Tarskian lines. See Thomson-Jones (2006) and Morrison (forthcoming). The account we arrive at may no longer be most aptly termed a “semantic” view of theories, but that is a discussion for another day.

"theoretical models". As an example, consider Newton's laws of motion and his law of universal gravitation. Given these, we can solve the general problem of what orbits are possible for two point masses interacting via the law of gravitation, and indeed using Newton's own argument we can move from point masses to extended spherical bodies of uniform density. Thus, we arrive at the generic solution of the two-body problem. The set of models thus obtained prescribes all and only the possible paths for theoretical Newtonian gravitational objects in two-body motion. These theoretical models are connected to the data models (models constructed from observations of the motions of heavenly bodies) via the hierarchy, in which mediating models serve such purposes as enabling the move from theoretical Newtonian gravitational objects to inhomogeneous extended masses that are not perfectly spherical.

With the semantic view thus modified, we retain the insight that the connection between high level theory and data is to be achieved via *structural relationships* between collections of *models*, these collections forming a *hierarchy*.

3.3 Structuralism and the Semantic View of Theories: Presenting Kinds of Objects

The structuralist finds a natural home within the semantic view of theories, asserting that the relationship of shared structure goes "all the way down" to the phenomena (the empiricist) or to the world (the realist): our theories in physics capture the *structure* of the phenomena or of the world. This needs spelling out in more detail, of course. Here, and in the remainder of this section, I confine my attention to the high level theory and its associated theoretical models. My position is this: the *theoretical kinds of objects* of a high level theory are exhibited in the *shared structure* of the models of that theory. What a theory talks about, at least primarily, is not particular physical objects, but theoretical kinds of objects. We use our theories in physics to talk about physical objects – planets, stars, electrons, atoms, quarks. But our theories are theories of "the electron", or of "G-type stars", or of "the hydrogen atom": what appears in our theories are theoretical *kinds* of objects, not particular objects (such as "*this* electron"). The structuralist view that I advocate claims that the way in which the theory succeeds in talking about these theoretical kinds is via the shared structure of the models of the theory: the shared structure between various models of the theory express the theoretical kinds of objects that are the subject of the theory. With respect to the above example, the claim is that the shared structure of the models constituting the generic solution of the Newtonian equations *present* the *kind* of object "Newtonian inertial-gravitational" for the case of the two-body problem.

I use the term "present" here, rather than "represent", with intent. It is important that we distinguish between theoretical objects and their physical realization. We need to maintain a level of description in which a physical theory can describe electrons, as a theoretical kind, without having to be about electrons as objects that are physically realized in the world: to talk about electrons (or unicorns) is not thereby to bring them into existence as physical objects. I will use the terminology of "presentation" versus "representation" to express this contrast.[6] While a scientific theory *presents* kinds of theoretical objects, it may also seek to *represent* particular physical objects. Indeed, any realist view must surely subscribe to a strong representational role for the theory. But the problems I am concerned with begin even within presentation, and for that reason I am at pains to separate these two roles, and to focus our attention in the first place on presentation. To sum up, the structuralist view I advocate is that the *kinds of objects* that a theory talks about are *presented* via the *shared structure* of the models of the theory.

3.4 Structuralism About Objects

According to the above version of structuralism, theories present kinds of objects. However, French and Ladyman (2003) have argued that we should give up the language of objects and commit ourselves to the ontological primacy of structure.[7] They appeal to two strands of argument, both stemming from "metaphysical individuality". Insofar as I have understood the discussions, the issue of metaphysical individuality concerns whether a particular object can be named such that it may be uniquely re-identified at later times and across possible worlds. There are numerous examples from quantum theory in which the most natural description of the objects involves no commitment to such metaphysical individuality. Paul Teller (1998) has a discussion of this issue where he argues for the superiority, in certain contexts, of the Fock space representation of atomic electrons: we model the electrons in a particular atom using occupation numbers, which are numbers describing how many times each property is instantiated, with no regard to "which" particle has which of the properties (see Teller 1998, p. 128). In other words, we get the *kinds* of electrons, plus the number of electrons instantiating each kind, but no labels enabling us to refer to any one electron in particular – we cannot name the electrons. French and Ladyman's argument is that the interpretation of our theories often fails to *determine* the question of whether the objects involved are individuals or not, and to avoid this "metaphysical underdetermination" (as Ladyman calls it, see his 1998, p. 419) they advocate ontic structural realism because, in committing ourselves to structure rather than to objects, no question can arise over whether the objects of physics are individuals or not.[8]

For French and Ladyman, a realism that commits us to objects but fails to determine the individuality or otherwise of those objects is so strange that they reject it in

---

[6] See Brading and Landry (2006).

[7] This has led to a lot of noise about whether there can be relations without relata.

[8] Other criticisms of the "argument from underdetermination" are to be found in the literature. See, for example, Chakravartty (2003; 2004, pp. 158–160).

favor of a commitment to "pure structure" as ontologically basic. My view is that individuality is distinct from object-hood, and that the "metaphysical underdetermination worry" over individuality can be avoided in a less dramatic-sounding manner. If a theory makes no commitments concerning whether or not the objects it purports to be about are individuals, then it is inappropriate to conjoin a metaphysics of individuals versus non-individuals to that theory. Indeed, the version of structuralism I have described here talks primarily about *kinds*, rather than particular objects, and as such may not entail any commitments as to whether particular objects instantiating the kind are individuals or not. In such a case, requiring that we discuss these objects in terms of metaphysical individuality (and perhaps even commit ourselves one way or another on the matter) demands that we go beyond the content of the theory: we have to add an interpretational layer not warranted by what the theory itself says.[9] Expressed in this way, the alleged "strangeness" of a commitment to objects that is not accompanied by a metaphysics of individuality doesn't sound strange at all – at least not to me.

The suggestion is that we can still say there are objects without this entailing that there is a substantive further issue about whether those objects are individuals or not. But in order to talk about such objects we require a *logical* notion of object, at least: we must be able to apply predicates. The philosophical questions about what conditions an object has to satisfy have long been given a double-sided treatment, having both a metaphysical and a logical face (think of Aristotle's treatment of individuals). On the metaphysical side, an object must be countable[10]; on the logical side it must be capable of serving as an object of predication. According to the view I am advocating, our theories talk about *kinds* of objects. These *kinds* must satisfy the logical condition of serving as objects of predication, but it is a further question whether the physical particulars (the objects) instantiating the kind are logical objects.[11] One point in the debate seems to be the claim that the possibility of *logical* predication depends on appeal to *metaphysically* robust objects – objects that can be named and then re-identified across possible worlds, and over time. However, Simon Saunders (2003a, b) has shown that the logical notion of object, as object of predication, is a weaker notion, requiring only numerical distinctness. He has argued for a version of Leibniz's Principle of the Identity of Indiscernibles on the basis of which the above example of Fock space poses no problem for the

---

[9] French and Ladyman (2003) go to strenuous lengths to resist the idea that we can continue talking of objects, simply reconceiving them as nothing more than the relations in which they stand, but I still take it that this is because the objects of physics fail to satisfy conditions which they assume are associated with object-hood. I don't find a conclusive argument here.

[10] This need not be a sharp criterion for when an object is present.

[11] In other words, I am leaving open the possibility that we can apply predicates to the theoretical kinds of objects without it following that the resulting statements can be re-expressed using a universal quantifier over particular theoretical objects that are the members of the kind. Relatedly, I am not yet convinced that the physical objects instantiating the kind must have theoretical counterparts that are *logical* objects, or that this is necessary in order for us to be able to represent and talk about the world. But I don't have arguments spelling out these latter points.

logical notion of object, because it admits two-place relations that cope with numerical distinctness for otherwise identical objects: "x is one meter away from y" (for example) gives numerical distinctness by failing to be true when $x = y$ and being true when $x \neq y$. He writes (2003b, p. 294):

> Consider the spherically symmetric singlet state of two indistinguishable fermions. Each has exactly the same mass, charge, and other intrinsic properties, and exactly the same reduced density matrix. Since the spatial part of the state has perfect spherical symmetry, each has exactly the same spatiotemporal properties and relations as well, both in themselves and with respect to everything else. But an irreflexive relation holds between them, so they cannot be identified (namely '... has opposite component of spin to...').

In Saunders' terminology, they are weakly discernible. Weak discernibility is indeed weak: we cannot refer to one of the two objects in preference to the other. Nevertheless, we can state of the pair that there are two objects, and we can make assertions concerning the properties of those objects. Thus, these objects serve as objects of predication, in the weakened sense given by Saunders' analysis, enabling Saunders to draw the following conclusion (2003a, p. 131):

> I think they [French and Ladyman] are mistaken in their view that failing transcendental individuality, the very notion of object-hood is undermined by particle indistinguishability in quantum mechanics... It is true that from exact permutation symmetry it follows that such particles... may in certain circumstances not be uniquely identifiable, in the sense that it may not be possible to refer to one member of the collective rather than another. But it does not follow, from logical principles, that such particles cannot be objects of predication. Indeed they can...

In short, on the basis of the metaphysical and logical considerations that French and Ladyman bring to bear, it is not clear to me why we should give up objects and move to "pure structure". They are right to see interesting metaphysical implications in modern physics for the concept of "object" – we need a weaker notion of "object" than we are used to dealing with. But they are wrong that no such notion is to be had and that we must give up talking about objects and instead talk only about structure.

In the absence of further arguments, I shall assume that the possibility of developing a structural construal of object-hood has not yet been ruled out.

3.5 Realism and the Semantic View of Theories

We come now to an obstacle that I don't know how to surmount. My purpose is to explain how it arises, and also why it appears to be a problem for the structural realist, while the scientific realist can address the issue with comparative ease. To this end, I begin by discussing a further modification of the semantic view, which leads to "a proliferation of models". I then discuss the relationship between realism (both scientific and structural) and the semantic view of theories. In the light of the "proliferation of models", scientific realists have a fairly straightforward response, whereas structural realists face a serious problem.

3.5.1 A Proliferation of Models

Recent work indicates that further modifications to the semantic view of theories, in addition to those described above (see Section 3.2), should be made. Morrison and Morgan (1999) have argued that an important feature of physical theorizing is that *different* mediating models, relating to the *same* theory, may be used for different purposes. Or, transforming their claim into the language of the view that I am advocating: mutually incompatible (collections of) mediating models may share structure with the same collection of theoretical models.[12] Therefore, the hierarchy must be widened as we go down as follows (Fig. 3.4).

The relationship of shared structure accounts for the applicability of high level theory to mediating models and vice versa, and the extent of that applicability.

Morrison (1999) has further shown that there is similar diversity in how data models relate to mediating models, and has argued persuasively that this is necessary to the way that high level theory applies to data models. Once again (as in Section 3.2), I accept Morgan's and Morrison's conclusions, and refer the reader to their work for the supporting arguments. My concern here is to elaborate the consequences of these conclusions for structural realist projects.[13]

In order to take account of this in considering the relationship of mediating models to data models, we must allow that mutually incompatible mediating models (a) may share structure with the same data models, and (b) may not share structure with *all* of the same data models (Fig. 3.5):

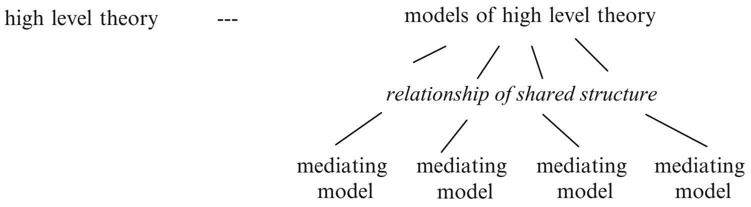

Fig. 3.4 Hierarchy with proliferation of models (I)

---

[12] Moreover, there may be structure important for certain purposes that is shared by certain mediating models and a *subset* of the theoretical models. What shared structure counts as relevant is dependent on the purpose for which we are using the theory and the models. I will not discuss this further here.

[13] As noted above, French and his collaborators seek to replace isomorphism with a weaker type of morphism (arguing for this in their "partial structures" approach), but reject the generalizing move to "shared structure", where the type of morphism is left unspecified at the general philosophical level and specified only in relation to specific examples. Nevertheless, they too will have to take into account the structural incompatibilities between collections of models within a single hierarchy. I think that the problem I am discussing here affects all structural realism projects once the isomorphism requirement is relaxed (as it needs to be).

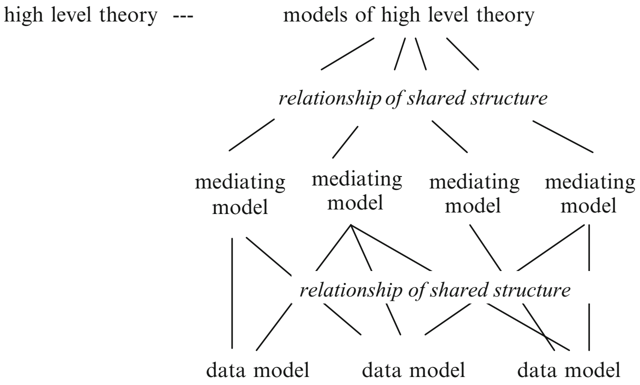

Fig. 3.5 Hiearchy with proliferation of models (II)

This proliferation of models, including incompatible mediating models, poses a serious problem for the structural realist, but not for the scientific realist. In order to see why, we need to compare the commitments made by each.

3.5.2 Scientific Realism

Drawing on van Fraassen (1980, p. 8), we might characterize scientific realism as the endorsement of the following two claims:

Scientific realism

(1) Science aims to give us, in its theories, a literally true story of what the world is like.
(2) We have good reason to believe that science is successful with respect to this aim.

Notice that this characterization of realism makes no reference to the hierarchy of theories. In Suppes' version of the semantic view, each level of the hierarchy consists of a theory and its associated models. However, in our modified version, we have dropped the theory from every level except the top, leaving ourselves with a hierarchy of models from there down. That is, we have the high level theory and its associated models at the top, and then the layers below are occupied by collections of models (mediating models, data models) standing alone. We are therefore faced with a choice concerning the term "theory" in the above characterization of scientific realism: either it refers to the high level theory, appearing at the top of the hierarchy, or it refers to the entire theoretical structure associated with the hierarchy.

The latter interpretation is problematic in the light of the "proliferation of models" discussed above: the models that make up the hierarchy contain mutual incompatibilities, and so some fancy footwork (at least) will be needed in order to extract (preferably in a non ad hoc manner) a logically self-consistent story that could serve as a candidate for a literally true story of what the world is like (as required by the above characterization of scientific realism).

However, if we adopt the former interpretation, then the content of our realist beliefs concerning the nature, make-up and structure of the world is given by the content of the high level theory and its associated models *alone*. Thus, on this view, the problem of mutual inconsistencies raised by the proliferation of models at the lower levels does not infect the content of our realist beliefs. When we are realist about the high level theory "quantum mechanics", and believe that the world indeed contains electrons (say) as described by quantum mechanics, the content of this belief is fixed by the high level theory and its models. The role of the data models and mediating models is to make the link between high level theory and the world, such that we are justified in our claim that our beliefs (expressed using the top level of the hierarchy, i.e. the high level theory and its models) are *about* the world. Expressing things another way, according to this approach the realist who subscribes to the semantic view of theories believes that there is *a model of the high level theory that accurately represents the world* (or some subsystem of it), and the intermediate levels of the hierarchy become "transparent" when it comes to the *content* of the realist's beliefs about the world.[14]

The remaining challenge is to justify the "transparency of the hierarchy". The scientific realist achieves this by appealing to the objects (and their properties) that are the subject of his realist claims. For him, kinds of objects are characterized by the high level theory as a whole. Having been characterized, these kinds of objects then appear in the particular models,[15] and may be traced up and down the hierarchy. For example, the label "electron" attached to a trajectory appearing in a model of the high level theory *means the same thing* as the label "electron" attached to a trajectory appearing in a mediating model or data model: the different levels of the hierarchy deal in the same kinds of objects (as characterized by the high level theory). Herein lies the key to the scientific realist's justification of the "transparency of the hierarchy". The kinds of objects appearing in any model at any level of the hierarchy are labelled *as that kind* from resources *outside* the model (and for mediating and data models, from outside that level of the hierarchy altogether). Therefore, it is legitimate to point to objects in a model of the high level theory and call them electrons (say), and then trace (with further pointing) the presence of these objects (or rather, their trajectories) down through the hierarchy to the data models (and, so the realist hopes, into the world[16]).

---

[14] This approach affords a special status to the models associated with the high level theory, and to the high level theory itself. It might be argued that this is a benefit of the approach. See, for example, Morrison (forthcoming), who argues that with the rise of the semantic view of theories and its emphasis on models we risk losing sight of the special role played by theory.

[15] It is compatible with this claim that the high level theory be *identified* with a collection of models, as some advocates of the semantic view of theories maintain.

[16] As noted above (see Section 3.3), representation is carried out in two steps: first the link between the models of the high level theory and the data models; second the link between the data models and the world. I will not discuss the second step here.

The scientific realist accepts that versions of ‘the same’ object appearing in different mediating models may have incompatible characteristics. Nevertheless, he asserts that these different versions of the same object are understood as different idealizations or approximations – different models – of the same *fundamental kind* of object, where that fundamental kind is characterized by the *high level theory* alone. Thus, he can believe that a model of the high level theory represents the particular physical system he is interested in, and justify that belief in part through appeal to the hierarchy, and nevertheless maintain that the *content* of his belief depends in no way on the lower levels of the hierarchy. “Transparency of the hierarchy”, when it comes to our beliefs about electrons (say), is achieved.

To sum up: the challenge posed by the “proliferation of models” can be overcome by the scientific realist as follows. The objects of the scientific realist are characterized by the high level theory, independently of any particular model in which they appear; and the scientific realist can therefore point to electrons (or their manifest traces) whichever model they occur in, without being in any way troubled by this proliferation of models. We are, quite simply, realist about electrons (say), as characterized by our high level theory.

3.5.3 Structural Realism

How does the structural realist fare in the face of the “proliferation of models”? Modifying the above characterization of scientific realism, we can arrive at a version of structural realism in the style of French and Ladyman:

Structural realism

(1) Science aims to give us, in its theories, an accurate representation of the structure of the world.
(2) We have good reason to believe that science is successful with respect to this aim.

A structural realist about a given theory maintains that there is a model of this theory that accurately represents the structure of the world.[17] We are interested in the structural realist who adopts the semantic view of theories, in the revised form that I have discussed. Suppose that this structural realist adopts the same strategy as the scientific realist, so that we interpret “theory” in the above characterization as picking out the top layer of the hierarchy. This leads again to the request for an account of the “transparency of the hierarchy”, where here the issue concerns the claim that the *content* of the structural realist’s beliefs depends on the structure of the models of the high level theory *only* (while the justification for realism depends in part on the entire hierarchy).

---

[17] Or, if you prefer, for any sub-system of the world there is a model of the theory that accurately represents the structure of that sub-system.

The structural realist accounts for the "transparency of the hierarchy" in a different way from the scientific realist. In the simplest case, isomorphism between the models at the different levels ensures the sought "transparency" through the "cascade effect", explained above (see Section 3.2): the structure of the theoretical model in question is isomorphic with that of the mediating model, which is in turn isomorphic with that of the data model.

However, according to the version of the semantic view of theories described above (also in Section 3.2), the relationship of isomorphism between the different levels of the hierarchy should be relaxed to one of "shared structure", of which isomorphism is a special case, but which includes weaker relationships. The inclusion of these weaker relationships means that the structural realist must demonstrate the relationship of shared structure on a case-by-case basis, showing that the relevant structure from the high level theoretical model (about which we are supposed to be realist) transfers down the hierarchy appropriately.[18] The task is more arduous than was the case for strict isomorphism, but nothing of principle has changed. When it comes to our beliefs about the structure of the world, the intermediate levels in the hierarchy between the high level theory and the world are rendered transparent by tracing shared structure from the high level theory all the way down the hierarchy: our realist beliefs are expressed in terms of the structure of the high level theory (or its models) alone.[19]

The problem comes when we ask the structural realist to meet the challenge posed by the "proliferation of models". The solution suggested above for the scientific realist is not available to his structural counterpart. The structural realist has been at pains to *reject* objects with the kind of independent characterization that the scientific realist relies upon to overcome the problem of "proliferation of models" and render the hierarchy transparent. The structural realist exhorts us to be realist about the structure of a particular model of our high level theory, claiming that it shares structure with how the world really is. In the face of the proliferation of models, it is unclear how the structural realist can justify the "transparency of the hierarchy" needed for his realist beliefs in the models of the high level theory. Structurally incompatible mediating models, each bearing a relationship of shared structure to a given model of the high level theory, and each being required for the application of that theory to data models, result in a proliferation of incompatible structures at the lowest level of the hierarchy. In the face of this lack of uniqueness, what does it mean to be realist about the structure of a particular model of our high level theory? While the proliferation arises at the level of presentation, it poses a problem for the claim that representation has been achieved: the absence of a unique structure "cascading" down the hierarchy poses a threat to the claim that the structural realist has, in his

---

[18] Note that this applies equally to the version of structural realism favored by French and his collaborators.

[19] Demonstrating the utility of the notion of shared structure requires the elaboration of detailed case studies, and that will not be carried out here. My concern in this paper is rather with highlighting one of the (problematic) consequences of moving to this more general notion, one that arises as a matter of principle even before the elaboration of case studies.

high level theory, “latched onto” *the* structure of the world.[20, 21] There is a richness in the hierarchy that the scientific realist can accept (or so I have argued), but which threatens to destroy the realism of the structuralist.

In fact, the problem gets worse. McAllister (see especially his 1997) has argued that there is ineliminable human choice in how the data models are structured: there is always an element of freedom in how we separate the data into pattern versus noise. Suppose that we have got as far as making measurements with outcomes plotted as dots on a graph.[22] There is always an element of freedom in how we draw a line through those points – in what counts as a pattern in the dots and what counts as noise. So different data models are compatible with the very same phenomena.[23] The problem that this, and everything discussed in this section, points to is the following: in giving up on isomorphism and allowing a proliferation of structures, it is no longer clear what it means to say that our higher level theory accurately represents *the* structure of the world.[24] And in the absence of this, it is no longer clear that we have anything that might reasonably be called *structural* *<u>realism</u>*.

---

[20] This is prior to, and independent of, any attempt to give a specific account of representation.

[21] In Section 3.5.1, above, we noted that one might insist that in the characterization of realism the term “theory” applies to the entire hierarchy, rather than to the high level theory alone, thereby avoiding the need to account for the “transparency of the hierarchy”. We are now in a position to see why this will not help the structural realist. What could it mean to be realist about incompatible structures?

[22] For now we ignore the enormous amount of work that gets us even to this point, since it is possible to construe this work as discovery of pre-existing quantities.

[23] Separating pattern from noise is not the problem of induction: so construed, it would assume that there is a correct separation (depending perhaps on the completed history of the universe) that – if we hit upon it – would remove the incompatibility between data models. I take McAllister’s point to be that the element of human choice is ineliminable *in principle*, not just in practice.

[24] Notice that this problem of different data models being compatible with the same phenomena poses a problem not just for the structural realist but also for the structural empiricist. Paralleling our characterizations of scientific realism and structural realism, we might attempt to characterize structural empiricism as follows:

Structural empiricism

(1) Science aims to give us, in its theories, an accurate representation of the structure of the phenomena.
(2) We have good reason to believe that science is successful with respect to this aim.

I reject both the above realist and empiricist versions of structuralism for two reasons. First, I think that we have good reason to suppose that there is no such thing as *the* structure of the world or *the* structure of the phenomena. (I offer support for this claim with my argument for objects of physics as objects FSPP (“for some practical purposes”), but I will not rehearse this argument here.) Second, I think that set up like this both the realist and the empiricist project are doomed to failure because of the problem of representation – the problem of what justifies us in believing that our theories represent the world or the phenomena. In the realist case, we will have only the “no miracles” intuition to justify our claim that our theories represent the structure of the world, and in the empiricist case it seems that in order to achieve representation van Fraassen (for example) ends up collapsing the distinction between data models and the phenomena, which it seems to me collapses empirical adequacy of a theory with respect to the phenomena into organizational adequacy of the theory with respect to the data models.

3.6 Realism and Modality

The third and final obstacle to structural realism that I will discuss here concerns modality. Ladyman (1998) indicates that the representation of modality plays a constitutive role in making his structuralism a structural *realism*: it "*must* go beyond a correct description of the actual phenomena to the representation of the modal relations between them" (p. 418, my emphasis). In emphasizing modality Ladyman follows Giere (1985), who states that "the crucial dividing line between empiricism and realism" concerns the status of modality, and urges that representing the world as modal is essential to the explanatory resources that the realist wishes to appeal to in explaining the actual. However, I think that there is an obstacle to the structural realist attempt to represent the world as modal. In this section I explain what the problem is, and I offer one doorway that might lead to a solution.

According to the semantic view of theories, a theory successfully represents a physical system if there is a model of the theory that "sufficiently resembles" the system in question. It seems, therefore, a prerequisite for *representing* modal properties of a physical system that these modal properties be captured by the associated model. This sounds banal, but my claim is that the structural realist faces a problem here, one that does not arise for the scientific realist.

In scientific realism the properties of the elements of a model are given by appeal to the wider theory, as discussed above (in Section 3.5), and these can include modal properties (perhaps via the claim that the theory describes "natural kinds", such as electrons (say)). Suppose we have a theoretical model of the hydrogen atom, containing one electron and one proton. We label these elements "electron" and "proton", and the properties of these elements are given not by the model alone, but by the theory as a whole. For example, the theory stipulates the allowed orbits for an electron bound to a proton, thereby ruling on aspects of what is possible and impossible for the electron. The theoretical electron in our model can, if we wish, be described in terms of these modal properties, and there is no immediate obstacle to the claim that modal properties are included in the content of the model.

My argument is that the situation is different in structural realism. Putting the above characterization of successful representation into structural terms, *representation* is achieved by a relationship of *shared structure* between a specified model of the theory and the physical system in question. (This is not a complete account, of course, but it is a necessary condition.) When we ask whether this model contains modal information, a great deal hinges – for the structuralist – on whether by "model" we mean generic or particular model, as I shall now argue.

Very often, when we are modelling a (closed) system, we are seeking to capture how that system is likely to behave given a range of possible initial and boundary conditions. In the example of Newtonian gravitational mechanics, we are seeking a generic solution of our equations (for whatever system is in question). In this sense, our "model" contains modal information: it contains a range of possible trajectories for the system, specifying what the system can and must do, corresponding to a range of initial and boundary conditions. The modal information is carried not by one particular solution/model/trajectory, but by the generic solution/model or collection of trajectories. One option for the structuralist might therefore be to assert that he is realist about the generic solution. But what does this mean? In any given case, only one of the trajectories is in fact realized. We can be structural realist about *this* solution by claiming that it accurately represents the structures and relations associated with the actual trajectory of the physical system in the world. But what does it mean to say that we are realist about the wider structure of the generic solution? In what way are the other, non-realized, trajectories structurally present in the world? I will return to this issue below (see Section 3.6.2), but first I want to pursue the alternative, and interpret "model" not as generic solution, but as particular solution.

3.6.1 Particular Models, and the Place of Modality

Let us assume that the structure that plays a representational role is the structure of *one particular model* of the theory, and not the structure of the generic model or of the wider theory. However (and this is the key point), *this structure, in and of itself, contains no modal information*. In the case of the scientific realist, we have an account of the relationship between the theory and its models that allows the modal commitments of the theory to be present also in the individual models. When it comes to the structural realist, on the other hand, we lack an account of the role of the wider theory, especially with respect to representation; I will discuss this in more detail in what follows. For now, the point is this: unlike the understanding of models used by the scientific realist, within the structuralist conception of a single model *there is no modality* present. And so, trivially, that model by itself cannot *represent* modality in the world. In short, the model describes *only* the *actual* structure of the physical system. We have arrived at something we might dub "structural actualism".

In what follows, I will attempt to clarify the place of modality in a structural actualist account of scientific theories, and thereby determine what is needed to move beyond it to modal structural realism. As introduced above, structural actualism appeals to a single model of the theory. An obvious place to start, therefore, is by looking for an account of what role the rest of the theoretical structure plays. What the structuralist cannot do is implicitly assume that the wider theoretical structure (a) has a representational role, and (b) is appropriate and adequate for the representation of modality. We are owed an explicit account of both these things.

Recall that a theory is characterized by a collection of models. As a result, not all the relations appearing in any given model have the same status with respect to the theory: some relations are part of the *shared structure* of this collection of models, and some are not. We may say that relative to a theory some features of a particular model are "necessary", in that they are part of the shared structure of the collection of models, and the remaining features of that model are "contingent". Thus, by appeal to the theory as a whole, characterized by a collection of its models, we can say more about the relations holding in the specific model of interest – which ones are necessary with respect to the theory, and which ones are contingent. (I will illustrate this with an example within the setting of my preferred version of structuralism; see below.)

I have argued that for the structuralist, modal information is contained in the shared structure of a collection of models, and that we can use this shared structure to talk about a particular model, distinguishing features of the model that are necessary with respect to the theory from those which are contingent. In this sense, the content of our theory allows us to say more about a particular model than is contained in that model. But this does not imply that the structure of any particular model contains modal content and can represent modal properties of a physical system through a relationship of shared structure. On the contrary, to reach that stronger modal realist claim, we would have to add further argument.

Let us be clear about this: if the structuralist is to make representational claims concerning modality, then he owes us either (a) an account of how modality is transferred from the wider theory to a single model, or (b) an alternative account of representation in which the wider theory plays a more direct role (i.e. in which it is not the case that representation is achieved through a relationship of shared structure between an individual model and the physical system in question).

We must first decide whether to attempt a form of modal structural realism – Ladyman's current project – or to follow van Fraassen (1980, pp. 196–203) in maintaining that modality belongs to theory, not to ontology.[25] Before adjudicating on whether to locate modality at the theoretical or the ontological level, I first want to motivate a little further why modal considerations are important. The theories that we have are not complete; we do not have a theory (or set of theories) for which there exists a model (or set of models) that we know to represent the actual world in its entirety. Far from it. Our commitment to a theory guides our actions, it guides our choices about how to act in the face of the unknown course of future events. Our commitment to a theory is equivalent to our placing a restriction on our beliefs about what will and will not happen based on what *theoretically* can, cannot, and must happen. This, I submit, is the main import of the modal features of our theories, and it is one on which the realist and the empiricist can agree. For the purposes of guiding our actions it is sufficient to maintain our modal discourse at the theoretical level, without making any commitment to modality at the ontological level.

My next move is to explain how modality at the theoretical level is captured within the version of structuralism that I advocate, and I will then return to the question of whether structuralists can also turn their modal claims into representational claims.

According to the version of structuralism that I advocate, the shared structure of a collection of models of a theory presents the *kinds of objects* that the

---

[25] See Ladyman (2000, 2004) for his criticisms of van Fraassen's position on modality.

theory talks about. These kinds stipulate the possible trajectories for objects instantiating the kind. With respect to the theory, the kind gives necessary and sufficient conditions for a particular object to be a member of that kind – these are modal conditions, they stipulate what an object can and must do if it is placed in certain conditions.[26] Recall the Newtonian-gravitational example I used earlier. Given Newton's laws of motion and his law of universal gravitation, we can solve the generic two-body problem. These solutions are models of the theory, and they prescribe all and only the possible paths for Newtonian inertial-gravitational objects in two-body motion. In doing so, they thereby present the kind of object that the theory talks about (viz., "Newtonian inertial-gravitational"). With respect to each particular solution of the equations, there will be features which are due to the objects being instantiations of this kind, and features which are peculiar to that model. The former are necessary with respect to the theory, and the latter contingent, as described above. As this example re-emphasizes, modality is a feature of the *collection* of models of the theory, and not of any particular model of the theory. According to the view that I am advocating, modality is *presented* through the *shared structure* of the models of the theory. Thus, in the first instance, modality concerns presentation and not representation; it is associated with how we present the theoretical kinds of objects that the theory *talks about*, and not with how we represent the particular physical objects that the theory purports to *be about*.[27]

How should the structural realist respond to this account of modality? One option would be to reject Giere's claim that modal realism is central to realism with respect to scientific theories, underwriting the explanations offered by realists, and to offer instead an actualist account, insisting that it is realist. I will not pursue the issue of whether "structural actualism", as I referred to it above, can be construed as a realist position. In what follows I will use the term "structural actualism" neutrally, and will reserve the term "structural realism" to include a commitment to modal realism. The structural actualist can endorse the above account, placing modality at the level of presentation, rather than representation. The emphasis on shared structure and kinds of theoretical object enables him to correctly locate the modal character of structuralism at the theoretical level and to explain in detail what it consists in. Since we don't know what the actual is going to be, we need our theories to be modal. We don't thereby commit ourselves to anything modal in the world, we are not committed to some kind of modal realism. So argues the structural actualist.

One line of argument that a modal realist might offer, in attempting to move from structural actualism to structural realism, is to say that it would be a miracle that this modal guide to action worked if there was no modality in the world. (From

---

[26] Note that in practice, for the theories that we have and are developing, the specification of necessary and sufficient conditions may not be complete, there may be consistency problems, and so forth.

[27] I follow E. Landry in my use of this terminology.

what I have heard about the approach Ladyman is developing, this is a line of argument he would endorse, but there is nothing in print by him on this at the moment, so far as I know.) But that's a bad inference: the actualist could claim that his commitment to the theory is a commitment to the view that the one of the models of the theory accurately represents the actual world, and so we can use the theory to rule on what the system actually will and will not do by means of what is possible/impossible at the level of theory (counterfactuals belong in the theory). I think the actualist is right that there is nothing miraculous here, and the "no miracles" line of argument is insufficient to move the position from structural actualism (asserted with respect to a particular model of the theory) to structural realism.[28] We are back to structural actualism.

But Ladyman is seeking structural *realism*, so are there any alternatives? On the basis of the discussion I have given here, Ladyman has the following two premises available:

(P1): *An individual model* of a theory *represents* the particular physical system in question through a relationship of shared structure.

(P2): *Modality* is a feature of a *collection of models*, deriving from their shared structure, and is *not* a feature of any individual model.

Our desired conclusion is

(C): Our theories *represent* the modal properties of the world.

Clearly, this argument is invalid. What is needed is a modification of one of the premises. For example, if we could modify (P2) such that modality is a feature of an individual model, then the argument becomes valid. However, my point is that the structural realist cannot assume this to be the case, he must give us an account of how an individual model could come to be imbued with modal content. How best to modify this argument for the representation of modality, and how to justify the modifications, is the third and final obstacle to structural realism discussed in this paper.

3.6.2 Generic Models, and the Place of Modality

I have argued for and endorse (P2), and so my conclusion is that structural realism requires the rejection of (P1): if structuralists are to represent the world as modal, representation must be something more than a relationship of shared structure between a particular model and physical system. Let us return to the alternative that we discussed briefly at the beginning of this section: could we be structural realists

---

[28] This is not to say that structural realism is an untenable position, when incorporating modal realism, but only that the arguments for structural realism do not get us to such a position: additional arguments relating explicitly to the modal claim need to be supplied.

with respect to the generic model, i.e., with respect to a structure that contains a range of possible trajectories of the system? *Prima facie*, this seems an odd claim: a particular trajectory is realized by a system, that trajectory shares structure with a particular model, and I am realist about that structure (no problem so far); but in addition it is meaningful to assert that the system shares structure with the other non-realized but possible trajectories, and I am realist about this structure too. Because of this oddness, it seems to me that the structural realist is obligated to fill out some details of these additional claims.

The version of structuralism that I have outlined in this paper allows for this possibility, by suggesting a route for moving from structural actualism to structural realism. The structural actualist asserts that there exists a particular model of the theory that shares structure with the physical system in question. The position I have outlined here requires something more of the structural actualist: for a theory (and not just a model) of that system to be acceptable, there must also be shared structure between the physical system and the *collection* of models, so that the objects of the physical system are displayed to behave in accordance with the theoretical kinds (i.e. the kinds given by the shared structure of the collection of models that characterize the theory). The structural actualist, as we have said, answers what it is for an object to be an instance of a given kind *not* by claiming that it somehow *has* all the modal properties expressed by the theoretical kind, but merely by asserting that its actual behaviour is consistent with the prescriptions deriving from the theoretical kind. But a door has also opened for the structural realist: if the *kinds* that the theory presents are also what the theory *represents*, then the way is clear for the objects instantiating a given kind to inherit the modal properties associated with that kind. In other words, this is one way in which representation of a given physical system might draw not just on a particular model, but on the theory as a whole. Clearly, however, this requires that the structuralist provide an account of representation that differs from (P1), and also from the brute claim that the *generic* model represents the structure of actual systems.

My conclusions are as follows. Insofar as Giere is right that modal realism is essential to the explanations that realists seek to offer, the standard approach to structural realism cannot get beyond structural actualism. The view that I advocate indicates a possible escape route, but it is one that is unlikely to be palatable: a great deal of the appeal of structural realism lies in its enchantingly simple claim that an individual model of a theory represents a particular physical system through a relationship of shared structure. Either way, modality is an obstacle to structural realism that is yet to be overcome.

3.7 Concluding Discussion

The above problems associated with (1) the “transparency of the hierarchy” combined with “the proliferation of models”, and (2) modality, have both been derived using a characterization of structuralism according to which there is *a* model of the theory that "shares structure" with the physical situation in question. On this approach, structuralism at the theoretical level collapses into relationism at the ontological level: the structure of the model displays the relations obtaining in the physical situation. Call this structural relationism. Structural relationism is compatible with, but not demanded by, my preferred version of structuralism given in Section 3.3. According to this view, structuralism enters first and foremost when discussing not an individual model of the theory but the *shared structure* of the models of the theory. The central claim is that the theoretical kinds of objects that a high level theory talks about are exhibited by the shared structure of the models of that theory. The example that I gave is of Newtonian gravitational physics, where the models (or, in this case, the generic solution of the Newtonian equations, for the two-body problem) present the theoretical kind of object "Newtonian inertial-gravitational" (for the case of the two-body problem). This is a structuralist construal of the *kinds* (perhaps the "natural kinds") that a theory talks about.[29] It is consistent with structural relationism, but other accounts of how a structuralist construal of kinds should be interpreted with respect to our representation of the particular phenomena are possible. This may be a loophole through which, one way or another, it is possible escape from the problems discussed in this paper.[30]

Acknowledgements Versions of material in this paper were presented at the Boston Colloquium for Philosophy of Science meeting on "Structural approaches in philosophy of physics" (February 2005), the University of Santa Clara conference on "The Metaphysics of Physics: Philosophical Implications of Contemporary Physical Theories" (May 2006), and at the Banff Workshop on Structuralism (August 2007). I am grateful to the participants at these meetings for their comments. I am also grateful to Elena Castellani, Elaine Landry, Margaret Morrison, Antigone Nounou, and Zanja Yudell for their comments on various aspects of this work. This paper makes extensive use of the "methodological structuralism" developed with Elaine Landry (see Brading and Landry 2006). My thanks also go to the editors of this volume. This paper was completed during research leave (2007–2008) supported by the National Science Foundation (NSF SES–0724383 Brading 201201), project title 'Structuralist Approaches to Physics'.

---

[29] Chakravartty (2004) has argued for a structuralism grounded in causal properties, in which he emphasizes the distinction between the nature of entities and the structural relations obtaining between those entities. On the view advocated here, the nature of entities (as members of kinds) is characterized in structural terms. Marrying the two approaches, the conclusion would be that it is not merely that our knowledge of the causal properties of entities is structural (knowledge of relations), as Chakravartty argues, but further that the ontological ground of these causal properties is itself structural. Working out the details of how this would go is a task for another day.

[30] How the loophole might work for (2) has been indicated above. In the case of (1), a first step would be to claim that the shared structure of the collection of models characterizing the high level theory gives sufficient conditions for an object to instantiate a kind, but that the necessary conditions need more careful handling in the face of the proliferation of models.

References

Brading, K., and Landry, E., 2006, Scientific Structuralism: Presentation and Representation. *Philosophy of Science* **73**, 571–581.

Chakravartty, A., 2003, The Structuralist Conception of Objects. *Philosophy of Science* **70**, 867–878.

Chakravartty, A., 2004, Structuralism as a Form of Scientific Realism. *International Studies in the Philosophy of Science* **18**, 151–171.

Da Costa, N., and French, S., 1990, The Model-Theoretic Approach in the Philosophy of Science. *Philosophy of Science* **73**, 248–265.

French, S., and Ladyman, J., 2003, Remodelling structural realism: quantum physics and the metaphysics of structure. *Synthese* **136**, 31–56.

Giere, R., 1985, Constructive realism, in P. M. Churchland and C. A. Hooker (eds.), *Images of Science*, 75–98. Chicago: Chicago University Press.

Ladyman, J., 1998, What is structural realism? *Studies in History and Philosophy of Science* **29**, 409–424.

Ladyman, J., 2000, What's Really Wrong with Constructive Empiricism? Van Fraassen and the metaphysics of modality. *British Journal for the Philosophy of Science* **51**, 837–856.

Ladyman, J., 2004, Constructive Empiricism and Modal Metaphysics: A Reply to Monton and van Fraassen. *British Journal for the Philosophy of Science* **55**, 755–765.

McAllister, J., 1997, Phenomena and Patterns in Data Sets. *Erkenntnis* **47**, 217–228.

Morrison, M., 1999, Models as Autonomous Agents, in M. Morgan and M. Morrison (eds.), 1999, 38–65.

Morrison, M., forthcoming, 2007, Where have all the theories gone? to appear in *Philosophy of Science* 74(2);195–228.

Morrison, M., and Morgan, M., eds., 1999, *Models as Mediators: Perspectives on Natural and Social Science*. Cambridge: Cambridge University Press.

Saunders, S., 2003a, Structural Realism, Again' *Synthese*, **136**, 127–133.

Saunders, S., 2003b, Physics and Leibniz's Principles, in K. Brading and E. Castellani (eds.), *Symmetries in Physics: Philosophical Reflections*. Cambridge University Press, 289–307.

Suppes, P., 1960, A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences. *Synthese* **12**, 287–301.

Suppes, P., 1962, Models of Data. *Logic, Methodology and Philosophy of Science*. Stanford University Press, Stanford.

Teller, P., 1998, Quantum Mechanics and Haecceities, in E. Castellani (ed.), *Interpreting Bodies: Classical and Quantum Objects in Modern Physics.* Princeton University Press, 114–141.

Thomson-Jones, M., 2006, Models and the Semantic View. *Philosophy of Science* **73**, 524–535.

van Fraassen, B., 1980, *The Scientific Image*. Oxford: Oxford University Press.

Chapter 4
Mathematical Structural Realism

Christopher Pincock

4.1 Varieties of Realism

Unrestricted or global scientific realism is the view that we should take seriously the whole content of empirically successful scientific theories. This attitude requires us to believe that the theoretical claims of the theory are true, or approximately true, and that scientific progress consists in increasing the scope and accuracy of these theories. A series of devastating objections to this position has been developed based on an examination of both the history and practice of science. On the history side, it is arguable that a majority of empirically successful scientific theories are not anywhere near approximately true as we now have evidence that the entities they posited do not exist. The practice of contemporary science raises different and more subtle concerns. Here we find scientists engaging in a wide array of seemingly ad hoc techniques of idealization and approximation. This suggests that we cannot explain the success of our theories by appeal to their truth as the assumptions deployed in the application of these theories have little bearing on the truth of the theoretical claims made by the theory.

Most scientific realists, then, have shifted to a more modest limited form of realism that endorses only those parts of our scientific theories that play a crucial role in the empirical success of our theories. Psillos' "divide and conquer" approach is the most sophisticated and influential proposal along these lines. Psillos responds to the history objections by examining the details of some past successful scientific theories and arguing that the scientists then had good reason to believe only certain parts of those theories. For example, commitment to the ether was not warranted by the success of Maxwell's electromagnetic theory. This is because the ether did not indispensably contribute to the generation of any successful prediction, in the following sense:

> Suppose that H together with another set of hypotheses H′ (and some auxiliaries A) entail a prediction P. H indispensably contributes to the generation of P if H′ and A alone cannot yield P and no other available hypothesis H* which is consistent with H′ and A can replace H without loss of the relevant derivation of P (Psillos 1999, 110).

C. Pincock (✉)
Department of Philosophy, Purdue University, West Lafayette, IN 47907, USA
e-mail: chrispincock@gmail.com

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_4,

Still, belief in some features of what turned out to be the electromagnetic field were justified and so a limited realism about Maxwell's theory was appropriate at the time. The objections to realism based on scientific practice are also considered by Psillos in his discussion of modeling and idealizing. While here I believe there is more work for Psillos to do, the broad outlines of a viable limited realism are now in place.

Psillos developed his own brand of limited realism with reference to an even more limited form of scientific realism, namely the structural realism advocated by Worrall. This is the view that, when it comes to the theoretical parts of our empirically successful theories, we only have a good reason to believe the structural parts.[1] For example, in the Maxwell case, Worrall argues that scientists had good reason to believe that something answered to the series of structural claims found in Maxwell's equations. This structural knowledge is consistent with the scientists not having a good reason to believe anything about the things that were responsible for the correctness of these structural claims. So, unlike Psillos, who says that belief in the ether was not warranted but that some beliefs about the electromagnetic field were, Worrall would argue that neither kind of belief was justified by the empirical success of the theory.

Worrall is explicitly focused on the objections to scientific realism based on history. By restricting what counts as an empirically successful theory, and narrowing his realism to structural claims, he is able to avoid many of the problematic cases from the history of science. In the examples he emphasizes, we can reconstruct a series of theories that preserve the original structural claims made by a successful scientific theory. Above all, Worrall focuses on mathematical relationships between theories when he tries to make this notion of preservation of structure more precise. In the ideal case for his proposal, we have the very same mathematical equation appearing in the succeeding theory. This occurs fairly rarely, though, so Worrall also allows cases where the mathematics of the two theories stand in a limiting relationship: "The much more common pattern is that the old equations appear as limiting cases of the new – that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit" (Worrall 1996, 160). For example, Maxwell's equations do not appear in either quantum mechanics or general relativity theory, but taking limits on the equations of the contemporary theories is said to recover Maxwell's equations.

The main objection that I will develop here against epistemic structural realists is that they have not clarified the positive contribution that mathematics makes to scientific theories in general or in the cases they have examined. This objection can be thought of as a special case of the objection to realism based on scientific practice that was noted earlier. For unless the structural realist can say what work the mathematics is doing, she has not dealt with the worry that the mathematics is unrelated to those aspects of the physical world that we have some reason to believe in.

---

[1] This is sometimes taken to be a claim about what it is in principle possible for us to know. I will not include this stronger position in my definition of structural realism. Also, I will not discuss here the view known as ontic structural realism, which maintains that the physical world is itself purely structural.

As I will argue below, it is not enough to say that the mathematics is ineliminable or indispensable to the generation of successful empirical predictions. The mathematics could very well be an indispensable part of the successful scientific theory and yet be a poor indicator of genuine features of physical world. In the end, I will do more than just raise this concern as an abstract possibility. Instead, I will argue that the most plausible proposal for explaining the value of mathematics to scientific representation entails that if some mathematics is indispensable to a given scientific theory, then there is good reason to think that the mathematics does not track the physical world. This stronger claim is hard to support conclusively, but it appears to put the burden of proof on the structural realist to say exactly when mathematics is needed and can be taken to represent the physical world more or less accurately.

4.2 A Coherent Structural Realism

Before turning to this objection to structural realism, it is worth explaining why I do not endorse the stronger objection developed by Psillos that structural realism is not even a coherent position. Psillos claims that there is no reasonable way to draw a line between the structural and non-structural claims made by a scientific theory:

> to say what an entity is to show how this entity is structured: what are its properties, in what relations it stands to other objects, etc. An exhaustive specification of this set of properties and relations leaves nothing left out. Any talk of something else remaining uncaptured when this specification is made is, I think, obscure. I conclude, then, that the 'nature' of an entity forms a continuum with its 'structure', and that knowing the one involves and entails knowing the other (Psillos 1999, 156–157).

We can take this worry to be based on the idea that all knowledge involves knowledge of some nature of the entities in question in addition to knowledge of their structural relations. So, there is no way to call into question all knowledge of the natures of the entities without also eliminating the structural knowledge as well.

The first step in responding to this objection is to give a proposal for what a scientific theory is. Broadly in step with the semantic view of theories, I will identify scientific theories with a collection of wholly mathematical models combined with a set of claims about how aspects of these models relate to aspects of the physical world. In the cases we will consider, the mathematical equations of the theory pick out the mathematical models. The models will be all the mathematical models consistent with the equations for all mathematically possible initial and boundary conditions. The claims of the theory will then link parts of each of these models to properties of the physical systems that the theory aims to represent. For example, in the case of Maxwell's electromagnetic theory, we might have a class of models representing discrete particles in motion.[2] Each particle could then be associated

---

[2] This theory also has continuous models, but I will not pursue the complications that arise from theories with models whose structures diverge to such a degree.

with six real numbers, representing its spatial coordinates at a time along with the mass and charge of the particle in some units. Additional components of each mathematical model might then represent the field strengths at each spatial point at each time. Combining the mathematical models and the claims places a vast array of stringent conditions on a physical system. When these conditions are met by some system, we will say that the model accurately represents the system. The hope, then, is that a theory will contain enough models to accurately represent all the systems in its domain.

Now, what are the structural aspects of the theory on this proposal and how do they differ from the non-structural aspects of the theory? My suggestion is that the structural claims of the theory are the propositions that follow from the weakest claims needed to link the wholly mathematical models to the relevant physical systems. Equivalently, the non-structural aspects of the theory are the propositions that are independent of the weakest claims needed to link the wholly mathematical models to the relevant physical systems. In our example, we have a required claim that this particular aspect of the mathematical models stands for charge. Without this claim linking these numbers to charge in some units that crucial part of the mathematical model would be idle. But any additional commitments that a scientist may have about charge would go beyond this. For example, the scientist may believe that charge is not a fundamental physical property and that it is due to some intra-particle features not otherwise represented by the theory in question. Relative to this theory, this additional claim is a non-structural assumption. It is idle with respect to the minimal conditions that the theory places on the physical systems in its domain.

The minimal non-mathematical claims needed to attach the mathematical models to the physical systems are included in the structural content of the theory on this proposal. This seems to be the sort of nature that Psillos has in mind when he argues that all knowledge of structure involves some knowledge of nature. As long as there is a principled way to divide this sort of nature from other more ambitious claims about the system, I do not see why making this concession would be problematic to an epistemic structural realist. The epistemic structural realist restricts our knowledge to what follows from the weakest claims needed to link the mathematical models to the physical systems. The non-structural realist aims to vindicate additional claims that involve additional features of the systems over and above this minimal structure.

This is my interpretation of what Worrall is after when he invokes gravity, in a Newtonian context, as a "primitive irreducible notion" (Worrall 1996, 162). Here Worrall helps himself to the basic semantic contents which are needed to attach the mathematics to the physical world. As long as these semantic contents are not combined with additional claims about the physical properties, I think it is fair to call the resulting content "structural". There are, of course, positions in the philosophy of science that would call into question the availability of these semantic contents, or that would demand that the philosopher of science explain how a scientist has access to them. Here I am thinking of the concerns presented most forcefully by William Demopoulos, and traceable to the writings of Russell and Ramsey among others (Demopoulos 2003a, b). From Demopoulos' perspective, the epistemic problems raised by knowledge of the unobservable are deeply intertwined with the difficulty in referring to properties of unobservable entities. Worrall's direct appeal to such semantic content, then, looks suspiciously like 'theft over honest toil'. As these issues are complex and cannot be pursued here, I will assume that it is acceptable to appeal to the minimal semantic contents that Worrall needs. Even if this semantic concession is made, epistemic structural realism is still in trouble.[3]

4.3 What Mathematics Contributes

As we have clarified it, epistemic mathematical structural realism is the view that we often have good reason to believe the structural claims made by our empirically successful scientific theories. These reasons are based on the role that the claims about mathematical structures play in allowing accurate predictions. Exactly what this role is has received almost no scrutiny in the philosophy of science, and this has led both advocates and critics of structural realism to miss the central problem with the view. In this Section I will outline my proposal for what mathematics contributes to scientific representation. This view arose out of reflection on debates in the philosophy of mathematics about the indispensability of mathematics to science (Pincock 2007), but can be defended independently of those considerations. In the following Section I will deploy this proposal to undermine mathematical structural realism.

In a nutshell, my proposal is that mathematics plays a crucial epistemic role in allowing the formulation of scientific theories that can be confirmed by the means that we have at our disposal. This epistemic proposal sits between two alternatives. On the one hand, there is the pragmatic view that says that mathematics is used purely for convenience. On this approach, for each mathematical scientific theory, there is a non-mathematical version of that scientific theory. Working with the non-mathematical version is said to be too difficult or practically daunting. But, in principle, all mathematics is dispensable from science. On the other hand, there is the metaphysical view which ascribes genuine physical significance to the entities studied in mathematics. On this view, the reason mathematics is so central to scientific theories is that mathematical entities figure as part of the physical world. So, just as we include talk of electrons in our theories, we should include talk of numbers as well.

---

[3] A different version of structural realism tries to overcome these difficulties by appealing to Ramsey sentences. See Cei and French (2006) and Melia and Saatsi (2006) for some recent discussion and references. I cannot pursue the relationship between mathematical structural realism and Ramsey-sentence realism here. However, I will say that I do not think Ramsey-sentence realism can overcome the problems I raise for mathematical structural realism.

Neither of these alternatives to the epistemic view is particularly attractive. The main problem with the pragmatic view is that it proves very difficult to formulate acceptable non-mathematical versions of our best scientific theories. The metaphysical view has the drawback that our best theories of mathematical entities, i.e. the theories developed by mathematicians, do not place these entities in the physical world or endow them with any direct physical significance. It is true that some philosophers of mathematics, for example David Lewis (Lewis 1993), have developed physical interpretations of mathematics. But nobody claims that these physical interpretations of mathematics relate to the ways mathematics is actually used in science.

The epistemic view emphasizes the boost in confirmation that results from formulating scientific theories in mathematical terms. In one respect, this point is fairly trivial. Consider, for example, the statistical techniques of data analysis that are used throughout the sciences to link raw data to scientific hypotheses. It is only after processing the data in mathematical terms that we are able to tell what the significance of the data is for a given theory. This mathematical analysis can give us a good reason to think that no bias of a certain kind has entered into the data collection process.

While this sort of deployment of mathematics is crucial to science and fits with the epistemic view, it has little bearing on a different use of mathematics in science, namely in formulating equations, analyzing their relationships and solving them. This is the central use of mathematics relevant to debates about scientific realism because it is these equations that we use when deciding what to believe about the physical world. Here I want to argue that the main benefit of using mathematics to formulate these equations is that mathematics allows us to specify a restricted range of claims about the physical systems in question. This restriction is crucial to developing claims that are modest enough to have a chance of being experimentally confirmed by the sorts of experiments we can actually run.

Consider, again, a case from classical electrodynamics. In an experimental context we might devise a system with a number of discrete particles with a certain charge distribution and see how their trajectories are altered when we impose some uniform magnetic field. In describing this system we appeal to some initial and boundary conditions that we believe accord with the experimental setup. In addition to this, we make claims only about the charge distribution, the position and momenta of the particles. We do not take an interest in the smaller or larger scale features of system. This restriction in the content of our claims is carried out using mathematics. We can assign a charge to a particle, for example, without being required to say why that particle has that charge or how that charge relates to entities outside the boundary of the system. More importantly, we can make testable claims about the electromagnetic field, how it interacts with charge, and how it will deflect the trajectory of the particles without taking a stand on what the electromagnetic field is or what its further physical basis might be.

This contribution by mathematics to restricting the content of our scientific claims occurs so routinely in science that we are liable to miss its significance. Imagine, then, how hard it would be to formulate a set of restricted claims about a given system of charged particles if we did not have mathematics at our disposal. One strategy would be to specify the causal mechanisms responsible for the observable features of the system. But typically these causal mechanisms operate on a scale that is not accessible to the scientist and an ingenious scientist can envision any number of ways in which microscale processes could contribute to what is observed. It is only by restricting our focus in some way that we can make a start at understanding the system. Here mathematics makes its crucial contribution by allowing the scientist to remain neutral on the wide range of questions that she does not know the answer to.

The epistemic proposal is consistent with the prospect of eliminating mathematics in some ideal end of science. This would occur if the scientific community was gradually able to zero in on the underlying causal mechanisms by first isolating stable larger scale mathematical structures and then proceeding to consider possible smaller scale underlying mechanisms. Perhaps this is what has started to happen in the case of Maxwell's electromagnetic theory. But our remaining ignorance of some details of the systems in question is signaled by the highly mathematical character of our current physical theories and the corresponding interpretative doubts that they give rise to. This is entirely in keeping with what the epistemic proposal would predict. On this proposal, mathematics is most useful when we are ignorant of basic causal mechanisms. Mathematical scientific theories about larger scale structures can be well confirmed, but what is confirmed are only beliefs about things at this scale. Highly mathematical theories have the benefit that we can actually confirm some of their claims, but only at the cost of undermining our confidence in our understanding of the underlying causal mechanisms responsible for the phenomena we are observing.

4.4 An Objection

Unsurprisingly, the proposed role for mathematics in science fits well with the conception of scientific theories developed earlier. We use the mathematics as a scaffolding and attach to it the restricted claims about the physical system. The mathematics does its job by allowing us to make testable claims about the system without forcing us to go further and engage in interpretative debate. On a first pass, this seems to accord perfectly with the position of the structural realist. For the structural realist restricts her belief to those parts of the theory that are required to minimally attach these mathematical models to the physical systems. So, she explicitly limits the scope of her claims about the system to what the mathematics is telling us. It seems that if anything is well confirmed in the theory, it will be the part that the structural realist has isolated. And the sort of empirical success that Worrall emphasizes seems to be all that is needed to reasonably conclude that some part of the theory is well confirmed.

This optimistic conclusion ignores the risks associated with mathematical scientific theories discussed in the last section. We can contrast two ways in which mathematics might be doing essential work in generating successful scientific predictions. First, the mathematics might be mirroring the structure of a stable phenomenon at a given scale of description. This would allow the scientist to confirm a whole host of claims about that phenomenon and there would then be every reason to think that these claims would survive further improvements in the theory. However, a second scenario is possible. This is that the mathematics does not mirror the structure of any stable phenomenon at the relevant scale. The disconnect might be because the mathematics has too much structure or too little. Too much structure would be involved if the mathematics involves more complexity than is needed to track the dynamics of the system. For example, we could imagine a competing electromagnetic theory which ascribed two kinds of charge to particles and had complicated equations relating these different kinds of charges to each other and to the trajectories of the relevant particles. It might appear that all this surplus mathematical structure was necessary to deriving the correct predictions, but this appearance would be illusory. Conversely, the mathematical scientific theory might have too little structure. Suppose (as in fact appears to be case) that electromagnetic interactions are affected by intra-particle forces at high energy regimes. Maxwell's electromagnetic theory ignored these possibilities and so this parameter did not appear in that theory. Additional structure is needed to account for phenomena at higher energies, but we have no hint of this from the experiments conducted in Maxwell's time.

My general objection to epistemic mathematical structural realism, then, is that the use of mathematics in deriving successful empirical predictions gives us no general assurance that the mathematics is mirroring the structure of the phenomenon in question. It could be appealing to too much structure or it might not have enough structure. If either of these mismatches occur, then there is no reason to think that the mathematical structures will persist through further developments in the theory. Note that on the epistemic view of the value of mathematics to science, this is not a merely abstract possibility. It is a consequence of that view that the presence of mathematics signals an ignorance of the underlying causal mechanisms. So, more often than not, we should expect that the structures invoked by a mathematical scientific theory will not mirror the underlying causal mechanisms. But if we lack this assurance, then there is no reason to think that the structures appearing in the successful theory will be preserved.

We can raise the same concern in a different way by focusing on the multiple realizability of any structure picked out by an abstract mathematical description. Consider again the successful description of an experiment involving discrete charged particles moving through a magnetic field. The description offered by Maxwell's electromagnetic theory, which the structural realist endorses, is consistent with any number of microscale realizations of the nature of charge, the electromagnetic field, the physical basis of the boundary conditions and so on.[4] This flexibility

---

[4] I do not intend to draw the difference between the macroscale and microscale at the level of what is observable. Also, it is worth noting that the same point could be made about the relationship between the medium scale description of the theory and the larger scale features of the physical system.

goes hand in hand with the description being given in mathematical terms and is precisely what allows us to confirm that the description is correct without knowing anything about the microscale interactions. Still, this has the disadvantage that the structural realist cannot be confident that the structures appealed to in this description will persist as the microscale details are resolved by further scientific investigation. It should strike the structural realist as extremely unlikely that this description happens to have hit on exactly the right level of structural detail. But unless we have exactly the right level of structural detail, there is no reason to think that the structures appealed to will survive theoretical change.

The point can be sharpened by drawing on Wimsatt's discussion of how rare it is that a physical property is aggregative (Wimsatt 2007). Aggregative properties are those that are scale invariant. Mass in classical physics is one of the few features of physical things that pass this test. So, if we understand how large objects move in virtue of their mass, we can extrapolate from this description at this scale to how smaller objects would move in virtue of their mass. Charge in classical physics initially also had this feature of scale invariance, but it was eventually realized that there was a minimal unit of charge. If we had a genuinely aggregative property and had some reason to think that it was aggregative, then the difficulties related to multiple realizability just canvassed would not arise. The structures observed at the medium scale would be reproduced at smaller and larger scales. Confidence about the medium scale behavior of the objects would warrant confidence in the behavior at different scales.[5]

It should be clear that we have little evidence that any of the features identified by contemporary science are aggregative in this sense. Scale matters, as shown by the presence of physical constants like Planck's constant in our most successful physical theories. This shows that as we scale up or down from the phenomena that we understand with our current best scientific theories, we should be prepared for a break with the structures countenanced by our current best science. Focusing on the role of mathematics, thus, in the end undermines the link between structure and predictive success so central to the structural realist position. Without this link, the position is no longer tenable.

4.5 An Appeal to Limits

There is an aspect of the structural realist position that I have not yet discussed in any detail that the persistent structural realist might insist is adequate to respond to my objections. This is the claim that mathematical structure need not be maintained exactly across theory change, but that limiting relationships are sufficient for the preservation of structure that the structural realist is after. Perhaps by emphasizing

---

[5] Techniques for this sort of scaling and their interpretative significance have been discussed extensively by Batterman. See, for example, Batterman (2002).

these limiting relationships the structural realist position can be made flexible enough to deal with the structural disconnect worries presented above.

It is worth asking why, on the structural realist position, these correspondences should be expected or how they accord with the history and practice of science. To begin with, as Mark Newman has recently noted, different scientific theories may draw on completely different mathematical theories: "It is far from obvious that we can successfully compare the equations of quantum mechanics with those of classical dynamics. In the former case we are dealing with operators operating on rays in Hilbert space, in the latter we are talking of continuous real valued functions" (Newman 2005, 1378). More generally, the mathematics used in classical physics changed considerably from Newton's original presentation through the eighteenth and nineteenth centuries (Lange 2004). If the mathematical theories used in the series of scientific theories are different in kind, it is hard to see what sort of limiting relationship might obtain between the two.[6]

Even when the mathematics is sufficiently similar, it seems to me that Worrall underestimates the complex interpretative questions that these limiting relationships give rise to. Typically what philosophers of science have in mind when they talk about taking limits is mathematically transforming one set of equations to another set of equations by letting one or more elements in the former set go to 0 or infinity. For example, classical dynamics is said to result from quantum mechanics if we take Planck's constant to 0. Assuming such mathematical transformations exist, the structural realist must still offer some reason to think that these transformations preserve claims about the structure of the physical systems that we should take seriously. Reverting to the worries of the last section, the existence of a mathematical transformation between sets of equations is perfectly consistent with both sets of equations failing to mirror the requisite structure. Simply because one set of equations involves ascribing more structure than another set, it does not follow that the former set is doing better or is preserving the structural claims of the latter set that we should take seriously.

We can think of cases of structure preservation in this sense as ordered between cases where mathematical considerations dominate and cases where physical considerations dominate. Mathematical structure may be preserved across theory change simply because some areas of mathematics are better understood than others. To take an extreme example, if only one mathematical theory is available that can do the job the scientists require, then the mathematics will be preserved across scientific theory change because there are no viable mathematical alternatives. So, the structural realist must explain why preservation of mathematical structure is not dictated by these mathematical considerations. If mathematical concerns are driving things, then there is little reason to believe the structural claims of the scientific theory.

Even at the other end of the spectrum, where it can be somehow demonstrated that physical reasons motivate the details of the mathematical transformations, things may not turn out as the structural realist desires. Consider, for example, a

---

[6] Cf. Worrall and Zahar (2001), 250.

case recently discussed by Batterman. Batterman focuses on the relationship between thermodynamics and statistical mechanics in their representation of critical points in phase transitions. An example is the disappearance of self-generated magnetic fields in some materials as temperature increases past a certain threshold. Batterman's point in developing this example is that the more recent and in some sense more fundamental theory, statistical mechanics, is not able to adequately represent these phase transitions without the aid of the less fundamental theory, thermodynamics. This is because it is only by taking the "thermodynamic limit" of the equations of statistical mechanics, in which the number of particles is increased to infinity, that the phase transitions can be recovered:

> thermodynamics is correct to characterize phase transitions as real physical discontinuities and it is correct to represent them mathematically as singularities. Further, without the thermodynamic limit [of infinitely many particles], statistical mechanics would completely fail to capture a genuine feature of the world. Without the thermodynamic limit, in fact, statistical mechanics is incapable of even establishing the existence of distinct phases of systems (Batterman 2005, 234).

The point that I want to make here is that this sort of relationship between scientific theories should be the best kind of case for the structural realist because the mathematical transformations are motivated by the need to explain some experimentally verified phenomena. Still, the relationship between the structural claims of the two theories is much more complex than what the structural realist is able to account for. Among other things, statistical mechanics does not preserve the relevant structural claims of thermodynamics because, as Batterman says, it is unable to even represent the phase transitions on its own. So there is no general conclusion that we can draw about the two theories simply in virtue of this limiting relationship obtaining between them. Furthermore, notice that the thermodynamic limit linking the two theories involves the assumption of infinitely many particles. As we believe this assumption is incorrect for the systems under consideration it is hard to see why the results of the claim should be taken realistically as reflecting genuine physical structures. Whatever the correct take on this complex interpretative question is, it is not something easily accommodated by epistemic structural realism.

We see, then, that invoking limiting relationships between theories does nothing to improve the plausibility of the structural realist position. Instead, it draws attention to the mathematical relationships between theories that may have nothing to do with those aspects of the theory that we should take seriously. And even when the mathematical relationships have some physical significance, it is far from clear that there is any general reason to think they can be fit into the structural realist position.

4.6 Problems for Limited Realism

So far my differences with Psillos' brand of limited realism have been downplayed as part of my attempt to clarify what I take the most serious challenge to epistemic structural realism to be. Against Psillos, I do not think the most difficult challenge is in articulating a coherent restriction on our commitments to the structural content of a given scientific theory. Still, everything said so far might be accepted by Psillos as a small amendment to his concerns about structural realism. Perhaps, as with Psillos, I think the flaws of structural realism support a more traditional form of limited realism.

I am not so sure about this happy resolution. Psillos himself maintains that Worrall's structure/nature distinction is "orthogonal" (Psillos 1999, 155) to Psillos' own limited realism, but this does not seem to be the case. Recall Psillos' test for belief in a theoretical constituent: whether or not that constituent is indispensable to generating a successful empirical prediction. If what I have said so far is correct, and there are serious problems taking seriously even the structural claims of our empirically successful scientific theories, then any version of realism that endorses these structural claims and other claims as well is in jeopardy. It seems clear that in many cases the mathematics is an indispensable theoretical component, so in those cases Psillos' realism inherits all the worries I have raised for structural realism.

To see this, return once again to the example from classical electrodynamics. Given the empirical success of the theory, Psillos argues that we have a reason to believe that the key parts of the theory are approximately true. The key parts are those parts of the theory that are genuinely required to generate the successful predications. Based on the conception of the contribution of mathematics to the theory presented in Section 4.3, the mathematics of Maxwell's equations is making such a crucial contribution. But the traditional realist has not given any reason to think that the mathematics mirrors some underlying physical reality. It may very well be the case that Maxwell's equations give the structure of some underlying entity, namely the electromagnetic field. But as far as the success of the predictions go, the mathematics used in the equations may have too much or too little structure and so not reflect the features of any physical thing.

We see, then, that paying attention to the role of mathematics in science threatens not only structural realism, but any form of limited realism that moves directly from the parts of the theory required for a successful prediction to a belief in those parts of the theory. It seems that additional conditions have to be met for us to be confident that the mathematics is latching on to something genuine in the physical world. I, for one, am not optimistic that any single, general style of argument can make this sort of fine differentiation. Instead, I would argue that it is only by working through the details of this or that particular case that can we come to some determination about what is entailed by a successful prediction.

References

Batterman, R. (2002), *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction and Emergence*, Oxford University Press.

Batterman, R. (2005), "Critical Phenomena and Breaking Drops: Infinite Idealizations in Physics", *Studies in the History and Philosophy of Modern Physics.* 36: 225–244.

Cei, A. and S. French. (2006), "Looking for Structure in All the Wrong Places: Ramsey Sentences, Multiple Realisability, and Structure", *Studies in the History and Philosophy of Science.* 37: 633–655.

Demopoulos, W. (2003a), "Russell's Structuralism and the Absolute Description of the World", in N. Griffin (ed.), *The Cambridge Companion to Russell*, Cambridge University Press, 392–419.

Demopoulos, W. (2003b), "On the Rational Reconstruction of our Theoretical Knowledge", *British Journal for the Philosophy of Science.* 54: 371–403.

French, S. and J. Saatsi. (2006), "Realism about Structure: The Semantic View and Nonlinguistic Representation", *Philosophy of Science* (Proceedings). 73: 548–559.

Lange, M. (2004), "Review Essay on *Dynamics of Reason* by Michael Friedman", *Philosophy and Phenomenological Research.* 68: 702–712.

Lewis, D. (1993), "Mathematics is Megethology", *Philosophia Mathematica.* 1: 3–23.

Melia, J. and J. Saatsi. (2006), "Ramseyfication and Theoretical Content", *British Journal for the Philosophy of Science.* 57: 561–585.

Newman, M. (2005), "Ramsey Sentence Realism as an Answer to the Pessimistic Meta-Induction", *Philosophy of Science* (Proceedings). 72: 1373–1384.

Pincock, C. (2007), "A Role for Mathematics in the Physical Sciences", *Nous.* 41: 253–275.

Psillos, S. (1999), *Scientific Realism: How Science Tracks Truth*, Routledge.

Psillos, S. (2001), "Is Structural Realism Possible?", *Philosophy of Science* (Proceedings). 68: 13–24.

Psillos, S. (2006), "The Structure, the Whole Structure and Nothing but the Structure?", *Philosophy of Science* (Proceedings). 73: 560–570.

Wimsatt, W. (2007), "Emergence as Non-Aggregativity and the Biases of Reductionisms", in *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*, Harvard University Press, 274–312.

Worrall, J. (1996), "Structural Realism: The Best of Both Worlds?", reprinted in D. Papineau (ed.), *The Philosophy of Science*, Oxford University Press.

Worrall, J. and E. Zahar. (2001), "Ramseyfication and Structural Realism", in E. Zahar (ed.), *Poincaré's Philosophy: From Conventionalism to Phenomenology*, Open Court, 236–251.

Chapter 5
Structural Empiricism, Again[1]

Otávio Bueno

5.1 What Is Structural Realism?

As is well known, there are two crucial arguments in the realism debate. According to the *no-miracles argument*, it would be a miracle if our best scientific theories – namely, those which successfully predict novel phenomena – were not true (or approximately true). So, we should take theories that yield novel predictions as being true or, at least, approximately so. Clearly, considerations of this sort are raised to support realism. On the other hand, according to the *pessimist meta-induction*, many of our best-confirmed theories have turned out to be false. So, how can we guarantee that current theories are true? Considerations such as these, in turn, are meant to provide support for anti-realism.

Since these arguments pull in opposite directions, the question arises as to whether there is some way of accommodating the intuitions underlying both arguments. A positive answer is provided by *structural realism*. According to the latter, scientific theories capture the "*structure* of the world", and that is why they are so successful in predicting novel phenomena. However, capturing the "structure of the world" is compatible with introducing *radically different ontologies*, since the same structure can be instantiated in different ways. So, our theories may well be false, as far as their ontological commitments are concerned, but this doesn't preclude them from correctly capturing the relevant overall structure.

---

[1] My thanks go to José Chiappin, Steven French, James Ladyman, Stathis Psillos, and Bas van Fraassen for stimulating discussions. My greatest debt is to Steven French, with whom I have discussed structural realism for several years, and who has developed, with James Ladyman, the best articulated version of the proposal. I also wish to thank Andrés Bobenrieth, Katherine Brading, Geoffrey Cantor, Anjan Chakravartty, John Christie, Jon Hodge, Don Howard, Michel Janssen, John Norton, Michael Redhead, Simon Saunders, and Adrian Wilson for helpful comments on earlier versions of this work.

O. Bueno (✉)
Department of Philosophy, University of Miami, Coral Gables, FL 33124, USA
e-mail: otaviobueno@mac.com

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_5, © Springer Science+Business Media B.V. 2011

This is an attractive move, which can be traced back to the works of Poincaré (1905), Duhem (1906), Russell (1927), and Cassirer (1936).[2] Recent developments of this proposal can be found in the works of a number of people; in particular, John Worrall, José Chiappin, Elie Zahar, James Ladyman and Steven French.[3]

As Ladyman (1998) argues, there are two formulations of structural realism. An *epistemic* formulation (that, to some extent, can be found in Worrall 1989), according to which (1) structural realism is an *epistemological* claim about our knowledge of structural preservation in science (following Poincaré), and (2) this claim is formulated in terms of the *syntactic* approach. According to the *ontological* reformulation (the one favored by Ladyman), (1′) structural realism is a *metaphysical* claim about the world – all there is to the world is structure – and (2′) this claim is better formulated via the *semantic* approach. In both cases, there is a clear realist component, since scientific theories uncover the truth about the world, by capturing the *structure* of the universe.

In this paper, I have two goals. First, I evaluate the cogency of structural realism, and argue that, as it stands, the approach still faces considerable difficulties. The problems are concerned with the elusive nature of structure (especially in the metaphysical form required by structural realism); the difficulty of carrying over structural realism into quantum mechanics; the existence of structural losses in scientific change; and problems that arise from Putnam's model-theoretic argument. Although structural realism is among the most attractive forms of realism in contemporary philosophy of science, it seems to offer little hope for the realist. My second goal is to argue that the criticisms raised leave intact a structural version of empiricism that I call *structural empiricism* (Bueno 1997, 1999). Although structural empiricism is not a form of realism, it still preserves the key structural features of this proposal. Given the difficulties that realism face, perhaps in the end we are better off leaving realism behind.

5.2 Problems for Structural Realism

5.2.1 A Metaphysical Problem: The Nature of Structure

What Is the Nature of Structure? The notion of structure is a crucial component of structural realism (see French and Ladyman 2003a, b). Thus, it seems fair to expect a clear account of structure. However, this is no easy task. As soon as the

---

[2] Both Worrall (1989) and Zahar (1996, 1997) claim that Poincaré was a structural realist. Chiappin (1989) argues that Duhem's work, rather than Poincaré's, is better understood as a structural realist view. According to Demopoulos and Friedman (1985), at least in 1927, Russell was a structural realist (see also van Fraassen 1997). In French and Ladyman's view, a similar point can be made about Cassirer (French and Ladyman 2003a, b).

[3] See Worrall (1989), Chiappin (1989), Zahar (1996, 1997), Ladyman (1998), French (1999), French and Ladyman (2003a, b). (See also Psillos 1995; Redhead 1995; van Fraassen 1997; Chakravartty 1998.)

structural realist is in the business of providing such account, a dilemma immediately arises. For given the extant accounts of structure, either they do not support structural realism, or they are not clear enough to be of much help for the realist. In either case, problems for the realist component emerge.

In brief, here is the argument: On the one hand, (i) we have a clear notion of structure (given by contemporary mathematics, in particular, set theory), but it is unclear how it could be used to support realism; on the other hand, (ii) the notion which would support realism (a metaphysical notion, as it were) is not clear at all. Why is this so?

Let's consider (i). In the set-theoretic account of structure, the latter is, of course, an abstract construction (it is part of the set-theoretic hierarchy). Thus, it seems that if the structural realist countenances this notion of structure, he or she will have to be a mathematical realist as well. Now, in the ontological formulation of structural realism provided by Ladyman, this seems to entail that the structural realist is committed to some sort of Pythagoreanism. Suppose that a particular structure has been preserved in theory change. This is because this structure appropriately captures – in some sense that has to be made clear – the structural component of the world. But then either the world itself seems to become a mathematical entity (an alternative that, I take it, few would be happy with), or we need some account of how abstract mathematical structures, used in the formulation of our theories about the world, capture the "structure of the world". Thus, we are back to where we started, that is, we would still need to provide an account of structure – except that now it is the "structure of the world" itself that has to be accommodated.

It might be argued that this is not a difficulty specific to structural realism, but that it applies to any account of mathematical representation. So, the point loses its force. However, even if this complaint were true, it wouldn't solve the problem. If this is a problem for any account of mathematical representation, in order to articulate his or her position, the structural realist will need to overcome it at some point.

But I think the difficulty is genuine, and it raises an issue *specific* to structural realism. The problem arises because of the *metaphysical* nature of the structures that are required by the structural realist; that is, such structures need to exist, be independent of us, and constitute the ultimate features of reality. If we drop these metaphysical requirements, the difficulty vanishes. For in this case, we need not assume that there are mathematical structures in the first place.[4] However, without demanding a metaphysical account of structure, it's unclear that structural realism is still a version of realism. After all, if structures have no metaphysical significance, there is nothing there for the structural realist to be realist about.

Thus, the structural realist needs a strong, metaphysical notion of structure. Of course, that's part of Ladyman's ontological version of structural realism. And this leads us to (ii). If structures have to be characterized in metaphysical terms, we seem to lose our grip on what exactly structure is. One could perhaps move to a Kantian

---

[4] For example, nominalists about mathematics will not grant the existence of mathematical structures, and they will provide an account of the usefulness of mathematics despite the non-existence of the such structures (see Field 1980; Hellman 1989; Azzouni 2004).

notion of structure.[5] But then it is unclear how to make this notion of structure compatible with the *ontological* version of structural realism. After all, according to Kant, "*All that we know* in matter is merely relations" (Kant 1787, p. 291; the italics are mine). This passage meshes nicely with structuralism, given the emphasis on relations. However, since it focuses on our *knowledge* of the relations, it seems to lead us to the *epistemic* form of structuralism. And Kant proceeds: "but among these relations some are self-subsistent and permanent, and through these we are given a determinate object" (ibid.). Here we seem to regain the *ontological* content that the structuralist needs, but at the expense of structuralism. Kant's talk of *object* doesn't mesh well with the structuralist's emphasis on structures.

Alternatively, one could move to a neo-Kantian account of structure,[6] such as the one provided by Cassirer (1936). According to the latter:

> A point, a curve, a straight line – these no longer have a firmly determined existence or a definite significance ascribed to them independently of their mutual relations. All these structures do not exist in order, subsequently, to enter into certain relationships; rather it is these relations themselves which determine and completely exhaust the being expressed in mathematical concepts. Likewise concepts like atoms or electrons fully share the logical character of these geometrical concepts. They do not admit of an explicit definition but basically can only be defined implicitly. In this respect there is no difference between the material point and the ideal mathematical point. To such a point also no being in itself can be ascribed; it is constituted by a definite aggregate of relations and consists in this aggregate. (Cassirer 1936, p. 195)

This passage is remarkable given the way in which Cassirer presents the content of subatomic particles in structural terms. French and Ladyman (2003a) don't fail to note that moves of this sort provide further grist to the structuralist's mill.

However, if the structural realist adopted Cassirer's approach to structure, we would move straightaway into Pythagoreanism. After all, on Cassirer's account, "there is *no* difference between the *material* point and the ideal *mathematical* point" from the way they are defined (ibid.; the italics are mine). The problem is that we need to recognize the fact that, even though subatomic particles are *expressed* in mathematical terms, they are still *physical* things that are causally active in space and time – in a way that mathematical objects are never taken to be. By blurring, to a certain extent, the distinction between a material point and a mathematical point, Cassirer and the structural realist face two main difficulties. First, it's very hard to understand how quantum mechanics could have any physical content if subatomic particles are ultimately mathematical entities, since the latter are causally inert. But perhaps what Cassirer means is that the mathematics used in the description of subatomic particles is to be *suitably interpreted* in a *physically significant way*. In this case, the physical content of quantum mechanics can be accommodated.

However, and this is the second difficulty, it's not clear that the structural realist can offer a physical interpretation of the mathematical formalism in question.

---

[5] This alternative is mentioned in French and Ladyman (2003a), without necessarily endorsing it.

[6] French and Ladyman (2003a) also mention this possibility, but again without exactly endorsing it.

According to Cassirer, mathematical objects, such as points and curves, don't exist independently of the mutual relations that hold between them. It's in virtue of these relations that the objects become what they are. Consider, for example, the second position in the structure of natural numbers. What "the number 2" stands for on a structuralist understanding is nothing but the fact that this number bears certain relations with other positions in the structure: it comes immediately after the first position and immediately before the third. There is nothing else to the number 2 but such relations (see also Resnik 1997; Shapiro 1997). Of course, no special theory regarding the constitution of mathematical objects is needed here, since these objects are implicitly defined by the structure they belong to. However, in the case of subatomic particles, an additional story needs to be told. It's not enough simply to *stipulate* that an electron is a certain set of invariants and leave it at that. One needs to connect the relevant features of the mathematical formalism – the set of invariants in question – with physically detectable results. But in order to establish such a connection, one needs to identify suitable features in the world that correspond to the relevant positions in the mathematical structure. And this, in turn, demands an understanding of the world itself in structural terms. But this understanding is exactly what the structural realist was trying to provide in the first place by blurring the distinction between mathematical and physical constructions! Moreover, if the world itself were to be understood in terms of objects and relations, so that some sort of interpretation mapping could be established between the mathematical structure and the world, the structural realist would end up relying precisely on the object-based ontology that he or she is trying to avoid.

Given these difficulties, the structural realist could entertain the idea of taking the notion of structure as primitive. This is certainly a possibility: every theory has its primitive notions. In its favor, we do seem to have an intuitive notion of structure. But is this notion strong enough to be taken as the basis for structural realism? And is it compatible with the philosophical commitments involved in that view? In particular, the notion of structure plays an *explanatory* role in structural realism: it is the crucial component of the structural realist's answer to the "no-miracles" argument. Thus, what is needed is a notion that goes beyond the models we use to represent the phenomena – a notion that is suitably related to the "structure of the world" and which has, in turn, a metaphysical import.

The major difficulty associated with articulating such a notion of structure is that, given the structural realist's rejection of the usual metaphysics of objects, what is needed is an understanding of structure that is not cashed out in terms of objects and relations. After all, this would simply bring the primacy of objects back into the structural realist's ontology. However, an intuitive notion of structure seems to presuppose objects and relations as the basic constituents of a structure. This is certainly the usual understanding of structure in mathematics. As a result, such an intuitive notion isn't suitable for the structural realist's needs. In response, the structural realist may argue that if the notion of structure is primitive, it's not going to be defined in terms of any other notions. In particular, it won't be characterized in terms of objects and relations. But, at this point, by detaching the notion of structure from the usual metaphysics of objects, the structural realist seems to have moved a long way from the intuitive understanding of structure we started with. It's now no longer transparent what exactly *is* a structure for the structural realist.

In other words, the structural realist owes us an account of structure, fleshed out in a suitably strong metaphysical way. However, at least so far, no such account has been provided.

Identity and Individuality of Structures. One of the advantages of structural realism, according to French and Ladyman (see Ladyman 1998; French and Ladyman 2003a, b; French 2006), is that it allows one to overcome a delicate issue in the foundations of quantum mechanics: the issue of identity and individuality of quantum particles. Whether such particles should be taken as individuals or not is an issue that emerged with quantum statistics. And it didn't take too long to become clear that classical statistical mechanics and quantum mechanics deal with the permutation of indistinguishable particles very differently. The difference can be illustrated with the following example (see French 1989; see also French and Krause 2006).

Suppose that we have to arrange two particles (i and j) in two boxes (I and II). According to classical physics, there are four possible arrangements: (a) both i and j in box I; (b) both i and j in box II; (c) i in I and j in II; (d) j in I and i in II. As a result, the probability of each case (if they are regarded as equipossible) is 1/4. However, in the quantum mechanics case, we have something very different; only three configurations are possible: (a) both i and j in box I; (b) both i and j in box II; (c) one of i and j in I and the other in II. Thus, the resulting probability is 1/3 for each case. In other words, quantum statistics considers as one configuration what is classically regarded as two different states of affairs. This is formulated in terms of the *indistinguishability postulate*. Let P_{ij} be an operator which interchanges the variables q_i and q_j of particles i and j. This operator commutes with the Hamiltonian H, i.e. $[P_{ij}, H] = 0$. The point is that, as far as the formalism of quantum mechanics is concerned, the permutation of indistinguishable particles is not observable.

As a result, the formalism of quantum mechanics doesn't settle the ultimate nature of quantum particles: it *underdetermines* two metaphysical packages that try to determine their nature (see French 1989; French and Krause 2006). According to the *individuality* package, quantum particles *are* individuals, but the principle of identity of indiscernibles should be rejected.[7] After all, in order to take particles as individuals and accommodate the results of quantum mechanics, this package must allow for the possibility that certain particles (such electrons) that share the same properties are not the same. The *non-individuality* package makes a different move. The best way to accommodate quantum mechanics and the fact that – under a certain interpretation – it is meaningless to apply the notion of identity to quantum particles is to conceive of the latter as non-individuals.

---

[7] According to this principle, if two objects have the same properties, they are the same. In symbols: $\forall P(Px \leftrightarrow Py) \rightarrow x = y$. The converse of this principle, which states that if two objects are the same, they share the same properties (in symbols: $x = y \rightarrow \forall P(Px \leftrightarrow Py)$), is sometimes called Leibniz's law.

Since both packages are compatible with the formalism of quantum mechanics (French 1989; French and Krause 2006), the resulting underdetermination raises a difficulty for the scientific realist: Which of the two alternatives should the scientific realist be realist about? Are quantum particles individuals? If so, the realist has to relinquish at the principle of identity of indiscernibles, which has been the basis for various metaphysical views. After all, as just noted, indiscernible (indistinguishable) quantum individuals need not be identical. If quantum particles are non-individuals, an entirely new metaphysics is called for.[8] In either case, quantum mechanics doesn't determine the fundamental nature of the entities the realist should believe. As Ladyman points out: "It is an *ersatz* form of realism that recommends belief in the existence of entities that have such ambiguous metaphysical status" (Ladyman 1998, p. 420).

According to French and Ladyman, this motivates the move to structural realism. The structural realist doesn't assume either that particles are individuals or that they aren't. As long as considerations about *structure* are the fundamental feature of the view, settling the issue regarding the nature of quantum particles becomes irrelevant. Rather, the structural realist argues that one should be realist about the *structure* provided by the indistinguishability postulate itself – any commitment beyond that is unnecessary. In this way, the structural realist doesn't go beyond the commitments that quantum mechanics itself has, and these commitments are ultimately restricted to the structures provided by the theory.

But this move faces a significant problem. Even if we grant that there is nothing beyond the *structure* provided by the indistinguishability postulate, one can still legitimately raise the question: is the resulting structure an individual or not? Of course, the structures that satisfy the indistinguishability postulate are *mathematical* – formulated in terms of convenient operators and Hamiltonians. And this raises a problem about the interpretation of the mathematics used in the formulation of quantum theory. Let's see how.

Similarly to what happens in the quantum mechanical case, there are two main interpretations of the nature of structures in mathematics. According to the *non-individuality* package, mathematical structures are better understood as non-individuals, since the identity of their elements depends on the structure one considers (see Resnik 1997, pp. 209–212; 246–250; 254–257). For example, is the number 2 in the real number structure the same as the number 2 in the natural number structure? Moreover, according to Resnik, a mathematical theory does *not* quantify over the very structure it is supposed to describe, but only over the *positions* in that structure. For example, number theory quantifies over numbers, but not over number-theoretic structures themselves (Resnik 1997, p. 211).

---

[8] Important work by Décio Krause, Steven French, and Newton da Costa has provided a much-needed formal framework for developing this alternative, independently of the issue of scientific realism (see Krause (1992, 1996), Krause and French (1995), French and Krause (1995, 2006), and da Costa and Krause (1994, 1997). As the authors acknowledge, however, one still needs to articulate in detail the metaphysical picture associated with this approach.

However, within *set theory*, for example, one *does* quantify over number-theoretic structures, and contrast these structures to others. One of the consequences of Resnik's view is that the notion of individuality suddenly becomes relative to the mathematical context one considers. In the context of set theory, a number-theoretic structure is an individual (since it is related by identity), but in the context of number theory, it isn't. (Note, however, that *given a structure*, their components *are individuals*, since they are related by identity.) In order to avoid this piece of metaphysical revisionism, the alternative *individuality* package has been formulated (see Shapiro 1997). In Shapiro's view, structures *are* individuals (they are related by identity), and he goes on to develop a theory of structure that mimics set theory (see Shapiro 1997, pp. 90–97). However, on this account, not only the structures are individuals, so are their constituents (the positions in the structure).

Therefore, both packages assume individuality at the level of mathematical objects. Note that it is also at the level of objects that quantum particles – as individuals – are found. After all, such particles are represented as a particular kind of mathematical object: a given set of invariants. Thus, both approaches to mathematical structuralism assume that mathematical objects, as part of a given structure, *are* individuals. Thus, if quantum particles are conceived of as mathematical entities, they are also individuals. It then follows that these structuralist approaches to mathematics are *not* neutral with regard to the individuality issue in quantum mechanics, but they favor the individuality package. If the structural realist adopts any of these approaches, as a way of fleshing out the required notion of *structure*, he or she will be committed to the *individuality* package of quantum mechanics. But this is inconsistent with the structural realist's requirement of neutrality regarding the two metaphysical packages about quantum theory.

In response, suppose that the structural realist adopts a very interesting version of set theory, called quasi-set theory, that doesn't require that all mathematical objects be individuals (see Krause 1992, 1996; French and Krause 2006). In terms of quasi-set theory, the underlying notion of structure can be articulated. Since not all objects are taken to be individuals, identity does not apply to certain objects in a quasi-set theory. The trouble is that if the structural realist advocates this set theory, he or she will be favoring the *non-individuality* package in quantum mechanics. After all, quasi-set theory is committed to the existence of non-individuals. In fact, the theory was devised in order to provide a formal framework for the non-individuality alternative. And once again, this conflicts with the structural realist's attempted neutrality on the issue of identity of quantum particles.

As a result, the structural realist faces a delicate predicament: the structures that he or she is committed to can't be taken as individuals (since this will bring back the standard metaphysics of objects), nor can the structures be conceived as non-individuals (since this clashes with the alleged neutrality regarding the nature of quantum particles). The metaphysical status of these structures, thus, becomes very unclear. Paraphrasing Ladyman's remark: it is an *ersatz* form of *structural* realism the one that recommends belief in the existence of structures that have such ambiguous metaphysical status.

5.2.2 Structural Realism About What?

If we examine quantum mechanics further, an additional difficulty for structural realism emerges. Before indicating the difficulty, let me say a few things about the status of two crucial mathematical theories that were central to the early formulation of quantum mechanics: group theory and the theory of Hilbert spaces.

Group Theory, Hilbert Spaces and Quantum Mechanics. As is well known, in 1925 and 1926, two entirely distinct formulations of quantum mechanics were devised. On the one hand, Heisenberg, Born, Jordan, and Dirac formulated, in a series of papers in 1925, the so-called matrix mechanics; on the other hand, in 1926, also in a series of works, Schrödinger articulated wave mechanics.[9] The two formulations couldn't be more different. Matrix mechanics is expressed in terms of a system of matrices defined by algebraic equations, and the underlying space is discrete. Wave mechanics is articulated in a continuous space, which is used to describe a field-like process in a configuration space governed by a single differential equation. However, despite these differences, the two theories seemed to have the same empirical consequences. For example, they gave coinciding energy values for the hydrogen atom.

It was therefore crucial to establish that the two theories were equivalent. Schrödinger and Dirac made partially successful attempts in this direction. The attempts, however, didn't completely succeed. In Schrödinger's case, only one side of the equivalence was actually established: he provided a mapping assigning a matrix to each wave-operator, but not the converse (Schrödinger 1926; for a discussion, see Muller 1997, pp. 49–58). Dirac, in turn, did establish the equivalence between the two theories, but his method required the introduction of the so-called δ-functions, which turned out to be inconsistent (Dirac 1930). In other words, in the first case, we have a consistent, but incomplete, attempt to establish the equivalence between the two theories; in the second, we have a complete, but inconsistent, proposal.

It is in this context that von Neumann formulated his approach to the foundations of quantum mechanics in terms of Hilbert spaces (von Neumann 1932). Von Neumann first noted that the mathematical spaces used in the formulation of wave and matrix mechanics were very different (one space was discrete, the other continuous). However, if we consider the *functions* defined over these spaces, in each case we obtain a particular instance of a Hilbert space. This suggested that the Hilbert space formalism provides the appropriate framework to develop quantum mechanics. And von Neumann's celebrated equivalence proof established an isomorphism between the two Hilbert spaces in question.

But there was an additional reason for the introduction of Hilbert spaces. They provide a straightforward setting for the introduction of probability into quantum mechanics. This is, of course, a crucial issue, given the irreducibly probabilistic character of the theory. In a paper written in 1927 with Hilbert and Nordheim, von Neumann explicitly addressed the problem of introducing probability into quantum

---

[9] For a detailed critical discussion, and references, see Muller (1997).

mechanics (see Hilbert et al. 1927). The approach was articulated in terms of the notion of the amplitude of the density for relative probability (for a discussion, see Rédei 1997). But the proposal faced a serious technical difficulty (which was acknowledged by the authors): the assumption was made that every operator is an integral operator, and therefore Dirac's problematic function had to be assumed. As a result, an entirely distinct account was required to introduce adequately probability into quantum mechanics. And this provided yet another path to von Neumann's introduction of Hilbert spaces.

In other words, by 1927 quantum mechanics could be seen as a semi-coherent assemblage of principles and rules for applications. And von Neumann provided a systematic approach to overcome this situation. Around the same time, Weyl developed a different approach. His 1931 book was an attempt to impose a degree of coherence via the introduction of group-theoretic techniques.[10] Weyl's approach, similarly to von Neumann's, was concerned with foundational questions, although not exactly the same sort of questions. As Mackey (1993, p. 249) points out, Weyl distinguished two questions in the foundations of quantum mechanics (Weyl 1927): (a) How does one *arrive at* the self-adjoint operators which correspond to various concrete physical observables? (b) What is the *physical significance* of these operators, i.e. how are physical statements deduced from such operators? According to Weyl, (a) had not been adequately treated, and is a deeper question; whereas (b) had been settled by von Neumann's formulation of quantum mechanics in terms of Hilbert spaces. But to address (a) Weyl needed a different framework altogether; he needed group theory.

According to Weyl (1931, p. xxi), group theory "reveals the essential features which are not contingent on a special form of the dynamical laws nor on special assumptions concerning the forces involved". And he continues:

> Two groups, the group of rotations in 3-dimensional space and the permutation group, play here the principal role, for the laws governing the possible electronic configurations grouped about the stationary nucleus of an atom or an ion are spherically symmetric with respect to the nucleus, and since the various electrons of which the atom or ion is composed are identical, these possible configurations are invariant under a permutation of the individual electrons. (ibid.; italics omitted.)

In particular, the theory of group representation by linear transformations, the "mathematically most important part" of group theory, is exactly what is "necessary for an adequate description of the quantum mechanical relations" (ibid.). Weyl then establishes that "all quantum numbers, with the exception of the so-called principal quantum number, are indices characterizing representations of groups" (ibid.; italics omitted). He also shows that Heisenberg's uncertainty relations and Pauli's exclusion principle can all be obtained via group theory (for a discussion, see Mackey 1993; French 1999). Given these considerations,

---

[10] Dirac's (1930) work represents a further attempt to lay out a coherent basis for the theory. However, as von Neumann perceived, neither Weyl's nor Dirac's approaches offered a mathematical framework congenial for the introduction of probability at the most fundamental level, and this was one of the major motivations for the introduction of Hilbert spaces.

Weyl's conclusion does not seem surprising: "We may well expect that it is just this part of quantum physics [the one formulated group-theoretically] which is most certain of a lasting place" (ibid.).

But it is not only in the foundations of quantum mechanics that group theory has a decisive role; it is also crucial for the *application* of quantum theory. Wigner, in particular, explored this role (see Wigner 1931). Here we find an important difference between Weyl's and Wigner's use of group-theoretic techniques in quantum mechanics (see Mackey 1993; French 2000). Weyl explored group theory at the foundational level – indicating how to obtain group-theoretically quantum mechanical principles. Wigner, on the other hand, was particularly concerned with the *application* of quantum mechanics (this is the main theme of Wigner's 1931 book). And as he argues, we cannot apply Schrödinger's equation directly, but we need to introduce group-theoretic results to obtain the appropriate idealizations (see French 2000). In Wigner's own words:

> The actual solution of quantum mechanical equations is, in general, so difficult that one obtains by direct calculation only crude approximations to the real solutions. It is gratifying, therefore, that a large part of the relevant results can be deduced by considering the fundamental symmetry operations. (Wigner 1931, p. v)

In particular, group theory allows the physicist to overcome the mathematical intractability of the many body problem, which is involved in a system with more than two electrons. Group-theoretic methods are invaluable in the process of connecting quantum mechanics to the empirical data (French 2000). As a result, group theory ends up entering both at the foundational level and at the level of application.

However, in order to use group theory in quantum mechanics, one has to adopt the prior reformulation of quantum theory in terms of Hilbert spaces. It is from the representation of the state of a quantum system in terms of Hilbert spaces that a group-theoretic account of symmetric and antisymmetric states can be provided (see Weyl 1931, pp. 185–191).[11] The group-theoretic approach also depends on the Hilbert space representation to introduce probability into quantum mechanics. Moreover, at the application level, despite the need for idealizations to apply quantum theory, Schrödinger's equation is still crucial (putting constraints on the accepted phenomenological models), and the representation of states of a quantum system in terms of Hilbert spaces has to be used. In other words, group theory is not an

---

[11] As French points out: "the fundamental relationship underpinning [some applications of group theory to quantum mechanics] is that which holds between the irreducible representations of the group and the subspaces of the Hilbert space representing the states of the system. In particular, if the irreducible representations are multi-dimensional then the appropriate Hamiltonian will have multiple eigenvalues which will split under the effect of the perturbation" (French 2000). In this way, "under the action of the permutation group the Hilbert space of the system decomposes into mutually orthogonal subspaces corresponding to the irreducible representations of this group" (ibid.; see also Mackey 1993, pp. 242–247). As French notes, of these representations, "the most well known are the symmetric and antisymmetric, corresponding to Bose–Einstein and Fermi–Dirac statistics respectively, but others, corresponding to so-called 'parastatistics' are also possible" (ibid.).

independent mathematical framework to articulate quantum mechanics – the Hilbert spaces representation is crucial. Roughly speaking, we can say that von Neumann's Hilbert spaces representation is "sandwiched" between Weyl's foundational use of group theory and Wigner's application program. Hence, there is a close interdependence between group theory and Hilbert spaces theory in the proper formulation of quantum mechanics in the mid-1920s.

Structural Realism in Quantum Mechanics. Given the need for *both* group theory and Hilbert spaces in the formulation of quantum mechanics, which of the resulting structures should the structural realist be realist about: (i) both group-theoretic structures and those derived from Hilbert spaces; (ii) neither of them; (iii) only one of them, or (iv) the other? I'll argue that the commitment to either (iii) or (iv) is arbitrary; (ii) is incoherent with realism about structure, and (i) is ontologically problematic. Therefore, in each case, the structural realist runs into trouble. Now, since quantum mechanics is one of our best-tested theories, if a philosophical proposal is unable to accommodate this theory, it faces a serious problem.

Why would it be arbitrary for the structural realist to believe, say, only in the group-theoretic structures used in quantum mechanics – and not in the Hilbert space formalism? As noted above, the group-theoretic representation depends on the formulation provided by Hilbert spaces. The "representation theorem" relating irreducible representations of a group and subspaces of a Hilbert space depends on the adequacy of the latter as a procedure to represent states of a quantum system. Moreover, as we saw, the introduction of probability into quantum mechanics is typically done in terms of Hilbert spaces. Thus, group theory depends on the latter. Now, given the crucial role of the Hilbert spaces representation – which is acknowledged and explored by Weyl – it would be ad hoc for the structural realist only to believe in the structures arising from group theory's side. Thus, option (iv) is not an option.

On the other hand, it would be similarly ad hoc for the structural realist only to believe in the Hilbert space formalism. As is stands, the formalism doesn't answer crucial ontological questions, such as the nature of quantum particles. It is straightforward, however, to claim that such particles are certain sets of invariants – as the group-theoretic approach highlights. In other words, given the close interplay between group theory and Hilbert spaces in quantum mechanics, it is arbitrary to believe in one and not in the other. This excludes option (iii).

What if the structural realist withholds belief in *both* types of structures altogether? In other words, what if he or she is neither a realist about Hilbert spaces nor about group-theoretic structures? In that case, the structural realist would relinquish realism. The *point* of the position is to defend realism about *structures*, and that is exactly what this option denies. After all, Hilbert spaces and group-theoretic structures are the crucial structures involved in this formulation of quantum mechanics. Thus, since option (ii) is incompatible with realism about structures, it is unacceptable to the structural realist.

The final option is then to be realist about *both* Hilbert spaces and the group-theoretic structures employed in quantum mechanics. But there are a number of difficulties with this move. First, there is no single "picture" of the quantum world associated with the two kinds of structure. We need an *interpretation* of quantum mechanics to explore this sort of question.[12] But the fact that there is a plurality of interpretations is enough to indicate that the quantum mechanical formalism is unable to settle the question about the "picture" provided by the theory. In other words, it is not enough simply to cling to the formalism and claim that the structures that satisfy the latter, whatever they are, are the ones the structural realist believes. For *without an interpretation*, no clear picture emerges from the formalism.[13]

However, if the structural realist moves *beyond* the formalism and actually articulates an interpretation, *non-structural* factors will have to be introduced. No interpretation is purely structural. We have to assert things like: "We take *this* part of the theory to refer to *that*". In other words, we need a *pragmatic* move to run any interpretation. As a result, we have moved *beyond* structuralism. Moreover, if we consider particular interpretations of quantum mechanics, they incorporate non-structural components. For example, the distinction made in the modal interpretation between value states and dynamic states is *not* structural. In van Fraassen's characterization, the *value state* is fully specified by stating which observables have values, and which they are; the *dynamic state* is completely determined by stating how the system will develop if acted upon in a particular way, and how it will develop if isolated (van Fraassen 1991, p. 275). Value states and dynamic states, however, are not *representations* of quantum states; they are *states of the system* itself. Although they can be represented by structures, such states are not structures themselves.

Perhaps this point can be made more clearly in the following way. Whether we consider collapse theories or non-collapse theories, there is *no purely structural* interpretation of quantum mechanics. Let's consider these two types of theories in turn.

(a) *Non-collapse theories*: According to these theories, all we need is Schrödinger's equation; there is simply no collapse of the wave function. What happens in measurement is that the world splits, and each component of a superposition is realized in a world. It might be argued that all there is in this interpretation is structure: a world is nothing but a connected branch in the tree of possibilities. This is *represented*

---

[12] How could the world possibly be the way this representation (in terms of Hilbert spaces and group theory) says it is? This is, of course, the typical foundational question (see van Fraassen 1991). The way to answer this question is by providing an *interpretation* of quantum mechanics.

[13] Furthermore, quantum mechanics is certainly more unified with the introduction of group theory, and some ontological questions (e.g. about quantum particles) can be better addressed group-theoretically. However, Hilbert spaces are also needed (for instance, as noted above, to introduce probability into quantum theory). But the ontological status of *these* spaces is far less clear. Such spaces certainly provide an important way of *representing* the states of a quantum system; but why should this be an argument for the *existence* of anything like a multi-dimensional Hilbert space in reality? Why is the *usefulness* of a representation an argument for its *truth*? This clearly conflates pragmatic and epistemic reasons – and even the realist should be careful in not conflating them.

by suitable sequences of vectors in a Hilbert space. The problem, however, is that although worlds are represented by structure, worlds themselves cannot be identified with these structures – unless we want to claim that the *world itself* is mathematical. But, clearly, it doesn't make much sense to claim that the world *is* a sequence of vectors! It makes sense to say that the world can be *represented* by such a sequence. However, this is not an option for the structural realist, according to which *all* there is to the world is structure. (Note that this is *not* a criticism of the many worlds interpretation. It's only an argument to the effect that this interpretation is not open to the structural realist.)

(b) *Collapse theories*: Collapse theories are similarly not open to a purely structural realist interpretation. For example, to develop the Copenhagen interpretation, Bohr introduces a distinction between the *measuring* apparatus and the *quantum* system. The measuring apparatus has certain "output" states that are correlated to the measured quantities of microsystems. No such distinction, however, is found in the formalism of quantum theory itself. Clearly, Bohr is not committed to any *particular* point where the line is drawn. But this doesn't make the distinction *structural*, given that what it represents – a line between apparatus and quantum system – is non-structural. Measuring devices are not structures. And the sense in which a quantum system might be a structure still needs to be specified. Of course, this needs to be done in a way compatible with structural realism, and as we saw above, the structural realist still owes us an account of structure. Hence, we have here a difficulty for the structural realist to accommodate collapse theories in purely structural terms. (Similar points can also be raised to other collapse theories.)

In summary, we can say that (a) the content of early quantum mechanics cannot be properly expressed without the close interplay between group theory and Hilbert spaces. However, (b) when we combine both mathematical frameworks, no clear picture of what is going on at the quantum mechanical level emerges. In particular, we don't have a clear picture that is compatible with a structural realist view. Since the last option, (i), is not available to the structural realist, I conclude that there are no grounds to claim that the approach can accommodate quantum mechanics.

5.2.3 Structural Losses and Scientific Change

If we still consider quantum mechanics, I think additional problems emerge. Shortly after the publication of his 1932 book, von Neumann became increasingly dissatisfied with the Hilbert space formalism (for an illuminating discussion of this episode, see Rédei 1997). I've already noted that one of his major motivations to use Hilbert spaces emerged from the need for introducing probability into quantum theory. Now, as part of the development of his ideas about quantum logic (see Birkhoff and von Neumann 1936), von Neumann realized that the geometry associated with a quantum system whose degrees of freedom is finite is a "projective geometry". However, if we consider a system whose degrees of freedom are infinite, the associated geometry changes dramatically. It actually leads to a new kind of mathematical structure, constructed by von Neumann in his formulation of continuous geometry, and it's called a called a type II_1 factor algebra (see von Neumann 1981). Now, what happens is that, for such infinite quantum systems, the Hilbert space formalism doesn't generate the appropriate notion of probability (see Rédei 1997). What is required is an entirely new conceptual setting: in terms of this II_1 factor algebra.

The outcome is that, according to von Neumann, one should move from Hilbert spaces to this factor algebra to properly accommodate the introduction of probability into quantum mechanics. Thus, we have two kinds of structures to be used in the foundations of quantum mechanics: the type II_1 factor algebra, and the algebra of bounded operators in a Hilbert space. However, these structures are not equivalent: the former is modular and non-atomic, the latter is modular, atomic, and non-distributive if the dimension of the Hilbert space is greater or equal to 2 (see Rédei 1997, p. 505). Interestingly, the two algebras lead to the same empirical results. In this sense, both frameworks are adequate for the development of quantum mechanics. Conceptually, however, the type II_1 algebra is better, since it leads to the appropriate probabilities for quantum systems (for a discussion, see Rédei 1997). Now, by moving to this algebra, a substantial structural change happens, since the atomicity of the algebra of bounded operators cannot be recovered. And it is unclear how the structural realist can accommodate this structural loss.

Of course, this is just one among many examples of structural losses in science. Laudan (1996) provides a number of other cases. For example, in the move from Cartesian to Newtonian mechanics, all the vortices integral to Descartes's theory were lost. Although the vortices explained the direction of the planets' orbits, the structural realist may say that such vortices were just part of a metaphysical theory, which wasn't well confirmed enough. However, given the crucial role played, in quantum mechanics, by the algebra of bounded operators in a Hilbert space, the same reply can't be used in this context. In the end, this poses a further problem for structural realism.

5.2.4 Putnam's Paradox Revamped

A few decades ago, Putnam devised an intriguing argument that relied on the Löwenheim–Skolem theorem to show that just by fixing the truth-values of the sentences of our language, we can't guarantee that the terms in these sentences refer.[14] According to the Löwenheim–Skolem theorem, if a first-order theory has an infinite model, then it has a model of each cardinality. In other words, there are non-standard models of first-order theories that have dramatically different structures, even though in all of them the theory's sentences are true.

---

[14] Putnam presented several, non-equivalent, formulations of his argument; see, for example, Putnam (1980, 1981).

In my view, a version of this argument can be used to raise problems for structural realism. The existence of non-standard and non-equivalent models of a first-order theory shows that there's no unique way of referring to that structure. In fact, there are *non-isomorphic* structures – whose domains don't have the same cardinality – that are *elementarily equivalent*, that is, which are such that a sentence is true in one structure if and only if it is true in the other. The problem is: which of these structures should the structural realist be realist about? Since these structures are different, the structural realist would have to decide between them. But on what grounds can that decision be made? The same theory is true in all of these structures!

In order to resist Putnam's argument, the structural realist can argue that we should move to a higher-order logic (such as second-order logic) in which the Löwenheim–Skolem theorem doesn't hold. This move is certainly possible, but it is not advisable for the realist. First, the incompleteness of second-order logic poses problems for the realist component, since the realist will have now to accept that there are truths that are not derivable in the system in use. Second, even if realism is made compatible with the incompleteness of the underlying logic, the move to a second-order logic won't settle the matter. For second-order logic has a non-standard semantics, called Henkin semantics, in which the Löwenheim–Skolem theorem does hold. The key feature of this semantics is that the relation and function variables may not range over *all* the relations and functions on the domain of interpretation, but only over a *fixed collection* of them (see Shapiro 1991). Hence, we obtain the same result again: the existence of non-isomorphic and elementarily equivalent models. Therefore, *different* structures will be equally adequate to accommodate the phenomena, and since these structures are *not* isomorphic, the structural realist will have to choose between them.

Note that, for the structural realist, the choice between these structures *cannot* be made on pragmatic grounds, otherwise the grounds for supporting realism will be lost. The empiricist is, of course, more than happy to select among the different structures based on *pragmatic* considerations, since he or she doesn't assert that such structures uniquely determine what is going on beyond the observable. But the structural realist has to be *realist* about such structures; that is, he or she has to claim that the structures in question ultimately capture the "structure of the world". Therefore, there should be a substantial, *epistemic* reason to prefer one such structure to the others. But if only *structural* considerations can be adduced – this is the structural component of structural realism – that choice can't be made. For besides being isomorphic, the structures are elementarily equivalent.

5.3 An Alternative: Structural Empiricism

The difficulties considered above motivate the need for an alternative account. The proposal shares with structural realism the emphasis on structure. But it completely abandons realism. Structures are employed not to capture the "structure of the world"; they only help us represent what is observable via the formulation of empirically adequate (or quasi-true) theories. Thus, the account sides with an empiricist interpretation, and can be called *structural empiricism* (see Bueno 1999).

By dropping the realist component, all the problems discussed above vanish. First, without realism, structures don't have to be cashed out metaphysically, and the difficulties of articulating a metaphysical account of structure don't emerge.

Second, with empiricism, a clear approach to the issue of identity and individuality of quantum particles can be taken (see van Fraassen 1991). In particular, the structural empiricist won't be trapped by adopting a view about structures in mathematics that interferes with his/her views about structures in science. Given that metaphysics is resisted in this approach, there is no incompatibility between the empiricist proposal and the non-metaphysical nature of structures.

Third, as I'll discuss in more detail below, without the realist component, the structural empiricist has no difficulty in accommodating the role played by mathematical structures in theory construction, such as the role played by group theory and the theory of Hilbert spaces in quantum mechanics. The empiricist withholds belief in anything that goes beyond the observable, and is not committed to the existence of the corresponding unobservable structures (whether they are mathematical or physical).

Fourth, the existence of structural losses in science, being an argument *against* realism, is very much well taken by the structural empiricist. In the end, such losses support an empiricist view.

Finally, Putnam's paradox does not pose a problem for a proposal that doesn't assume that structures in science should capture the "structure of the world". The structural empiricist has no qualms about choosing between different, non-equivalent, structures on pragmatic grounds. As a result, the empiricist structuralist doesn't face the difficulties that plague structural realism.

The structural empiricist is also able to accommodate two additional issues: (i) How can we use mathematical theories without being committed to the existence of the corresponding objects? (ii) How does the empirical success of mathematical and physical theorizing take place?

To answer question (i), the structural empiricist can make two moves. (a) For the structural empiricist, a scientific theory – including its interpretations and the relevant mathematical theories – need not be true. Only the empirical adequacy of the resulting "package" of physical and mathematical theories plus their interpretations is required (see also van Fraassen 1980). Hence, the structural empiricist is not committed to the existence of unobservable entities postulated by the mathematical and physical theories. In this way, it's possible to use the mathematical theories without claiming that the corresponding entities exist.

(b) The second move consists in insisting that there is a distinction between *ontological commitment* and *quantifier commitment* (see Azzouni 1998, 2004; Bueno 2005). As is well known, Quine elaborated a clear criterion for ontological commitment: we are ontologically committed to whatever the regimentation of our best theories in a first-order language quantifies over. In other words, Quine ultimately identified ontological commitment and quantifier commitment. But this identification is neither required nor justified. After all, we often quantify over objects for which we have no reason to believe that they exist, such as fictional objects, perfectly frictionless planes etc. As soon as this fact is acknowledged, it becomes clear that we may be quantificationally committed to mathematical entities, given that we quantify over these entities, and accept that they are indispensable to our best physical theories. However, a quantifier commitment is not sufficient for ontological commitment. Without a *criterion for what exists*, we cannot conclude that the objects we quantify over do *exist*. In other words, we don't have to follow Quine in his identification of quantifier commitment and ontological commitment. To be committed to the existence of something, we need to show that the objects in question satisfy the relevant criterion for what exists.

For the structural empiricist, a sufficient (but not necessary) criterion for what exists is tied to the observable, and it involves four conditions. An object is taken to exist if we have an access to this object such that: (1) The access is *robust* (it doesn't depend on our beliefs about the object). (2) It can be *refined* (we can get closer to the object for a better look). (3) The access allows us to track this object (in space and time). And (4) the access is counterfactually dependent on the object (if the object weren't there, we wouldn't observe it).[15] Note that these conditions leave it open whether unobservable entities (such an electrons or vectors in a Hilbert space) exist. After all, these are only sufficient, and not necessary, conditions for us to take something to exist. Thus, if unobservable entities don't satisfy these conditions, we can't conclude that they don't exist.

Consider the first three conditions. What is *robust* is *not* our access to electrons, but our *observation* of patterns in a cloud chamber. Strictly speaking, we also don't *track* an electron. We use a theory (quantum mechanics) to guide us in *postulating* that there is an object (the "electron") behaving in such and such a way, and we *infer* that there is such an object based on *tracking certain patterns in a cloud chamber*. Similarly, what is refined is not our *observation of the electron* (we could never get closer to an electron for a better look). Note that mathematical objects also don't satisfy these conditions, given that our observation of these objects is not robust; we cannot track them; nor can we refine our observation of them (of course, mathematical objects aren't observable to begin with). However, since these conditions are only sufficient, and not necessary, for us to take something to exist, the fact that the conditions fail doesn't entail the non-existence of unobservable entities.

But there is also another, weaker, form of access to an object, and it should be distinguished from the stronger form of access just presented.[16] We have this weak form of epistemic access to an object if our access to it is *through a theory* that has five virtues: (a) simplicity, (b) familiarity, (c) scope, (d) fecundity, and (e) success under testing.[17] This epistemic access gives us reason to accept a

---

[15] These conditions are adapted from Azzouni (1997, 2004) and Lewis (1980).

[16] Azzouni calls this weaker form of access *thin epistemic access* (see Azzouni 1997, 2004).

[17] These are the five theoretical virtues taken by Quine as providing good epistemic reasons for adopting a theory (see Quine 1976, p. 247).

theory, but not to *believe* in the existence of the corresponding objects. So, with regard to mathematical and unobservable physical entities, we may quantify over them, but this doesn't entail that we need to believe in the existence of these objects. After all, such objects don't satisfy the criteria for what exists, even though they do satisfy the above theoretical virtues. And so, the structural empiricist need not believe in their existence, but may use mathematical and unobservable physical entities in science.

Let's now address question (ii): How can the structural empiricist explain the empirical success of certain scientific theories? There are two strategies to provide such an explanation. First, we can develop a deflationary account of empirical success. Given that scientific theories that turn out *not* to be empirically adequate typically are rejected, it's not surprising that theories in science end up being empirically adequate. Ultimately, the empirically inadequate theories are abandoned (see van Fraassen 1980). Of course, the exception here comes when there is no alternative theory to an empirically inadequate one. In this case, the scientific community searches for a better alternative while it still tentatively uses the empirically inadequate theory.

This first way of addressing the issue, however, fails to engage with the main point that the realist has with regard to the no-miracles argument, namely that *novel* predictions (that is, predictions that a theory was not constructed to make) are the truly surprising ones, and they need explanation. To address this issue, the second strategy is developed, and I'll only have space to sketch it here. The strategy involves exploring the structures developed by the particular scientific theories under consideration. These theories are, by assumption, empirically adequate, and so they have models in which the phenomena are embedded. What does it take for such theories to be able to successfully predict novel phenomena? Briefly, to have models that yield observationally correct results. Without these models, the theories would be unable to achieve that. Is there anything to be explained here besides the fact that the successful theories do have the relevant models?

Well, the (scientific or the structural) realist will claim that there *is* something to be explained here. It's only by claiming that the relevant theories are true (or approximately true) that we can accommodate the fact that they yield novel predictions. That is, the scientific realist will insist that it's only because the terms in the successful theories refer to the right kind of objects (uncovering along the way the right properties of the domain) that we obtain novel predictions from these theories. Alternatively, the structural realist will insist that to explain novel predictions we need to have uncovered the right structures, those that correspond to the "structure of the world". Without the right sort of structures (or objects), no such explanation can be put forward.

The structural empiricist provides an explanation for the predictive success of scientific theories by first challenging that the scientific and structural realists provide adequate explanations of the success of science. Both versions of the realist explanation presuppose that there are *things* of the relevant sort that provide the explanation. In the case of scientific realism, the relevant unobservable entities (electrons, photons etc.) are thought of as the *cause* of the observable phenomena, and that's why the theories under consideration are successful in their predictions. In the case of structural realism, the relevant *structure* causes the observable phenomena to be the way they are, and hence it explains the success of the theory's predictions.

The problem, however, is that the existential claims associated with each explanation are open to a serious charge: the same empirical phenomena can be equally well explained by the existence of radically different entities (in the case of scientific realism) and radically different structures (in the case of structural realism). This is, of course, the familiar underdetermination argument: nothing in the phenomena uniquely determines the nature of the objects that are supposed to explain the successful predictions. This point is very clear in the case of quantum mechanics; a theory that, despite its tremendous empirical success, leaves the issue of the nature of quantum particles completely undecided. These particles can be thought of as Copenhagen objects (lacking either a sharp position or a sharp momentum, at least when measured simultaneously), or the particles can be conceived as Bohmian objects (in which case they do have well-defined position and momentum). But nothing in the phenomena favors one interpretation over the other. So, the scientific realist cannot specify the nature of the objects that are supposedly explaining quantum theory's success. Similar points can be made in the case of other scientific theories. Consider, for example, debates about the unit of selection in evolutionary biology. The existence of deep disagreements about the correct interpretation of these theories clearly indicates that such theories fail to determine uniquely the nature of their ontological posits.

The same problem also arises in the case of structural realism. As we saw above, we also face underdetermination: now at the level of structures. Multiple, non-equivalent structures can be used to explain the same phenomena, and there is *no purely structural* way of selecting among such structures. To invoke *pragmatic* criteria in structure selection, being a typical anti-realist move, would only reinforce a structural *empiricist* view. As a result, just as the scientific realist, the structural realist doesn't seem to be in a position to claim that he/she is able to explain the empirical success of science.

For the structural empiricist, novel predictions are just a feature of a selective process of theory construction. Since in the process of theory construction most of the time scientific theories don't quite work, and are swiftly ruled out, it's not surprising that the theories that do yield correct, empirically successful, results are preserved. From this perspective, it's not clear that novel predictions are so mysterious as the realist takes them to be. In the end, all it takes to make sense of empirical success, even regarding novel phenomena, is to have models that relate to the phenomena in the right way – yielding empirically adequate theories, even though not true ones (see van Fraassen 1980).

5.4 Conclusion

Despite its many attractive features, structural realism still faces some difficulties. The ones I raised here are not conclusive, of course. Rather, they are an invitation for structural realists to develop the position further.

Meanwhile, empiricists who are sympathetic with the structural components in structural realism have the beginning of an alternative in structural empiricism. Here also more work is needed. But, in the end, that's how philosophical debates advance: one step at a time.

References

Azzouni, J. (1997): "Thick Epistemic Access: Distinguishing the Mathematical from the Empirical", *Journal of Philosophy 94*, pp. 472–484.

Azzouni, J. (1998): "On 'On What There Is'", *Pacific Philosophical Quarterly 79*, pp. 1–18.

Azzouni, J. (2004): *Deflating Existential Consequence*. New York: Oxford University Press.

Birkhoff, G., and von Neumann, J. (1936): "The Logic of Quantum Mechanics", *Annals of Mathematics 37*, pp. 823–843. (Reprinted in von Neumann (1962), pp. 105–125.)

Bueno, O. (1997): "Empirical Adequacy: A Partial Structures Approach", *Studies in History and Philosophy of Science 28*, pp. 585–610.

Bueno, O. (1999): "What is Structural Empiricism? Scientific Change in an Empiricist Setting", *Erkenntnis 50*, pp. 59–85.

Bueno, O. (2005): "Dirac and the Dispensability of Mathematics", *Studies in History and Philosophy of Modern Physics 36*, pp. 465–490.

Butterfield, J., and Pagonis, C. (eds.) (1999): *From Physics to Philosophy*. Cambridge: Cambridge University Press.

Cassirer, E. (1936): *Determinism and Indeterminism in Modern Physics*. (Translated by O. Theodor Benfey in 1956.) New Haven: Yale University Press.

Chakravartty, A. (1998): "Semirealism", *Studies in History and Philosophy of Science 29*, pp. 391–408.

Chiappin, J.R.N. (1989): *Duhem's Theory of Science: An Interplay Between Philosophy and History of Science*. Ph.D. dissertation, University of Pittsburgh.

da Costa, N.C.A., and Krause, D. (1994): "Schrödinger Logics", *Studia Logica 53*, pp. 533–550.

da Costa, N.C.A., and Krause, D. (1997): "An Intensional Schrödinger Logic", *Notre Dame Journal of Formal Logic 38*, pp. 179–194.

Dalla Chiara, M.L., Doets, K., Mundici, D., and van Bentham, J. (eds.) (1997): *Logic and Scientific Methods*. Dordrecht: Kluwer Academic Publishers.

Demopoulos, W., and Friedman, M. (1985): "Critical Notice: Bertrand Russell's *The Analysis of Matter*: Its Historical Context and Contemporary Interest", *Philosophy of Science 52*, pp. 621–693.

Dirac, P.A.M. (1930): *The Principles of Quantum Mechanics*. Oxford: Clarendon Press.

Duhem, P. (1906): *The Aim and Structure of Physical Theory*. (An English translation, by P.P. Wiener, was published in 1954.) Princeton: Princeton University Press.

Field, H. (1980): *Science Without Numbers: A Defense of Nominalism.* Princeton, N.J.: Princeton University Press.

French, S. (1989): "Identity and Individuality in Classical and Quantum Physics", *Australasian Journal of Philosophy 67*, pp. 432–446.

French, S. (1999): "Models and Mathematics in Physics: The Role of Group Theory", in Butterfield and Pagonis (eds.) (1999), pp. 187–207.

French, S. (2000): "The Reasonable Effectiveness of Mathematics: Partial Structures and the Application of Group Theory to Physics", *Synthese 125*, pp. 103–120.

French, S. (2006): "Structure as a Weapon of the Realist", *Proceedings of the Aristotelian Society 106*, pp. 167–185.

French, S., and Krause, D. (1995): "Vague Identity and Quantum Non-Individuality", *Analysis 55*, pp. 20–26.

French, S., and Krause, D. (2006): *Identity in Physics*. Oxford: Clarendon Press.

French, S., and Ladyman, J. (2003a): "Remodelling Structural Realism: Quantum Physics and the Metaphysics of Structure", *Synthese 136*, pp. 31–56.

French, S., and Ladyman, J. (2003b): "The Dissolution of Objects: Between Platonism and Phenomenalism", *Synthese 136*, pp. 73–77.

Greffe, J.-L., Heinzmann, G., Lorenz, K. (eds.) (1996): *Henri Poincaré: Science and Philosophy*. Berlin: Akademie Verlag.

Hellman, G. (1989): *Mathematics Without Numbers: Towards a Modal-Structural Interpretation.* Oxford: Clarendon Press.

Hilbert, D., Nordheim, L., and von Neumann, J. (1927): "Über die Grundlagen der Quantenmechanik", *Mathematische Annalen 98*, pp. 1–30. (Reprinted in von Neumann 1961.)

Kant, I. (1787): *Critique of Pure Reason*. (Translated in 1929 by Normal Kemp Smith.) London: Macmillan Press.

Krause, D. (1992): "On a Quasi-Set Theory", *Notre Dame Journal of Formal Logic 33*, pp. 402–411.

Krause, D. (1996): "Axioms for Collections of Indistinguishable Objects", *Logique et Analyse 153–154*, pp. 69–93.

Krause, D., and French, S. (1995): "A Formal Framework for Quantum Non-Individuality", *Synthese 102*, pp. 195–214.

Ladyman, J. (1998): "What is Structural Realism?", *Studies in History and Philosophy of Science 29*, pp. 409–424.

Laudan, L. (1996): *Beyond Positivism and Relativism*. Boulder: Westview Press.

Lewis, D. (1980): "Veridical Hallucination and Prosthetic Vision", *Australasian Journal of Philosophy 58*, pp. 239–249. (Reprinted, with a postscript, in Lewis (1986), pp. 273–290.)

Lewis, D. (1986): *Philosophical Papers*, Volume II. Oxford: Oxford University Press.

Mackey, G.W. (1993): "The Mathematical Papers", in Wigner (1993), pp. 241–290.

Muller, F.A. (1997): "The Equivalence Myth of Quantum Mechanics", *Studies in History and Philosophy of Modern Physics 28*, pp. 35–61; 219–247.

Poincaré, H. (1905): *Science and Hypothesis*. New York: Dover.

Psillos, S. (1995): "Is Structural Realism the Best of Both Worlds?", *Dialectica 49*, pp. 15–46.

Putnam, H. (1980): "Models and Reality", *Journal of Symbolic Logic 45*, pp. 464–482. (Reprinted in Putnam (1983), pp. 1–25.)

Putnam, H. (1981): *Reason, Truth and History*. Cambridge: Cambridge University Press.

Putnam, H. (1983): *Realism and Reason*. Cambridge: Cambridge University Press.

Quine, W.V. (1976): *The Ways of Paradox and Other Essays*. (Revised and enlarged edition.) Cambridge, Mass.: Harvard University Press.

Rédei, M. (1997): "Why John von Neumann did not Like the Hilbert Space Formalism of Quantum Mechanics (and What He Liked Instead)", *Studies in History and Philosophy of Modern Physics 28*, pp. 493–510.

Redhead, M. (1995): *From Physics to Metaphysics*. Cambridge: Cambridge University Press.

Resnik, M. (1997): *Mathematics as a Science of Patterns*. Oxford: Clarendon Press.

Russell, B. (1927): *The Analysis of Matter*. London: Routledge.

Schrödinger, E. (1926): "Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen", in Schrödinger (1927), pp. 45–61.

Schrödinger, E. (1927): *Collected Papers on Wave Mechanics*. (Translated by J.F. Shearer.) New York: Chelsea.

Shapiro, S. (1991): *Foundations Without Foundationalism: A Case for Second-Order Logic*. Oxford: Clarendon Press.

Shapiro, S. (1997): *Philosophy of Mathematics: Structure and Ontology*. New York: Oxford University Press.

van Fraassen, B.C. (1980): *The Scientific Image*. Oxford: Clarendon Press.

van Fraassen, B.C. (1991): *Quantum Mechanics: An Empiricist View*. Oxford: Clarendon Press.

van Fraassen, B.C. (1997): "Structure and Perspective: Philosophical Perplexity and Paradox", in Dalla Chiara *et al*. (eds.) (1997), pp. 511–530.

von Neumann, J. (1932): *Mathematical Foundations of Quantum Mechanics*. (English translation, by Robert T. Beyer, first published in 1955.) Princeton: Princeton University Press.

von Neumann, J. (1961): *Collected Works, vol. I. Logic, Theory of Sets and Quantum Mechanics*. (Edited by A.H. Taub.) Oxford: Pergamon Press.

von Neumann, J. (1962): *Collected Works, vol. IV. Continuous Geometry and Other Topics*. (Edited by A.H. Taub.) Oxford: Pergamon Press.

von Neumann, J. (1981): "Continuous Geometries with a Transition Probability", *Memoirs of the American Mathematical Society 34*, No. 252, pp. 1–210.

Weyl, H. (1927): "Quantenmechanik und Gruppentheorie", *Zeit. für Phys. 46*, pp. 1–46.

Weyl, H. (1931): *The Theory of Groups and Quantum Mechanics*. (Translated from the second, revised German edition by H.P. Robertson. The first edition was published in 1928.) New York: Dover.

Wigner, E.P. (1931): *Group Theory and Its Application to the Quantum Mechanics of Atomic Spectra*. (English translation published in 1959.) New York: Academic Press.

Wigner, E.P. (1993): *The Collected Works of Eugene Paul Wigner. The Scientific Papers*, volume 1. (Edited by Arthur S. Wightman.) Berlin: Springer-Verlag.

Worrall, J. (1989): "Structural Realism: The Best of Both Worlds?", *Dialectica 43*, pp. 99–124.

Zahar, E. (1996): "Poincaré's Structural Realism and his Logic of Discovery", in Greffe *et al.* (eds.) (1996), pp. 45–68.

Zahar, E. (1997): "Poincaré's Philosophy of Geometry, or does Geometric Conventionalism Deserve its Name?", *Studies in History and Philosophy of Modern Physics 28*, pp. 183–218.

Chapter 6
Structural Realism: Continuity and Its Limits

Ioannis Votsis

6.1 Introduction

Structural realism comes in various shapes and sizes. First there is the epistemic kind which holds that at best we can have knowledge of the structure of the world. This comes in two main flavours: à la Ramsey (e.g. John Worrall and Elie Zahar 2001) claiming that the structure of the world is reflected in the Ramsey sentence of successful scientific theories and à la Russell (e.g. Ioannis Votsis 2005) claiming that we can infer certain things about the structure of the world from the structure of our perceptions. Then there is the ontic kind which also comes in a multitude of flavours, three of which stand out: (i) the 'no objects view' (e.g. James Ladyman 1998) according to which there exist no objects only structures, (ii) the 'no individuals view' (e.g. Steven French and Decio Krause 2006) which maintains that there exist no individuals but only structures and objects lacking individuality and (iii) the 'no intrinsic natures view' (e.g. Ladyman 2007) which eliminates intrinsic natures in favour of haecceity-free individuals and structures.[1] Finally, there is the methodological kind which concentrates on the role shared structure plays in characterising scientific theories, in relating high-level theory to low-level data, and in identifying links between predecessor and successor theories (see Katherine Brading and Elaine Landry 2006).

That the different kinds of structural realism share less than their name suggests is something that is increasingly becoming apparent.[2] One major disagreement

---

[1] Concerning the 'no objects view', Ladyman insists (in private communication) he never intended to say that no objects exist. He admits, however, that certain of his early pronouncements have contributed to this misinterpretation.

[2] In a recent workshop on structural realism organised in Banff by Elaine Landry, Ladyman urged the participants, who parenthetically represented almost the whole spectrum of different structural realist positions and included most of the main players, to find a mutually agreeable formulation of what is common to all. What became clear by the end of the workshop was that no such formulation can easily be produced.

I. Votsis (✉)
Philosophisches Institut, Heinrich-Heine-Universitaet Duesseldorf,
Universitaetstrasse 1, Duesseldorf 40225, Germany
e-mail: votsis@phil-fak.uni-duesseldorf.de

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_6,

relates to the way structure gets demarcated. It is not merely a question of which formal tools are best equipped for the job, e.g. Ramseyfication, set theory, group-theory, category theory, etc. It is also a question of how we draw the line between the structural and the non-structural. For example, some circumscribe the structural so as to potentially include structural information about intrinsic properties (e.g. Votsis), while others deny the very existence of intrinsic properties (e.g. Ladyman).

Disagreements aside, all structural realists (bar perhaps the methodological ones) appropriate the preservation of structure through historical theory change as evidence for their respective views. They thus endorse what I will henceforth call ‘the structural continuity claim’. Roughly put, this is the claim that the structure of successful scientific theories survives theory change in virtue of the fact that it correctly reflects structural features of the world. In other words, structures are preserved through theory change on account of their truth or approximate truth – hereafter designated by the phrase ‘(approximate) truth’.

The structural continuity claim makes its debut appearance in Henri Poincaré’s *Science and Hypothesis*. As John Worrall (1989) first pointed out, Poincaré utilises the structural continuity claim to motivate a version of epistemic structural realism.[3] Poincaré argues that only structural features of theories survive theory change and the reason for their survival is that they have somehow latched on to the structure of the world. He cites the preservation of Augustin-Jean Fresnel’s equations in James Clerk Maxwell’s electromagnetic theory as evidence for that claim. The equations successfully describe the reflection and refraction of light when it is passing through media of different refractive indices. Under Fresnel’s interpretation, light consists of vibrations that are transmitted through the ether, a ubiquitous yet virtually imperceptible material medium. Of crucial importance is the fact that Fresnel’s interpretation of the nature of light is not necessary for the empirical exploitation of Maxwell’s equations and, a fortiori, not necessary for the empirical exploitation of Fresnel’s own equations since the latter can be derived from the former. It is no wonder then that the interpretation is made redundant in Maxwell’s mature electromagnetic theory. This is taken by Poincaré and subsequent structural realists to mean that Fresnel’s (and Maxwell’s) equations provide at most a structural account of light.

Let us now reconstruct the argument whose conclusion is the more polished structural continuity claim:

1. Only structural elements of predictively and explanatorily successful scientific theories have been (and will be) preserved through theory change.
2. Preservation of an element implies its (approximate) truth.
3. Non-preservation of an element implies its (approximate) falsity.

∴ The preservation of structural elements of predictively and explanatorily successful scientific theories through theory change implies their (approximate) truth. The non-preservation of non-structural elements implies their (approximate) falsity.

---

[3] The structural continuity claim is not the sole motivation for Poincaré’s epistemic structural realism as I indicate in Votsis (2004, ch. 2).

A few qualifications are in order. First, by 'elements' I mean statements about the world that have a truth value. Structural elements are truth-valued statements that have both observable and unobservable content but whose unobservable content is purely structural. Second, what counts as unobservable may differ in some of these accounts. Third, since different conceptions of structure may lead to different formulations of this argument I have formulated the argument in the most general way possible, i.e., without specifying where to draw the line between structure and non-structure. After all, structural realists of different stripes agree on the relevant historical facts when they are neutrally stated, e.g. that a set of equations belonging to some past theory is derivable from its successor. Their disagreement lies only in the interpretation of such facts as evidence for their specific brand of structural realism.

In this paper I aim to elucidate, improve, and extend the structural continuity claim and its associated argument. In so doing, I will not presuppose a particular conception of structure that favours this or that kind of structural realism, but will instead focus on how structural realists can best handle the historical facts. A positive consequence of this approach is that the results will be pertinent to both epistemic and ontic structural realists. A negative consequence is that various significant issues, such as how best to draw the structure vs. non-structure distinction as well as whether the distinction is even feasible (questioned, for example, in Bas van Fraassen 2006, p. 290 and in Stathis Psillos 1999, p. 157), will be left untouched.[4] My intended audience is therefore those who at least provisionally accept that the structure vs. non-structure distinction can be drawn but are unclear about the details – those that are independent of the particular form of the distinction – of the structural continuity claim and its associated argument.

6.2 Not All Structures Are Preserved

Not all structures are created equal. Some play no active role in the predictive and explanatory success of a theory because they do not correspond to any structure in the world. Their non-preservation does not therefore encumber the structural realist. Traditional scientific realists have long employed a distinction between essential and idle posits to weed out those elements of theories that played no substantial role in their predictive and explanatory success. An analogous distinction is required for the structural realists. Henceforth I will brand *operative* those structures that are responsible for a theory's genuine predictive and explanatory success. Those that do not meet this condition, I will brand *inoperative*.

Under the foregoing characterisation, Fresnel's equations count as operative structural elements, for they are arguably the sole purveyors of the success enjoyed

---

[4] I have defended a version of the structure vs. non-structure distinction in Votsis (2007).

by Fresnel's theory of light. More examples of operative structural elements will be discussed in the sections below. For now let us consider an example of an inoperative structural element. Such examples are plentiful in the history of science. After all, most conjectures, structural or other, are likely to be predictively and explanatorily unsuccessful. Take August Weismann's claim that different cells contain different components of hereditary material and are distributed to different parts of an organism's body so as to locally oversee that part's development. In his bid to defeat structural realism, Kyle Stanford (2006, p. 181) offers Weismann's claim as an example of a structural element from biology that did not survive theory change. Contra Stanford, we can simply point out that Weismann's claim did not enjoy genuine predictive success. Thus Weismann's claim is not merely a structural element that did not survive theory change but also an inoperative element. For this reason its abandonment does not threaten the structural realist.

Modified accordingly, premise one now reads as follows.

1a. All and only operative structural elements of scientific theories have been (and will be) preserved through theory change.

Two provisos need to be made here. First, the clause 'predictively and explanatorily successful', which is now packaged inside the concept 'operative,' is applied to structural elements, not to whole theories. Second, the reformulation makes clear that not only are all predictively and explanatorily successful elements that survive structures, but also that all predictively and explanatorily successful structures survive.

6.3 Not All Preserved Structures Are Intact

As many authors have rightly pointed out the neat preservation of structure exhibited by the Fresnel-Maxwell case is atypical in the history of science (e.g. Michael Redhead 2001).[5] More often a structure belonging to a superseded theory can be recovered only as a limiting case of a successor theory's structure. Aware of this, Worrall (1989) reasoned that structural realism benefits from 'limiting case' survival when appeal is made to the general correspondence principle. According to Heinz Post's formulation, "this is the requirement that any acceptable new theory L should account for its predecessor S by 'degenerating' into that theory under those conditions under which S has been well confirmed by tests" (Post Heinz 1971, p. 228). Worrall notes that given that the principle operates solely on the mathematical level, its applicability "is not evidence for full-blown realism – but, instead, only for structural realism" (Worrall 1989, p. 161).

---

[5] It is atypical but not unique. Several structures postulated within the framework of the caloric theory of heat have survived the theory's demise and are still with us today, e.g., Sadi Carnot's principle of maximum efficiency.

A refinement of premise one that takes into consideration the need to employ the general correspondence principle takes the following form:

1b. All and only operative structural elements of scientific theories have been (and will be) preserved through theory change either intact by derivation or suitably modified in accordance with the correspondence principle.

Worrall's remarks on the link between the general correspondence principle and structural realism are rather brief and suggestive. Luckily, Redhead has made some progress on this front.

> Consider a one-parameter family of structures $\{S_p\}$ where the parameter p is a continuously variable real number. Let us suppose for values of p unequal to zero the structures S_p are all qualitatively the same, as p varies the structure changes, but in a continuous way. But suppose the change in structure suffers a discontinuity at the point $p=0$, S_0 is qualitatively distinct from all the S_p with $p \neq 0$. We may say that the family of structures is *stable* for $p \neq 0$, but exhibits a *singularity* at $p=0$ (ibid., p. 86) [original emphasis].

Redhead thus identifies two kinds of structure transformations: continuous and discontinuous.[6] Whether or not a structure transformation is discontinuous depends on what makes a structure the kind of structure it is, i.e., what we deem to be its essential (read: defining) features. Redhead offers an instructive example from geometry. Think of transforming a circle on a Euclidean plane into any other closed curve. Suppose the essential features in this case are the following: (a) that the shape completely encloses an area and (b) that it has no endpoints. Since these two conditions are essential features of closed curves (including circles), the transformation is continuous with respect to the relevant group of homeomorphisms. Contrast this with the transformation of any closed curve (including a circle) into a straight line. In this case (a) and (b) are lost and so the transformation qualifies as discontinuous.

In his example Redhead neglects to highlight that some continuous transformations can easily be turned into discontinuous ones if the appropriate essential features are available and chosen. Think of the circle-to-closed curve transformation again. One of the defining characteristics of circles is that they possess a unique point equidistant from the set of points that bounds them, i.e., a centre. Modulo this essential feature the aforementioned transformation becomes discontinuous, as no other closed curves share this feature with circles.

As it stands, the notion of discontinuous transformation fails to do justice to the varying degrees of discontinuity. For example, there is a clear sense in which the circle-to-closed curve transformation is less discontinuous than the circle-to-straight line transformation. To redress this issue we need to establish a more fine-grained account of discontinuous transformations. A first step in the right direction is to divide the original notion into two notions: 'partially discontinuous' and 'fully discontinuous'. The first notion applies when the transformation brings about the loss of some but not all of the essential features that a structure possesses. In general, the less essential the features lost are, the more continuous the transfor-

---

[6] Though not a structural realist, Robert Batterman (2002, pp. 17–19) draws a similar distinction between reduction (where the limit is regular) and intertheoretic relations (where it is singular).

mation is likely to be. The second notion applies when all essential features are lost. Changes of this magnitude make it difficult, if not impossible, to claim that successor structures have a non-accidental kinship to predecessor structures.[7] Although refinements to these notions and perhaps even additional notions are required to deal with further problems, e.g. some essential features may be more essential than others and hence will need to be differentially weighted, the two notions will do for the purposes of this paper.

At this point it is worth asking how the different kinds of structure transformations fit into the puzzle of relating old and new structures. The simple answer is that they are either all exemplified in the history of science or they could be so exemplified. Since discontinuous transformations are quite prevalent in modern physics and indeed more challenging to legitimate as genuine cases of substantial continuity I will focus the discussion on them. To explain the rationale behind such transformations, imagine, as a first approximation, a successor structure as typically possessing one or more additional parameters than its predecessor. We can think of the predecessor structure as a less approximately true, more idealised version of the successor structure (e.g. Wladyslaw Krajewski 1977). Neutralising these parameters from the successor structure thereby allows us to recover the predecessor one. In the above framework, the neutralisation of a parameter is achieved by suitably modifying its value, e.g. by setting it to zero. Assuming, as it seems we must, that the parameter at issue corresponds to an essential feature of the successor structure entails that neutralising it amounts to the removal of that feature and hence to a discontinuous transformation. In cases where some essential features survive the transformation we can speak of partial continuity or partial discontinuity. When all essential features are lost we can speak of full discontinuity. Only fully discontinuous transformations are undesirable for the task of supporting structural realism.

We are now in a position to unveil a correspondence relation for the structural realist:

A structure S' and its predecessor structure S correspond if and only if with respect to a given parameter class there is a transformation from S' to S that is either (a) continuous or (b) partially discontinuous.

In light of our discussion of discontinuous transformations, I suggest that we modify the first premise thus:

1c. All and only operative structural elements of scientific theories have been (and will be) preserved through theory change either intact by derivation or via a transformation from new to old structure that is either (i) continuous or (ii) partially discontinuous.

What makes discontinuous transformations capable of supporting the structural continuity claim? Astonishingly, one finds little by way of argument in Redhead's otherwise fecund paper. He resorts to metaphorical language, claiming that, if, like

---

[7] It may still be possible that two structures are somehow partially continuous on the basis of non-defining features. I mention this only as food for thought as I do not really put much trust in the claim that continuity of this kind is sufficient for (structural) realist purposes.

the mathematician, we see how natural the leap is to introduce or remove a feature from a structure, then we realise that discontinuous transformations of structures in physics are cases of structure preservation (ibid., p. 88). Discontinuous transformations must be put on firmer footing than this. I have already intimated how this may be done. The introduction of the two notions of discontinuous transformation brings out the fact that some transformations are unreservedly radical while others less so. Surely the latter are capable of supporting the structural continuity claim for they display constancy with respect to some essential features between old and new structures. To stress this point in a different way, just think how improbable it would be that any two structures accidentally happen to be connected via partially discontinuous transformations. To test this, take an algorithm that generates (pseudo-) random pairs of structures. Because a great many structures share no essential features at all, the odds of getting a pair that corresponds via partially discontinuous transformations are very small.

6.4 Not All Structures Have Predecessors

Not all successor equations have limiting case analogues in the predecessor theory. Hans Radder (1996) cites the relativistic equation $E = m_0c^2$ for a particle's energy with rest mass m_0. No analogue of it exists in classical mechanics so any talk of structure transformation from new to old theory would be pointless. Some philosophers tout this fact as detrimental to the general correspondence principle. Since the structural continuity claim banks on the principle, the objection threatens to derail structural realism itself.

A more careful look at the general correspondence principle reveals how remarkably easy it is to answer this objection. It is merely a matter of revealing how the objection confounds the scope of the principle. The principle does not require that all (successful) successor structures correspond to (successful) predecessor structures. Let's not forget that in the (structural) realist's eyes successor theories will venture beyond their predecessors, describing and predicting new classes of phenomena with the help of completely new structures. What the principle requires is that all (successful) predecessor structures correspond to (successful) successor structures. As such the objection leaves the general correspondence principle unfazed.

6.5 Kuhn Loss

The term 'Kuhn loss' seems to have been coined by Post (ibid.). He quotes a relevant passage from Thomas Kuhn who says "new paradigms seldom or never possess all the capabilities of their predecessors" (Kuhn [1962]1996, p. 169). What does Kuhn mean by capabilities? His scattered thoughts on the matter seem to mostly point to the capability of explaining phenomena. For example he speaks of the loss of such capabilities in terms of the new paradigm being deprived of "some actual and much potential explanatory power" and of its "failure to explain" (Kuhn [1962]1996, p. 107). If such losses exist, they seem to undermine the realist claim that successor theories incorporate all of the successes of their predecessors and hence are strictly more approximately true than their predecessors. How does this affect the structural continuity claim? Suppose such losses were operative structural elements. Under this supposition it would no longer be true that all operative structural elements survive theory change and therefore the structural continuity claim would be false.

There are two main readings of Kuhn's view. According to the narrow reading, offered by Post, a Kuhn loss is the "loss of *successful* explanatory power" (ibid., p. 229, emphasis added). Post goes on to clarify that Kuhn-losses are those well-confirmed parts of a superseded theory that were not saved in its successor and rejects that any such losses ever occur (p. 230). By contrast, Alexander Bird's interpretation of Kuhn's view is more relaxed, requiring only that a phenomenon "in an earlier period was *held to be successfully* explained" (Bird 2004, emphasis added). As we shall shortly see the controversy over the occurrence of Kuhn-losses hinges on how widely one reads the loss of explanatory power.

Let us first consider the wide notion of Kuhn loss. Thus defined the notion has various historical instantiations. A frequently discussed example concerns the loss by Newtonians of the Cartesian ability to explain why "the planets lie in approximately the same plane" and why "planets orbit the sun in the same direction [and indeed in the same direction as the Sun's spin]" (McAllister 2007, p. 18).[8] According to this explanation, the planets, and any other celestial objects for that matter (including comets), are kept in orbit around a star by hitching a ride on the same fluid vortex. As the vortex turns only in one direction so do the objects that ride on it.

The explanation was certainly 'held to be successful' by some and hence qualifies as an instantiation of the wide notion of Kuhn loss. It does not, however, qualify as an instantiation of the narrow notion since the explanation was never well-confirmed. Over hundreds of years no such thing as a fluid vortex has ever been detected. Moreover, there are positive reasons to reject the Cartesian explanation because it does not account for the following anomalies. Various objects in our solar system, e.g. Neptune's moon Triton as well as comet Halley, travel in the opposite direction to the Sun's spin. The same irregularities seem to hold for solar systems other than our own. A recently discovered exoplanet (WASP-17b) is the first known to travel against its star's rotation.[9] That the orbit of objects in a solar system all lie in approximately the same plane is also falsified by the existence of objects like the dwarf planet Pluto whose orbit is highly inclined.

The above problems clearly illustrate that the Cartesian explanation was never a serious contender. We still do not have a well-confirmed explanation regarding the orbits of objects around stars.[10] Realists (structural or other) need explanations, but

---

[8] For a similar point see also Paul Hoyningen-Huene (1993, p. 261).

[9] It is worth noting that we currently have evidence for the orbits of only around a dozen exoplanets.

[10] We only have a tentative account in the guise of the nebular hypothesis which provides sketches of the formation and evolution of solar systems.

not those lacking robust empirical merits. In sum, although the Cartesian explanation qualifies as a Kuhn loss under the wide construal of the notion, it is not the kind of loss that could challenge the cumulativity of scientific knowledge, or, in the case at hand, the structural continuity claim.

Let us then consider the narrow notion of Kuhn loss, i.e. the one that demands genuine empirical success from the lost ability. Despite all the commotion surrounding Kuhn loss, finding examples that satisfy this stronger notion is not an easy task. Radder (ibid., p. 63) puts forth Poiseuille's law as one such example – the only one it seems. The law $Q = \pi r^4 P/8\eta L$ determines an (nearly) incompressible fluid's rate of laminar flow Q along a tube as a relation between the following quantities: the fluid's viscosity η, the radius r and length L of the tube and the pressure difference between the tube's two ends P. The law is arguably a structural element, as it requires no 'non-structural interpretation of the ontology of the involved quantities. It is also an operative element since it has been used to provide explanations and accurate quantitative predictions in a number of different domains including medicine where it is used to calculate blood flow. Crucially, and according to Radder, it is impossible to reproduce this law from quantum mechanical accounts of fluids. It thus seems to be a bona fide case of Kuhn loss in the narrow sense, threatening to undo the structural continuity claim.

Alas, one plain fact has been neglected. Poiseuille's law was never abandoned! It is in use today and can be found in numerous scientific textbooks. Even so, according to Post's narrow definition, Poiseuille's law still counts as a Kuhn loss precisely because it is not preserved in the successor theory. This only indicates the need for a narrower definition of Kuhn loss. The sense of loss that really matters to the realist is when the said theory part is no longer available for predictive and explanatory exploitation. That is clearly not the case here. Hence Poiseuille's law is not a genuine Kuhn loss according to this more sensible account of Kuhn loss. Having said this, Poiseuille's law presents another problem for (structural) realism. Up till now we have required that old structures be suitably preserved in new structures. Poiseuille's law is preserved but independently of any new structure. This contradicts what we required previously, namely that all successful predecessor structures have corresponding successor structures.

Despite appearances, the game is not lost for the (structural) realist. Some realists will no doubt argue that Poiseuille's law will eventually be derived from quantum mechanics when the right auxiliary hypotheses emerge. Bar that prospect, I want to maintain that there is nothing dire about the independent survival of a predictively and explanatorily successful structure. The structural continuity claim merely needs to be amended. New paradigms, theories or structures need not replace old ones in toto.[11]

---

[11] Radder (in e-mail communication) points out that this idea spells the end of convergent realism. Although strictly speaking correct, this does not mean that we slip back into anti-realism. After all, what I say here, i.e. that predictively and explanatorily successful theory parts still survive theory change, is consistent with realism. Moreover, not always having full convergence towards one or more successor theories does not mean having no convergence whatsoever. Remember, in the case at hand, Poiseuille's law is the exception, not the rule!

That is, they need not range over all the old domains of phenomena, though we certainly expect them to unify a substantial chunk of the old domains with new domains of phenomena. So long as the unaccounted for domains are preserved nothing is really lost. Designations like '*the* successor' are therefore clearly hyperbolic. The same point can be demonstrated in a much simpler way by reminding oneself of the fact that there exist two successors to the Newtonian paradigm, viz., relativity theory and quantum mechanics.

This brings us to the final qualification of premise one.

1d. All and only operative structural elements of scientific theories have been (and will be) preserved through theory change either (a) intact by derivation or (b) via a transformation from new to old structure that is either (i) continuous or (ii) partially discontinuous or (c) intact but independent of any currently accepted structures.

6.6 Inferences from Preservation

Premises two and three of the structural continuity argument add up to the following claim: The preservation of an element is a necessary and sufficient condition of its (approximate) truth.[12] No realist, I hope, ought to be happy to adopt such a strong claim. The preservation of an element through theory change is neither a necessary nor a sufficient condition for its (approximate) truth.

It is not a necessary condition because the preservation of a(n) (approximately) true element is not guaranteed. An element might be cast aside because it is, or at least it seems to be, incompatible with certain parts of other theories. Perhaps instruments capable of assessing its empirical merits have not yet been invented. Even worse, it might be that no instrument capable for this assessment can be constructed.[13] Thus an (approximately) true element may find itself thrown into the wastebasket of history. Kuhn losses, in the narrower construal of the concept, are after all genuine possibilities.

Far from being outlandish, the necessity condition's failure can be witnessed in the actual historical record. Take the central claim of the kinetic theory of heat. The idea that heat is due to the motion of particles can be traced back to antiquity. It thereafter vanished only to reappear in the sixteenth century. Francis Bacon famously remarked that 'heat itself, its essence and its quiddity, is motion and nothing else'. Yet it was not until the nineteenth century when the work of bold experimentalists like Count

---

[12] That the third premise amounts to preservation being a necessary condition of an element's (approximate) truth is more clearly seen when formulated in its contrapositive form, i.e. the (approximate) truth of an element implies its preservation.

[13] This last scenario finds support in some interpretations of the measurement problem in quantum mechanics.

Rumford, Humphry Davy and James Prescott Joule, as well as the advent of new ideas like energy conservation, allowed the successful development and rise to dominance of the kinetic theory of heat. To those itching to point out that the kinetic theory's central claim did eventually survive, it is worth reminding that premise three is tenseless. In other words, one should be able to apply the inference at any historical period, including the period between antiquity and the sixteenth century and not merely from the sixteenth century onward. It is also worth reminding that we could not be talking about a specific (approximately) true element that did not survive (during some period) unless that element did in due course survive. Needless to say that some (approximately) true elements may be lost, never to be rediscovered.

Preservation is not a sufficient condition because the mere survival of a given element does not guarantee its (approximate) truth. This point has also been made by Hasok Chang (2003), though for reasons that do not exactly coincide with mine. Preservation does not guarantee (approximate) truth because it might simply be a by-product of the conservativeness of scientific theorizing. A well-document aspect of this conservativeness is our penchant for anthropomorphic, anthropocentric and teleological explanations. Thus for a long time it was natural to suppose the truth of the principle that an external force is required to keep things in motion. Our trust in this principle, as is well known, was withdrawn as a consequence of our acceptance of the law of inertia.

A strict preservationist will no doubt protest against both my necessity and sufficiency objections. Had the scientific community been able to test the elements at issue sooner, the preservationist will insist, they would have surely uncovered their empirical merits or lack thereof. Thus (approximately) true elements would be duly preserved and (approximately) false ones duly abandoned. Though this statement is largely correct, notice that now the empirical merits of elements take centre stage, not their state of preservation. In a nutshell, the issue of preservation becomes parasitic on the issue of empirical merits.

It has not been my intention to dismiss preservation as a hopeless idea but rather to shed light on its scope and the origin of its strength. A theoretical component's empirical merits and its preservation are substantially correlated simply because scientists preserve those components that have empirical merits. It is for that reason highly unlikely that narrowly construed Kuhn losses will be found in abundance.[14] This brings us to the final modification of the structural continuity argument.

1e. Approximately all and only operative structural elements of scientific theories have been (and will be) preserved through theory change either (a) intact by derivation, or (b) via a transformation from new to old structure that is either (i) continuous or (ii) partially discontinuous or (c) intact but independent of any currently accepted structures.

---

[14] Under the current qualification, isolated incidents of narrowly construed Kuhn losses are not sufficient to undermine the structural continuity claim.

2a. Preservation is a reliable guide to (approximate) truth.
3a. Non-preservation is a reliable guide to (approximate) falsity.
∴ The preservation of approximately all and only operative structural elements of scientific theories through theory change either via (a), (b) or (c) is a reliable guide to their (approximate) truth. The non-preservation of non-structural elements and inoperative structural elements is a reliable guide to their (approximate) falsity.

6.7 Conclusion

I do not expect what has been said above to be the final word on these matters. The more one studies the history of science the more one finds cases that deserve special attention. This in turn translates into amendments of the premises of the structural continuity argument and ultimately amendments of the structural continuity claim itself. These amendments will probably continue the tendency of relaxing the link between preservation and (approximate) truth. For this reason it is perhaps better to think of the structural continuity argument as inductively strong rather than as deductive. I hope that this essay has laid the foundation for a more focused debate on the shared commitments, desiderata and limits of structural realists.

Acknowledgements My sincerest thanks to Hans Radder who has provided helpful feedback. This work was in part made possible by funding from the German Research Foundation (Deutsche Forschungsgemeinschaft).

References

Bas C, Van Fraassen (2006), Structure: Its Shadow and Substance, *British Journal for the Philosophy of Science*, vol. 57(2): 275–307.

Batterman, Robert (2002), *The Devil in the Details: Asymptotic Reasoning in Explanation, Reduction, and Emergence*, New York: Oxford University Press.

Bird (2004), Thomas Kuhn, *The Stanford Encyclopedia of Philosophy*, Edward N. Zalta (ed.), URL = http://plato.stanford.edu/entries/thomas-kuhn/

Brading, Katherine and Elaine Landry (2006), Scientific Structuralism: Presentation and Representation, *Philosophy of Science*, vol. 73(5): 571–581.

French, Steven and Harmke Kamminga (1993), *Correspondence, Invariance and Heuristics: Essays in Honour of Heinz Post*, Boston Studies in the Philosophy of Science, vol. 148, Dordrecht: Kluwer.

French, Steven and Decio Krause (2006), *Identity in Physics*: *A Historical, Philosophical and Formal Analysis*, Oxford: Oxford University Press.

Hans, Radder (1996), *In and About the World: Philosophical Studies of Science and Technology*, New York: S.U.N.Y. Press.

Hasok, Chang (2003), Preservative Realism and Its Discontents: Revisiting Caloric, *Philosophy of Science*, vol. 70(5): 902–912.

Hoyningen-Huene, Paul (1993), *Reconstructing Scientific Revolutions*, Chicago: University of Chicago Press.

Ioannis, Votsis (2007), Uninterpreted Equations and the Structure-Nature Distinction, *Philosophical Inquiry*, vol. 29(1–2): 57–71.

James, Ladyman (1998), What Is Structural Realism? *Studies in History and Philosophy of Science*, vol. 29: 409–424.

John, Worrall (1989), Structural Realism: The Best of Both Worlds? in Papineau, D. (ed.) *The Philosophy of Science*, Oxford: Oxford University Press, 1996.

John, Worrall and Elie Zahar (2001), Ramseyfication and Structural Realism, Appendix IV, in E. Zahar, *Poincaré's Philosophy*: *From Conventionalism to Phenomenology*, Chicago and La Salle (IL): Open Court.

Kuhn ([1962]1996), *The Structure of Scientific Revolutions*, 3rd edition, Chicago: University of Chicago Press.

Kyle, Stanford (2006), *Exceeding Our Grasp*: *Science*, *History*, *and the Problem of Unconceived Alternatives*, Oxford: Oxford University Press

Ladyman, James (2007), Structural Realism, *The Stanford Encyclopedia of Philosophy*, Edward N. Zalta (ed.), URL =http://plato.stanford.edu/entries/structural-realism/

McAllister (2007), Introduction. Dilemmas in Science: What, Why, and How, in *Knowledge in Ferment*, Leiden: Leiden University Press.

Poincaré, Henri ([1905]1952), *Science and Hypothesis*, New York: Dover.

Post, Heinz R. (1971), Correspondence, Invariance and Heuristics, repr, in S. French and H. Kamminga (eds.), *Correspondence*, *Invariance and Heuristics*: *Essays in Honour of Heinz Post*, Boston Studies in the Philosophy of Science, vol. 148, Dordrecht: Kluwer Academic Press.

Redhead, Michael L.G. (2001), The Intelligibility of the Universe, in A.O'Hear (ed.) *Philosophy at the New Millennium*, Cambridge: Cambridge University Press.

Stathis, Psillos (1999), *Scientific Realism: How Science Tracks Truth*, London: Routledge.

Votsis, Ioannis (2004), *The Epistemological Status of Scientific Theories*: *An Investigation of the Structural Realist Account*, London School of Economics, unpublished PhD thesis.

Votsis, Ioannis (2005), The Upward Path to Structural Realism, *Philosophy of Science*, vol. 72(5): 1361–1372.

Wladyslaw, Krajewski (1977), *Correspondence Principle and Growth of Science*, Dordrecht: Reidel.

Zahar, Elie (2001), *Poincaré's Philosophy*: *From Conventionalism to Phenomenology*, Chicago and La Salle (IL): Open Court.

Chapter 7
Structuralism About Scientific Representation*

Martin Thomson-Jones

7.1 Introduction

I have two central aims in this paper, both relatively modest. The first is to present some ways of thinking about structuralism about scientific representation. After distinguishing two distinct structuralist theses about representation – "vehicle structuralism" and "content structuralism" – from one another and from various other structuralist theses about the sciences (Section 7.2), I will separate three different non-formal concepts of structure (Section 7.3.1), discuss their relationship to the familiar formal concepts (Section 7.3.2), and consider two different ways of explicating vehicle structuralism (Section 7.4). I will then go on (and this is the second aim) to present a line of argument for the conclusion that structural realists of a certain familiar sort should reject both vehicle structuralism and content structuralism, and, relatedly, the semantic view of theory structure (Sections 7.5 and 7.6). Appallingly, this conclusion may not be at odds with the commitments of any particular philosopher or group of philosophers, but I think it will be useful to spell out the connections explicitly nonetheless.

Along the way, I will show that both content structuralism and (one version of) vehicle structuralism face a threat of triviality, and limn some of the difficulties which result from the more obvious ways of trying to evade that threat (Sections 7.4 and 7.5); and, building on a discussion of the relationship between vehicle structuralism and content structuralism, I will suggest a counter to Callender and Cohen's (2006) scepticism about the idea that understanding scientific representation is a worthwhile philosophical pursuit in its own right (Section 7.5).

---

*Some of the points made in this paper were first presented as part of a talk given at PSA 2004, but did not make it into the paper which appeared in the proceedings (Thomson-Jones 2006). Thanks to Craig Callender, Anjan Chakravartty, James Ladyman, and Bas van Fraassen for helpful correspondence and discussion.

M. Thomson-Jones (✉)
Thomson-Jones Department of Philosophy, Oberlin College, Oberlin, OH 44074

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_7,

7.2 Structuralisms

We should begin by distinguishing a number of distinct structuralist theses about the sciences: two about scientific representation, two about scientific knowledge, and one about the metaphysics of the world we are investigating when we engage in scientific work.

First, vehicle structuralism, a thesis about the means we employ in scientific representation:

> (VS) All scientific representation is representation by means of structure

This is view is certainly in the air, and it may plausibly be claimed to be in the literature.[1] It is motivated at least in part, and for some, by an attachment to one version or another of the semantic view of theory structure: if we focus on the role of theories in scientific representation, see theories as being or representing via collections of models, and then take representation via models to be representation by means of structure, the view that scientific representation is representation by means of structure acquires a clear impetus.[2]

Second is content structuralism, a thesis concerning the content of our representations:

> (CS) All scientific representation is representation of structure

(We will consider the relationship between vehicle and content structuralism in Section 5.) Third comes limit structuralism, a thesis about the limits of scientific knowledge:

> (LS) We can know only the structure of the world

Fourth, scope structuralism, a more positive epistemological thesis:

> (SS) We can and do know much about the structure of the world

Fifth, and finally, metaphysical structuralism:

> (MS) There is only structure in the world

The two varieties of contemporary structural realism distinguished by Ladyman (1998), epistemological and metaphysical, can then be characterised by saying that although they are both committed to LS and SS, they differ over MS: metaphysical structural realism affirms it, and epistemological structural realism denies it (or perhaps takes no stand on it).[3]

---

[1] It is at least suggested by passages in French and Ladyman (1999), Brading and Landry (2006), and van Fraassen (2006a,b) and (2008, ch. 11), for example – which is not to say that all these authors would embrace the view, or that any of them would embrace it exactly as I have formulated it here. See also the last paragraph of this section for an important qualification.

[2] I will address the question of how vehicle structuralism might be understood in some detail in Section 7.4; and see the end of Section 7.6 for more on the connection to the semantic view.

[3] These are only partial characterisations of the two varieties of structural realism; in particular, no claim has been made about the knowability of the structure of the *unobservable* world. Note also that there is a harmless redundancy involved in this characterisation of metaphysical structural realism: given MS, a commitment to LS is unavoidable.

My focus is, first, on the two theses about scientific representation, and second, on the links between those theses and the epistemological theses.[4] One such link is straightforward: content structuralism entails limit structuralism. (That is, if all scientific representation is of structure, then scientific knowledge can only be knowledge of structure, as knowing that *P* involves representing the world as being such that *P*). In Section 7.6 we will encounter another, less direct link, but the point for now is just that the various theses can bear upon one another; and that, of course, adds to the interest of the theses about representation.

One last but very important point before we proceed: It might be objected that I have set up straw men by making vehicle and content structuralism claims about *all* scientific representation. If this is a concern, then we can insert a qualification into both theses throughout the following discussion: instead of making claims about all scientific representation, we can read them as making claims about all scientific representation via models and theories. They then become the claims that all scientific representation via models and theories is representation by means of structure, and that all scientific representation via models and theories is of structure, respectively.[5] Taken this way, there is much less room for complaints about straw men (or so it seems to me). In any case, it is worth noticing that the arguments given below go through whether they are read as concerning the stronger or the weaker versions of vehicle structuralism and content structuralism.[6] As the stronger formulations are simpler and more easily stated, I will employ them throughout; the reader can insert the relevant qualifications if she so chooses.[7]

7.3 Structure

Arguments in favour of any of the structuralist theses we have just delineated need to include some elaboration of the notion or notions of structure in play. If the four negative theses – VS, CS, LS, and MS – are to be interesting, furthermore, then the pressing issue is to ensure that the notions of structure being employed are thick enough to guarantee that something is ruled out. (Indeed, the philosophers who defend structuralist theses like these clearly take themselves not just to be ruling something out, but to be ruling out views actually held by other philosophers.) I will not be arguing for any of the theses in question, and so I am spared the full burden of that task; but I do want to recommend a particular set of concepts of structure as especially helpful for the purposes of thinking about structuralism concerning

---

[4] I will, for the most part, leave MS aside.

[5] Note that this way of putting things assumes nothing about the relationship between models and theories.

[6] See n. 23 for the one point at which it makes a difference.

[7] Though I will not be presenting an overall assessment of VS or CS in this paper, I offer arguments against VS in "Thomson-Jones" (in preparation a,b). I do not and would not argue that *no* scientific representation is representation by means of structure, however, nor (of course) that no scientific representation is of structure.

scientific representation. These concepts are relatively intuitive, and they are not formal concepts. (Or at least, they are relatively non-formal. For simplicity of presentation, I will leave aside this nuance in what follows, and speak instead in crudely dichotomous terms.) As I will explain, however, I think they have some advantages over the (significantly more) formal notions which tend to predominate in current discussions.

7.3.1 Three Notions of Structure

The first concept of structure I want to identify is the one at work in the architectural remark "There's a remarkable modernist structure on the corner of 57th and Parker." Here a structure is a particular object, an object which has parts that have various properties and stand in various relations to one another. In this sense, the only objects which do not count as structures are mereological atoms, if such there be; but we only *call* something a structure in this sense when we want to call attention to…well, to its structure in one of the other two senses we are about to consider. Those other senses will be primary in what follows. To avoid confusion, I will say 'structured object' to capture this first notion; 'structure' will be used exclusively in the second and third ways.

In the second sense, a structure is an arrangement of properties and relations amongst the parts of an object. A structured object has, or instantiates, or exemplifies, a structure in this second sense (I will use the three verbs interchangeably). Structures of this sort are thus good candidates for being universals, whereas structured objects are particulars.[8]

We can characterise a given structure of this sort by specifying the conditions under which an object, *X*, has it. So, for example, consider the concrete structure **S1**:

> *X* has **S1** iff there is a partition of *X* into five parts, *a*, *b*, *c*, *d*, and *e*, such that (i) *a* is above each of the other parts, but no other relations of aboveness obtain between the parts, and (ii) *b*, *c*, *d*, and *e* are all metallic, but *a* is not.

My desk provides an example of an object which instantiates **S1**; the partition I have in mind is one on which *a* is the desktop (which is glass), and *b*, *c*, *d*, and *e* are the legs.

A structure in this second sense can thus be characterised by a sentence of the form:

> *X* has *S* iff there is a partition of *X* into *n* parts such that…

where the ellipses are filled in by some collection of statements about properties possessed by the parts, and relations holding amongst them.[9] (We can also allow, as

---

[8] It does not matter for present purposes whether structures of this second sort are in fact universals, or even whether there are any universals; I mention the issue only as a way of characterising the notion of structure I have in mind.

[9] If we so wish, this schema can be treated as providing a reductive analysis of the '*X* has *S*' locution when that locution is employed in the sense I am hereby specifying (so that the reductive analysis is correct by stipulation, so to speak). This means that we need not worry about the ontological status of structures as entities in themselves, or of such things as "arrangements of properties and relations"; talk of structures can be treated simply as indirect talk of parts, properties, and relations, and so as no more mysterious than talk of those things (however mysterious that may or may not be).

a generalization of this schema, characterisations which refer to partitions into infinite collections of parts.)[10] I will call structures of this second sort *concrete structures*; the reason for this will become clearer when we meet the third sort.

Note that there is no requirement of completeness built into the notion of a concrete structure – to say that object *X* has concrete structure *S* is not to say that *S* contains *all* the properties of and relations amongst the parts of *X*. (This point is entailed by the mere fact that **S1** is counted as a concrete structure instantiated by my desk, given that the parts of the desk have other properties and stand in other relations to one another than those included in **S1**.) Relatedly, an object can instantiate more than one concrete structure.[11] So, for example, considering just the partition on which *a* is the desktop and the other parts are the legs, my desk also instantiates **S2**:

> *X* has **S2** iff there is a partition of *X* into five parts, *a*, *b*, *c*, *d*, and *e*, such that *a* is above each of the other parts, but no other relations of aboveness obtain between the parts.

and **S3**:

> *X* has **S3** iff there is a partition of *X* into five parts, *a*, *b*, *c*, *d*, and *e*, such that *b*, *c*, *d*, and *e* are all metallic, but *a* is not.

And there are other partitions of my desk: into the electrons, protons, and neutrons it contains, for example, or (if one's ontology is sufficiently generous) into its left half and its right. The existence of these other partitions clearly multiplies further the range of concrete structures my desk instantiates.[12]

Note also that **S2** is a *relational concrete structure* (or *purely relational concrete structure*, for emphasis). That is, instantiating such a structure is purely a matter of having parts which stand in various relations to one another; no intrinsic properties are involved. Relational structures will become important in Section 7.4.2.

Our third and final notion of structure is the notion of an *abstract structure*. As with concrete structures, we can characterise an abstract structure by specifying the conditions under which an object has it, using a sentence of the form:

> *X* has *S* iff there is a partition of *X* into *n* parts such that…

where, again, the ellipses are filled in by some collection of statements about properties possessed by the parts, and relations holding amongst them. The crucial

---

[10] We might also want to allow schemas of the form '*X* has *S* iff there is a partition of *X* into *at least n parts* such that…'; this would amount to allowing structures which are instantiated by an object purely in virtue of the properties and relations of just *some* of its parts.

[11] Whether one should think that there is, or might be, a structured object which instantiates only one concrete structure will depend on one's views about the metaphysics of properties, amongst other things. But even if there are such objects, they are clearly atypical.

[12] Incidentally, the present approach to thinking about structures can easily accommodate the notion of a substructure: we can simply say that *S* is a substructure of *S′* iff necessarily, for all *X*, if *X* has *S′*, then *X* has *S*. (So **S2** and **S3** are both substructures of **S1**, for example.)

difference is that when S is an abstract structure, the statements in question make no mention of any *specific* properties or relations.

As an example, suppose we start with the description of **S1**, and substitute for the specification of particular properties and relations the mere mention of some of the general characteristics of those properties and relations:

> X has **S4** iff there is a partition of X into five parts, a, b, c, d, and e, such that (i) there is an asymmetric, transitive two-place relation which a stands in to each of the other parts, but which none of the other parts stands in to any part, and (ii) there is a property which b, c, d, and e all have, but which a does not.

S4 is another structure my desk instantiates, but one which it is natural to think of as abstract in a way that **S1**, **S2**, and **S3** are not.[13] Hence the labels: on the usage I am laying out here, **S1**, **S2**, and **S3** are all concrete structures, whereas **S4** is an abstract structure. The distinction rests on the question of whether there are specific properties or relations the parts of an object must possess in order for the object to have the structure in question.[14]

Note that various points made about concrete structures carry over in obvious ways to abstract structures: there is no requirement of completeness on an abstract structure; an object on a given partition can instantiate more than one abstract structure; many, perhaps all objects can be partitioned in more than one way, giving rise to even more abstract structures; and we can carve out, as a subclass of the abstract structures, the purely relational abstract structures.[15]

Finally, note that in picking out an abstract structure, we are indirectly picking out a *kind* of concrete structure. In picking out **S4**, for example, we are picking out that kind of concrete structure which involves: a partition of an object into five parts: an asymmetric, transitive two-place relation which one of the parts stands in to each of the other parts, but which none of the other parts stands in to any part; and a property which is possessed by all and only those parts which do not stand in the aforementioned relation to any part.

In what follows, I will use the unmodified term 'structure' to cover both concrete and abstract structures, for much of what I want to say about the structuralist theses we are considering will apply whether those theses are understood as concerned with concrete or abstract structures.

---

[13] Note that I am not thinking of the relation in question as defined exhaustively by its extension on the set $\{a, b, c, d, e\}$; if it were, the mention of transitivity would be redundant.

[14] The examples I have given are ones in which either every mention of a property or relation is specific (**S1**, **S2**, and **S3**), or none are (**S4**). I will take it that those are the cases of interest, and leave aside the category of "mixed" cases.

Note also that I am using the labels 'abstract' and 'concrete' differently than (Redhead, 2001, pp. 74–75) and (Votsis, 2003, p. 881, and 2005, p. 1363); see Section 7.3.2 for more, and especially n. 17.

[15] The substructure relation can also be defined for abstract structures in just the same way as for concrete structures – see n. 12.

7.3.2 Formal Notions of Structure

Proponents of structuralism tend to employ notions of structure which are more formal than the ones I have just laid out. One standard notion is what I will call the “tuple” notion of structure, on which a structure is an ordered tuple of the form $< U, P_i, R_j^n >$, where U is a set, the P_i are subsets of U, and the R_j^n are subsets of the n-fold Cartesian product of U with itself (for a range of values of n). The P_i are thought of as properties-in-extension (on U) possessed by the members of U, and the R_j^n are, similarly, relations-in-extension (on U) holding amongst the members of U.[16] A second formal notion is what I will call the “isoclass” notion, on which a structure is an isomorphism class of such tuples. I am drawing particularly on (Votsis, 2003, p. 881) here, but both of these formal notions, and others closely related to them, can be found throughout the literature.[17] The tuple notion, for example, plays a central role in the partial structures approach (e.g., French and Ladyman (1999); Da Costa et al. (1990); Bueno (1997)).[18]

---

[16] The ‘P_i’ is often absent. Sometimes this is because properties-in-extension are treated as unary relations-in-extension, so that the ‘R_j^n’ is read as covering them; and sometimes it is because the focus is exclusively on relations-in-extension, and so on a formal analogue of what I have called purely relational structures. I have nonetheless included the ‘P_i’ for greater ease of comparison to the informal notions laid out in the last subsection. The relevant adjustments are easily made when we want to focus on the special case of purely relational structures. Similarly, we might in some contexts want to consider tuples also containing distinguished members of U, or functions-in-extension, or both.

It is worth noting that a structure of this sort is also a structure in one established logical sense – namely, it is the sort of thing which provides an interpretation for a set of sentences in a first-order language. This is no coincidence: see n. 26.

[17] Votsis uses the labels ‘concrete’ and ‘abstract’ rather than ‘tuple’ and ‘isoclass’; I have not followed his usage for obvious reasons.

Incidentally, (Votsis, 2005, pp. 1362–1363) seems to attribute both the tuple and the isoclass notions to Redhead (2001), but it is not clear to me that Redhead means to be employing either of them. Redhead gives as one example of what *he* calls a concrete structure “a pile of bricks, timbers and slates, which are then ‘fitted together’ to make a house” (2001, p. 74), and makes no mention of ordered tuples, so his notion of a concrete structure does not seem to be Votsis’s (i.e., the tuple notion). Redhead then says that we can think of what he calls an abstract structure as *either* an isomorphism class of the sorts of things he calls concrete structures, *or* “in an ante rem Platonistic sense as the second-order Form which is shared by all the concrete relational structures in a given isomorphism class…” (ibid. p. 75). The second notion is clearly not equivalent to Votsis’s notion of an abstract structure (which is the isoclass notion), and if I am right that Redhead’s notion of a concrete structure is not the tuple notion, then the notion of an isomorphism class of (his) concrete structures is not Votsis’s notion of an abstract structure, either.

[18] In that approach, the relations-in-extension are partial relations – relations-in-extension which may be defined only on a proper subset of U – but that difference will not bear on the present discussion.

Contrary to what we might initially be inclined to suppose, there are no simple one-to-one correspondences between these formal notions of structure and the informal notions laid out in the last subsection. Consider, for example, the relationship between structures as tuples and concrete structures. First, a given $< U, P_i, R_j^n >$ will very often fail to single out a unique concrete structure, because specifying the P_i and the R_j^n will not, in general, single out a unique collection of properties and relations: two or more properties or relations can share an extension on a restricted domain.[19] For example, although it is true of the property of being metallic that it is a property which all the legs of my desk have but the desktop lacks, the same is true of the property of being cylindrical and the property of weighing 5 lbs.[20] Secondly, it is obvious that there is no simple correspondence in the other direction: a given concrete structure does not typically single out a unique $< U, P_i, R_j^n >$, as many concrete structures are possessed by more than one object. Even choosing both a concrete structure and an object which has that structure will not always fix a unique $< U, P_i, R_j^n >$, as some objects instantiate some concrete structures relative to more than one partition. Thus there is no simple way of pairing up concrete structures with structures-as-tuples. And although there is not the space to go through every option here, this result generalises: there are no simple one-to-one correspondences between structures of either formal kind (tuples of the form $< U, P_i, R_j^n >$ or isomorphism classes of such tuples) on the one hand, and structures of either informal kind (concrete or abstract) on the other.[21]

The informal notions are thus not equivalent to the more standard formal notions. I would argue, however, that the informal notions have at least three advantages over their formal cousins. The first is their greater simplicity. The second is that the notion of a concrete structure does not tie the structure to a particular object or system which has it, unlike the tuple notion. If we are using the informal notion, we can point first at my desk and then at another desk of the same model, back at the IKEA store, and say “Look, same structure!”; with the formal notion, we cannot. In this respect it seems to me that the notion of a concrete structure

---

[19] Of course, it may also be true that two or more properties or relations can share their extension *simpliciter* – perhaps even necessarily – but the claim I need here is a much less controversial one.

[20] This example suggests that the “underdetermination” of concrete structure by structures-as-tuples will be a widespread phenomenon even if our ontology of properties is a sparse one, or (alternatively) we are restricting attention to a limited range of natural properties. On a more promiscuous ontology of properties, or given a less restricted field of attention, such underdetermination will be ubiquitous.

[21] Or between structures of either formal kind and structured objects, for that matter. (To see that there is no one-to-one correspondence between isomorphism classes of tuples and abstract structures, incidentally, consider the fact that the extension of a relation on a restricted domain does not, in general, determine its logical properties.)

captures something central to our intuitive notion of structure which the tuple notion does not.[22]

The third advantage of the informal notions is that they yield readings of vehicle structuralism and content structuralism on which those theses have greater *prima facie* plausibility. (a) Vehicle structuralism is the thesis that all scientific representation is representation by means of structure. If this is interpreted as the claim that all scientific representation employs set-theoretical tuples, or isomorphism classes of such tuples, say, it is less plausible than if it is interpreted as the claim that all scientific representation involves concrete structures, or even abstract structures: the claim that scientists are always singling out and employing arrangements of properties and relations, or kinds of such arrangements, when representing the world is surely easier to swallow than the claim that they are always employing formal structures of (say) a set-theoretical variety.[23,24] (b) Content structuralism is the thesis that all scientific representation is of structure. This, again, is clearly more plausible if it is read as the claim that all scientific representation is representation of arrangements of properties and relations, or kinds of arrangements of properties and relations, than if it is read as the claim that all scientific representation is representation of set-theoretical tuples, or isomorphism classes of tuples (for example). We do not normally think of the physical world around us as containing such formal structures,

---

[22] The isoclass notion *does* allow one to say "Same structure!" when pointing at the two desks, of course; but that notion does not enable us to capture the sense in which there is a structure shared by the two desks which is shared only by objects which have parts instantiating a certain pattern of relations of aboveness to one another. If, on the other hand, we are interested in focussing on a more abstract sort of structure the desks have, one which is *not* tied to the possession of any specific properties or relations, then the informal notion of abstract structure will serve our purposes just as well as the isoclass notion.

[23] See the next section, when it will become clearer what I mean by talk of singling out arrangements of properties and relations and employing them in representation.

Incidentally, this is the one point at which the strength of my argument might be thought to depend on the fact that I have adopted an unqualified formulation of VS. It will seem to many less implausible that all scientific representation *by means of models and theories* involves the employment of formal structures than that *all* scientific representation does so, and not only because the former is a logically weaker thesis: the weaker claim may, in fact, have a fairly high degree of plausibility for adherents of at least some central varieties of the semantic view. Leaving aside the big question of whether the semantic view gets things right, however, we can say that even the qualified version of VS – the weaker claim – seems (pre-theoretically) more plausible when construed in terms of the informal notions of structure than the formal, even if the plausibility gap is smaller than the gap between the informal and formal construals of the unrestricted version of VS. I would thus maintain that this argument for using the informal notions goes through either way.

[24] This is quite compatible with the claim that it might be helpful, for certain purposes, to use formal structures in the philosophical representation of the workings of scientific representation. (French and Ladyman, 1999, p. 107) and French (2010) emphasise that various of their claims are to be taken in this latter spirit. It seems to me, nonetheless, that if we want to understand how scientific representation works, then sooner or later we will need to know what sorts of thing scientists in fact employ when representing the world.

but we surely do normally think of the world as containing arrangements of properties and relations (and so, kinds of arrangements of properties and relations).[25]

I want to recommend the informal notions of structure as useful conceptual tools, then, when it comes to the understanding and examination of various elements of the structuralist approach to science. This is not to say that we should eschew the formal notions entirely, of course, nor that they may not be better tools for certain purposes. Clearly the formal notions do have features which may be advantageous in some contexts. The set-theoretical formal notions described above, for example, allow us to bring model theory to bear, and some would maintain that doing so makes philosophical progress possible on certain fronts. Nonetheless, I want to claim that the informal notions are clear and precise enough for many purposes, and better-suited than the formal notions to some tasks. I also think that my arguments in the remainder of this discussion would carry over to construals in more formal terms of the various structuralist theses we are considering.[26]

7.4 Explicating Vehicle Structuralism

We can now turn to the task of spelling out the content of vehicle structuralism, the thesis that all scientific representation is representation by means of structure. I will lay out two readings of this claim.

7.4.1 Representation Via Structured Objects

In order to represent by means of structure, we need to single out the structure by means of which we aim to represent. One way of doing this is to draw attention to a structured object which has the structure in question. Of course, given that any structured object instantiates numerous structures, one cannot single out a particular structure simply by drawing attention to a structured object which has it; what one can do, however, is draw attention to a structured object and make one of the structures it instantiates salient, somehow or other. On the first understanding of VS

---

[25] Especially given the points made in n. 9.

[26] Attachment to the $<U, P_i, R_j^n>$ notion of structure may come from a commitment to a certain programme in the semantics of natural language (or at least the language of science), or, relatedly, from linking a notion of model appropriate to the philosophy of science to the notion of model found in the standard Tarskian semantics of first-order languages. I do not wish to presuppose the former commitment, however, and have argued that the latter tendency is ill-advised (Thomson-Jones (2006)). Note, too, that although my emphasis here is on making room for non-formal notions of structure, we can also challenge the assumption that a formal approach should rely primarily on set-theoretic tools; see (Thomson-Jones, 2006, esp. p. 534) and Landry (2007).

I have in mind, all scientific representation involves picking out a structure in this way – via a structured object which has it – and then using the structure in question to represent whatever it is we are representing as being whatever way we are representing it to be. On this view, then, the tools we use to represent the world, the vehicles of representation, are structures. As the structures we use are (on this view) always introduced by way of structured objects, however, it also follows that all scientific representation is representation by means of structured objects.

To put this a little more precisely: Say that we are *displaying* a structure when we pick out a structure by (i) drawing attention to a structured object which has that structure and (ii) singling out the structure in question from amongst the many structures the object instantiates. Then count an act of representation as an act of representation by means of structure if and only if (a) it involves displaying a structure which plays a central role in the representing, and (b) the displayed structure then carries all the content conveyed by the representational act.[27] Vehicle structuralism, on this first explication, is the thesis that all scientific representation works that way. I will call the view encapsulated in this thesis *display vehicle structuralism*, or *DVS*.

This explication of vehicle structuralism employs the notion of the content conveyed by an act of representation, and the notion of x's carrying all the content conveyed by a given representational act. I will not attempt precise accounts of either notion; the hope is that the discussion in the rest of this subsection will make them clear enough.

When we ask whether some structure which plays a role in a representational act carries all the content conveyed by that act, we are asking: Is everything that is being said about the target system being said via the structure in question? Would any content still be conveyed if we took that structure out of the picture? It is not difficult to construct a definite counter-case: a case in which, although a structure plays an important role in a representational act, it clearly does not carry all the content conveyed. Suppose, for example, that I construct a scale model of the Golden Gate Bridge out of (unpainted) balsa wood. I then hold the model up in front of you, point at it, and say "The Golden Gate Bridge has this sort of spatial structure; oh, and it's red."[28] Part of the content I have conveyed is that the target system is red, but clearly the structure displayed (namely, a certain spatial structure possessed by the model) does not carry that content. To put it another way, if we imagine the events described taking place *sans* the displayed structure – suppose there is nothing in my hands when I point and utter the words in question – I would still succeed in conveying the content that the bridge is red.

---

[27] Note that no claim is made here about when an act counts as an act of representation.

[28] I am not assuming here that the act in question counts as an act of *scientific* representation; the point here is only to get some purchase on the notion of x's carrying all the content conveyed by an act of representation, whatever the do problem of saying what it is for a representational act to count as an act of scientific representation is one I have purposefully left aside.

It is less easy to find cases of representation in actual scientific practice which unquestionably fit the picture at the heart of DVS.[29] Here, though, are two attempts to locate examples which *might* fit, attempts which should at least help to make the idea underlying DVS more vivid. The first example, though not typical of most scientific representation, and more of a strain, is relatively simple, and so heuristically useful; the second is more typical, and points towards one connection between vehicle structuralism and the semantic view.

First, then, consider the famous model of DNA which Francis Crick and James Watson constructed in the early 1950s using copper wire, carefully crafted tin plates, and (in occasional moments of desperation) pieces of cardboard.[30] One thing Crick and Watson did with this physical model – the main thing – was to represent the DNA molecule as made up of parts of various kinds standing in certain spatial relations. Suppose, more precisely, that at least on some occasions, Crick and Watson used the tin-plate model to convey *just* the idea that DNA is made up parts of four different kinds standing in such-and-such spatial relations to one another.[31] That is, suppose they conveyed specific ideas about the geometry of the molecule on such occasions, and conveyed the thought that the parts of the molecule are of four different kinds by giving each relevant part of the model one of four different shapes, but conveyed nothing about what the kinds in question are (e.g., that kinds 3 and 4 are the kinds *cytosine base* and *guanine base*, respectively). Such representational acts would seem to fit the DVS picture in a particularly simple way: they are acts in which the agents first display a structure (in this case, one which consists in having parts of four different kinds standing in such-and-such spatial relations) by holding up a structured object which has it (the tin-plate model), and then simply attribute *that very structure* to the target system. Given this understanding of what was going on in the representational acts we are imagining, it seems entirely plausible to claim that all the content conveyed was carried by the displayed structure: if the tin-plate model had somehow been removed from the representational acts in question, Crick and Watson would have failed to pick out the structure which their representational acts were intended to attribute to DNA; and there was, *ex hypothesi*, no other content conveyed by those acts.[32]

---

[29] It is easier to construct artificial cases which fit the picture, especially if we are not concerned with limiting ourselves to scientific representation: see the "British are coming" case discussed at the beginning of Section 5, below.

[30] See Watson (1969), pp. 45, 67, 97, 108, and 113.

[31] I am leaving aside the sugar and phosphate molecules.

[32] I will not attempt to settle here the question of whether a representational act which centres on the tin-plate model but specifies the four kinds can be fitted to the DVS picture. There are at least some complications there, however. In particular, no such representational act could be taken to be of the especially simple sort just described, in which it is a straightforward matter of attributing the displayed structure to the target system, because the tin-plate model does not instantiate any structure which involves the property of being, say, a guanine base (which is just to say that no part of the tin-plate model is a guanine base). Relatedly, if no representational acts of the more limited sort described in the text took place, the proponent of DVS has more work to do to make her view seem plausible.

A second example which seems to fit the DVS picture is provided by the use of the real line to represent time. Or rather: the use of the real line to represent time seems to fit the DVS picture if we are willing to adopt a Platonist understanding of our mathematical discourse.[33] Then we can say that in talking about the real line we are drawing attention to a certain structured object, one which has real numbers as parts (where real numbers are taken to be objects in their own right). And a certain abstract structure the real line instantiates – a structure which is (say) a matter of having a non-denumerable collection of parts standing in a transitive, asymmetric, connected relation – can be made salient, so that that structure has been displayed. The structure in question then carries the content we convey when, using the real line, we represent time as having just the structure in question. Such acts of representation are thus acts of representation by means of structure.[34]

It is worth addressing a misunderstanding which might arise around the talk of a structure's "carrying all the content" conveyed by a given act of representation. Such talk is not meant to suggest that no factor other than the relevant structure plays a role in bringing it about that representation takes place, or in bringing it about that the content conveyed is such-and-such. Indeed, it is an explicit feature of this explication of the notion of representation by means of structure (and of the examples given) that agents are involved. The intentions of those agents can play a role, as can background sets of rules, conventions, beliefs, and the like, many of which will play a role in virtue of being shared by the agent and her audience. Linguistic acts may be central, too: it might be an important part of the process for the agent to say "Now, look at this tin-plate construction, and see the way it forms a sort of double helix," or "Consider the real line…." Even physical gestures might be crucial – acts of pointing, say. These allowances are all consistent with an insistence that the displayed structure carries all the content conveyed by the act of representation, as that phrase is intended. The intuitive picture is that a sort of "funnelling" takes place in an act of representation understood this way: various factors play a role in making representation happen, and in fixing the content conveyed, but there is a *stage* in the process by which content "moves" from representer to audience at which all the content involved is conveyed via some structure.[35]

---

[33] We will consider the consequences of taking a different view of mathematics – the structuralist view – in a few moments.

[34] Note that here I have considered a representational act in which we do no more than represent time as having a certain abstract structure. If more is being said when we say that the parts are times, or moments, and that the relation is (say) the earlier-than relation, then there will be complications involved in fitting a representational act which conveys that additional content to the DVS picture, complications of the same sort as the ones discussed in n. 32.

[35] Bear in mind that the point here is not to decide whether it is accurate, in the final analysis, to claim that any or all scientific representation works this way; I am merely trying to fill out the structuralist picture of scientific representation.

Incidentally, the points made in this paragraph apply in equal measure to the second explication of VS, to which we are about to turn.

To round out the discussion of the first explication of vehicle structuralism, and segue into the second, consider again the use of the real line to represent time. Suppose that in place of Platonism, we adopt a structuralist view of mathematical discourse. Then it is still true that we are singling out a structure when we say "Consider the real line…"; but on the mathematical structuralist's view, that is all we are doing. Crucially, we are not picking out a structured object (or "system," in the mathematical structuralist's preferred terminology) which has the structure in question. On the assumption that the use of the real line to represent time is a paradigmatic example of at least one type of scientific representation (albeit a very simple example), it follows that if structuralism is the correct philosophy of mathematics, then display vehicle structuralism is false. This is because that thesis insists that all scientific representation involves the displaying of a structure, and so involves the use of a structured object. Here, then, we have one quick way of arguing against display vehicle structuralism. It is a significant disadvantage of this argument, however, that it relies on a particular philosophy of mathematics.[36]

7.4.2 Structure Without a Structured Object

The mathematical structuralist's take on the use of the real line to represent time reminds us that displaying a structure is not the only way of picking one out, which is to say that we can single out a structure without employing any object which has that structure. For example, we might simply say "Consider the structure which involves being partitioned into five parts, *a*, *b*, *c*, *d*, and *e*, such that…," and thus pick out a structure my desk instantiates without involving the desk or any other structured object.[37]

On the second explication I want to consider, then, vehicle structuralism is just the thesis that all scientific representation involves singling out a structure which then carries all the content conveyed by the act of representation. *Simple vehicle structuralism* (*SVS*), as I will call this thesis, differs from display vehicle structuralism only in containing no insistence that we single out structures in any particular way. It is thus a weaker thesis, and so, presumably, easier to defend.[38] But there is also trouble here, for there is a significant danger that simple vehicle structuralism weakens vehicle structuralism to the point of triviality, given the notions of structure in play.[39]

---

[36] The arguments I lay out against display vehicle structuralism in "Thomson-Jones" (in preparation a, b) do not rely on such a heavy independent philosophical commitment.

[37] I am using 'single out' and 'pick out' interchangeably; unmodified, both phrases are intended to leave open the means, so to speak, and that is how I have been using them thus far.

[38] For one thing, note that simple vehicle structuralism has no difficulty in accommodating the mathematical structuralist's understanding of the real line/time case.

[39] By saying that simple vehicle structuralism is in danger of seeming trivial, I do not mean that it begins to look like an analytic or logical truth. I mean rather that it begins to look too weak and uncontroversial to be staking out a specifically "structuralist" approach to scientific representation.

Here's why: It is plausible that all (or virtually all, or most) scientific representation is a matter of presenting some target system or type of system, real or imaginary, either as having various parts which have various properties and stand in various relations to one another, or at least as having various parts which instantiate some *kind* of arrangement of properties and relations.[40] On the notions of structure I introduced earlier, this is just to represent the target system, or type of system, as having a certain structure (concrete or abstract). But representing target system or type of system *X* as having structure *S* can always be thought of as singling out *S*, and then using it to represent *X* as being a certain way (namely, as having *S*). Given that on this way of looking at it, the representational act consists entirely of attributing *S* to *X*, it also seems clear that *S* carries all the content conveyed by that act. It thus looks as though any act of scientific representation (or virtually all, or most) will fit the account offered by simple vehicle structuralism for very general reasons. Simple vehicle structuralism is thus in danger of seeming trivial (or nearly so) on this second explication.[41] (Note, on the other hand, that there is no argument here for the triviality of display vehicle structuralism, as that thesis insists that *S* is always singled out by being displayed, and that, I take it, is not trivial.)

Given the background of recent discussion, two ways of avoiding this sort of triviality suggest themselves. Both involve modifying simple vehicle structuralism by restricting attention to certain special sorts of structure: abstract structures in the first case, and purely relational structures in the second.[42] That is, the vehicle structuralist can announce that the thesis she means to defend – and, perhaps, intended all along – is either

> (SVSa) All scientific representation is representation by means of abstract structure

or

> (SVSr) All scientific representation is representation by means of purely relational structure

where we are representing "by means of" a structure when our representational act involves singling out a structure which then carries all the content conveyed by that act.[43]

---

[40] This is especially plausible if we count as a partitioning the trivial partitioning of a thing into its sole improper part.

[41] If the parenthetical 'virtually all, or most' is needed in this argument, then although simple vehicle structuralism might still be true, it will only apply for the very general reasons just presented in virtually all or most cases, and thus will only be *nearly* trivial.

[42] We have already seen (in Section 7.3.2) that there is a formal notion of abstract structure in the literature, often traced back to Russell (e.g., Votsis (2003), p. 881); and some who employ the tuple notion of structure sometimes focus exclusively on relations. The proposals I am considering here are distinct, of course, because they centre on informal notions of structure; but the parallels are clear.

A third option, incidentally, is to impose both constraints, but the points I am about to make will extend to that option automatically.

[43] An exclusive focus on abstract structures might be taken to yield the purest form of structuralism. If so, it seems that structuralists differ in their degree of purity: see Ladyman (2007), Section 3.

I will not attempt a final assessment of either of these theses here, but it is worth noting certain aspects of what is involved in defending them. Consider, for example, a representational act which represents the hydrogen atom as being the way the Bohr model of hydrogen says it is – call this a "Bohr act" for short. We can, of course, see a Bohr act as attributing a certain structure to the hydrogen atom; and the defender of either (SVSa) or (SVSr) is committed to the claim that such an act involves singling out a structure (abstract or purely relational, respectively) which then carries all the content conveyed by the act. There are then two options for the defender of either (SVSa) or (SVSr): either the structure employed in the Bohr act (the one which carries all the content) *is* the structure attributed to the hydrogen atom by that act, or there are two distinct structures.

The first option is attractive in that it tells a relatively simple story: the agents who are doing the representing do it by singling out a certain structure, and then attributing that very structure to the hydrogen atom. But there are difficulties here nonetheless. The defender of (SVSa) is committed to claiming that the structure singled out and employed for the purposes of representation is an abstract structure, and so the identification of the structure employed with the structure attributed yields the claim that Bohr acts attribute only a certain abstract structure to the hydrogen atom. The defender of (SVSa) will thus have to square such purported abstractness with the fact that it seems as though we attribute specific properties to the hydrogen atom in the course of Bohr acts – we attribute a charge of $+1.6 \times 10^{-19} C$ to one part of it, for example. Similarly, the defender of (SVSr) who takes the first option is committed to claiming that we attribute no intrinsic properties to any part of the hydrogen atom in performing a Bohr act; not a trivial commitment.

The defenders of (SVSa) and (SVSr) can avoid these difficulties by taking the second option, but the cost will be a more complex story about how representation works in such cases. The defender of (SVSa) who allows that the structure attributed to the hydrogen atom in Bohr acts is not abstract, and who thus pictures those acts as involving two distinct structures, the structure employed and the structure attributed, will need to provide a more detailed account of how an abstract structure, involving no specific properties or relations, is used (in a representational act in which it carries all the content conveyed) to attribute certain specific properties and relations to the hydrogen atom. Similarly, the defender of (SVSr) who is not comfortable insisting that the structure attributed to the hydrogen atom in Bohr acts is purely relational, and who thus has a "two-structure" picture of such acts, will need to say more about how purely relational structures are used to attribute intrinsic properties (amongst other things) to the parts of the hydrogen atom.[44]

The upshot is that if the simple vehicle structuralist attempts to avoid the triviality charge outlined above by strengthening her central thesis in either of the ways

---

[44] I do not mean to insinuate that it will be impossible to provide such accounts; my point is just that there are questions that need to be answered on this sort of approach, and it is not *obvious* how those questions are to be answered.

we have just considered – that is, by claiming that all scientific representation is by means of either abstract or purely relational structures – she will have more work to do to make her account seem plausible.

7.5 Vehicle Structuralism and Content Structuralism

In Section 7.6 we will consider some connections between structuralism about scientific representation, structural realism, and the semantic view. First, in this section, we will examine the relationship between the two basic structuralist theses about representation, vehicle structuralism and content structuralism.

It might seem for a moment as though there is no logical room for manoeuvre between vehicle and content structuralism. If all scientific representation involves displaying a structure, or at least singling one out, and then using it to represent some part of the world in such a way that the entire content of the representation is carried by the structure in question, then, we might think, it must follow that all scientific representation is representation of structure. Once we say this out loud, however, it becomes clear that there is a logical gap between the two theses after all. Certainly one particularly straightforward way of using a structure *S* to represent some part of the world is by singling *S* out and saying "Look: *that* part of the world (object, system,…) has *this* structure"; and if that is all we do, then our representation will indeed have been entirely of structure. But there are other ways we might choose to employ a structure for the purposes of representation. For example, suppose I set in place a background rule which says: *whenever I bring structure S to your attention, that will be my signal that the British are coming*. Then clearly I can employ *S* to represent the British as being on their way (or the arrival of the British as imminent, or the world as containing approaching British forces/Britpop bands). Thus it is possible to represent, by means of structure, something other than structure. And so unless we are willing to insist that restricting our attention to *scientific* representation makes the right sort of difference here, it seems we should take it to be logically possible that scientific representation is always by means of structure, but not always of structure. That is, it seems we should conclude that VS does not entail CS.

One objection to this line of argument would involve claiming that representing the British as being on their way *is* representation of structure (and of nothing else). Given the notions of structure I delineated in Section 7.3.1, this claim seems entirely plausible, and so the objection seems reasonable. But this simply draws attention to the fact that CS, as it stands, is vulnerable to just the sort of triviality charge SVS faced. Indeed, the charge arises in a very similar way. It is plausible that the content of any act of scientific representation is of this sort: that some target system or type of system, real or imaginary, either has various parts which have various properties and stand in various relations to one another, or at least has various parts which instantiate some *kind* of arrangement of properties and relations. This is just to say that the content of any act of scientific representation is of this sort: that some target system or type of system has a certain structure (concrete or abstract). And this, in turn, is just to say that all scientific representation is of structure. Thus CS, as stated, looks plausible for reasons which are so general that it is hard to see it as a peculiarly "structuralist" thesis. Presumably, then, the structuralist will want to strengthen CS – perhaps by restricting attention to either abstract or purely relational structures, as before – so that some sort of representational content would count as being about something other than structure in the appropriately restricted sense. But then we can simply substitute some content of *that* sort for the content of 'The British are coming' in the argument given above, and so establish that the interesting, strengthened form of CS is not entailed by VS.[45]

Another objection might be that in the envisioned case, the structure employed is not carrying all the content conveyed by the representational act. The obvious thought here would be that the stage-setting (in which I tell you the rule about what you should take me to mean if I draw *S* to your attention) is doing some central work. The response to this objection is that although it is indeed true that the stage-setting is crucial, that fact does not distinguish this case from the cases we have been taking to be paradigmatic of representation by means of structure (namely, the Crick and Watson case and the case of the real line). As we noted earlier, factors other than the mere singling out of a structure are essential to bringing it about that an act of representation occurs in those cases, too; and indeed, such additional factors play a role in determining the content which is conveyed in those cases, just as the relevant bit of stage-setting does in the "British are coming" case. Simply displaying a structure possessed by the tin-plate model is not enough to bring it about that we are representing the parts of the DNA molecule as standing in various spatial relations to one another; it is not even enough to bring it about that we are representing. In addition to singling out the structure in question, we also need to identify a representational target, and make it clear what we mean to convey about that target by singling out the structure in question. Certainly the simple option in the latter respect is that we mean to convey that the target just has the structure in question; but even that needs to be fixed somehow, and fixing it is part of the stage-setting. Thus if representation is not by means of structure in the "British are coming" case for the sort of reason given, neither is it by means of structure in our paradigmatic cases. Furthermore, as an additional reason for rejecting this objection, note that the "British are coming" case passes the counterfactual test I proposed to help fix the sense of the idea that some structure involved in an act of representation carries all the content conveyed: if we imagine my going through the same motions but without displaying the structure in question to you at the crucial moment, it seems clear that I would fail to convey anything to you.

---

[45] In other words, although VS may entail CS as stated, given the notions of structure in play, strengthening CS to yield a sufficiently substantive thesis will undermine the entailment.

Note also that a defender of CS who responds to this triviality problem by restricting attention to abstract structures will have to claim that Bohr acts attribute no specific properties or relations to parts of the hydrogen atom, whereas one who responds by restricting attention to purely relational structures will have to claim that Bohr acts attribute no intrinsic properties.

So CS is not a logical consequence of VS (at least, not once it has been strengthened enough to be interesting). This claim, we might note, is in line with a Goodmanian insistence that relations of representation are fundamentally arbitrary, and that anything can be used to represent anything else.[46] Nonetheless, there is a connection between VS and CS worth spelling out: even given the relevant Goodmanian points, and the lack of a straightforward entailment, VS gives us reason to believe CS.

The simple but crucial observation is that it is one thing to say that anything *can* represent anything, and quite another thing to say that there are no general patterns in the ways we in fact represent. Once we consider our actual representational practices, it is obvious that practical considerations come into play, for one thing.[47,48] So, for example, there are two obvious practical disadvantages to representing the British as being on their way by employing some arbitrary structure *S* in the way described above: first, the representational act requires its own special bit of stage-setting; second, it would be easy for my audience to forget what it is I mean to convey by displaying *S*. (To make the second point vivid, imagine that there is a large range of things I might want to be able to convey, and that for each of them we set up a similarly arbitrary link to some structure or other.) No special stage-setting is required, in contrast, if I represent the British as being on their way by declaiming the words ‘The British are coming,’ and my audience is unlikely to forget what I mean to convey by uttering those words.[49] Similarly, consider a case in which I use *S* to represent something as having that very structure – by identifying *X* as the target and saying “*X* has *S*,” for example. Here again there is no need for special acts of stage-setting peculiar to the representational task, and no danger that my audience will forget something crucial to the success of my attempt to convey to them what it is I am attempting to convey. In other words, there are practical reasons for using structures just to represent the structure of things; and this makes it plausible that when it comes to representation, that is in fact what we use structures for. Thus if all scientific representation is representation by means of structure, then it is plausible enough that, *de facto*, all scientific representation is of structure: VS gives us reason to believe CS. Correspondingly, the rejection of CS would give us some reason to reject VS; and that point will be important in the next section.

Of course, the argument of the last paragraph assumes that the members of my audience are competent English-speakers, and producing a competent speaker of English amounts to a great deal of “stage-setting” in itself. Thus it is not as though

---

[46] See Goodman (1976). On p. 5, for example, Goodman makes the slightly qualified claim that “almost anything may stand for almost anything else,” and follows up with the claim that a particular sort of standing-for, denotation, is “the core of representation.” (Of course, even the correspondingly qualified claim that almost anything can represent almost anything else does not follow from these two claims, but Goodman has been taken to assert that, too.)

[47] We could also call them “pragmatic” considerations, provided we do not allow the term to trigger unwanted associations.

[48] I do not mean to suggest that Goodman failed to take account of these points – he clearly did not.

[49] The emphasis here is on the word ‘special’ – see the next paragraph.

we have dispensed with the need for an appropriately prepared audience when I convey the relevant content simply (!) by uttering the words 'The British are coming.' Very little representation, if any, comes out of the blue. What is more, language might have been set up to work very differently than it does. The claims made in the argument of the last paragraph are thus quite compatible with a Goodmanian insistence on the ultimate arbitrariness of representation, and on the role of convention. But when we are interested in scientific representation as it actually is, it is perfectly appropriate to take into account the fact that acts of scientific representation are performed by creatures who, in fact, are already speakers of various natural languages, have already learned a set of conventions governing the activity of (literal) pointing, and so on.

Herein, incidentally, lies the beginnings of a response to Callender and Cohen (2006). It may be that certain fundamental questions about how representation works fail to take on a different cast in the scientific case than in other contexts, but it is nonetheless entirely possible that given the sorts of representational task we are attempting in the sciences, and given the various practical constraints at work, there are some particular ways of representing that *de facto* predominate in the sciences – some characteristic kinds of representational vehicle which are employed, for example. So there can be a special question about how representation works in the sciences. That question may well be worth asking, and answering, furthermore, for it may be that articulating an account of the specific ways of representing which predominate in the sciences will aid us in our attempts to understand scientific explanation, theory testing, modelling, and the various other aspects of scientific practice which concern us in the philosophy of science.

7.6 Structural Realism, and the Semantic View

My last piece of business is to draw out certain connections between structuralism about scientific representation, structural realism, and the semantic view of theory structure. In particular, I want to trace out a line of reasoning which takes us from one standard defence of structural realism to the rejection of content structuralism, vehicle structuralism, and the semantic view. As I mentioned in the introduction, it may be that no actual philosopher holds a combination of views in tension with the conclusions I will draw here; nonetheless, I think it is worth bringing the connections out into clear sight.

As a starting point, let us see why a certain sort of structural realist does well to reject content structuralism. Here, and throughout the remainder of the discussion, I have in mind a modified content structuralism which manages to avoid the charge of triviality discussed in the last section. This modified content structuralism might be the thesis that all scientific representation is of abstract structure, perhaps, or the thesis that all scientific representation is of purely relational structure – the details will make no difference. In fact, the arguments of this section will swing free of our choice of notion of structure, and so they are independent of the ideas in Section 7.3, above. All that matters is that the term ‘structure’ is used in the same way throughout (including in the statements of vehicle structuralism, content structuralism, and the tenets of structural realism), so that we are not trading on ambiguities.

So: Consider the epistemological structural realist, or ‘ESRist,’ who, in the terms of Section 7.2, accepts LS and SS, which is to say she believes that although we can know only the structure of the world, we can and do know a good deal about that. There is, of course, no contradiction between this view and content structuralism, the claim that scientific representation only ever tells us about the structure of the world (i.e., between LS, SS, and CS).[50] But consider the main argument for ESR: that it is the only position which both does justice to the intuitions underlying the miracle argument, and escapes the jaws of the pessimistic induction. The crucial part of this argument for the present point is the second. In responding to the pessimistic induction, the ESRist grants that there are radical discontinuities between successive theories in the history of even the mature sciences, but insists (and this is the definitive manoeuvre) that these are discontinuities only with regard to what theories say about the *nature* of things; at the level of postulated *structure*, there is enough continuity to make realism tenable. The alleged evasion of the pessimistic induction thus relies on the idea that theories tell us both about the structure of the world, *and* about the nature of the things in it.

Exactly how we should understand the distinction between structure and nature need not detain us here: all that matters is that the proposed response to the pessimistic induction presupposes that theories tell us both about the structure of the world and about something else. It follows straightforwardly from that fact that CS undercuts this argument for ESR. If content structuralism is correct, so that everything scientific theories say is about structure, the diagnosis of discontinuity offered in the course of the argument is no longer an option. The radical differences of content that obtain between Fresnel’s aether theory of light and Maxwell’s electromagnetic theory (to use Worrall’s (1989) example) will have to be differences of postulated structure, and the pessimistic induction will rise again at the level of structure. If she wished both to embrace content structuralism and to retain some version of this central argument for her position, the ESRist would thus have to provide us with a distinction between kinds of postulated structure – the kind we should believe in, and the kind we should not. Insofar as this would be a new problem with no obvious solution, the ESRist would do better simply to reject content structuralism.[51]

---

[50] At worst, there is some redundancy involved in declaring a commitment to CS, LS, and SS: if our representations tell us only about structure (CS), there is little point in saying that we can know only what they tell us about structure (LS). Note, however – and this is to anticipate the point that the ESRist should reject CS – that this combination of views is clearly not what the ESRist has in mind, as she clearly means to be describing a *restriction* on what we should believe, from amongst all the things our scientific representations say.

[51] Of course, this point, and the points which follow, extend to any metaphysical structural realist who might wish to draw support for his position from the argument we have been considering.

The next step is a simple one: it follows from the arguments in Section 7.5 that vehicle structuralism and the denial of content structuralism make for uneasy bedfellows, and so the ESRist has some reason to reject vehicle structuralism, too. Despite the initial appearance of a natural affinity between structuralism about scientific representation and structural realism, then, these positions do not play happily together. More carefully, and less colourfully: structuralism about scientific representation does not combine well with the best-known argument for one central form of structural realism.

Finally, there is a connection here to the semantic view. As I mentioned in Section 7.2, the semantic view of theory structure provides one central source of support for vehicle structuralism. Invoking the standard first approximation, we can say that the semantic view is the view that theories are collections of models, or are usefully viewed as such. On several versions of the semantic view, moreover, the models in question are mathematical structures of one sort or another (e.g., Suppes (1957, 1960, 1967); van Fraassen (1970, 1972, 1980, 1987); French and Ladyman 1999).[52] Or at least, that is how it is often put. With the stipulations of Section 7.3 and the discussion in Section 7.4 in mind, we should more carefully say this: On several versions of the semantic view, the models invoked are either structured mathematical objects, or structures of a mathematical sort, depending on one's philosophy of mathematics. In either case, however, it seems a small step to say that theories represent by means of structure, or that we do so in using theories to represent. The semantic view thus lends credence to vehicle structuralism.

Building on this connection, we can see that the ESRist should feel some pressure to reject the semantic view as long as she supports her position by appealing to the usual argument. That argument requires the ESRist to reject content structuralism for theories (and so to reject it *simpliciter*), as we have seen; she then has reason to reject vehicle structuralism for theories, given the arguments of the last section (and so to reject VS *simpliciter*). As the semantic view leads naturally to vehicle structuralism for theories, this means that the ESRist should be inclined to reject that view of theory structure. Unsurprisingly at this point, perhaps, the view of theory structure which most naturally accompanies structuralism about scientific representation looks to be at odds with the central argument for structural realism, too.

---

[52] For my own view about the best way of understanding the seminal variants due to Patrick Suppes and Bas van Fraassen, see Thomson-Jones (2006). Despite the fact that the work of Ronald Giere and Frederick Suppe is often mentioned in the same breath as that of Suppes and van Fraassen, their accounts of theory structure diverge in a number of significant (and different) ways from the Suppes-van Fraassen approach. (See, e.g., Giere (1988); Suppe (1989)). Particularly relevant here is the fact that models, on Giere's account, are not mathematical structures; see Thomson-Jones (2010).

References

Brading, K. and Landry, E. (2006), “Scientific Structuralism: Presentation and Representation,” *Philosophy of Science* 73: 571–581.

Bueno, O. (1997), “Empirical Adequacy: A Partial Structures Approach,” *Studies in History and Philosophy of Science* 28: 585–610.

Callender, C. and Cohen, J. (2006), “There Is No Special Problem About Scientific Representation,” *Theoria* 55: 67–85.

Da Costa, N. C. A. and French, S. (1990), “The Model-Theoretic Approach in the Philosophy of Science,” *Philosophy of Science* 57: 248–265.

French, S. and Ladyman, J. (1999), “Reinflating the Semantic Approach,” *International Studies in the Philosophy of Science* 13: 103–121.

French, S. (2010), “Keeping Quiet on the Ontology of Models,” *Synthese* 172: 231–249.

Giere, R. N. (1988), *Explaining Science: A Cognitive Approach*. Chicago: The University of Chicago Press.

Goodman, N. (1976), *Languages of Art: An Approach to a Theory of Symbols*. Indianapolis: Hackett Publishing Company, Inc.

Ladyman, J. (1998), “What is Structural Realism?”, *Studies in History and Philosophy of Science* 29: 409–424.

Ladyman, J. (2007), “Structural Realism,” in E. N. Zalta, ed., *The Stanford Encyclopedia of Philosophy (Summer 2008 Edition)*. URL = <http://plato.stanford.edu/archives/sum2008/entries/structural-realism/>.

Landry, E. (2007), “Shared Structure Need Not be Shared Set-Structure,” *Synthese* 158: 1–17.

Redhead, M. (2001), “The Intelligibility of the Universe,” in A. O’Hear, ed., *Philosophy at the New Millennium*. Cambridge: Cambridge University Press, pp. 73–90.

Suppe, F. (1989), *The Semantic Conception of Theories and Scientific Realism*. Urbana: University of Illinois Press.

Suppes, P. (1957), *Introduction to Logic*. Princeton: Van Nostrand.

Suppes, P. (1960), “A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences,” *Synthese* 12: 287–301.

Suppes, P. (1967), “What is a Scientific Theory?”, in S. Morgenbesser, ed., *Philosophy of Science Today*. New York: Basic Books, pp. 55–67.

Thomson-Jones, M. (2006), “Models and the Semantic View,” *Philosophy of Science* 73: 524–535.

Thomson-Jones, M. (2010), “Missing Systems and the Face Value Practice,” *Synthese* 172: 283–299.

Thomson-Jones, M. (in preparation a), “Modelling Without Mathematics.”

Thomson-Jones, M. (in preparation b), “Mathematical and Propositional Models.”

van Fraassen, B. C. (1970), “On the Extension of Beth’s Semantics of Physical Theories,” *Philosophy of Science* 37: 325–339.

van Fraassen, B. C. (1972), “A Formal Approach to the Philosophy of Science,” in R. Colodny, ed., *Paradigms and Paradoxes*. Pittsburgh: University of Pittsburgh Press, pp. 303–366.

van Fraassen, B. C. (1980), *The Scientific Image*. Oxford: Clarendon Press.

van Fraassen, B. C. (1987), “The Semantic Approach to Scientific Theories,” in N. J. Nersessian, ed., *The Process of Science*. Dordrecht: Martinus Nijhoff, pp. 105–124.

van Fraassen, B. C. (2006a), “Structure: Its Shadow and Substance,” *The British Journal for the Philosophy of Science* 57: 275–307.

van Fraassen, B. C. (2006b), “Representation: The Problem for Structuralism,” *Philosophy of Science* 73: 536–547.

van Fraassen, B. C. (2008), *Scientific Representation: Paradoxes of Perspective*. Oxford: Clarendon Press.

Watson, J. D. (1969) *The Double Helix: A Personal Account of the Discovery of the Structure of DNA*. London: Readers Union.

Worrall, J. (1989), “Structural Realism: The Best of Both Worlds?”, *Dialectica* 43: 99–124.

Votsis, I. (2003), “Is Structure Not Enough?”, *Philosophy of Science* 70: 879–890.

Votsis, I. (2005), “The Upward Path to Structural Realism,” *Philosophy of Science* 72: 1361–1372.

Chapter 8
Ontic Structural Realism as a Metaphysics of Objects*

Michael Esfeld and Vincent Lam

8.1 Introduction

In a first approach, ontic structural realism (OSR) is a realism towards physical structures in the sense of networks of concrete physical relations, without these relations being dependent on fundamental physical objects that possess an intrinsic identity as their relata. In that vein, OSR has been developed in recent years as a metaphysics of contemporary fundamental physics, mainly non-relativistic quantum mechanics (QM), relativistic quantum field theory (QFT) and the general theory of relativity (GTR). The fundamental physical features of permutation invariance in many-particles quantum theory (Muller 2009), quantum entanglement in QM (Esfeld 2004) and in QFT (Lam 2010a), gauge invariance in quantum gauge theories (Lyre 2004) as well as background independence and gauge-theoretic diffeomorphism invariance in GTR (Rickles 2006; Esfeld and Lam 2008) have all been shown to support OSR in the following sense: these fundamental physical features can with good reason be taken to suggest all the same conclusion, namely that the fundamental physical objects – whatever they are according to the theory under consideration – are parts (relata) of a physical structure in the sense of a network of concrete physical relations. These objects do not have any existence – and in particular not any identity – independently of the structure they are part of (that is, the relations they bear to each other).

---

* We are greatful to Tim Raez, Jakob Sprickerhof and Christian Wüthrich for helpful comments on the draft of this paper.

M. Esfeld (✉)
University of Lausanne, Department of Philosophy
e-mail: Michael-Andreas.Esfeld@unil.ch

V. Lam
University of Queensland, School of Mathematics and Physics
& School of History, Philosophy, Religion and Classics
e-mail: v.lam@uq.edu.au

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_8, © Springer Science+Business Media B.V. 2011

To be more precise, the original papers on OSR – mainly Ladyman (1998) and French and Ladyman (2003) – consider OSR to be supported by a fundamental underdetermination about individuality in QM and about quantum fields in QFT. Ladyman and Ross (2007, Chapters 2 to 4), French (2010, Sections 3–4) and French and Ladyman (Section 2.1, this volume) still defend such a view. Similarly, Bain (2006, 2009) argues in favour of an OSR interpretation of GTR on the basis of the different mathematical formulations of the theory. However, it is in dispute whether there really is such an underdetermination (see Cao 2003; Pooley 2006, p. 90, against French and Ladyman 2003). We therefore take OSR to be directly and explicitly supported by the above mentioned features of QM and QFT as well as GTR.

These empirical arguments from contemporary fundamental physics stand against a long metaphysical tradition, which can be traced back to Aristotle. According to this tradition, physical objects, notably the fundamental ones, have to be something in themselves, enjoying an intrinsic identity. To be more precise, one can distinguish between the following three possible situations:

(a) The fundamental physical objects are equipped with an intrinsic identity: each fundamental physical object has intrinsic properties, that is, possesses properties that are independent of whether the object is alone or accompanied by other objects (see the definition of intrinsic properties by Langton and Lewis 1998). These properties are furthermore such that they distinguish each object from all the other objects that there are in the world. Aristotle sets out such a position, if one applies his considerations in *Metaphysics* Book VII to fundamental physical objects.

(b) The fundamental physical objects are not equipped with an intrinsic identity: they have (or at least can have) intrinsic properties, but no fundamental physical object has intrinsic properties that distinguish it from all the other fundamental physical objects. However, there are – asymmetric – relations that provide for identity conditions in that they distinguish each fundamental physical object from all the other ones. Time instants in Galilean space-time can serve to illustrate this situation: there are no intrinsic properties that distinguish each time instant from all the other ones. But the irreflexive and asymmetric relation "earlier than" does so.

(c) Neither intrinsic properties nor relations provide for identity conditions of the fundamental physical objects: two or more fundamental physical objects can have all the same intrinsic properties and stand in the same relations. This description has to be further qualified in view of what is known as weak discernibility in the current discussion; we will go into that issue at the end of the next section.

While situation (b) arguably is already a form of OSR, it is situation (c) that the mentioned features from contemporary fundamental physics on which OSR draws suggest. Whereas in situations (a) and (b), there are identity conditions that distinguish each object from all the other ones, provided for by either intrinsic properties (a) or relations (b), there are no identity conditions that distinguish objects from each other in situation (c). This situation therefore raises the question in what sense there are objects at all. Against this background, the main reservations about OSR are directed at the view of objects that this position is taken to imply. Accordingly, this paper aims to sketch out a metaphysics of objects within OSR, thus answering the question of the relationship between objects and relations.

We will first set out five different views of the relationship between objects and relations; three of them are versions of OSR (Section 8.2). We will then introduce our own account that takes the distinction between objects and relations to be a conceptual rather than an ontological one, thus also amending our previous publications on OSR (notably Esfeld 2004; Esfeld and Lam 2008) (Section 8.3). Finally, we briefly go into the issues of intrinsic properties (Section 8.4) and objective modality (Section 8.5).

8.2 Ontological Primacy and Ontological Dependence

The issue of the relationship between objects and relations within OSR has mainly been addressed in the literature in terms of ontological primacy (Stachel 2006; Ladyman and Ross 2007, Section 3.4; French 2010). We can distinguish five possibilities in order of increasing ontological importance of the relations over the objects (relata).

1. There are only objects, but no non-supervenient relations. Objects have only intrinsic properties. All physical relations are reduced to the intrinsic properties of objects in the sense that they strongly supervene on them. Suppose that mass is an intrinsic property of objects. Relations such as being heavier than, being lighter than, having the same mass as then supervene on the masses that objects have each independently of one another. If one sets out to account for what there is in the world, it suffices to mention the intrinsic properties; the relations then come for free. The intrinsic properties furthermore equip the objects with an intrinsic identity: there is at least one intrinsic property of each object by means of which it is distinct from all the other objects in the world. Leibniz proposes such a radical atomistic conception in his *Monadology* (1714). Since there are no relations and thus no structures admitted in fundamental ontology, such a conception obviously does not count as a form of OSR.

   This conception faces serious objections. At least spatio-temporal relations are generally admitted as being non-supervenient relations, even in classical physics. As is well known, Leibniz puts forward a good argument against spatio-temporal relations existing independently of matter in his correspondence with Newton and Clarke, but he by no means succeeds in showing spatio-temporal relations to supervene on intrinsic properties of objects (such as the monads).
2. There are non-supervenient relations among objects – such as spatio-temporal relations in classical space-time prior to the theory of general relativity –, but there is an ontological primacy of the objects (relata) over the relations (and thus over the structures they are part of) in the following sense: the objects are equipped with an intrinsic identity independently of the relations in which they stand and thus independently of the other objects that happen to be in the world. Relations are irrelevant as far as the identity of the objects is concerned.

Admitting non-supervenient relations among objects whose identity does not depend on these relations is the standard way in traditional metaphysics to conceive the relationship between objects and relations. This view is not a form of OSR either, since it takes an intrinsic identity of the fundamental physical objects for granted. David Lewis' thesis of Humean supervenience can be regarded as the most famous example of such an atomistic position within contemporary metaphysics (e.g., Lewis 1986, Introduction): Lewis recognizes spatio-temporal relations as being non-supervenient relations that hold the world together. However, these relations do not contribute to the identity of the fundamental material objects. That identity is constituted by fundamental physical, intrinsic properties occurring at space-time points, the fundamental physical domain consisting in the distribution of intrinsic properties at space-time points. However, this conception is clearly at odds with quantum physics, since it is now commonly recognized that there are no intrinsic properties on which the relations of quantum entanglement could supervene (the same argument applies to QFT, even with a stronger force; see Lam 2010a).

3. There are relations as well as objects standing in the relations without there being any ontological priority between them. Relations and objects are both genuine fundamental ontological entities. They are on the same ontological footing, being given "at once" in the sense that they are mutually ontologically dependent on each other. Note two consequences of this view:

   - Since relations and objects are on the same ontological footing, it has to be accepted as a primitive that there is a numerical diversity of objects. Their numerical diversity is neither grounded in relations, nor is it grounded in intrinsic properties (since these do not constitute identity conditions for objects). Nonetheless, there is no question of haecceitism here (see Ladyman 2007; French and Ladyman, Section 2.3, this volume).
   - Contrary to what Ainsworth (2010, pp. 53–54) claims, this conception does not deny that properties are part of fundamental ontology. Properties do not have to be intrinsic, but can also be relational (extrinsic) (see Hoffmann-Kolss 2010 for an elaborate study of extrinsic properties). If there are physical relations among objects as relata, these objects have relational properties, and the other way round.

   This conception clearly is a variant of OSR. We have developed it under the name "moderate structural realism" in Esfeld (2004) and Esfeld and Lam (2008). This conception can accommodate both situation (b) and situation (c) described at the end of the preceding section, although it is (c) that is relevant in the context of current fundamental physics. Consequently, this conception has mainly been elaborated and advocated within the context of entanglement and non-separability in QM (Esfeld 2004) and with respect to GTR in order to account for the gauge-theoretic diffeomorphism invariance and the background independence postulated by that theory (Pooley 2006; Rickles 2006; Esfeld and Lam 2008; as regards moderate structural realism, see also Floridi 2008).

4. Relations are ontologically primary and objects are ontologically secondary in the sense that they derive their existence from the relations in which they stand

and thus from the structures they are part of. Objects are mere nodes within structures. The relations bear all the ontological weight: objects are literally constituted by the relations in which they stand. To the extent that this view recognizes intrinsic properties, it has to reconstruct them on the basis of relations as well. It can accommodate both situation (b) and situation (c) described at the end of the preceding section as well. Ladyman favours such a view on the basis of the mentioned physical features (see notably Ladyman and Ross 2007, Chapters 2 to 4, and French and Ladyman, Section 2.3, this volume).

5. There are only relations and no objects, thus no relata, in the domain of fundamental physics. There are only objectless structures in the sense of networks of relations without relata. Objects are not genuine fundamental ontological entities, it's relations (structures) all the way down. According to this radical conception, fundamental physics not only undermines the notion of intrinsic identity but the very notion of objects being part and parcel of fundamental ontology. This is the radical, eliminativist variant of OSR. French and Ladyman (2003) are commonly seen as setting out this version of OSR. It is still recently explicitly defended in French (2010, Section 7). This position fits only situation (c) described at the end of the preceding section. There is no question of identity conditions for objects, since there simply are no objects in the last resort.

French (2010, Section 7) provides an alternative characterization of the OSR variants (3)–(5) in terms of the ontological dependence between the objects and the structure to which they belong. Accordingly, he distinguishes three options for this ontological dependence: the case of a symmetric dependence, the case of an asymmetric dependence and the case where the dependence concerns the very essence of objects. Symmetric dependence characterizes (3), since if entities are mutually ontologically dependent on each other, none of them has an ontological priority over the others. Asymmetric dependence characterizes (4): objects derive their existence from, and are constituted by, the relations in which they stand. French's third option claims that it belongs to the very essence of an object that it exists only if the structure it is part of does. He elaborates on this option by maintaining that objects only exist "if the relevant structure exists and the dependence is such that there is nothing to them – intrinsic properties, identity, constitution, whatever – that is not cashed out, metaphysically speaking, in terms of this structure. What exists then are not objects in any ontological sense" (2010, p. 18). Hence, this option precisely characterizes (5). French (2010, Section 7) furthermore argues that (3) and (4) entail such a thin notion of object that they in effect collapse into the eliminativist variant (5).

The alleged elimination of objects in fundamental ontology has provoked the major objection against OSR, namely that in repudiating objects, it is hardly intelligible as a metaphysical conception of the concrete physical world (Busch 2003; Cao 2003; Chakravartty 2003; Psillos 2006). Three aspects of this objection can be distinguished: (i) a metaphysical one according to which relations require some sort of relata as that what stands in the relations, even if these relata do not necessarily possess an intrinsic identity; (ii) an empirical one according to which the physical features on which OSR draws its support by no means suggest abandoning a commitment to there being objects in the fundamental physical domain (see notably Ainsworth 2010, p. 53); (iii) a logical one, concerning the quantification over objects in standard first order logic and the apparently unavoidable use of set-theoretical concepts within physical theories.

To counter (iii), Bain (2009) argues that the use of category-theoretic concepts in fundamental physics may provide the right framework for eliminativist OSR (5). Nonetheless, it is not clear to what extent these concepts are free from standard set-theoretic ones and what is the relevance of category theory for fundamental physics. We are therefore not convinced that we have any workable alternative at our disposal that is capable of avoiding the notion of objects or relata altogether and in what sense such an alternative would be preferable for the formulation and understanding of fundamental physical theories. As regards (i) and (ii), we do not wish to rule out on a priori reasons that a metaphysical position that rejects objects altogether may be consistent, and we clearly do not have any objection to a revisionary metaphysics based on science, being favourable to the methodological attitude advanced in Ladyman and Ross (2007, Chapter 1). However, metaphysics should not be more revisionary than is required to account for the results of science, and in that respect, we do not see a cogent reason to abandon a commitment to objects.

As regards version (4) of OSR, this position sets out to reconstruct relata on the basis of relations, objects somehow emerging from relations as being nodes in a network of relations. However, the commitment to an ontological priority of relations over relata again invites the above mentioned objection in all its three aspects, for if objects somehow derive from relations, one still is committed to there being relations without relata in the fundamental physical domain in the first place. This objection hence applies not only to (5), but also to (4).

Nonetheless, it may seem that the idea of an ontological primacy of relations over relata, objects somehow deriving from relations, can draw support from recent claims about weak discernibility within QM. According to these claims, the identity of fundamental quantum physical objects – be they fermions, be they bosons – can always be grounded in the weak version of the principle of the identity of indiscernibles (Saunders 2006; Muller and Saunders 2008; Muller and Seevinck 2009). However, the physical significance of weak discernibility is in dispute, and we do not see how weak discernibility could ground an ontological priority of relations over relata.

The fact that two objects are weakly discernible is compatible with the two objects having all the same intrinsic properties and standing in the same relations, so that they cannot be distinguished. Weak discernibility merely means that there is a symmetric and irreflexive relation between the two objects. Nonetheless, the fact that the relation is irreflexive makes clear that there are two objects and not just one object, since nothing can stand in an irreflexive relation to itself. Consequently, weak discernibility tells us how many objects there are in the domain under consideration and thus gives us an epistemic, empirical access to these objects, but it does not distinguish these objects from each other. As Ladyman and Bigaj (2010) recently put it: "… the formal condition of weak discernibility, while being sufficient to establish numerical diversity, is not properly thought of as establishing that two objects can be discerned from each other. … The relation that weakly discerns them does not amount to a difference between them over and above their numerical distinctness" (p. 130). In a similar vein, Dieks and Versteegh (2008) say: "… because of the symmetry any property or relation that can be attributed to one object can equally be attributed to any other and we can therefore not single out any specific object" (p. 926).

By way of consequence, weak discernibility does nothing to show how objects could be derived from relations, as claimed by position (4): it is not that the relations somehow constitute the relata and their plurality, these latter somehow deriving their existence from the relations, as intrinsic properties in traditional metaphysics may be taken to constitute the identity and plurality of objects by distinguishing each object from all the other ones (at least on a view that takes the identity of objects to be somehow constituted by their intrinsic properties instead of consisting in a primitive thisness or haecceity).

Nonetheless, there is a lesson to be learnt from weak discernibility in this context. As the quotation from Ladyman and Bigaj also makes clear, due to weak discernibility, there is no need to accept a numerical diversity of objects as a primitive, as in version (3) of OSR. Being committed to such a primitive numerical diversity is the main reservation against version (3). Due to weak discernibility, there is no primitive, naked numerical diversity of objects, being entities on a par with relations, there being a mutual ontological dependence between both of them. Insofar as there are objects, these stand in irreflexive, symmetric relations, although these relations do not distinguish them. We therefore take weak discernibility to support the position that we will set out in the next section. In case weak discernibility holds firm in the case of symmetric space-times such as the FLRW solutions of the Einstein field equations as well (Muller and Saunders 2008, p. 529, claim so), the reservations that Wüthrich (2009) voices in challenging the space-time structuralist could also be adressed.

One may object against these considerations that the very posing of a domain of elements over which physical variables range presupposes acknowledging a numerical diversity of objects. However, Muller and Saunders (2008, pp. 542–544) convincingly argue that one should not mix formal, logical objects and their logical identity conditions with physical objects and their physical identity conditions, these latter being provided by physically meaningful predicates. As Muller (2009) puts it, "The numerical diversity is accounted for physically, that is, by means of physically meaningful and permutation-invariant relations" (p. 9).

We agree that the symmetric and irreflexive relations that amount to weak discernibility are physically meaningful. Consequently, we do not have to presuppose a numerical diversity of naked physical objects as a primitive in the metaphysics of fundamental physics. But we also wish to point out that the name "weak discernibility" is a misnomer, as Ladyman and Bigaj say (2010, p. 130), since the symmetric and irreflexive relations in question by no means amount to distinguishing one object from the other objects in the domain in question. As far as these relations are concerned, the objects are qualitatively, albeit not numerically, all the same. This simply is a lesson that current fundamental physics teaches us, admitting no stronger form of discernibility than the one that goes under the name of weak discernibility.

In sum, current fundamental physics does not make an intrinsic identity of the fundamental physical objects, whatever they may be, available. The relations or structures acknowledged in current fundamental physics cannot provide for an identity that distinguishes each object from the other ones either, since they yield no more than what is known as weak discernibility. However, weak discernibility does not contribute to vindicating the idea of relations enjoying ontological primacy over relata in that objects somehow emerge out of relations (4), and the other two versions of OSR – symmetric ontological dependence between objects and relations (3), eliminativism with respect to objects (5) – are not convincing either.

8.3 A Conceptual in Contrast to an Ontological Distinction

In order to overcome these metaphysical dead ends, we suggest that the distinction between objects and relations (or properties in general) is not to be regarded as an ontological one, as usually assumed, but only as a conceptual one, anchored in our thinking and language. The question of the ontological relationship between objects and relations is ill-posed. We predicate properties, including relations, of something, we quantify over objects, and we define a structure on a domain of objects by indicating how these objects are related to each other. However, this is the way in which we represent the world, its philosophical analysis going back to Aristotle's treatise *On interpretation (Peri hermeneias).* It does not match a real distinction in the world. Consequently, there is no point in enquiring into the relationship between objects and properties, including relations or structures, and, in particular, to talk in terms of a mutual ontological dependence between objects and properties, including relations or structures, or an ontological priority of the one over the others. There are not two types of entities, objects and properties including relations or structures, that entertain a certain relationship of ontological dependence. The dependence is only conceptual.

In the recent philosophy of physics literature, Earman (2002) suggests a similar move in the interpretation of the GTR observables when he proposes that the subject-properties (or attributes) distinction belongs only to the representation of reality, but not to reality itself. French (2010, p. 18) also agrees that the distinction between objects and relations is only a conceptual one, but he does so against the background of an ontology that eliminates objects, namely radical OSR (5).

We can draw on Spinoza's metaphysics in order to make the idea more precise according to which objects and relations exist without the question of an ontological priority of the one over the others, or the question of a mutual ontological dependency, arising. Following Spinoza's *Ethics* (1677), properties are modes, that is, concrete, particular ways in which objects are. There is no ontological distinction between objects and their properties in the sense of modes: the modes are the way in which the objects exist. Objects do not have any existence in distinction to their ways of existence, and their ways of existence do not have any existence in distinction to the objects. One can draw a conceptual distinction between objects and their ways of existence, but not an ontological one, applying to reality. In reality, there is only one type of entity, namely objects that exist in particular ways. This theory of Spinoza is well worked out with respect to intrinsic properties, regarding the intrinsic properties as the ways in which the objects exist and providing for an identity of the objects that distinguishes each object from all the other ones in a given domain. This theory is taken up in contemporary metaphysics notably by Heil (2003, Chapter 13) and Strawson (2008) (cf. also Armstrong 1989, pp. 96–98).

Nothing hinders applying this theory to relations as well. Like intrinsic properties, relations can be the ways in which objects exist. In this case, the objects in a given domain do not exist independently of each other, being equipped with an intrinsic identity each. By contrast, insofar as there are identity conditions for the objects, these are provided for by the relations in which they stand. Thus, relations may make available identity conditions in the sense of distinguishing each object from all the other ones in a given domain (situation (b) described in Section 8.1). Nonetheless, relations can be the ways in which objects exist without yielding identity conditions. For instance, relations of entanglement are the ways in which quantum objects exist, metrical relations are the ways in which space-time points exist, but these objects can stand all in the same relations. They are only weakly discernible, due to the relations being irreflexive, but symmetric.

Consequently, a structure consists in objects whose ways of existence are certain relations that they bear to each other. The objects hence do not exist apart from the structure they are part of. *Qua* ways in which the objects exist, the structures are networks of concrete physical relations, by contrast to abstract second order structures defined on objects and their intrinsic properties and by contrast to abstract mathematical structures (see Esfeld and Sachse 2010, Chapter 2, in particular 2.5, for an elaboration of this view in the context of the metaphysics of properties).

As with any metaphysical position, there is a certain price to pay in order to make this position available. One has to abandon the view of properties being universals that are instantiated by particulars, that is, objects. In that framework, there indeed is an ontological distinction between properties *qua* universals and objects *qua* particulars that instantiate the universals. However, there are good reasons, independently of OSR and the interpretation of fundamental physics, to abandon that traditional metaphysical framework. Suffice it here to mention the fact that since the days of Plato and Aristotle, is has not been proven possible to render the notion of the instantiation relation that is central to this metaphysical framework intelligible. If one conceives of properties *qua* universals as something that exists beyond the empirical world, the question remains unanswered what it means that the particulars, the objects, in the empirical world take part in or instantiate the universals. The problem that this question highlights has already been pointed out by Plato himself in the *Parmenides* (130e-133a), and it still is an open issue. Conceiving the universals as abstract mathematical structures that are instantiated by concrete physical objects in the world does not help to make the relationship of instantiation intelligible. If one regards, following Aristotle, the universals as being inherent in the particulars (the objects) in the world, it remains an open question how numerically one and the same universal can exist in many different objects.

The main alternative in contemporary metaphysics to the view of properties as universals that are instantiated by particulars (objects) in the empirical world is to conceive properties as tropes. One may take the distinction between properties as tropes and modes in the sense of the ways in which objects exist to be only a verbal one. After all, "modus" is in this context the Latin translation of "tropos" in Ancient Greek. However, the trope theory of properties is often linked with the view of objects being bundles of tropes. If the tropes are relations instead of intrinsic properties, objects thus being conceived as bundles of relations, one gets at least close to version (4) of OSR and faces the objections that this view provokes.

The position according to which relations are the ways (modes) in which objects exist clearly is a version of OSR. We propose to call this position "moderate ontic structural realism," thus amending our earlier view in Esfeld (2004) and Esfeld and Lam (2008) in which we defined moderate structural realism in terms of a mutual ontological dependence between objects and relations. This position is a moderate in contrast to a radical form of OSR (such as (5) and, less radical (4)), because it recognizes objects. If relations are the ways (modes) in which objects exist, there is a clear and meaningful sense in which there are fundamental physical objects in the world, thus avoiding the mentioned objections against the versions (4) and (5) of OSR. These are not some sort of Ersatz objects, but objects tout court, as conceived in one of the main metaphysical positions on objects, namely the one associated with Spinoza, according to which properties are the ways (modes) in which objects exist. Maintaining that intrinsic properties or relations are the ways in which objects exist is one thing, the question of whether or not intrinsic properties or relations are capable of providing for identity conditions of objects in that they are able to distinguish each object from all the other ones in a given domain is another matter. In other words, the question whether the relationship between objects and properties is only a conceptual or also an ontological affair and the question of identity conditions for objects are two separate issues.

By shifting from the mainstream metaphysical view of the modes being intrinsic properties to the view of the modes being relations, there is indeed no question of an intrinsic identity of objects, given by certain intrinsic ways of their existence. It is not required for an entity to be an object that it possesses an intrinsic identity or that it be distinct from all the other objects that there are in the world (see also Brading, Section 3.4, this volume). If one wishes to call objects that do not possess an intrinsic identity, following French and Ladyman (Chapter 2, this volume), thin objects, we do not have any objection against that manner of talking. Objects, as conceived by moderate ontic structural realism, still are genuine objects, but they are remarkably distinct from what traditional metaphysics takes objects to be, namely possessing an intrinsic identity. We prefer to make this issue clear by maintaining that the objects that moderate ontic structural realism admits are not individuals – since there is nothing, not even relations, that distinguishes each object in a given domain from all the other ones –, but one may also talk in terms of thin objects.

Moderate structural realism, thus conceived, has to accept a numerical plurality of objects being related in certain ways as primitive – such as a numerical plurality of quantum objects being related by entanglement, or a numerical plurality of space-time points being related by metrical field properties. Note however that this numerical diversity is not one of naked objects, being ontologically distinct from properties including relations, but a diversity of objects related in certain manners and being weakly discernible due to the relations being irreflexive and symmetric. The relations thus tell us how many objects there are.

Any metaphysical position has to accept something as primitive. The numerical plurality of objects being related in certain ways is a primitive on a par with the plurality of fundamental intrinsic properties occurring at space-time points that Lewis accepts as primitive in his thesis of Humean supervenience, the plurality of instantiations of properties *qua* universals that the theory of properties as universals accepts as primitive, the plurality of tropes that may perfectly resemble each other which trope ontology endorses as primitive, etc. Insofar as these primitives are unobjectionable in the respective metaphysical framework, so is the numerical plurality of objects being related in certain ways in the framework of moderate ontic structural realism against the background of the fact that current physics does not yield more than what is known as weak discernibility for the fundamental physical objects.

Furthermore, this position is able to take into account a view in the philosophy of fundamental physics that may be called super-holism. It is sometimes suggested that in an EPR-Bell-experiment and thus in the singlet state, there are, prior to measurement, not two quantum objects that are related by entanglement (as proposed by Teller 1986 in his seminal paper on relational holism), but only one total object that has the disposition to manifest itself in measurement as two objects whose observables are correlated in a certain way (see e.g. Morganti 2009, p. 1031, Ainsworth 2010, p. 54, and the reservations that Dieks and Versteegh 2008, pp. 928–934, voice against speaking of two weakly discernible objects in a case like the singlet state). Since entanglement is generic in quantum theory, this view amounts in the end to the position that, as regards fundamental ontology, there is only one object that has the disposition to manifest itself in the form of quantum objects that are correlated in certain ways. According to the position Spinoza sets out in his *Ethics* (1677), there is only numerically one substance, that is, one object, which Spinoza identifies with God; all the properties in the world are ways (modes) in which that one substance exists. If there is only one object in the last resort, there is of course no longer a question of physical relations being fundamental. OSR would thus not be the most fundamental ontology. There would rather be only one object that has the disposition to give rise to structures in the sense of a plurality of objects that are correlated in certain ways.

However, we remain sceptical with respect to such a super-holistic position, at least for the time being. It is entirely unclear how the one object could lead to structures in the mentioned sense as something that emerges out of it. The field interpretation of Spinoza put forward by Bennett (1984) is of no help in this context, since it conceives Spinoza's one object as classical, three-dimensional space. In the context of applying this super-holism to quantum theory, by contrast, a classical space or space-time – or a field defined on classical space-time – is obviously not a suitable candidate for the one object. Taking GTR into account, space-time cannot even be presupposed as a background. Nonetheless, given the current research programmes in quantum gravity that envisage reconstructing space-time as somehow emerging out of a fundamental quantum domain, we do not wish do exclude that such a super-holism may in the future become a serious contender in the philosophy of physics. Our point here merely is that if this should turn out to be the case – and OSR hence not hold firm as a candidate for a most fundamental ontology – the view of properties, whatever they may be in concrete physical terms, being modes, that is, the ways in which an object exists, will presumably still stand. In a nutshell, this view is fruitful for OSR in that it dispels the ontological worries about the relationship between objects and relations (structures), and it would still remain valid even if it turned out that the structures can be traced back to ways of existence (modes) of numerically one object, the only object that there is.

8.4 OSR and Intrinsic Properties

Fundamental physical intrinsic (state-independent) properties, such as notably rest mass and the various quantum charges within quantum theory, as well as total spin, constitute a notorious difficulty for OSR (for a recent version of this objection against OSR, see Ainsworth 2010, p. 54). If these properties are understood as genuine intrinsic properties – as they usually are, although they do not provide for identity conditions for quantum objects –, then it seems that none of the above three versions (3)–(5) of OSR can take them into account. Given that these are fundamental physical properties, this fact would constitute an important objection to OSR.

Of course, proponents of OSR in any of the three versions (3)–(5) have elaborated a defence. The main idea is to consider these properties as being defined in terms of symmetry relations (Castellani 1998): mass and spin can be defined as eigenvalues of the Casmir operators of the relevant symmetry group (the Galilei group in the non-relativistic case, the Poincaré group in the relativistic case), that is, as invariants of this group; similarly, quantum charges are defined in terms of the generators of the relevant quantum gauge groups (for instance, U(1) for the electric charge). Fundamental state-independent properties of quantum objects can therefore be understood in terms of symmetry relations. In that sense, OSR (in any of the above three versions) can readily account for them (Muller 2009, Section 4).

However, it is questionable to what extent such an account is satisfactory. There are two main reservations: first, in order to conceive the fundamental physical state-independent properties in terms of physical relations, the symmetry relations have to be regarded as concrete physical relations, being in that respect on a par with the concrete physical relations of entanglement in which the state-dependent properties of quantum systems are implicated. It is debatable to say the least whether such an understanding of quantum gauge symmetry relations is a serious option. Second, even within this group-theoretic understanding of the fundamental state-independent properties, it seems that these latter can still be considered as intrinsic in the standard sense of being independent of accompaniment or loneliness. This second difficulty could possibly be avoided if one takes into account that strictly speaking the definition of state-independent properties requires some fixed space-time background structures (this is especially true for mass-energy within the dynamical space-times of GTR; see Lam 2010b).

Refusing to recognize the distinction between objects and relations (and properties in general) as an ontological one puts us in the position to avoid these troubles about state-independent properties. Against this background, the OSR proponent can coherently accept them as candidates for genuine intrinsic properties without affecting the central claim of OSR: even if some of the ways fundamental physical objects are consist in intrinsic properties, important features of fundamental physics show that there always are central ways in which these objects exist that consist in relations, so that the objects do not have any existence – and in particular not any identity – independently of the structure they are part of. This is the third point in which we wish to amend the position set out as moderate ontic structural realism in Esfeld (2004) and Esfeld and Lam (2008): we no longer take OSR – at least in the moderate version that we defend, recognizing objects – to be opposed to acknowledging the existence of intrinsic properties, as long as such intrinsic properties do not amount to an intrinsic identity of the objects. It may be that there are intrinsic properties, or it may finally turn out that there are only relations in the fundamental physical domain. We wish to leave this question open. It is of minor importance for OSR as we propose to conceive this position.

8.5 OSR and Modality

The structures to which OSR is committed are usually conceived as being modal structures (French and Ladyman 2003; Ladyman and Ross 2007, Chapters 2 to 4, Esfeld 2009; French 2010, Section 3; French and Ladyman, Section 2.1, this volume; but see Sparber 2009, Chapter 4; Lyre 2010 for Humean versions of OSR that reject objective modalities). OSR is opposed to a position that accepts physical laws as something primitive (Maudlin 2007 can be read as endorsing such a position). If structures are the fundamental ontological ingredient, then the laws supervene on the structures. That is to say, what the laws of nature are depends on what are the concrete physical structures that exist in the world. However, taking the laws to supervene on the structures does not imply a commitment to objective modality, regarding the structures themselves to be modal in some sense, for such a position is also available to a Humean. According to Lewis' thesis of Humean supervenience, the laws of nature that hold in a world supervene on the distribution of the fundamental intrinsic properties that happens to exist in the world in question, the laws being, in short, the salient regularities that this distribution exhibits (see, e.g., Lewis 1994). Such a position is also available to a proponent of OSR, replacing the distribution of the fundamental physical intrinsic properties with the distribution of the fundamental physical structures (Sparber 2009, Section 4.3.1). Hence, the mere fact of the laws supervening on the structures according to OSR does not amount to a commitment to the structures being modal.

One sense in which the structures can be modal is by being causal. One thus is committed to a realism about causation that takes causation to be a feature of the fundamental physical domain. Indeed, not being able to account for causation is one of the stock objections against OSR (e.g. Psillos 2006). There are three ways of replying to this objection: (a) One can maintain, in the tradition of Russell's (1912) attack on causation, that this objection is misplaced, since there is no causation in the domain of fundamental physics (Ladyman 2008). (b) One can take this objection at face value and admit causal properties that underlie the structures, thus abandoning OSR (Chakravartty 2007, Chapters 3 to 5, can be read as making such a move). (c) One can regard the structures as being causal in themselves (Esfeld 2009; see furthermore Esfeld and Sachse 2010, Chapter 2, for metaphysical and physical arguments in favour of objective modality and causal realism in the domain of fundamental physics).

Taking the structures to be causal is to say that insofar as there are concrete physical relations as the ways in which the fundamental physical objects exist, these are powers or dispositions to bring about certain physical effects (or, in other words, these bestow on the objects whose ways of existence they are the power to bring taken together certain effects about). To indicate an example of what this idea says in concrete terms, within the framework of the GRW interpretation of QM, one can maintain that entangled states are the power or disposition to bring about classical physical properties that have a rather precise value through spontaneous localizations (see Dorato and Esfeld 2010 for details). Since the entangled states are non-separable and thus not states that are localized at a certain point in physical space-time, the idea that quantum objects standing in entangled states have, insofar as the relations of entanglement are their way of existence, together the power or disposition to bring about rather well-localized, classical physical properties through spontaneous localization does not pose any additional problems to making a collapse interpretation of QM such as GRW compatible with special relativistic physics (but does nothing to solve these problems either; see Tumulka 2006 for an important step towards achieving a relativistic version of GRW).

However, if in future research on quantum gravity, the quantum structures of entanglement should indeed turn out to be more fundamental than classical space-time, these problems may find a solution. Suffice it here to mention that the research programmes that take space-time to emerge out of a more fundamental quantum domain are compatible with considering this emergence in causal terms. It may thus turn out to be possible to regard the fundamental quantum structures as the power or disposition to bring about space-time. In brief, it is conceptually possible to conceive the fundamental physical structures as modal structures *qua* being causal structures, and such a view is not only supported by certain interpretations of QM, but may also prove to be fruitful for a future metaphysics of quantum gravity.

In conclusion, we have set out in this paper a revised version of the position introduced as moderate ontic structural realism in Esfeld (2004) and Esfeld and

Lam (2008) by maintaining that the distinction between objects and properties, including relations and thus structures, is only a conceptual one by contrast to an ontological one: properties, including relations, are modes, that is, the ways in which objects exist. Moderate OSR maintains that at least some central ways in which the fundamental physical objects exist are relations so that these objects do not have any existence – and in particular not any identity – independently of the structure they are part of. There thus clearly are fundamental physical objects (moderate OSR being a metaphysics of objects), but there is no question of a mutual ontological dependence between objects and relations, no question of having to accept a naked numerical diversity of objects as primitive, and this position also enables us to recognize genuine intrinsic properties as ways in which the fundamental physical objects exist without jeopardizing OSR. It can furthermore accommodate a modal nature of structures in conceiving them as causal powers.

References

Ainsworth, Peter M. (2010): "What is ontic structural realism?" *Studies in History and Philosophy of Modern Physics* 41, 50–57.

Armstrong, David M. (1989): *Universals. An opinionated introduction*. Boulder (Colorado): Westview Press.

Bain, Jonathan (2006): "Spacetime structuralism". In: D. Dieks (ed.): *The ontology of spacetime*. Amsterdam: Elsevier. Pp. 37–66.

Bain, Jonathan (2009): "Motivating structural realist interpretations of spacetime". Manuscript.

Bennett, Jonathan (1984): *A study of Spinoza's "Ethics"*. Indianapolis: Hackett.

Brading, Katherine (2010): "Structuralist approaches to physics: objects, models and modality".

Busch, Jacob (2003): "What structures could not be". *International Studies in the Philosophy of Science* 17, 211–223.

Cao, Tian Yu (2003): "Can we dissolve physical entities into mathematical structure?" *Synthese* 136, 57–71.

Castellani, Elena (1998): "Galilean particles: an example of constitution of objects". In: E. Castellani (ed.): *Interpreting bodies: classical and quantum objects in modern physics*. Princeton: Princeton University Press. Pp. 181–194.

Chakravartty, Anjan (2003): "The structuralist conception of objects". *Philosophy of Science* 70, 867–878.

Chakravartty, Anjan (2007): *A metaphysics for scientific realism: knowing the unobservable*. Cambridge: Cambridge University Press.

Dieks, Dennis and Versteegh, Marijn A. M. (2008): "Identical quantum particles and weak discernibility". *Foundations of Physics* 38, 923–934.

Dorato, Mauro & Esfeld, Michael (2010): "GRW as an ontology of dispositions". *Studies in History and Philosophy of Modern Physics* 41, 41–49.

Earman, John (2002): "Thoroughly modern McTaggart: or, what McTaggart would have said if he had read the general theory of relativity". *Philosophers Imprint* 2 (3).

Esfeld, Michael (2004): "Quantum entanglement and a metaphysics of relations". *Studies in History and Philosophy of Modern Physics* 35, 601–617.

Esfeld, Michael (2009): "The modal nature of structures in ontic structural realism". *International Studies in the Philosophy of Science* 23, 179–194.

Esfeld, Michael & Lam, Vincent (2008): "Moderate structural realism about space-time". *Synthese* 160, 27–46.

Esfeld, Michael & Sachse, Christian (2010): *Kausale Strukturen. Einheit und Vielfalt in der Natur und den Naturwissenschaften*. Berlin: Suhrkamp. English translation Conservative reductionism forthcoming London: Routledge 2011.

Floridi, Luciano (2008): "A defence of informational structural realism". *Synthese* 161, 219–253.

French, Steven (2010): "The interdependence of structure, objects and dependence". Forthcoming in *Synthese*.

French, Steven & Ladyman, James (2003): "Remodelling structural realism: quantum physics and the metaphysics of structure". *Synthese* 136, 31–56.

Heil, John (2003): *From an ontological point of view*. Oxford: Oxford University Press.

Hoffmann-Kolss, Vera (2010): *The metaphysics of extrinsic properties*. Frankfurt (Main): Ontos.

Ladyman, James (1998): "What is structural realism?" *Studies in History and Philosophy of Modern Science* 29, 409–424.

Ladyman, James (2007): "On the identity and diversity of objects in a structure". *Proceedings of the Aristotelian Society. Supplementary* Volume 81, 1–22.

Ladyman, James (2008): "Structural realism and the relationship between the special sciences and physics". *Philosophy of Science* 75, 744–755.

Ladyman, James & Bigaj, Tomasz F. (2010): "The principle of the identity of indiscernibles and quantum mechanics". *Philosophy of Science* 77, 117–136.

Ladyman, James & Ross, Don with Spurrett, David & Collier, John (2007): *Every thing must go. Metaphysics naturalised*. Oxford: Oxford University Press.

Lam, Vincent (2010a): "The non-intrinsic identity of fundamental quantum objects". Manuscript, submitted.

Lam, Vincent (2010b): "Gravitational and non-gravitational energy: the need for background structures". Forthcoming in *Philosophy of Science*.

Langton, Rae & Lewis, David (1998): "Defining 'intrinsic'". *Philosophy and Phenomenological Research* 58, 333–345.

Lewis, David (1986): *Philosophical papers. Volume 2*. Oxford: Oxford University Press.

Lewis, David (1994): "Humean supervenience debugged". *Mind* 103, 473–490.

Lyre, Holger (2004): "Holism and structuralism in U(1) gauge theory". *Studies in History and Philosophy of Modern Physics* 35, 643–670.

Lyre, Holger (2010): "Humean perspectives on structural realism". *Proceedings of the 1st plenary conference of the ESF programme Philosophy of science in a European perspective*. Berlin: Springer.

Maudlin, Tim (2007): *The metaphysics within physics*. Oxford: Oxford University Press.

Morganti, Matteo (2009): "A new look at relational holism in quantum mechanics". *Philosophy of Science* 76, 1027–1038.

Muller, Fred A. (2009): "Withering away, weakly". Forthcoming in *Synthese*.

Muller, Fred A. & Saunders, Simon (2008): "Discerning fermions". *British Journal for the Philosophy of Science* 59, 499–548.

Muller, Fred A. & Seevinck, Michiel (2009): "Discerning elementary particles". *Philosophy of Science* 76, 179–200.

Pooley, Oliver (2006): "Points, particles, and structural realism". In: D. Rickles, S. French & J. Saatsi (eds.): *The structural foundations of quantum gravity*. Oxford: Oxford University Press. Pp. 83–120.

Psillos, Stathis (2006): "The structure, the whole structure and nothing but the structure". *Philosophy of Science. Proceedings* 73, 560–570.

Rickles, Dean (2006): "Time and structure in canonical gravity". In: D. Rickles, S. French & J. Saatsi (eds.): *The structural foundations of quantum gravity*. Oxford: Oxford University Press. Pp. 152–195.

Russell, Bertrand (1912): "On the notion of cause". *Proceedings of the Aristotelian Society* 13, 1–26.

Saunders, Simon (2006): "Are quantum particles objects?" *Analysis* 66, 52–63.

Sparber, Georg (2009): *Unorthodox Humeanism*. Frankfurt (Main): Ontos.

Stachel, John (2006): “Structure, individuality, and quantum gravity”. In: D. Rickles, S. French & J. Saatsi (eds.): *The structural foundations of quantum gravity*. Oxford: Oxford University Press. Pp. 53–82.

Strawson, Galen (2008): “The identity of the categorical and the dispositional”. *Analysis* 68, 271–282.

Teller, Paul (1986): “Relational holism and quantum mechanics”. *British Journal for the Philosophy of Science* 37, 71–81

Tumulka, Roderich (2006): “A relativistic version of the Ghirardi-Rimini-Weber model”. *Journal of Statistical Physics* 125, 821–840.

Wüthrich, Christian (2009): “Challenging the spacetime structuralist”. *Philosophy of Science* 76, 1039–1051.

Chapter 9
Scientific Explanation and Scientific Structuralism

Mauro Dorato and Laura Felline

9.1 Introduction

An interpretation of the formalism of quantum mechanics that can be regarded as uncontroversial is currently not available. Consequently, philosophers have often contrasted the poor explanatory power of quantum theory to its unparalleled predictive capacity. However, the admission that our best theory of the fundamental constituents of matter cannot explain the phenomena it describes represent a strong argument against the view that explanation is a legitimate aim of science, and this conclusion is regarded by the vast majority of philosophers as unacceptable.

On the other hand, it is well known that for most working physicists the question of the explanatory power of quantum mechanics does not even arise, and quantum theory is regarded as explicative (or as non-explicative) with respect to quantum phenomena as any other physical theory with respect to its own domain of application.

Granting that there is such a chasm between the attitude of the "working physicists" and the philosophers of quantum mechanics, how can we explain it? One possible answer is that physicists are instrumentalists on Mondays, Wednesdays and Fridays, and scientific realists on the rest of the days, depending on the theory they are using. However, rather than attributing physicists such an opportunistic pragmatism, could we not partially make sense of their attitude by hypothesizing that they implicitly use a different criterion for individuating what counts as an "explanation"?

In this paper we try to answer in the positive this crucial question by defending the claim that quantum theory provides a kind of *mathematical* explanation of the *physical* phenomena it is about. Following the available literature, we will refer to such explanations as *structural explanations*. In order to illustrate our main claim,

---

M. Dorato (✉)
Dipartimento di Filosofia, Università degli Studi Roma Tre, Rome, Italy
e-mail: dorato@uniroma3.it

L. Felline
Dipartimento di Scienze Pedagogiche e Filosofiche, Università degli Studi di Cagliari, Cagliari, Italy

P. Bokulich and A. Bokulich (eds.), *Scientific Structuralism*,
DOI 10.1007/978-90-481-9597-8_9,

we will present two case studies, involving two of the most typical and puzzling quantum phenomena, namely Heisenberg's Uncertainty Relations and quantum non-locality.

To the extent that structural explanations rely only on the formal properties of a theory, they are obviously independent of interpretive questions concerning its ontological posits. Consequently, they justify the claim that, even in the current absence of an uncontroversial interpretation of the formalism, quantum theory, regarded as the family of its mathematical models, can provide effective explanations of physical phenomena. While structural explanations should by no means be regarded as a replacement for a sound interpretation of quantum mechanics, they can nevertheless give some philosophical support to a well-established scientific practice, one that so far has received very little attention.

Our program is therefore to reassess the traditional claim that scientific explanations are necessarily based on *physical* or *causal* models of phenomena, and to stress the explanatory force of the *mathematical models* used by quantum mechanics: as one of the precursors of this idea put it: "if we are to understand quantum theory… we will have to take seriously the idea that locating phenomena within a coherent and unified mathematical model is explanatory in itself" (Clifton 1998, p. 6).

The paper is structured as follows. Sections 2 and 4 will present, respectively, two case studies, aimed first of all at illustrating the existence of *genuine* structural explanations of puzzling quantum phenomena. Secondly, these sections will flesh out Hughes' and Clifton's seminal but thin intuitions about the nature of structural explanation (see the beginning of next section). While in Section 3 we will defend the claim that structural explanations are not mere *redescriptions* of the relevant physical phenomena, in Section 5 we will compare the main features of structural explanations with the Deductive-Nomological (D-N) and the unificationist models. By inquiring into the possible link between structural explanations and structural realism, in Section 6 we will offer some sketchy suggestions for future research.

9.2 Structural Explanations and Heisenberg's Uncertainty Relations

Unfortunately, the previous literature is not very generous in offering detailed characterizations of structural explanations. Despite the highly interesting claim that in the quantum case structural explanations provide a decisive alternative to other types of explanation, in his 1989a Hughes did not offer more than a metaphor to characterize it explicitly:

> [A] structural explanation displays the elements of the models the theory uses and shows how they fit together. More picturesquely, it disassembles the black box, shows the working parts, and puts it together again. "Brute facts" about the theory are explained by showing their connections with other facts, possibly less brutish. (Hughes 1989a, p. 198).

To be honest, by discussing the case study of the EPR correlations, Hughes tried to give a concrete and clear illustration of what it means "to disassemble" a black box and put it together again. However, in order to characterize a structural explanation, it seems to us more useful to draw attention to the already quoted, unpublished paper by Robert Clifton, in which we find the following sketch of a definition:[1]

> We explain some feature B of the physical world by displaying a mathematical model of part of the world and demonstrating that there is a feature A of the model that corresponds to B, and is not explicit in the definition of the model. (Clifton 1998, p. 7).

Consider as a first case study Heisenberg's famous Uncertainty Relations between position (x) and momentum (p) as our *explanandum B*. These relations are usually taken to entail that the values of the corresponding magnitudes are not simultaneously sharp, independently of limitations of our knowledge about the system. In a less metaphysically committed language, we could simply say that there is a limit to the simultaneous predictability of position and momentum. Why?

We will take as our *prima facie* formal representative of the non-simultanous-definiteness of the two incompatible observables the well-known equation:

$$\Delta x \bullet \Delta p \geq \frac{\hbar}{2}. \tag{9.1}$$

The explanation of such a relation usually invokes the description of a typical 'experimental setting' (a thought experiment, really), namely, the measurement of the position of an electron by the so-called Heisenberg microscope. Heisenberg's account of this thought experiment[2] made use of a qualitative argument, according to which, due to its impact with a gamma ray generated by the microscope "[a]t the instant of time when the position is determined, that is, at the instant when the photon is scattered by the electron, the electron undergoes a discontinuous change in momentum" (Heisenberg 1927, pp. 174–175).

However, Heisenberg's 1927's derivation was quite confusing. For instance, if the meaning of an observable is determined, as he claimed, by the measurement apparatus, the crucial notion of 'imprecision' becomes quite *vague and contextual*, as it receives different meanings in different experimental situations. For measures of position, such imprecision corresponds to the limited resolving power of the microscope, while the imprecision of the observable momentum is due to the unpredictability of the behaviour of a particle *after* a measurement of its position, causing an unknown recoil due to highly energetic light. Since in Heisenberg's philosophy physical concepts are defined operationally, it follows that the 'imprecision' of position and momentum receive two different meanings.[3]

---

[1] Clifton appropriates this definition, with some modifications, from Hughes' (1993) definition of *theoretical* explanation.

[2] The inequality derived by Heisenberg was not exactly the one given in (9.1). Heisenberg originally derived the formula:

$$p_1 q_1 \sim \hbar \tag{9.2}$$

The standard formulation was for first proved by Kennard, in 1927.

[3] For a more detailed reconstruction of Heisenberg's relations, see Uffink (1990, p. 96).

The confusion in dealing with the notion of uncertainty continued also after Heisenberg's first formulation, so that the uncertainty principle is nowadays typically defined as a bound to the degree to which the results of the two measurements are predictable. Moreover, the oversimplified character of this quasi-classical picture of a collision, and the well-known complications of the standard account of measurement, render a clear physical account of these relations quite difficult.

Given this state of affairs, if the availability of a clear physical interpretation were a necessary condition for having some insight into the relevant physical phenomena, the Heisenberg Uncertainty Principle ought to be considered a mystery. However "working physicists" (and not just them) do not regard such a principle as unintelligible.

In the reading we propose, this attitude of the working physicists is justifiable by the fact that the mathematics of quantum theory provides a solid *structural explanation* of the relations in question. Among physicists the better-known explanation of the position-momentum relation is *analytic*, insofar as it appeals to Fourier analysis, and shows how the formal representative $\Phi(p_x, p_y, p_z)$ of the momentum of the electron is the Fourier transform of the function $\Psi(x, y, z)$, which formally represents the coordinates specifying the position of the particle. In this functional-analysis kind of approach, the structural explanation of Heisenberg's Relations exploits a well-known *mathematical property* of the Fourier transform, on the basis of which the narrower the interval in which one of the two functions differs significantly from zero, the larger is the interval in which its Fourier transform differs from zero, in such a way that Eq. 9.1 must be satisfied.

The Uncertainty Relation between position and momentum, therefore, is understood as a direct consequence of the *mathematical*, formal properties of the Fourier transform. In other words the existence of a minimum for the product of the uncertainties of these two measurements, or the *physical*, non-simultaneous sharpness of the two observables, is explained first of all by showing how quantum systems are represented within quantum theory (i.e. by the model *M* of Hilbert spaces of square summable functions) and secondly by showing how in such a model $\Phi(p_x, p_y, p_z)$ is the Fourier transform of the function $\Psi(x, y, z)$. From these two assumptions, and in virtue of the mathematical properties of the Fourier transform, not only does it follow that $\Delta x \bullet \Delta p \geq \frac{\hbar}{2}$ (the formal representative of the physical explanandum) is also an element of *M*, but also that the *explanandum* must possess the required properties.

In the reading we propose, therefore, the properties of *the explanandum* are constrained by the general properties of the Hilbert model *M*. In this sense the *explanandum* is made intelligible *via* its *structural similarities* with its formal representative, the *explanans*. Given the typical axioms of quantum mechanics (for instance, the typical correspondence between observables and Hermitian operators, physical states and rays of the Hilbert space, a probability and a scalar product, etc.), any quantum system exemplifies, or is an instance of, the formal structure of the Hilbert space of square summable functions.

To be sure, there are many features of the uncertainty principle that are still the object of dispute, and by claiming that there is a structural explanation of the position/momentum uncertainty relations we don't mean to imply that these problems have all been solved.[4] However, physicists hold that, despite the lack of a unanimous interpretation of the physical processes leading to the limits in the predictability of the results of the measurements of position and momentum on the same particle, *such unpredictability is indeed explained by quantum mechanics*.

It follows that structural explanations provide a common ground for understanding the *explanandum* in question, independently of the various different ontologies underlying the different interpretations of quantum theory. Even under the pessimistic assumption that the *explanandum* could not be given an interpretive account in terms of a future, precise ontology, we can still claim that quantum theory is capable of explaining Heisenberg's Uncertainty Relations.

9.3 A Crucial Objection: Are Structural Explanations *Genuine* Explanations?

However, how can a *mathematical* model be explicative of a *physical* phenomenon in the first place? According to what one could call the Principle of the Explanatory Closure of the physical world, *only* a physical fact or a physical law should be allowed to function as *explanans* of another physical fact or event. In order to understand how such a Principle can be violated, it is necessary to consider that what enables us to say that the structural explanation provided *via* a mathematical model *M* is about a *physical* fact is the existence of a *relation of representation* between *M* and the physical explanandum. Such a representational aspect of *M* is obviously *necessary*, since otherwise the structural account provided by the model would be completely "unanchored" to the physical world. Without a representational or referential relation of some kind holding between *the formal explanans and the physical explanandum*, no explanation of a physical, concrete phenomenon could be forthcoming, since we would simply explain an abstract fact *via* another abstract fact.

Consequently, and more generally, at the basis of the possibility of tranferring knowledge from the abstract mathematical model to a physical target, or of performing so-called *surrogate reasoning* (Swoyer 1991), we claim there is the existence of a representation relation between the model and its target system.

In order for such a representational relation to be also sufficient for a structural explanation, however, we have to accept the idea that we understand the physical phenomenon in terms of the properties of its formal representative, by locating the latter in the appropriate mathematical model. This sufficiency, we take it, has been illustrated in the previous case study. The explanatory character of the

---

[4] For some of these problems, we refer the reader to the work already cited in Note 3.

mathematical/formal features of the Fourier functions *vis-à-vis* Heisenberg's Uncertainty Relations has to do with the fact that such features are exactly those that are required to make intelligible the relevant, represented physical phenomena. As such, they are an answer to the following "why question": *why do position and momentum not assume simultaneously sharp magnitudes?* Answer: *because their formal representatives in the mathematical model have a property that makes this impossible*. Consequently, the physical fact that the greater the precision of our measurement of one magnitude, the more undefined is the value of the other—being structurally equivalent to formal features of the Fourier functions in the mathematical model of the physical system — is made intelligible by this very model. Our claim is therefore that the existence of structure-preserving morphisms from the Hilbert space associated to the physical system to the structure of physical "relations" (observables) characterizing the system ensures that properties of the physical world can be made intelligible by properties of the model.

Clearly, the specification of what is required for a successful scientific representation is subject to a great deal of current philosophical research, which here can only be mentioned.[5] Under the structural realist view of scientific representation, however, one typically requires that the physical system be a concrete example, or a concrete instance of, the abstract mathematical model. For example, the use of Minkowski's spacetime model to explain geometrically the phenomenon of length contraction is justified by the fact that bits of the physical world (say, electromagnetic phenomena) exemplify the abstract spatiotemporal structure postulated by the model. Accordingly, it is the assumption that quantum systems exemplify (relevant parts of) the structure of Hilbert spaces of square summable functions that allows us to use properties of the latter in order to explain properties of the former.

This claim, nevertheless, could still not resolve all possible doubts. What we have described so far as a structural *explanation*, someone could note, is really a mere *translation* or redescription in the convenient language of mathematics of the truly explanatory account, to be given in terms of those entities or processes which constitute the physical or the categorial framework of the theory.[6] To be concrete, think of a balance with eight identical apples, three on one pan and five on the other. If someone explained the dropping of the pan with five apples (or the rising of the side with three) by simply saying "5 > 3", he/she would not have provided a genuine explanation.[7] The side with five apples drops because it is heavier and because of the role that its gravitational mass has *vis-à-vis* the earth, not because 5 > 3!

---

[5] For two recent essays, see Debs and Redhead (2007) and Van Fraassen (2008).

[6] A *categorial framework* is the set of fundamental *metaphysical* assumptions about what sorts of entities and processes lie within a theory's domain (see Hughes 1989b, pp. 175–176).

[7] This objection is due to Jim Brown, and was addressed to one of us during a presentation of a previous version of this paper at the first European Philosophy of Science Association (EPSA) conference, held in Madrid in 2007.

In one sense, of course, the above point is correct. In this example, it is the physical property weight that does the explanatory job. Consequently, one *could* explain the tipping of the scale by simply saying: "look, there is *more* weight here than there"; consequently, it would not be the structure of the natural numbers that works as the *explanans*, but the fact that there is a force that causes one pan to drop. But even in this example, if we have eight identical apples, a *quantitative* rather than a merely *comparative* explanation of the tilting of the scale must *also* rely on the fact that, out of eight identical apples, *5 apples weigh more than 3 because 5 > 3*. This simple example shows that arithmetic enters the explanation, depending on the context, and depending on the kind of information we want. Typically, in physics we are interested in *quantitative* descriptions of phenomena, so that often the descriptions afforded by mathematical models are explanatory.

More generally, we claim that structural explanations are not so easily translatable into non-mathematical terms without loss of explicative power. It is to be noted, in fact, that the use of mathematical models as *explanans* offers various advantages that in a non-mathematical explanation would be lacking. On the one hand, and in virtue of the postulated representation relation, it enables us to exploit the more solid knowledge that we have of the model as if it were knowledge of the structural relations of the target. On the other hand, the abstraction of the mathematical representation lets us carry on our reasoning independently of the unknown properties of the target (see Pincock 2007), in this case the categorial, intrinsic nature of the quanta.

Finally, it is important to clarify that the usefulness of mathematical explanations is not just a *faute de mieux* due to our ignorance of the exact ontology of the quantum world. In order to discard also this objection, consider again the case of the Uncertainty Relations. One, more general, algebraic explanation valid for all pairs of non-commuting observables and not just for the position-momentum relations, typically involves the non-simultaneous-diagonalizability of the matrices representing non-commuting Hermitian operators, and therefore relies on the non-commutativity of the algebra of observables related to a quantum system. Due to its greater generality, this algebraic explanation is not only shared by the different interpretations of the formalism, but is also common to the different uncertainty relations holding between the various non-commuting observables (time and energy, spin in different directions).

What kind of non-mathematical explanation could be given in place of the non-commutativity of the algebra of the quantum observables? Even granting the possibility of a physical or causal model of the position/momentum uncertainty relation, an all-purpose physical explanation (presumably in terms of a common mechanism), common to all non-commuting observables, seems hardly plausible. Just to give an example, think of the possibility of a common mechanism or physical process explaining equally well both the position/momentum and the spin-x/spin-y relations. It seems reasonable to believe that, in view of the difference in the relevant phenomena, the only possibility to meaningfully translate a general structural explanation into a physical one would be to provide different accounts for each uncertainty relation holding between two non-commuting observables. Needless to say, such different causal accounts would lose much of their explanatory power, in contrast to the single unifying universal mathematical feature offered by non-commutativity. We will come back to this unificationist feature of structural explanations in due course, when analyzing the difference between the latter and other theories of explanations, in particular the D-N and the unificationist model.

For now, we will close this section by noting that structural explanations seem important especially in those areas of physics that are very remote from the world of our experience. And the quantum world is so distant from the manifest world in which we evolved over millennia that there is no a priori reason that it should obey the same principles. Consequently, attempts to apply to it "classical" categories like "causation" (or "property" or "substance" and the like) might simply fail forever, a claim that will be further illustrated by our next case study.

9.4 Entangled States and Non-locality

In the introduction we have argued that one of the aims of a theory of structural explanation is to reduce the chasm between the physicists' explanatory practice and the philosophers' gloomy analysis of the explanatory virtues of quantum mechanics. The next case study we shall present is aimed exactly at achieving this goal, by showing how the hypothesis that physicists make more or less tacit use of structural explanations accounts quite well for their unproblematic attitude towards one of the most typical quantum phenomena, namely non-locality or non-separability.

Let instances of non-local phenomena be the *physical explanandum B*, and let non-factorizable, entangled states be their *formal* counterpart *A* in the mathematical model *M*. In virtue of what we already specified in the previous section, the non-factorizable states *represent* physical systems exhibiting non-local behaviour, say, correlated measurement outcomes in EPR-Bohm types of experiments, regarded as concrete physical events.

In order to realize how quantum theory explains non-local correlations *structurally*, one must consider once again the models with which the theory represents physical systems. By representing quantum systems *via* the well-known formalism, quantum theory 'invites us' (Hughes 1997) *to see* the former *as* Hilbert spaces, and this allows us to perform surrogate reasoning about the physical systems themselves. In particular, since composite physical systems are formally represented by the tensor product of the Hilbert spaces representing each separate quantum system, such surrogative reasoning invites us to look at measurement outcomes of correlated systems as joint elements of such tensor products.

Now, it can be proved that a model *M* exemplifying the structural properties typical of the quantum (mathematical) description of the microscopic world must be such that some of its states are entangled. Entangled states can explain non-local behaviour structurally in virtue of the following two facts:

(i) M – which, together with its structural properties, constitutes the *explanans* of the nonlocal behaviour of quantum systems – obeys the *principle of superposition*, a crucial formal feature that ensures that the sum of vectors of the Hilbert space (physical states) is also a vector of the space (a physically possible state);

(ii) Some such superpositions of state vectors in the tensor products of the Hilbert spaces associated with subsystems of a composite system *cannot* be written as a tensor product of any of the state vectors belonging to the component Hilbert spaces. This crucial feature, known as entanglement or non-factorizability, is the key to explaining non-locality, since it is responsible for the peculiar holism of the quantum world.

It could be objected that these two formal features, taken by themselves, do not suffice to provide a full explanation of the correlations of measurement outcomes in experiments with, say, particles emitted in the singlet state:

$$\frac{1}{\sqrt{2}}\left(|\uparrow\rangle_1|\downarrow\rangle_2-|\downarrow\rangle_1|\uparrow\rangle_2\right) \tag{9.3}$$

For this purpose, so the objection continues, we also need

(iii) The law of conservation of intrinsic angular momentum, and additional information on the initial state of the emitter of particles.

However, the crucial claim that we want to put forward is that whatever goes on in (iii) is necessary but not sufficient to explain non-locality,[8] and that *reference to the formal structure of the theory*, i.e., to (i) and (ii), *is needed essentially*. The physical *explanandum*, corresponding to the existence of non-local correlations across *spacelike* separated regions, is understood in terms of the properties of its formal representatives in the model by realizing that non-factorizable states in the Hilbert space M, *qua* formal counterpart of non-local measurement outcomes, *share* some essential properties of the latter. In particular, the tensor product formalism implies that any possible (definite) outcome on one side of an EPR-Bohm experiment is inseparably linked to another (definite) outcome on the other side.

This is clear in singlet states for a spin ½-particle as expressed in (9.3). The mere formal structure of the singlet state makes it clear that, in the hypothesis of completeness, before measurement the fermion does not have any definite spin in any direction. If it is the act of measurement that creates a definite "element of reality" in one of the two spacelike-separated wings of the experiment, the spin pertaining to other side must, in virtue of the mere existence of anticorrelated states of this sort, be instantaneously "determined" in a non-local fashion, independently of whether such "non-locality," or such "determination," admits or does not admit a

---

[8] As we will see in more details in the next section, this also marks the difference between structural explanations and the D-N model of explanation.

causal interpretation.[9] Since, after measurement, one of the two possible outcomes in an entangled state like (9.3) must be observed, the properties of the formal state (9.3) constrain those of the physical systems that they represent and, in cases of measurements apparatuses that are spacelike separated, imply by themselves non-local behaviour.

In sum, the non-separability or non-locality of the physical outcomes is understood in terms of the fact that that before measurement the joint state has no definite property, that each separate tensor product in (9.3) represents the two possible outcomes, and that the two elements of each tensor product cannot be separated, and *have* to occur always together, given the way that composite systems are formally represented. The fact that entangled states are independent of distance is then sufficient for non-locality.

A couple of striking analogies with the previous case study should be noticed. First of all, the explanation just offered is valid across all the various interpretations of quantum theory, and it is therefore wholly independent of any of them.[10] Furthermore, *if* one rejects (along with Fine 1989) the possibility of a *causal* explanation of non-local behaviours in quantum theory, then any interpretation of the formalism of the theory can hardly add any significant explanatory information to that already provided by the mathematical model of the phenomenon.[11]

Can we justify the above antecedent? We claim that the possibility of regarding quantum non-locality as not needing a causal explanation is grounded in the conceptual and explanatory switch required by all major scientific revolutions. Exactly as, before Galileo, we thought that inertial motion required a causal account and then we discovered that it didn't require one; and exactly as, before Einstein's special relativity, we thought that the fact that light seemed to have the same speed in all inertial frame needed a dynamical/causal explanation, and then we discovered that it didn't need one; and exactly as before Einstein's general relativity, we thought that free fall required a force and therefore a cause, and then we discovered that it didn't require any cause; also after the quantum revolution, we may have to regard quantum non-locality as explanatorily primitive or fundamental and therefore non-caused, instead of trying to use old, classical causal categories in order to understand it. What needs to be explained causally is rather the loss of coherence, or the non-entangled nature of the macroscopic world.[12]

Secondly, also in this case study there exists another, more abstract and general way to explain non-locality structurally or formally *vis-à-vis* classical separability, one that has been illustrated by John Baez (2006), and that hinges on the formal difference between the properties of the tensor product in the category "HILBERT SPACES" and the Cartesian product in the category "SET." In this explanatory

---

[9] For instance, according to the Bohmian interpretation, non-locality has a causal reading, corresponding to action at a distance.

[10] A possible exception is Everettian interpretations, where non-locality could be doubted in virtue of the fact that all outcomes are simultaneously realized. However, relative to a single branch or world, there must be a non-local correlation.

[11] If one adds the physical law and the initial conditions mentioned in (iii).

[12] For a full justification of this claim, we refer the reader to Dorato (2011).

approach, involving category theory, the classical intuition that a joint system can be accurately described by specifying the states of its parts corresponds to, or is denoted by, the mathematical properties of the Cartesian product: if the set of states of the first system is S and that of the second is T, the joint system has the Cartesian product $S \times T$ as its formal counterpart, where $S \times T$ is the set of all ordered pairs (s, t) such the first member s is in S and t is in T.

In order to make the idea of Cartesian product applicable in category theory and generalize it, Baez introduces projection functions (morphisms) from the set $S \times T$ to its components:

$$p_1 : S \times T \to S \qquad p_2 : S \times T \to T \tag{9.4}$$

such that "for any set X and any function (morphism) f_1 and f_2, with f_1: $X \to S$ and f_2: $X \to T$, there exists a unique function f: $X \to S \times T$ such that $f_1 = p_1 f$ and $f_2 = p_2 f$" (Baez 2006, p. 256). Figure 9.1 illustrates the definition by showing how, by composing f with the two projections, we get, respectively, f_1 and f_2.

With such a generalized definition of (Cartesian) product, one can propose a comparison with the category given by Hilbert spaces, and give a somewhat *more general* structural explanation of non-locality. In the category of Hilbert spaces (HILBERT), the formal representative A of the joint state of a composite physical system (our explanandum B) is still the tensor product of the component Hilbert spaces, which does *not* obey the condition stated above for the product in the category SET. In particular, given two Hilbert spaces H and K, if $H \otimes K$ is their tensor product, then there are *no* morphisms (linear operators) p_1 and p_2 that project pure states in $H \otimes K$ onto pure states in the component states[13]:

$$p_1 : H \otimes K \to H \qquad p_2 : H \otimes K \to K \tag{9.5}$$

It could be noted that the formal language employed in this example is not a mere redescription of the non-factorizability of the states in $H \otimes K$ that was alluded to before, but yields a *different* way of understanding it, even though the formal representative A of nonlocal states is still the tensor product. *The difference in*

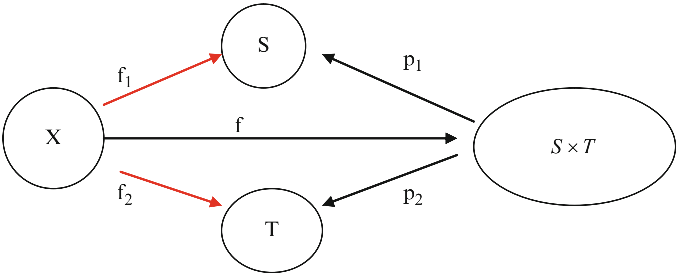

Fig. 9.1 Projection functions from the set $S \times T$ to its components, Eq. 9.4

---

[13] See Baez (2006, pp. 259).

understanding depends exclusively on the fact that the same formal representative is embedded in different models, and therefore possesses different structural properties, in such a way that in order to produce an explanation, different procedures are required.

However, even the existence of two different structural explanations, if it were to be conceded, would not represent an embarrassment for our account of structural explanation. The fact that the language of n-category theory can provide a framework to unify – formally at least – Hilbert spaces with n-cobordisms and therefore with the mathematical language of general relativity, may shed new future light on non-locality regarded as a physical phenomenon.

9.5 Structural Explanations, the D-N Model, and Explanations by Unification

The illustration of the above case studies naturally leads to a further question: it could be argued that *locating* the formal representative of the *explanandum* within the mathematical model of the theory M is always equivalent to a *deduction* of the sentence expressing the *explanandum* from a set of sentences expressing the initial conditions. This deduction would involve laws of nature, e.g., in our two cases, the conservation of angular momentum for non-locality or Heisenberg's Principle itself for the uncertainty principle. If what we call "structural explanations" ended up being a mere relabeling of the D-N model, then a theory of structural explanation would in effect be superfluous.

However, as already implicitly shown in our former discussion, the D-N model cannot actually cover the presented case studies: when a phenomenon is explained structurally, the purely *mathematical* features of the model become essential, while laws of nature and initial conditions might be necessary but insufficient for the explanation. Consider the latter example discussed above. We claimed that the non-separable character of the measurement outcomes is a consequence of the existence of non-factorizable states in the Hilbert space model of the theory, but these formal properties do not also express, strictly speaking, a law of nature. In a word, even though the physical system *exemplifies* some structural features of the mathematical model, the formal representative A of the physical *explanandum B*, a mathematical property like the tensor product, or the non-existence of morphisms that project pure states in $H \otimes K$ onto pure states in the component states H and K does *not* denote physical laws, even though it carries the explanatory weight.

The same remark applies to the former case study. We have seen how, in order to explain the position/momentum uncertainty relation, we have used the Fourier transform's rule, according to which the narrower is the interval in which $\Phi(p_x, p_y, p_z)$ significantly differs from zero, the larger is the interval in which its Fourier transform $\Psi(x, y, z)$ differs from zero, and conversely. Obviously this "mathematical law," analogously to the more general non-commutativity of an algebra of observables, represents a physical fact. But even if the represented fact were a physical regularity, in the structural account of Heisenberg relations, as well as in the non-locality case, *it is the physical regularity that is explained/understood in terms of the mathematical fact*, *and not the other way around.*

A second argument separating the D-N account from the theory provided here is implicit in the following quotation:

> [I]f one believes (as I do) that scientific theories [...] provide explanations, then one's account of explanations will be tied to one's account of scientific theories. (Hughes 1989a, p. 257)

While structural explanations are a natural by-product of the *semantic* view of theories, the natural environment for the D-N model is the 'received', *syntactic* view of theories, and it is not evident how the latter view can be consistently adapted into the framework of the semantic view.

In the previous section we have stressed the importance of unification as evidence that the structural accounts of Heisenberg's relations in terms of non-commutativity or n-category theory are genuinely explanatory. To be sure, generality and scope are important virtues of structural explanations (remember that one of the crucial advantages of structural explanations is that they are independent of the non-structural properties of a system, and therefore of the specific interpretation of quantum theory one advocates); however, the thesis that structural explanation is merely explanation by unification, or that unification is a necessary feature of every structural explanation, need not follow, and is refuted by the first of our case studies. The explanation in terms of the Fourier transform, in particular, is not achieved via a unification *per se*, and yet it is explanatory in virtue of the commonality of relations that the represented physical systems and the model exemplify.[14]

On the other hand, the unificationist models of explanation suffer from the well-known difficulty of defining what counts for unification, a difficulty that is solved by structural explanations: the unification of diverse physical phenomena sharing the same mathematical structure is a unification enabled by the possibility of regarding all of these phenomena as different exemplification of the same structural features characterizing the mathematical model.

Finally, notice that to the extent that traditional unificationist models are committed to the possibility of logically deducing the explananda from the unifying laws of nature or from those of the reducing theory, the structuralist view is not likewise committed. In fact, the previous examples should have convinced the reader that a representation relation between the *explanans* and the *explanandum* may suffice (Fourier transform explanation of the position-momentum relations, or the tensor product explanation of non-locality), and that in structural explanations the unification is not achieved in a syntactic fashion.

To conclude, what is said above is not meant to deny the existence of deep analogies between unificationist theories of scientific explanation and structural explanations.

---

[14] As a fact of sociology, it could be guessed that while the philosophically educated person tends to prefer the most unifying algebraic approach as the most explanatory, this attitude is far from being typical among physicists, who, in explaining the momentum/position Uncertainty Relation, more often than not rely on the less-general analytic explanation.

Unification is surely a virtue of any explanation (whether structural or causal); what we deny is simply that the received unificationist accounts can be regarded as sufficient to explain quantum phenomena.

9.6 Structural Explanations and Structural Realism

Is there any connection between the effectiveness of structural explanations and the current discussions on structural realism? In this final section we will lay our cards on the table for further inquiry into this issue.

An often heard complaint about the blossoming literature on structural realism is that, so far, there is no clear definition available of what a *physical structure* is. Besides the clear distinction between *epistemic* and *ontic structural realism* – where the main divide is, respectively, whether all we can know is structure, or rather that we can know only the structure of physical entities since structure is all there is – there remains the crucial problem of clarifying once and for all what is this physical structure that theories regarded as mathematical models are meant to capture.

However, if the key idea to make sense of a physical structure is to think of it as *a net of physical relations*, the kind of net and the type of relations instantiated by physical systems must necessarily depend on the particular mathematical model one is working with. So there might simply be no question of *the* structure of the world, or of what a physical structure is, since these questions are contextually dependent on particular mathematical models. This "contextualism" fits in well with the claim, illustrated by our two case studies, that one could have different structural explanations of the same physical phenomenon simply by locating its formal representative (the uncertainty relations between position and momentum for the first case study, the tensor product for the second) in *prima facie different* mathematical models (the space of L^2 functions and a non-commutative algebra of observables on the one hand, an Hilbert space and n-category theory on the other hand). It is clearly always possible to discover that the two explanations are equivalent, either because the two models are isomorphic to each other, or because the more abstract model contains the less abstract as a particular case.[15]

If this is correct, a particularly clear formulation of structural realism advocates the *primacy* of *relational* properties over *intrinsic* properties of physical entities. Now, there are various ways to analyze the crucial notion of "primacy," One involves the *identity* of objects: relational properties are more important than intrinsic properties in *defining* what an entity is. A second, more *ontological* and quite radical way of understanding the idea of primacy is to deny the existence of entities *tout court* (Ladyman 1998). A more moderate form of ontic structural realism consists instead in trying to reconceptualize all physical entities as bundles of relations which instantiate second-order relations with other "entities." This less radical form of ontic

---

[15] In this paper, we have not further studied these possibilities.

structural realism claims that there *are* indeed relata or entities, but that there are no intrinsic or monadic properties of physical entities, since all entities have extrinsic or relational properties (Esfeld and Lam 2008).[16]

A third, so-far unexplored way to capture this idea of *primacy*, one that is much more natural in the context of this paper, involves our *explanatory practices*: the idea is that structural properties of entities are *explanatorily more important than their intrinsic properties.* Is a claim about the explanatory primacy of relations sufficient to endorse some form of structural realism?

To the extent that explanatory power is an epistemic virtue, one could argue that this third way is close to a form of epistemic structural realism. However, we don't think that the effectiveness of structural explanations of physical phenomena can be used to defend *epistemic* structural realism or *ontic* structural realism of various sorts (radical or moderate). From our thesis that relational properties are explanatorily central in contemporary physical theories it need not follow, in fact, that we can only know relational properties of objects, or that the nature of things will always be hidden to us (epistemic structural realism). Nor does it follow that we can use (epistemic or) ontic structural realism in order to *justify* or explain the centrality of structural explanations in our explanatory practices: inferences to best explanations, i.e., attempts at explaining structural explanations, are a risky business.

Finally, it seems to us that the importance of structural explanations cannot be used to decide between epistemic and ontic structural realisms, since both camps acknowledge the existence of physical webs of relations, and this recognition alone is sufficient to account for the genuine explanatory character of structural explanations. Since the advantage of structural explanations lies in stressing the explanatory information that is common to various interpretations of quantum theory, it would be surprising if it could contribute to solving metaphysical issues pertaining to question of scientific realism.

In conclusion, there are only two aspects of structural explanations that might be relevant for structural realism: (i) the claim that the *physical* world and the *mathematical* world share the same structure, in the precise sense that data models of the former are isomorphic to theoretical models belonging to the latter,[17] and (ii) the claim that such structure is explanatory central. As stressed in the previous part of the paper, the requirement of representation is the key to ensuring the fact that mathematical structures refer to the physical world and can explain it. However, while the existence of a representation relation between model and world is required to guarantee the effectiveness of structural explanations, it cannot be regarded as committing us to any of the known forms of structural realism, neither is it sufficient to exclude other forms of scientific realism (entity realism or theory realism).[18]

---

[16] This latter conception is committed to regarding mass, charge and spin as extrinsic rather than intrinsic properties of particles, a claim that, on the face of it, looks quite implausible. (For a revised version of their view, see Chapter 8.)

[17] For an articulation of these claims, see Suppes (2002) and Dorato (2005).

[18] Entity realism commits us to the existence of entities endowed with intrinsic properties, while theory realism commits us to the (approximate) truth of empirically successful laws or theories.

Acknowledgements: This paper has a long story. Some parts of it were presented at the EPSA07 and BJPS 2007 conferences. We are very thankful for the useful comments that came out in these occasions. One of us (L.F.) would also like to warmly thank her fellows at the Pittsburgh Center for the Philosophy of Science (Claus Beisbart, Erik Curiel, Urlich Krohs, Flavia Padovani, Chris Pincock, Federica Russo) and its director John Norton, for comments on a previous version of this paper from which this final version highly benefitted.

References

Baez, J. (2006), Quantum Quandaries: A Categorical-Theoretic Perspective, in S. French, D. Rickles, J. Saatsi (eds.), *Structural Foundations of Quantum Gravity*, Oxford University Press, pp. 240–265.

Clifton, R., 1998, Scientific Explanation in Quantum Theory. http://philsci-archive.pitt.edu/archive/00000091/

Debs, T., and Redhead, M. (2007), *Objectivity*, *Invariance and Convention*, Harvard University Press, Harvard, Mass.

Dorato, M. (2005), *The Software of the Universe*, Ashgate, Aldershot, UK.

Dorato, M. (2011), Scientific revolution as transformations of questions into postulates, forthcoming

Esfeld, M., and Lam, V., (2008) Moderate Structural Realism About Space-Time, *Synthèse*, 160, N. 1, pp. 27–46.

Fine, A. (1989), Do Correlations Need To Be Explained?, in J. Cushing and E. McMullin (eds.), *Philosophical Consequences of Quantum Theory*, Notre Dame University Press, Notre Dame, IN, pp. 175–194.

Heisenberg, W. (1927), Ueber den anschaulichen Inhalt der quantentheoretischen Kinematik and Mechanik"*Zeitschrift für Physik*, 43, pp. 172–198. English translation in J.A. Wheeler and W.H. Zurek, (eds.) (1983), *Quantum Theory and Measurement*, Princeton NJ: Princeton University Press, pp. 62–84.

Hughes, R.I.G. (1989a), Bell's theorem, ideology, and structural explanation, in Cushing, J. and McMullin, J. (eds.), *Philosophical Consequences of Quantum Theory*, Notre Dame, pp. 195–207.

Hughes, R.I.G. (1989b), *The Structure and Interpretation of Quantum Mechanics*, Harvard University Press.

Hughes, R.I.G. (1993), *Theoretical Explanation*, Midwest Studies in Philosophy XVIII: 132–153.

Hughes, R. I. G. (1997), Models and Representation, *PSA 1996: Proceedings of the 1996 Biennial Meeting of the Philosophy of Science Association*, vol. 2. East Lansing, MI: Philosophy of Science Association, S325–S336.

Ladyman, J. (1998). What Is Structural Realism? *Studies in History and Philosophy of Science*, 29, pp. 409–424.

Pincock, C. (2007), A Role for Mathematics in the Physical Sciences, *Nous* 41: 253–275.

Suppes, P. (2002), *Representation and Invariance of Scientific Structure*, CSLI Publication, Stanford CA.

Swoyer, C. (1991), Structural Representation and Surrogative Reasoning, *Synthese* 87: 449–508.

Uffink, J. (1990), *Measures of Uncertainty and the Uncertainty Principle*, PhD thesis, University of Utrecht. http://www.phys.uu.nl/igg/jos/publications/proefschrift.pdf

Van Fraassen, B. (2008), *Scientific Representation. Paradoxes of Perspectives*, Oxford University Press, Oxford.

Index