8-12-2014

On the Unity and Continuity of Science: Structural Realism's Underdetermination Problem and Reductive Structuralism's Solution

Anthony Blake Nespica

ON THE UNITY AND CONTINUITY OF SCIENCE:

STRUCTURAL REALISM'S UNDERDETERMINATION PROBLEM AND REDUCTIVE STRUCTURALISM'S SOLUTION

by

ANTHONY BLAKE NESPICA

Under the Direction of Daniel A. Weiskopf

ABSTRACT

Russell's claim that only structural knowledge of the world is possible was influentially criticized by Newman as rendering scientific discoveries trivial. I show that a version of this criticism also applies to the "structural realism" more recently advocated by Worrall, which requires continuity of formal structure between predecessor and successor scientific theories. The problem is that structure, in its common set-theoretical construal, is radically underdetermined by the entities and relations over which it is defined, rendering intertheoretic continuity intolerably cheap. I show that this problem may be overcome by supplementing the purely formal relation of intertheoretic isomorphism with the semiformal "Ontological Reductive Links" developed by Moulines and others of the German "structuralist" approach to the philosophy of science.

INDEX WORDS: Structural realism, Structuralism, Scientific realism, Reduction, Semantic view of theories, Philosophy of science

ON THE UNITY AND CONTINUITY OF SCIENCE:

STRUCTURAL REALISM’S UNDERDETERMINATION PROBLEM AND REDUCTIVE STRUCTURALISM’S SOLUTION

by

ANTHONY BLAKE NESPICA

A Thesis Submitted in Partial Fulfillment of the Requirements for the Degree of

Master of Arts

in the College of Arts and Sciences

Georgia State University

2014

ON THE UNITY AND CONTINUITY OF SCIENCE:

STRUCTURAL REALISM'S UNDERDETERMINATION PROBLEM AND REDUCTIVE STRUCTURALISM'S SOLUTION

by

Anthony Blake Nespica

Committee Chair: Daniel A. Weiskopf

Committee: Andrea Scarantino

Neil Van Leeuwen

Electronic Version Approved:

Office of Graduate Studies

College of Arts and Sciences

Georgia State University

August 2014

DEDICATION

To Paxton Dean Nespica. You inherit a natural world of great beauty and still greater mystery. A world savage with unapologetic wonder. May you likewise inherit a sociopolitcal world possessed of the will and wherewithal to interrogate that wonder with commensurate boldness. For a paradise obscured is a paradise unstewarded. A paradise lost.

ACKNOWLEDGEMENTS

This work would likely never have escaped Meinong's jungle without the support, guidance, advice, and solidarity of a great many others.

My thanks to my Thesis Director, Dr. Dan Weiskopf, whose discipline has kept me on task and resistant—for the time being—to the siren's song of concerns largely tangential to the one with which I'm here centrally occupied, and whose challenging insight has helped me scrutinize this core concern from perspectives which might otherwise have remained remote to me.

My thanks as well to the other members of my Thesis Committee, Dr. Andrea Scarantino and Dr. Neil Van Leeuwen for many helpfully incisive comments and questions on drafts of this thesis. The final product benefits greatly from their input.

Thanks to my mother, Rose Anne, my own Mickey Goldmill, for always being in my corner, and to all other members of the clans Nespica and Holdsworth for their unflagging support.

A special thanks to Drs. Janice Daurio and Paul Ford, who first put the philosophy bug in me and whose hospitality and moral support over the intervening years can only be described as angelic. Thanks also to Dr. Patricia Churchland, whose warm advice and encouragement proved pivotal for a confused undergraduate torn between two great academic loves.

This work has also benefitted in innumerable ways small and large from conversations, communications, consultations, commiserations, and good-natured contestations with a great many friends and colleagues. Foremost among them are Emily Cahill, Michael Mahoney, Morgan Thompson, Michael Cawley, Michael Nielsen, Archie Fields, Marcus McGahhey, Jessie Green, Adam Fox, Alexandra Lee, Chris Wargo, John Alexander, Ross Delkhaste, Dylan Belton, and Richard Nutley. You have all, after your own fashions, sharpened me, and I say without reservation that this work and I are both the better for your presence in my life. *Grazie,* all.

TABLE OF CONTENTS

LIST OF FIGURES

INTRODUCTION

Structural realism is a deflationary strain of scientific realism that eschews ontological or epistemological commitment to all but the *relational structure* of our best scientific theories. It was advanced in its modern iteration by John Worrall (1989) as an answer to Laudan's (1981) anti-realist "Pessimistic Meta-Induction" (PMI) and the general problem of apparently radical ontological discontinuity between predecessor and successor theories of a common phenomenal domain. Structural realists have argued that despite pre- and post-succession incommunsurability at the level of *object-ontology,* there is nevertheless frequently a large degree of intertheoretic continuity at the level of mathematical or other formal *structure*.

However, Worrall's structural realism, couched within the syntactic or sentential view of theories, is vulnerable to a version of an argument Max Newman (1928) raised against an early structuralist thesis briefly advocated by Russell (1927). The problem Newman identified is that any purely structural constraint, whether imposed by a phenomenal domain or by another theory, can be satisfied by an indefinite number of theories of the right cardinality. The problem has recently been generalized by French and Saatsi (2005) and shown to also applicable, in a residual but still significant form, to newer species of structural realism developed within the semantic or model-theoretic view of theories.

The crucial question for the committed structural realist is: To what, if anything, beyond representational structure can one legitimately appeal in order to establish a trans-successional continuity both strong enough and nontrivial enough to satisfactorily avert the PMI?

The answer, I think, can be provided by examining a solution developed in response to an analogous problem in the context of a different intertheoretic relation: reduction. In the early nascence of the model-theoretic framework, Patrick Suppes (1957; 1962) devised a novel account of intertheoretic reduction based on structural isomorphism. Briefly put:

> For any two theories Θ_1 and Θ_2, and any two models $\mu_1(\Theta_1)$ and $\mu_2(\Theta_2)$ (set-theoretically construed), Θ_1 reduces Θ_2 just in case there is a model μ_1^*, constructible within $\mu_1(\Theta_1)$ that is *isomorphic* to $\mu_2(\Theta_2)$.

This account was influentially criticized by Ken Schaffner (1967) as being too permissive and, as such, unable to rule out spurious mappings between models (e.g., of classical collision mechanics and exchange microeconomics) that may be set-theoretically isomorphic but which clearly stand in no direct reductive relation. To address this particular version of what we may call the "Generalized Newman Problem" (GNP), Moulines (1984, 2006) introduced the concept of an "Ontological Reductive Link" (ORL) as a semiformal adjunct to structural morphism that connects the "kinds" (Moulines, 2006) that figure in one model/theory to those that figure in its putative reductive parter. I will argue that, under an appropriately non-essentialist reading of "kinds," the structural realist may adapt this solution to her own version of the GNP, viz., the problem of establishing non-spurious continuity between diachronic theories of a common phenomenal domain.

In Chapter I, I will briefly introduce structural realism (SR) and the PMI it was invoked to overcome. I will then describe Newman's criticism as it applies to both syntactic and semantic versions of SR, developing a concise, general statement of the problem, the GNP. Structural realists grappling with this problem have tended to appeal to context in order to fix the right structures in the desired continuity relation (Brading & Landry, 2007; French & Saatsi, 2005; see also, van Fraassen, 1997). It has been difficult, however, to make these appeals both sufficiently metaphysically innocuous that they do not undermine the crucial entity-agnosticism of SR and sufficiently principled that they do not appear merely *ad hoc*.

In Chapter II, I will briefly describe Suppes' early account of intertheoretic reduction and the challenge to it raised by Schaffner, viz., that it is unable to rule out obviously spurious reductions. I will show that the difficulty arises because the isomorphism on which Suppesian reduction depends is, in set theory at least, a purely structural relation, and that Schaffner's problem is, therefore, another instance of the GNP.

In Chapter III, I will introduce the set-theoretic characterization of models developed by Sneed (1971), Stegmüller (1976), Mayr (1976), Moulines (1984, 2006), and others in the German "structuralist" tradition. Each model μ of a theory Θ on such an account has the following set-theoretic structure:

$$\langle D_1,\ldots D_m, [A_1,\ldots A_n], R_1,\ldots R_p\rangle,$$

where the D_i pick out the model's base sets (i.e., its empirical ontology), the A_j its auxiliary base sets (i.e., its purely mathematical or formal "ontology"), if applicable, and the R_k the family of relations obtaining among and between the D_i and A_j. I will next describe how structuralists conceive of theories, will introduce two additional formal constraints on Suppes' account of reduction based on the work of Balzer, Moulines and Sneed (1987) and Bickle (1998). I will then introduce Moulines' (1984) notion of an ORL as a set of connections between the base sets figuring in the appropriate models of reduced theory Θ_R to some combination of base sets, auxiliary base sets, and relations figuring in the models constituting the reduction base of Θ_R within the reducing theory Θ_F, precisifying the account formally using the set-theoretic construct known as an *echelon* (after Moulines, 2006). ORLs are legitimated (and to some extent determined) by what the structuralists call the "intended empirical applications" of the theories in the putative reductive relation (Balzer, Moulines, & Sneed, 1987; Bickle, 1998). I will demonstrate, using examples, how ORLs allow reductionists working within a model-theoretic framework to avoid in a systematic way spurious reductions of the sort that worried Schaffner.

I will argue that ORLs may also be applied, *mutatis mutandis*, to the SR version of the GNP. This may at first blush seem counterintuitive, since it is tempting to think of a model's "base sets" as the *objects* whose behavior the model tracks. Moulines (2006), however, sees the base sets as designating *kinds* (encompassing, potentially, both the natural and non-natural sort). Following non-essentialist views of kinds, such as those of Boyd (1991) or Millikan (1999), a structural realist needn't see the use of ORLs as committing her to anything more than clusters of *relational* properties to which theorists have found it useful to assign names. What ORLs effectively are in the hands of the structural realist are tools for making precise and principled the hitherto only vague appeals to context by which structural realists have (unsuccessfully, in my judgment) tried to banish the specter of the GNP.

CHAPTER I

1.1 Scientific realism and its discontents

Scientific realism, broadly construed, is the thesis that science aims at providing accurate descriptions of the objective kinds into which nature divides itself and accurate representations of their interrelations, and succeeds in doing both in a generally (if not uniformly) progressive manner. Scientific *anti*-realism is the denial of either of the central conjuncts of scientific realism. Over the past several decades, the shape of the debate between these rival views has been largely dictated by two intuitively compelling but opposing arguments. Realists, beginning with the early Hilary Putnam (1975), have claimed that the accretionary explanatory, predictive, and manipulatory successes of the scientific enterprise would have to be reckoned *miraculous* if its theories were not reliably latching onto the objective facts and ontic categories of the world. That is to say, the realist avers, the *best explanation* for the success of science is that its theories and models are (approximately) true. This has come to be called the *No Miracles Argument* (NMA; Worrall, 1989). Anti-realists, on the other hand, have observed that the history of science is a intellectual graveyard packed with the remains of theories once thought true—even obviously so—but now regarded radically false by the lights of successor theories and the new, incompatible ontologies they invoke. This state of affairs as famously moved Larry Laudan to claim (1981) that it is almost certain that most if not all of our *current* scientific theories will one day be deemed radically false as well. If this is correct, then we then have no rational justification for believing in their approximate truth. This argument is commonly known as the *Pessimistic Meta-Induction* (PMI).

The force of the PMI depends heavily upon a view of scientific theory change most influentially articulated by Thomas Kuhn (1962). Most realists, committed as they are to only the *approximate* truth of our best scientific theories, would see little trouble for their view in admitting the *strict* falsity of any theory at time *t*. The received picture of scientific progress is that such theories are continuously being patched up, refined, refitted, and in various other ways corrected as increases in observational data and advances in experimental apparatus and mathematical formalism warrant, such that realist credence in these theories grows increasingly justified over time. Kuhn, however, observed that as this "normal science" proceeds, intractable problems tend to accumulate alongside pragmatic successes, clogging the gears of research until at some point the theory and its associated investigative framework must simply be replaced whole cloth by a new paradigm in which the anomalies of the previous no longer arise.

Crucially for Laudan's argument, the theory that organizes this new paradigm rests upon an ontology that cannot be mapped in any tidy and straightforward way onto the ontology of the predecessor theory. The kinetic theory of heat was not a mere refinement or emendation of the caloric theory; the central ontological posit of the latter—a fluid-like heat *substance*—simply finds no home nor obvious analogue in the former. The luminiferous ether of Fresnel's mechanical theory of light was similarly excised in Maxwell's succeeding electromagnetic framework. Even where successor theories borrow terms from their predecessors, it is often the case that the intended referent varies between them. Relativistic mass, for example, exhibits properties—e.g., inertial frame-dependence—that do not feature in classical Newtonian mass. If these ontologies are as incommensurable as the radical Kuhnian alleges, then there is little sense in which predecessor and successor theories can be said to be the same or even continuous, though their empirical successes may largely overlap. If this pattern of stagnancy and replacement truly constitutes the norm of scientific progress, then, Laudan invites us to induce, we should expect even our best contemporary theories to one day be jettisoned as well. We've little reason, therefore, to think that *any* theory at *any* time is capturing something real.

1.2 From traditional realism to structural realism

In his foundational 1989 article, John Worrall sought to steer a precarious course between the Scylla of the NMA and the Charybdis of the PMI, developing a form of realism sufficiently robust to respect the former while avoiding the direst implications of the latter. His *structural* realism (SR), the intellectual origins of which he credited to Poincaré (1905), restricts realist commitment to only the *relational structure* of our best scientific theories. It is not the family of *relata* themselves, Worrall alleges, but the system of relations into which they enter that should be the target of the realist's assent. Worrall observes that even in cases where the shift to a new theory is accompanied by a radical revision of basic ontological categories there is nevertheless significant retention of structure as captured in the respective theories' mathematical equations. For example, he shows that even though nothing like Fresnel's elastic ether figures in Maxwell's electromagnetic theory, Fresnel's equations can be readily recovered from Maxwell's by little more than term substitution (1989, p. 119). Significantly, it is a theory's relational structure, Worrall alleges, that is primarily responsible for the pragmatic successes that have so impressed scientific realists. If, therefore, structural retention is a sufficiently common feature of theory change, then realism—a deflationary realism to be sure, but one still adequate to the NMA—can be rescued from pessimism.

Worrall couched his structural realism within the so-called "syntactic" view of theories. On this account a theory's content is exhausted by a set of sentences or their equivalents in a formal (almost always first-order) language. The purely structural component of a theory, Worrall thought, was given by its *Ramsey sentence* (Worrall & Zahar, 2001). The Ramsey sentence of a given theory Θ may be constructed as follows. First, Θ is formalized in a first-order language: $\Theta(o_1,\ldots,o_m; t_1,\ldots,t_n)$, where the o_i are the observational terms and the t_j the purely theoretical terms (i.e., terms that refer to unobservables, such as electrons, fields, and spacetime points). All theoretical terms are then replaced by existentially quantified variables. The Ramsey sentence of Θ, then, would be:

$$Я\Theta = (\exists x_1),\ldots,(\exists x_n)\Theta(o_1,\ldots,o_m; x_1,\ldots,x_n)$$

While the formalized theory connects observational consequences with *particular* theoretical entities, the *Ramseyfied* theory states only that there are *some* entities responsible for particular observations within the phenomenal domain to which the theory applies.

Consider (a relevant part of) a physical theory describing the behavior of alpha particles. It would contain sentences such as the following: “An alpha particle consists of a pair of protons and a pair of neutrons”; “An alpha particle can usually be stopped by a sheet of paper”; “An alpha particle produces a wide, irregular track in a cloud chamber,” etc. The Ramseyfied theory would then state: “There is some *x*, some *y*, and some *z* such that: *x* consists of a pair of *y*s and a pair of *z*s; *x* can usually be stopped by a sheet of paper; *x* produces a wide, irregular track in a cloud chamber;” and so forth (“sheet of paper,” “cloud chamber,” and “track” are not replaced here because they are observational terms with respect to the theory in question). What the Ramsey sentence of a theory does, in effect, is capture the theory’s *relational structure* while remaining neutral as to the non-relational or intrinsic *natures* of the entities that collectively instantiate that structure. It appears, then, to be a promising locus of commitment for the structural realist (for more information on Ramsey sentences, see Carnap, 1958, and Ramsey, 1929).

The Ramsey sentence formalism has been adopted and endorsed by Grover Maxwell (e.g., 1970) and by Worrall and Zahar (2001) as the preferred way of explicating their own structuralist epistemic commitments. However, Ladyman (1998) has influentially challenged this approach by invoking a criticism first leveled by Max Newman (1928) and later recapitulated by Demopoulos and Friedman (1989) against an early precursor to structural realism briefly entertained by Russell (1927).

1.3 Russell's epistemic structuralism and Newman's objection

The structuralist view Russell developed in *The Analysis of Matter* proceeded not from a felt need for a more realistic scientific realism, but from more fundamental considerations stemming from the presumed impossibility of epistemic access to the first-order or intrinsic properties of individuals. Stathis Psillos (2001) has dubbed this philosophical trajectory the "upward path" to structural realism, in contrast with the "downward path" taken by Worrall of weakening standard scientific realism. Chief among the considerations motivating Russell were what Psillos (*Ibid*.) calls the *Helmholtz-Weyl Principle*:

> HW: Percepts supervene on stimuli. That is to say, if there are differences between percepts, then there are differences between the stimuli causative of those percepts; (Russell, 1927, pp. 226-227)

and what Ioannis Votsis (2004) calls the *Mirroring Relations Principle*:

> MR: The relations between percepts mirror the relations between their causative stimuli. (Russell, 1927, p. 252)

The upshot of the conjunction of HW and MR with basic empiricist principles, Russell contended, was that only *structural* knowledge of the world could be "validly infer[red]" from [the structure of] perception (*Ibid*., p. 254). It is the business of the empirical sciences to tease this structure out in a systematic way.

Newman (1928), however, argued that Russell's conclusion is incompatible with the intuitive and widely held notion that science reveals surprising facts about the world. The problem is that (1) relations overdetermine structure, and (2) HW and MR alone do not suffice to pick out the relations in the world that give rise to the structure of interest, viz., the structure mirrored in some focal subset of our percepts.

To see that (1) is the case, let A denote a system of objects with structure W given by some relation R. Newman then asks us to consider the following:

> A might be a random collection of people, and R the two-termed relation of being acquainted. A *map* of A can be made by making a dot on a piece of paper to represent each person, and joining with a line those pairs of dots which represent acquainted persons. Such a map is itself a system, B, having the same structure [W] as A, the generating relation, S, in this case being "joined by a line". …[I]t is not at all necessary for the objects composing A and B, nor the relations R and S, to be qualitatively similar. In fact to discuss the structure of the system A it is only necessary to know the *incidence* of R; its intrinsic qualities are quite irrelevant. (*Ibid*., p. 139, emphases in original)

Note as well that MR itself demands the overdetermination of structure by relation. Given only HW and MR, then, the most it seems we can say with regard to the putative cause of some particular ordering of percepts is that there exists *some* relation in the world that generates a structure that our percepts come to mirror. To say anything further would require more than just structural knowledge of the world. The problem is not merely one of fixing the correct extensional domain; even assuming this could be specified without breaching structuralist epistemic constraints, we would still be unable to specify the generating *relation*, for it follows from a theorem of set theory or second-order logic that the domain contains *every* relation of every arity compatible with the domain's cardinality (Demopoulos & Friedman, 1989).

It thus becomes a very nearly trivial matter whether for any perceptual aggregate P, a mirroring structure-in-extension exists, for any domain can be carved out of the world and organized under some relation or other to yield the required structure, provided only the domain has the same cardinality as P. As long as this condition is met, any inference from the structure of our percepts to the structure of the world will be valid. Hence, the only interesting questions open to science are those concerning the cardinality of the domains it investigates; everything else is given a priori.

1.4 Newman's objection generalized

Demopoulos and Friedman (1989) contend that the problem raised by Newman spells doom for the Ramsey sentence approach to structural realism, for it follows from the above that the Ramsey sentence *ЯΘ* of a theory *Θ* is guaranteed to be true so long as (1) *Θ* is consistent; and (2) *Θ* is empirically adequate, i.e., entails no false observation statements (see also Ketland, 2004). If the Ramsey sentence is taken to be the correct expression of the structural content of a theory, then it seems the structural realist's commitments do not go beyond those of the strict empiricist after all.

1.4.1 The semantic view of theories

According to Ladyman (1998), parrying Newman's objection requires a shift from a syntactic to a semantic or model-theoretic account of scientific theories. Ladyman particularly has in mind the approach of Giere (1988), according to which a theory is not a set of statements or formulae but a power set of possible models of shared structure, each model itself being a set containing at minimum a subset of entities (real or abstract), and a subset of relations defined thereon. Within such a framework, high-level theories are connected to the world via a hierarchy of model families terminating in models of experimental data (Suppes, 1962). Each model in a hierarchy is *directly presented* by its embedding theory, but the data models have the additional putative role—on a realist reading at least—of *re*presenting (via a suitably appropriate morphism) the phenomena the theory is ultimately supposed to be tracking.

One clear advantage the semantic approach has over the Ramsey sentence approach is that, qua family of *possible* models, a theory has a *modal* structure which goes beyond what the theory's Ramsey sentence alone can capture (Giere, 1985). This allows the structural realist to distinguish herself in her commitments from the strict empiricist and thus to carve out a unique position within the landscape of the scientific realism debate (Ladyman, 1998).

Additionally, though he never states it explicitly, Ladyman seems to hold that the direct presentation of a model by its theory suffices to fix the relations generating the structure of interest (French & Saatsi, 2005). Whether this is correct or not, it is difficult to see how this putative fact affords the structural realist any additional traction against Newman's objection. If the structure at issue is still set-theoretic structure, then, since structure of this sort, as we've seen, is always overdetermined by its generating relations, it makes little difference that the relations can be specified *in the model*; they will still be underspecified in any structurally isomorphic phenomenal domain obeying the cardinality and empirical adequacy constraints. Thus it remains a very nearly trivial truth that any model of any theory structurally represents (*some* slice or other of) the world.

A tempting gambit for the structural realist at this juncture might be to concede that very little indeed can be known about the world solely via deduction from perception but maintain that inferences to further properties—e.g., *particular* relational properties—can be justified by other means, say, the abductive force of the NMA. However satisfactory this reply may be vis-à-vis theory-world relations, the problem, unfortunately, merely resurfaces for structural realism in a slightly different guise.

1.4.2 Intertheoretic continuity and the problem of unintended models

The structural realist, qua *realist*, still owes the anti-realist a response to the PMI. The difficulty is that if theories are families of merely *possible* models, then structural equivalence between theories is *at least as cheap* as structural representation by a theory of some phenomenal domain. For any model family M_1 at t_0, there is at least one (and perhaps infinitely many) possible model family M_2 at t_1 and at least one (and perhaps infinitely many) possible model family M_3 at t_{-1} that are structurally equivalent. The diachronic structural continuity that the Worrallian structural realist requires in order to resist the PMI is guaranteed within the model-theoretic framework, but it no longer does the realist work demanded of it.

The Worrallian structural realist needs a way of ruling out "unintended models" (French & Saatsi, 2005), of fixing only the intended M_i in the desired successional relation. Here it may be thought that Ladyman's point about the direct presentation of models can purchase some traction, but it seems that even if we grant that relations can be specified in a model, the same cannot be said of the model's intended phenomenal domain—not, at least, if we are to refrain from going beyond purely structuralist commitments, since specifying the domain seems to require specifying the entities that enter into the modeled relations. French and Saatsi (2005) at this juncture gesture at "contextual information," but they are unsuitably vague as to how this information can be both strong enough to fix the domains of the models or theories in question and yet weak enough to permit the structuralist to avoid committing herself to any sort of de facto entity realism.

I think we can be more precise, and I think we can do so in a way that does not contravene structural realism's core "epistemic humility" (Saatsi, 2010). In order to see how, we must first take a look at a startlingly similar problem faced by proponents of a different intertheoretic/intermodel relation and the solution they developed in response.

CHAPTER II

2.1 Intertheoretic reduction: Nagel's account

Reduction has been interpreted as an epistemic relation, a metaphysical relation, a linguistic/semantic relation, or some combination of these. From the latter half of the 20th century onward, philosophers of science have tended to give it a principally epistemic reading. On this view, reduction is a relation between scientific theories or other similarly organized representations which enables the phenomena falling within the domain of one theory to be (at least in principle) predicted and explained using only the terms, equations, laws, and other resources of the more fundamental theory.

These *intertheoretic reductions* may be thought desirable for a number of reasons. Where successful, they simplify our ontology, constrain theorizing in useful ways, and bring the explanatory and predictive resources of each theory in the reductive relation to bear on the other. Where they are not fully successful but only approximative, they may help scientists hone in on problem areas in either theory which might otherwise have served as seed crystals for the accretion of ultimately catastrophic Kuhnian anomaly. They may even, as Dizadji-Bahmani, Frigg, and Hartmann (2011) have recently argued, enable novel confirmation to accrue to each theory in the reductive relation from the other theory's (formerly) independent evidential basis. In this manner, and *modulo* their strength and smoothness, reductions function, empirically, like a bonanza of confirmed predictions or concordant discoveries.

One of the earliest and most influential accounts of intertheoretic reduction was that presented by Ernst Nagel in *The Structure of Science* (1961). Let Θ_R denote the phenomenal theory (i.e., the theory putatively to be reduced) and Θ_F the fundamental or reducing theory. On Nagel's account, Θ_F reduces Θ_R just in case the laws of Θ_R can be derived from the conjunction of (1) the laws of Θ_F, (2) boundary conditions on Θ_F or other auxiliary assumptions, and (3) "bridge principles" connecting the theoretical terms of Θ_R to those of Θ_F. Reduction, on this account, is therefore a sort of *de*duction or translation, with the bridge principles specifying the term substitutions in Θ_F required to yield the laws or other empirical generalizations of Θ_R.

Problems with Nagel's account were quickly and widely recognized. For one, it did not seem to adequately capture much actual scientific practice. The derivability constraint was exceptionally strict, and most of what scientists generally recognized as straightforward cases of intertheoretic reduction fail to satisfy it. Even the vaunted reduction of classical thermodynamics to statistical mechanics falls somewhat short, for the laws of the former are deterministic, incapable of being derived, except as a limiting case, from the probabilistic laws of the latter. For two, it seemed tightly wedded to the syntactic view of theories, which was, even at the time of Nagel's writing, beginning to lose favor to the alternative, semantic view. It was unclear whether and how models, which are not sets of sentences as Nagel conceived theories to be and which appear to be the principal representational vehicles in many of the biological and social sciences, could enter into reductive relations in the Nagelian sense.

2.2 Intertheoretic reduction: Suppes' account

Patrick Suppes, one of the chief architects of the semantic view of theories, was also among the first to propose an account of reduction that could be situated within this new framework (1957; 1962). In his *Introduction to Logic* (1957), Suppes writes:

> To show in a sharp sense that thermodynamics may be reduced to statistical mechanics, we would need to axiomatize both disciplines by defining appropriate set theoretical

predicates, and then show that given any model T of thermodynamics we may find a model of statistical mechanics on the basis of which we may construct a model isomorphic to T. (p. 271)

Suppes does not provide a detailed formulation of isomorphism in his account. However, because he defines models set-theoretically—as do many proponents of the semantic view—we may say that two models μ_1 and μ_2 are isomorphic just in case there is a *bijection* of μ_1 onto μ_2, which is to say there is a function f: $\mu_1 \rightarrow \mu_2$ that maps every element $x \in \mu_1$ onto a unique element $y \in \mu_2$ (i.e., an injection), and a corresponding function f: $\mu_2 \rightarrow \mu_1$ that maps every element $y \in \mu_2$ onto a unique element $x \in \mu_1$.

We may then say that, on Suppes' account, a theory Θ_F reduces another theory Θ_R just in case for any model $\mu_R \in \Theta_R$, there is a model μ_F within Θ_F such that f: $\mu_R \rightarrow \mu_F$ is a bijection. Formally:

$$\rho_S(\Theta_F, \Theta_R) \Leftrightarrow [(\forall x)(\exists y)(\exists f)([\mu x \wedge (x \in \Theta_R)] \Rightarrow \{[\mu y \wedge (y \in \Theta_F)] \wedge [f\text{: } x \underset{bij}{\rightarrow} y]\})],$$

where the expression $\rho_S(\Theta_F, \Theta_R)$ is to be read: "Θ_F Suppes-reduces Θ_R."

2.3 Schaffner's objection

Kenneth Schaffner (1967) famously argued that Suppes' account of reduction was weaker than Nagel's, and in fact too weak to capture or justify, without augmentation, the reductions scientists claim to have successfully effected. Suppes-reductions were, Schaffner contended, necessary but not sufficient for proper scientific reductions. The problem is that isomorphism is a purely structural relation, and, as noted in the previous chapter, set-theoretic structure underspecifies both the relations and the relata that instantiate it. Indeed, a bijection obtains for any two sets of the same cardinality. Because of this, an isomorphic model for any model of the reduced theory is guaranteed to be found within the reducing theory provided only that the latter has a cardinality at least equal to that of the former. Suppes-reductions, then, are secured far too cheaply.

As a consequence, Schaffner noted, Suppes' account allows for spurious "reductions" between theories with formally similar presentations that are nevertheless not reductively related in any direct way. Schaffner's examples of choice to illustrate this point are hydrodynamics and "the theory of heat" (*Ibid*., p. 145). Concerning this particular theory pair, one might wish to contend that the formal similarity is due to both theories having a common reduction base in a third theory, such as fluid dynamics or statistical mechanics. However, Suppes' account, on its own, leaves us unable to designate which among these three theories are reducing and which are reduced.

More extreme examples abound in the large, heterogeneous field that has come to be called "econophysics," wherein models of energy exchange between various kinds of physical systems are used to predict and retrodict the flow and distribution of capital and other economic resources. For example, dyadic monetary exchanges and particle collisions are widely claimed to follow a conservation equation of the following general form:

$$\mathrm{a}_i(t_1) + \mathrm{a}_j(t_1) = \mathrm{a}_i(t_2) + \mathrm{a}_j(t_2)$$

where a_i and a_j denote the quantities of the exchanged medium possessed by the two exchanging agents (i.e., particles or economic actors), t_1 denotes the time immediately prior to the exchange, and t_2 the time immediately following the exchange. More complex systems have also been characterized by models isomorphic to those in the physical sciences. For example, Chatterjee and

Chakrabarti (2007) adapted models from the kinetic theory of gases to describe the behavior of the tails of income and wealth distribution curves, and Chen (2001) showed that the Cox-Ross-Rubinstein binomial options pricing formula:

$$C_0^N = (1 + \mathrm{r})^{-N} \sum_{n=0}^{N} \frac{N!}{n!(N-n)!} q^n (1 - q)^{N-n} [S_0(1 + \mathrm{b})^n (1 + \mathrm{a})^{N-n} - K]^+$$

was formally equivalent to a multi-step equation used in quantum mechanics to model probability distributions within disks of a Euclidian unit sphere:

$$C_0^N = \mathrm{tr}[(\otimes_{j=1}^{N} \rho_{\mathrm{j}})(S_N - K)^+]$$

2.4 Schaffner's objection as a version of Newman's objection

Reductions are typically thought of as synchronic relations between independently established theories in distinct research programs. Wimsatt (1974) famously drew philosophers' attention to the notion, seemingly already held at least implicitly by many scientists, that reductions could also be *diachronic* relations between theories ranging over phenomenal domains related by set inclusion or identity. These "successional" or "intra-level" reductions (*Ibid*., p. 675) are characterized by large-scale retention of theoretic structure (and, less commonly, content), and thus run contrary to the Kuhnian picture of violent overthrow by incommensurable successor theories. Widely recognized reductions of this sort include the reduction of Kepler's laws of planetary motion to Newtonian mechanics and (with some revision and, therefore, some controversy) of Newtonian mechanics in turn to relativistic mechanics.

Given the above, we may then recast the Worrallian structural realist's aim as the establishment of non-trivial successional reductions or, more broadly, as showing that a pattern of successional intertheoretic reduction predominates over a pattern of radical successional replacement throughout the history of science. The problem Schaffner identified with Suppes-reductions, then, can be considered of a piece with the problem Newman identified in Russell's early epistemological structuralist excursion. It is another instance of what we may call the *Generalized Newman Problem* (GNP), and what French and Saatsi (2005) call the "problem of unintended models." Indeed, the only constraint on both Suppes-reductions and Worrall-continuities appears to be the cardinality of the representational systems entering into the relation in question.

Because the core problem in each case follows from the specifically set-theoretic notion of structure, one option for avoiding it might be to opt for a characterization of models that does not rely on set theory. Elaine Landry (2007), for instance, has explored category-theoretic characterizations of models and morphisms for the specific purpose of heading off modern variants of the theory-world version Newman's challenge to structural realism. Another option might be to retain the set-theoretic foundation, but to supplement or augment the isomorphism relation with additional constraints sufficient to rule out spurious instances of the desired relation. A very detailed proposal of this sort, aimed squarely at the problem of adequately characterizing reduction within the semantic view of theories, was developed over many years by a group of philosophers working within the so-called "structuralist school." We now turn to their solution and to the question of whether a version of it can be safely adapted to the particular species of the GNP confronting the structural realist.

CHAPTER III

3.1 German structuralism: models and theories

Throughout the 1970s and 1980s, a small number of primarily German philosophers calling themselves "structuralists" (to be confused neither with Worrallian structural realists nor with Russellian structuralistic epistemologists) were tirelessly at work expanding and elaborating the Suppesian version of the semantic view of theories. Central to their project was a semantic reformulation of intertheoretic reduction that would avoid the sorts of spurious mappings that rendered Suppes' account intolerably weak. In general, the structuralists have retained Suppes' set-theoretic characterization of models, so their strategy has largely been to supplement model-model isomorphism with additional constraints such that reductions only obtain, if they do at all, between the desired theories.

The development of the structuralists' accounts of scientific models, theories, and their interrelations is lengthy and complex, and it will take us too far afield to recapitulate it in any great detail here (for competent overviews, see Pearce, 1982; Balzer, Moulines, & Sneed, 1987; Mormann, 1988; and Bickle, 1998, Ch. 3). In what follows, I will be drawing principally upon the distillation of the approach articulated in a recent paper by Moulines (2006) and on the extensive treatment Bickle develops in his 1998.

The structuralist characterization of a model is fair bit more detailed than that of Suppes. On the structuralists' understanding, a model μ of theory Θ is a set comprised of three distinct, disjoint proper subsets:

$$\mu = \langle D_1, \ldots D_m, [A_1, \ldots A_n], R_1, \ldots R_p \rangle$$

Here, the D_i denote the model's *base sets*, or the *kinds* of things in its phenomenal domain. Collectively, they constitute the model's ontological commitments. The A_j denote the model's *auxiliary base sets*, or the purely mathematical or abstract entities deployed in its formal apparatus (if applicable). The R_k are *relations* and magnitudes defined over elements in $D_i \cup A_j$ (Moulines, 2006).

A model μ of, say, classical collision mechanics (CCM) would be characterized as follows:

$$\mu_{\text{CCM}} = \langle P, T, \mathbb{R}, m, v \rangle, \text{ such that}$$

$$\sum_{\varphi \in P} m(\varphi) \cdot v(\varphi, t_1) = \sum_{\varphi \in P} m(\varphi) \cdot v(\varphi, t_2);$$

i.e., the law of the conservation of momentum is satisfied (Bickle, 1998, pp. 62-63).

Here, P is a finite, nonempty set (viz., of interacting particles), and T is an ordered-pair set $\langle t_1, t_2 \rangle$ of time instances. P and T constitute the model's base sets. $\mathbb{R}$, the set of real numbers, is the model's auxiliary base set. m is the magnitude *mass*, which assigns to each $\varphi \in P$ a positive real number, and v is the relation *velocity*, which maps onto certain elements in the Cartesian product $P \times T$ (i.e., particles at particular time instances) an ordered triple of real numbers.

However, a theory or model family is not, on the structuralist construal, exhaustively comprised of its *actual* models but of the typically much larger set of its *potential* models. Let us denote this set M_{P} and its (seemingly always proper) subset of actual models M_{A}. A potential model of a theory is a model with the same formal composition as the theory's actual models but about which it cannot yet be said that it satisfies the theory's fundamental law(s). A theory also contains a proper subset of *intended empirical applications*; let us denote this set I. The totality of a theory's *empirical assertions*, Bickle notes, amounts to the claim that its empirical applications are a subset of its actual models (*Ibid*., p. 63). In reality, this is rarely, if ever, the case. Typically, a theory will only contain a nonempty intersection $I \cap M_A$ consisting of its *confirmed* empirical applications (i.e., its confirmed actual models). Let us denote this I_C. On a structuralist construal, then, for any theory that is not radically false, the following generalizations hold (*Ibid*., pp. 63-64):

(A) $M_A \subseteq M_P$ (and usually $M_A \subsetneq M_P$)

(B) $I \subsetneq \mathrm{M_P}$

(C) $I \nsubseteq M_A$

(D) $I \cap M_A \neq \emptyset$ (= I_C)

These may be represented graphically by the following Euler diagram (Fig. 3-1):

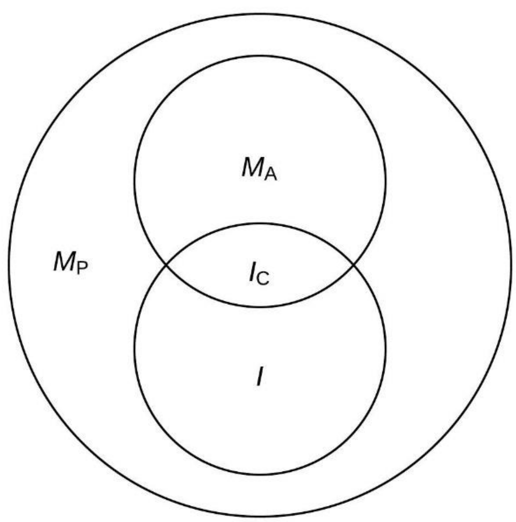

Figure 3-1 Theoretic structure as conceived by the German structuralists
A theory is described as a set of potential models (M_P) containing as proper subsets both the theory's actual models (M_A) and its intended empirical applications (I). The intersection $M_A \cap I$ contains the theory's *confirmed* empirical applications (I_C). Adapted from Bickle, 1998.

3.2 Intertheoretic reduction: The structuralist account

Suppes' account of reduction does not advert to this finer-grained general theory structure, so it is not immediately clear which parts of a theory thus conceived must be bijectively mapped in order for a reduction to obtain. According to Suppes, recall, a theory, Θ_{F} reduces another theory Θ_{R} just in case for "any model [μ_{R} of Θ_{R}] we may find a model of [Θ_{F}] on the basis of which we may construct a model isomorphic to [μ_{R}]" (1957, p. 271).

In most if not all historical cases of reduction, be they synchronic or diachronic, the reducing theory has a broader phenomenal scope than the reduced. Indeed, it would perhaps be impossible to effect a reduction—a smooth, largely retentive one, at any rate—without the reducing theory ranging over at least all the phenomena subsumed under the reduced theory. Let us then characterize the *reduction base, B* of Θ_{R} as a subset within $M_{\mathrm{P}}(\Theta_{\mathrm{F}})$ having possible intersections with $M_{\mathrm{A}}(\Theta_{\mathrm{F}})$, $I(\Theta_{\mathrm{F}})$, and $I_{\mathrm{C}}(\Theta_{\mathrm{F}})$[1]. In Suppesian terms, we may think of B as the set of all constructible models of Θ_{F} that are isomorphic to models in some appropriate subset of $M_{\mathrm{P}}(\Theta_{\mathrm{R}})$. This subset, put another way, is the *range* of the reduction, while B is its *domain*. Suppes' account, unfortunately, does not sufficiently specify the range of his proposed reduction relation. Should "any model [μ_{R} of Θ_{R}]" be interpreted as any *potential* model of Θ_{R}, such that $\mathrm{Rng}(\rho_{\mathrm{S}}) = M_{\mathrm{P}}(\Theta_{\mathrm{R}})$, as any *actual* model of Θ_{R}, such that $\mathrm{Rng}(\rho_{\mathrm{S}}) = M_{\mathrm{A}}(\Theta_{\mathrm{R}})$, or as any *empirical application* of Θ_{R}, either merely *intended* or *confirmed*, such that $\mathrm{Rng}(\rho_{\mathrm{S}}) = I(\Theta_{\mathrm{R}})$ or $I_{\mathrm{C}}(\Theta_{\mathrm{R}})$, respectively? Fig. 3-2 visually represents each of these four possibilities.

[1] What I am here designating B bears some resemblance to the "analog structure" T_{R}* in Bickle's (1998) reconstruction of the structuralist account of reduction. With T_{R}*, Bickle was seeking to incorporate into the structuralist framework Clifford Hooker's (1981) notion that what in fact often gets reduced in a successful reduction is not Θ_{R} but a theoretic analog having the same scope as Θ_{R} but constructed wholly from the theoretical vocabulary of Θ_{F}. *B*, as I am here using it, serves the more modest role of the domain of the reduction in Θ_{F} following the application of Nagel-style boundary conditions.

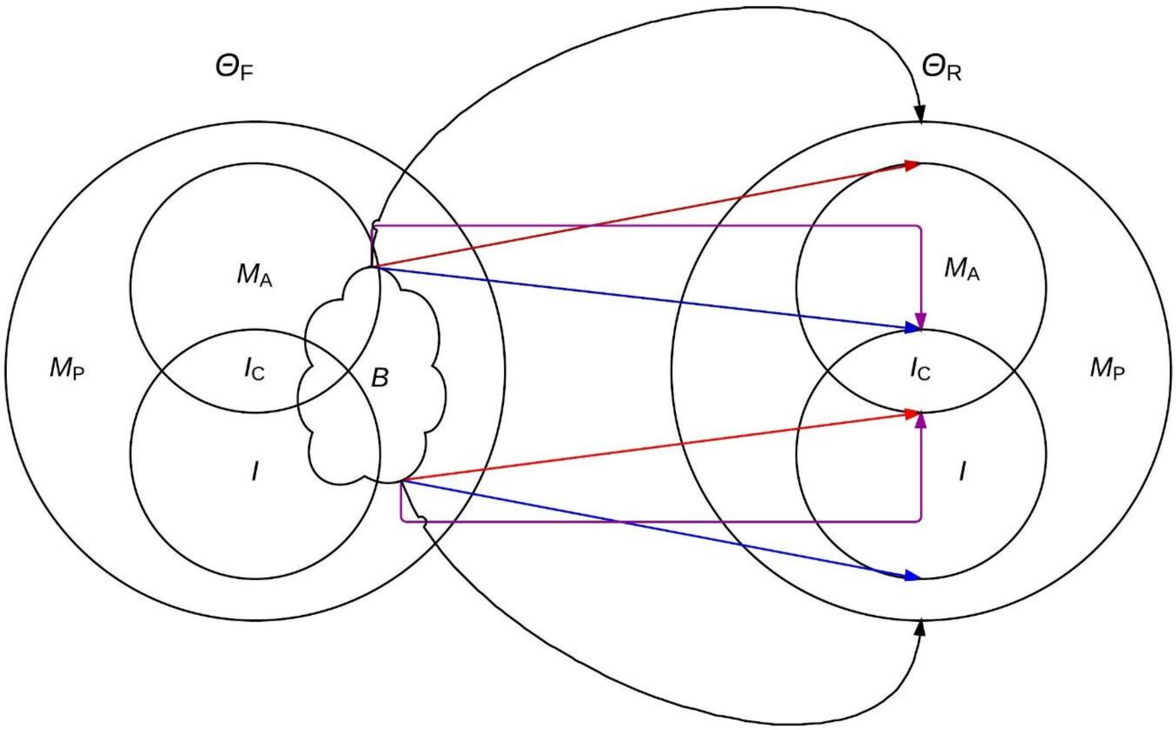

Figure 3-2 Possible structuralist interpretations of Suppesian reduction
Red: Dom(ρ_S) = $B(\Theta_F)$; Rng(ρ_S) = $M_A(\Theta_R)$. Blue: Dom(ρ_S) = $B(\Theta_F)$; Rng(ρ_S) = $I(\Theta_R)$.
Purple: Dom(ρ_S) = $B(\Theta_F)$; Rng(ρ_S) = $I_C(\Theta_R)$. Black: Dom(ρ_S) = $B(\Theta_F)$; Rng(ρ_S) = $M_P(\Theta_R)$.

3.2.1 Additional formal constraints on Suppesian reduction

Bickle (1998), following Balzer, Moulines, and Sneed (1987, Ch. 6), suggests that the appropriate range may in fact be as heterogeneous as the domain, pursuant to two conditions. Adapting and formalizing Bickle's treatment, these may be stated as follows:

(1) $(\forall z)(\forall w)(\{[(\mu z \wedge \{z \in [B(\Theta_F) \cap M_A(\Theta_F)]\}) \wedge \{\mu w \wedge [w \in M_P(\Theta_R)]\}] \wedge (\langle z, w\rangle \in \rho_S)]\} \Rightarrow [w \in M_A(\Theta_R)])$

(2) $(\forall w)(\exists z)\{[\{\mu w \wedge [w \in I_C(\Theta_R)]\} \wedge (\langle z, w\rangle \in \rho_S)] \Rightarrow (\mu z \wedge \{z \in [B(\Theta_F) \cap I_C(\Theta_F)]\})\}$

The first condition, simply put, states that a successful reduction must relate any *actual* models of Θ_F falling within its domain only to *actual* models of Θ_R. This seems reasonable once one recalls that the merely potential models of a theory do not necessarily satisfy its fundamental laws. In fact, Bickle (*Ibid*., pp. 68-69) interprets this condition as an attempt by the structuralists to capture the essence of Nagel's (1961) requirement that the laws of the reduced theory be derivable from those of the reducing theory. The second condition demands that a successful reduction relate every *confirmed* intended application of the reduced theory to some *confirmed* intended application of the reducing theory. This also makes sense, for it would be odd to regard as successful or complete any putative reduction that left any confirmed actual models of the reduced theory without isomorphs in the reducing theory. Fig. 3-3 represents these two conditions by illustrating all permissible isomorphism pairings on the above account.

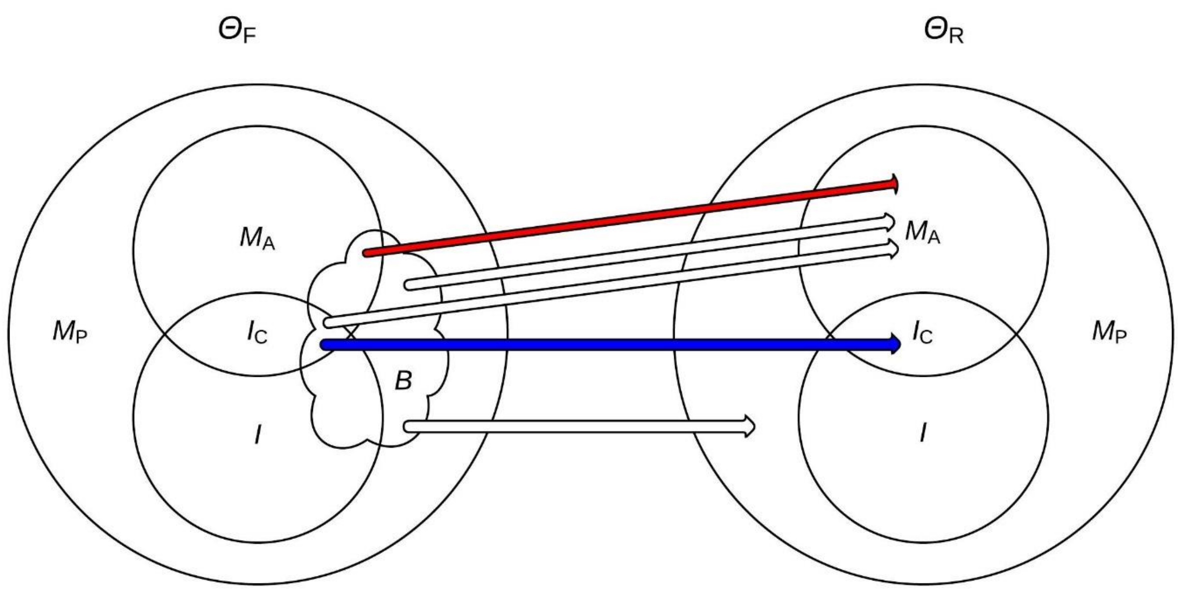

Figure 3-3 Permissible isomorph pairs in a structuralist reduction
The red arrow follows Condition (1) above, while the blue arrow follows Condition (2). White arrows indicate other permissible pairings. Adapted from Bickle, 1998.

These two conditions serve to constrain Suppes' account of reduction considerably, but they do not on their own defuse the Newman-style objection leveled against that account by Schaffner. The problem Schaffner identified concerns not the possible domains and ranges of reduction, but the purely formal character of the individual inter-model relations (viz., isomorphisms) of which reductions are composed.

3.2.2 Ontological Reductive Links

To address this issue, Moulines (1984) introduced the concept of an *Ontological Reductive Link* (ORL) as an additional, semiformal constraint on any putative reduction. Recall the structuralist definition of a model given in §3.1. Let us consider a superset O_R containing the empirical base sets of all models in the range of a reduction; this O_R comprises the reduced theory's *ontology*. Let us then group *all* intra-model subsets (i.e., empirical base sets, auxiliary base sets, and relations) in the domain of the reduction under the superset E_F. An ORL between Θ_R and Θ_F obtains when every member of O_R can be identified with either an atomic element or a nonempty subset of E_F. ORLs, in effect, show that (and in a limited sense how) the ontology of the reduced theory is *realized* by elements in the phenomenal domain of the reducing theory.

ORLs may be *homogeneous*, *heterogeneous*, or *mixed*. In a homogeneous ORL, each empirical base set in O_R is either identified with exactly one of the empirical base sets in E_F or is a proper subset of an empirical base set in E_F, such that $O_R \subseteq O_F \subsetneq E_F$ (i.e., the reducing theory's ontology is itself a superset of O_R). This kind of ORL, Bickle (1998, p. 78) notes, characterizes the reduction of classical collision mechanics to Newtonian particle mechanics; the particles that figure in the former simply *are* Newtonian particles. In a heterogeneous ORL, on the other hand, each empirical base set in O_R is connected to a subset in E_F of > 1 members which may include empirical base sets, auxiliary base sets, or relations. Lastly, a mixed ORL is both homogeneous and heterogeneous, with some empirical base sets in O_R connected to individual empirical base sets in E_F, and some connected to combinations of empirical base sets, auxiliary base sets, and relations in E_F. Heterogeneous or mixed ORLs will be characteristic of any mereological or inter-level reductions, wherein the ontology of the reduced theory is composed of combinations of elements of the reducing theory.

In his 2006, Moulines subsumes all three types of ORLs under a general characterization using the concept of an *echelon*. As he puts it: "A set A is an echelon-set over sets $B_1, \ldots, B_n$ iff A comes out of $B_1, \ldots, B_n$ by successively applying the set-theoretical operations of power-set formation and Cartesian product to $B_1, \ldots, B_n$" (p. 319). Put more simply, to say that an ORL obtains between O_R and E_F is to say that O_R is an echelon on E_F, which is to say that O_R is *constructible* out of E_F.

This notion of constructibility may be stated more precisely and formally. Individual models, recall, are composed of two to three disjoint proper subsets: the superset of its empirical base sets, the superset of its auxiliary base sets (if any), and the superset of relations defined on subsets of the union of its empirical and auxiliary base sets. We may regard E_F as itself a single large model and divide it likewise. We have already carved off from it a proper subset of empirical base sets appearing in any model within the domain of the reduction, O_F. Let us additionally denote with A_F the proper subset of all auxiliary base sets appearing in any model $\in \mathrm{Dom}(\rho_S)$ and with R_F the proper subset of all relations appearing in any model $\in \mathrm{Dom}(\rho_S)$. To say that O_R is an echelon on E_F $(=\langle O_F, A_F, R_F\rangle)$, then, is to say that:

$$O_R \in \wp \ldots \wp([\wp]O_F \times [\wp]A_F \times [\wp]R_F),$$

where [$\wp$] denotes the merely possible application of the power-set operation, and $\wp \ldots \wp$ indicates arbitrarily many such applications (after Moulines, 2006, p. 320). Reworked a bit, this yields our third constraint on Suppesian reduction:

(3) $(\forall v)[(v \in O_R) \Rightarrow \{v \in \wp \ldots \wp([\wp]O_F \times [\wp]A_F \times [\wp]R_F)\}]$

3.3 Schaffner's objection defanged

The identifications or other local connections that secure an ORL are the deliverances of extra-formal judgments determined principally by the intended empirical applications of the theories standing in the putative reduction relation. Indeed, ORLs are only possible if there is substantial overlap between $I(\Theta_F)$ and $I(\Theta_R)$. Thus, Bickle notes (1998, p. 81), one cannot effect spurious identifications between elements of O_R and E_F without thereby altering the intended empirical applications of at least one of the theories in question. The intended empirical applications also enable us to determine (albeit fallibly) which of a pair of theories satisfying the formal conditions on a structuralist reduction is the reducing theory and which is the reduced. With very few exceptions, it seems, $\{I(\Theta_F) \setminus [I(\Theta_F) \cap I(\Theta_R)]\} > \{I(\Theta_R) \setminus [I(\Theta_F) \cap I(\Theta_R)]\}$.

ORLs, thus, enable us to distinguish legitimate reductions from the kinds of pseudo-reductions that troubled Schaffner. We have noted already that classical collision mechanics bears a homogeneous ORL to Newtonian particle mechanics. This is because the former, simply put, is a part of the latter; its models are all models of Newtonian particles mechanics as well. Let us now examine a more complex case: the reduction of rigid body dynamics (RBD) to Newtonian particle mechanics (NPM). The ontology of rigid body dynamics (O_{RBD}) contains an empirical base set C, the set of rigid bodies. While C is neither identical to nor a proper subset of P, the set of Newtonian particles in E_{NPM}, every element in C can be identified with some construct of empirical base sets, auxiliary base sets, and relations in E_{NPM}. (after Bickle, 1998, p. 80). That is to say, every element in *C is a system of* Newtonian particles having certain masses, positions, velocity vectors, force vectors, and so on. These identifications are permitted by the large overlap between the intended empirical applications of RBD and NPM, and they enable the derivation of RBD's equations of motion (i.e., Euler's laws) from Newton's laws of motion.

Compare this to the spurious "reduction" between exchange microeconomics (EME) and classical collision mechanics (CCM) discussed in §2.3. It might seem at first blush as though a heterogeneous (or mixed) ORL obtains here as well, since economic actors (i.e., elements of the empirical base set $Æ \in O_{\mathrm{EME}}$), like rigid bodies, are composed of particles. It beggars belief, though, to suppose that anything like a full *identification* of any element of Æ could be effected using *only* elements of the models of CCM (viz., $\langle P, T, \mathbb{R}, m, v\rangle$). Both the ontology of CCM (particles and time instances) and its fundamental relations (mass and velocity) are insufficient for the task, even given arbitrarily many power-set and Cartesian product applications. We could, of course, expand the family of models in the putative reduction base to include as well those of Newtonian particle mechanics (since CCM is connected to NPM via a homogeneous ORL) or, more controversially, quantum mechanics, adding to our basal ontology such further elements as states, wavefunctions, and fields and to the superset of basal relations spin, charge, and many others. The problem with this strategy is that the resulting reduction base would now satisfy many more laws, the vast majority of which will not have structural analogues in EME. The global ismorphism between the two theories, then, would be lost.

Further, even if an identification or some other suitably strong ontic connection could be established between economic actors and systems of particles specifiable using only the resources of CCM, this would not provide evidence of any overlap in the intended empirical applications of the two theories. It is not (merely or directly) in virtue of being composed of particles that economic actors follow a law analogous to that of the conservation of momentum. That is to say that the putative mereological connection between the two ontologies would not permit *explanations* of the behavior of economic actors couched solely in terms CCM in the way that the identification of rigid bodies with systems of Newtonian particles permits explanations of the behavior of rigid bodies couched solely in terms of NPM. The ontological link, if such there be, would not be an Ontological *Reductive* Link. The intended empirical applications are simply far too disjoint.

3.4 ORLs and structural realism

If ORLs can be used to overcome Schaffner's version of the Newman Problem, can they likewise be deployed against the version faced by the structural realist? One might initially suspect that the structural realist is barred from availing herself of ORLs, given that her epistemic commitments cannot by definition go beyond the structure of a theory and that structure's generating relations. Recall, however, that the elements of O_R are not individual objects but *kinds*[2]. I believe a structural realist may advert to these elements if they are conceptualized under a non-essentialist reading of kinds, subject to an important constraint.

[2] Moulines resists referring to the kinds connected in an ORL as natural kinds, claiming he has "never found a convincing, clear-cut criterion to distinguish natural from non-natural kinds…" (2006, p. 315). I will follow suit here, since I suspect a good number of the objects of present scientific study (particularly in the social or other special sciences) do not qualify as natural kinds under most definitions presently on offer. I will therefore omit any mention in what follows of Boyd's (1991) homeostatic causal mechanisms or similar such requirements property-cluster kinds are commonly regarded as needing to satisfy in order to qualify as natural kinds.

3.4.1 ORL anchors as (relational) property-cluster kinds

On property-cluster accounts, such as those developed by Boyd (1991) and Millikan (1999), membership in a kind is determined not by the satisfaction of certain necessary or sufficient conditions, but by the possession of a number of properties shared among other members of the kind. Approaches may here diverge, with some treating kind membership as bivalently conditional upon the possession of a minimal number of the shared properties and others treating kinds like *fuzzy sets* admitting of graded membership according to the number of shared properties possessed. Either view seems acceptable as far as the structural realist's aims go. What is crucial for her is that she appeal exclusively to *relational* properties in her determinations of kind membership; any putatively *intrinsic* properties are verboten.

Are relational properties alone sufficient to delimit (however fuzzily) a scientifically interesting kind? In the vast majority of cases, I suspect so. All so-called "functional kinds"—common in the ontologies of the biological and social sciences—are already specified relationally. Even many fundamental physical properties admit of relational characterization. The mass of a body, for example, may be given by the degree to which it deforms its surrounding spacetime. The charge of a particle may be known by the amounts of attractive and repulsive forces it exerts on other particles. In fact, any *measurable* property admits of a minimal relational characterization of the form: "…induces state x in instrument y."[3]

Kinds, then, on a structural realist reconstruction, are clusters of relational properties—hubs, we might say, of structure-generating relations. To show that a kind K in O_R is connected

[3] A theory-wide application of the relational reconstruction of kinds may be reckoned as a sort of model-theoretic analogue to the Ramseyfication procedure discussed in §1.2.

to some construct of E_F in an ORL, then, one would need to show that the construct is capable of instantiating all (or at least some requisite percentage) of the relations by which K is picked out. This determination will be a largely empirical matter, facilitated to the extent that the intended applications of the two theories in which K and the construct figure overlap.

3.4.2 The problem of unintended models solved

For any predecessor/successor theory pair for which the structural realist wishes to claim continuity, then, she must demonstrate two things: (1) that a Suppes-style reduction obtains between the two theories, subject to the additional formal constraints discussed in §***3.2.1***; and (2) that an ORL obtains between the two theories. Traditional Worrallian structural realism goes only as far as (1), leaving it vulnerable, as we've seen, to the Newman-esque "problem of unintended models" (French & Saatsi, 2005). The existence of an ORL shows that the structural continuity between the theories is more than a trivial artifact of their cardinality and set-theoretic conceptualization. It shows that the theories are continuous because they pick out common relations (ensured by the local substitutions of which the ORL is comprised) in a common phenomenal domain (ensured by the theories' overlapping intended empirical applications).

It might be tempting to suppose that the incommensurability thesis on which the PMI rests amounts to just the denial that ORLs tend to obtain interparadigmatically, and that, therefore, the structural realist adopting the above strategy is doing little more than begging the question against the Laudanian anti-realist. Consider Worrall's preferred interparadigm theory pair: Fresnel's optics and Maxwell's electromagnetism. Does not the abolition of an elastic mechanical ether in the latter suggest that no ORL obtains between them? Not, I think, on the relational property-cluster account of kinds given above. Worrall writes:

> [F]rom [the vantage point of Maxwell's theory], Fresnel's theory has exactly the right structure—it's "just" that what vibrates according to Maxwell's theory, are the electric and magnetic field strengths. And in fact if we interpret *I*, *R*, *X etc*. [i.e., the intensities of the various components of unpolarized light] as the amplitudes of the "vibration" of the relevant electric vectors, then Fresnel's equations are directly and fully entailed by Maxwell's theory. (1989, p. 119)

This is to say, in the terms of the German structuralists, that what was really doing the work in Fresnel's theory—what was anchoring all the essential structure-generating relations—was some construct composed of electromagnetic fields, real numbers, and various relations assigning real numbers to those fields and to changes in the real numbers assigned to those fields. Does this mean that the ether has been, in some substantive sense, *preserved* under a new definition (given by Maxwell's theory)? No; at least, not on a scale beyond the intended applications of Fresnel's optics. As a metaphysical background assumption and as an element in the ontology of many other theories cotemporaneous with Fresnel's, the ether had a great deal of content beyond what Fresnel's theory alone assigned to it. In what we may call its supratheoretical totality, this ether is rightly now regarded as a fiction. But the subset of its relational properties on which Fresnel's equations depended for their empirical adequacy may be regarded as preserved in the form of the above-described construct in Maxwell's theory. For this reason, the structural realist may regard Fresnel's more minimal "ether" as ontologically linked to Maxwell's theory while maintaining an appropriately eliminitivistic stance toward ether in the broader, supratheoretical sense.

The above concern aside, it will likely prove desirable to have an account of partial or approximate ORLs for cases in which not every kind in the ontology of the predecessor theory is preserved in the strict sense of having *every* putative relational property instantiated by some construct of the successor theory. This will be the case whenever the successor theory *corrects* the predecessor theory to some extent. Perfectly smooth Nagelian reductions are, in fact, the rare exception in science; most recognized reductions, whether synchronic or diachronic, are at least somewhat revisionary.

This fact may complicate the structural realist's response to the Laudanian anti-realist, for she will need to modulate her credence in any extant theories in proportion to the completeness of the ORLs in which they enter[4]. Two broad strategies suggest themselves: (1) The structural realist may attempt to defend a *global* scientific realism in the spirit of the definition given in §1.1 by showing that scientific progression is better characterized by a pattern of significant ORL-mediated intertheoretic continuity than by a pattern of radical elimination and replacement; (2) Alternatively, she may assess the relative prevalence of the two patterns more locally—at the level of individual scientific fields, or even individual theory "lineages"—and tailor her realist commitments accordingly. This may ultimately require her to adopt anti-realist stances toward particular theories and even whole sciences, but as long as a pattern of continuity prevails in *some* empirical domains, she can resist the *global* anti-realism toward which the PMI, in its strongest form, is supposed to compel her.

[4] For this and for other reasons, I suspect a reconstruction of the structuralist account of reduction within fuzzy set theory would be immensely helpful, though that is a project for another day.

CONCLUSIONS

We have seen that the problem Newman identified in Russell's structural realism, its intertheoretic parallel in Worrall's structural realism, and the problem Schaffner identified in Suppes' model-theoretic account of reduction are all species of a common genus: what I have been calling the Generalized Newman Problem. Underlying each is the core difficulty that, where models and the theories they compose are characterized purely set-theoretically, their structure will be underspecified by their elements. While some philosophers cognizant of this problem have urged a shift to a different formal framework within which to characterize science's representational vehicles (Landry, 2007; see also van Fraassen, 1972), I have attempted to show that the problem may be obviated from within a set-theoretic framework by supplementing the purely formal relation of inter-model isomorphism with the semiformal Ontological Reductive Links developed by the German structuralists.

ORLs do not constitute a silver bullet against all anti-realist worries. They do, however, furnish a framework within which the structural realist can provide a variety of nuanced responses to the PMI while avoiding Newmanesque charges of triviality.[5] In §***1.4.2***, we noted that French and Saatsi (2005) attempt to circumvent these charges by appealing to "contextual information." They argue that no science has ever given a *purely* structural presentation of any theory but always a presentation couched within a particular language having determinate referents for competent speakers of that language. I found this appeal unsatisfactorily vague, but I believe the

[5] It should not escape notice that the benefits of ORLs are available to the traditional scientific realist as well. The Worrallian structural realist may therefore be reluctant to endorse their use, for such an act would seem to undercut the "downward path" to structural realism (Psillos, 2001) and weaken its case against traditional realism even while fortifying it against their common Laudanian nemesis. I do not find this an especially worrisome bullet to bite, for I think structural realism's principal advantage over traditional realism has always been, as Russell rightly glimpsed, its greater epistemic humility.

application of ORLs to the SR version of the GNP may permit us to say in a more precise way what this contextual information consists in. What are crucial for fixing the correct models in the relevant structural morphisms are the intended empirical applications of the theories in which the models figure. These applications specify the relations and relational properties by which the empirical base sets in each model are picked out. They thus ensure that two isomorphic models in a predecessor/successor theory pair are not just structurally continuous, but “ontologically” (qua relationally) continuous as well.

REFERENCES

Balzer, W., Moulines, C. U., & Sneed, J. (1987). *An architectonic for science*. Dordrecht: Reidel.

Bickle, J. (1998). *Psychoneural reduction: The new wave*. Cambridge, MA: The MIT Press.

Boyd, R. (1991). Realism, anti-foundationalism and the enthusiasm for natural kinds. *Philosophical Studies, 61*, 127-148.

Brading, K. & Landry, E. (2007). Shared structure and scientific structuralism. *Philosophy of Science (Proceedings), 73*, 571-581.

Carnap, R. (1958). Observation language and theoretical language. In J. Hintikka (Ed.), *Rudolf Carnap, Logical Empiricist* (1975), Dordrecht: Reidel. Translation of Beobachtungssprache und theoretische sprache. *Dialectica 12*, 236–248.

Chatterjee, A. & Chakrabarti, B. K. (2007). Kinetic exchange models for income and wealth distribution. *The European Physical Journal B, 60*, 135-149.

Chen, Z. (2001). Quantum theory for the binomial model in finance theory. *arXiv preprint quant-ph/0112156*.

Demopoulos, W. & Friedman, M. (1985 [1989]). Critical notice: Bertrand Russell's *The analysis of matter*: Its historical context and contemporary interest. *Philosophy of Science, 52,* 621-639.

Dizadji-Bahmani, F., Frigg, R., Hartmann, S. (2011). Confirmation and reduction: A Bayesian account. *Synthese, 179*(2), 321-333.

French, S. & Saatsi, J. (2005). Realism about structure: The semantic view and non-linguistic representations. *Philosophy of Science, Supplement: Proceedings of PSA 2004*.

Giere, R. N. (1988). *Explaining science: A cognitive approach*. Chicago and London: University of Chicago Press.

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

Hooker, C.A. (1981). Towards a general theory of reduction. *Dialogue* 20, 35–59, 201–236, 496–529.

Kuhn, T. S. (1996/1962). *The structure of scientific revolutions* (3rd ed.). Chicago: University of Chicago Press.

Ladyman, J. (1998). What is structural realism? *Stud. Hist. Phil. Sci., 29*(3), 409-424.

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

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

Maxwell, G. (1970). 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, volume IV*, pp. 181-192. Minneapolis: University of Minnesota Press.

Mayr, D. (1976). Investigations of the concept of reduction, I. *Erkenntnis, 10,* 275-294.

Millikan, R. G. (1999). Historical kinds and the special sciences. *Philosophical Studies, 95*, 45-65.

Mormann, T. (1988). Structuralist reduction concepts as structure-preserving maps. *Synthese, 77*(2), 215-250.

Moulines, C. U. (2006). Ontology, reduction, emergence: A general frame. *Synthese, 151*, 313-323.

Moulines, C. U. (1984). Ontological reduction in the natural sciences. In W. Balzer, D. Pearce, and H. J. Schmidt (Eds.), *Reduction in science*, 51-70. Dordrecht: Reidel.

Nagel, E. (1961). *The structure of science*. London: Routledge and Keagan Paul.

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

Pearce, D. (1982). Logical properties of the structuralist concept of reduction. *Erkenntnis, 18*, 307-333.

Psillos, S. (2001). Is structural realism possible? *Philosophy of Science, 36*(Supp. Vol.), S13-S24.

Putnam, H. (1975). *Mathematics, matter and method*. Cambridge: Cambridge University Press.

Ramsey, F. P. (1929). Theories. In R. B. Braithwaite (Ed.), *The foundations of mathematics and other logical essays*, pp. 212-236. Paterson, NJ: Littlefield and Adams.

Russell, B. (1927). *The analysis of matter*. London: Routledge Kegan Paul.

Saatsi, J. (2010). Whence ontological structural realism? In M. Suárez, M. Dorato, & M. Rédei (Eds.), *EPSA epistemology & methodology of science*, pp. 255-265. Springer Netherlands.

Schaffner, K. F. (1967). Approaches to reduction. *Philosophy of Science, 34*, 137-147.

Sneed, J. (1971). *The logical structure of mathematical physics*. Dordrecht: Reidel.

Stegmüller, W. (1976). *The structure and dynamics of theories*. Berlin: Springer-Verlag.

Suppes, P. (1962). Models of data. In E. Nagel, P. Suppes, and A. Tarski (eds.), *Logic, methodology and philosophy of science: Proceedings of the 1960 international congress*, 252–261. Stanford: Stanford University Press.

Suppes, P. (1957). *Introduction to logic*. Princeton: D. Van Nostrand Co., Inc.

van Fraassen, B. C. (2007). Structuralism(s) about science: Some common problems. *Aristotelian Society Supplemental Volume, 81*(1), 45-61.

van Fraassen, B. C. (1997). Structure and perspective: Philosophical perplexity and paradox. In M. e. A. Dalla Chiara (Ed.), *Logic and Scientific Methods*, 511-530. Dordrecht: Kluwer.

van Fraassen, B. C. (1980). *The scientific image*. Oxford: Oxford University Press.

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.

Wimsatt, W. C. (1974). Reductive explanation: A functional account. In R. S. Cohen, et al. (Eds.), *Philosophy of Science Association Proceedings*. Dordrecht: Reidel.

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

Worrall, J. & Zahar, E. (2001). Ramsification and structural realism. Appendix in E. Zahar, *Poincaré's philosophy: From conventionalism to phenomenology*, pp. 236-251. Chicago: Open Court.