Microfoundations in Strategy and Explanatory Reductionism

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Abstract

The history of research on strategy has been dominated to the point of exclusivity by holistic conceptual models and empirical studies of firm-level means and ends. In this paper we examine the call for the development of microfoundations for strategic management research within the broader call for microfoundations in organization theory (Abell, Felin, and Foss 2008; Greve 2013). We argue that the project of establishing microfoundations for strategy research requires a thorough understanding of the mainstream philosophy of science approach to reductionism. We discuss three issues from the philosophy of science that should govern further methodological development of the microfoundations project. First, there are limits to mechanistic explanations, including the non-decomposability of strategy systems in complex organizations. Second, proper arguments for and against explanatory reduction center around concepts that have appropriate empirical confirmation, which we find lacking for some conceptual constructs in the microfoundations project, including routines. Finally, we examine the issue of the multiple realizability of micro-level explanations for macro-level phenomena. Once we have argued for the value of debating explanatory reductionism, our thesis is ecumenical: the presence of micro-level explanations does not render macro-level explanations dispensable. Whether we ought to prefer micro-level or macro-level explanations depends on the *explanandum*.

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From its inception as a field of study within the domain of management scholarship, strategic management has focused on decision processes and outcomes at the firm level. The nascent field was dominated by the planning or "design"school, wherein detailed top-down cascades of objectives and strategies were elaborated (Mintzberg 1994; Mintzberg, Ahlstrand and Lampel 1998). Later schools of strategy involving positioning, corporate portfolio management, resource-based and re-configuration have been likewise focused on decisions at the top of strategic business units, conglomerates, and firms. We see this as a holistic approach to management, particularly strategic management, where behavior is observed at the firm-level, as are outcomes. The antecedents of firm level growth, profits, and other outcome measures are strategy choices made in the strategic apex of the organigram. There has been virtually no attention paid to processes at the lower levels of the organigram; that is, there are no microfoundations to the corpus of strategic management.

There is a movement among some management scholars to establish microfoundations to strategic management and the research on it. (cf. Felin and Foss 2005; Foss and Lindenberg 2013; Felin and Barney 2013) This effort may be seen as interior to a larger project on microfoundations to organization theory and evolutionary economics (Abell, Felin, and Foss 2008; Gavetti 2005; Vromen 2006). One might characterize the protagonists in this movement in two ways: microfoundationalists and microfundamentalists. The former acknowledge the existence and supplementary value of macro- or firm-level explanations of strategic outcomes, while expressing clear preference for micro-level explanations of strategic outcomes. The latter believe that micro-level explanations render macro-level explanations dispensable. Both genres see themselves as presenting a superior research program to the received canon, at the ontological and methodological levels.

We argue that the project of establishing microfoundations for strategy research requires a thorough understanding of the mainstream philosophy of science approach to reductionism, where the central issue concerns not mysterious appeals to holism but *explanation*. Explanatory reductionism is <u>not</u> metaphysical reductionism, as we discuss below. After a brief discussion of reductionism, we highlight three important issues from the philosophy literature that are largely neglected in the discussion of micro-foundations. The first issue concerns the decomposability of complex systems. The degree to which a system, such as a complex organization governed by a coherent strategy, can be decomposed into separable and distinct units – down to the level of teams and individuals -- constrains the feasibility of micro-level explanation. The second issue concerns the practice of empirical confirmation, an important consideration in choosing an explanation, particularly at the micro-level in a complex system. The best arguments for and against explanatory reduction center around concepts that have the appropriate empirical confirmation. The third issue involves the practice of scientific explanation. In particular we will evaluate the limits of

dispensability arguments in scientific reduction. Once we have argued for the value of debating explanatory reductionism, our thesis is ecumenical: the presence of micro-level explanations does not render macro-level explanations dispensable. Whether we ought to prefer micro-level or macro-level explanations depends on the *explanandum*.

An Introduction to Reductionism

As Timothy Devinney (2013) puts it, the literature on microfoundations of management highlights a number of reoccurring yet unresolved basic philosophical issues, including "reductionism". Indeed, business and strategic management scholars, including Devinney, have done an admirable job invoking the philosophical literature to see what can be usefully applied to the discussion on microfoundations. But, from a trained philosophers' perspective, the discussion lacks cohesion. Management scholars often fail to identify the most relevant aspects of the vast philosophical literature on reductionism. We hope to provide some guidance by discussing key philosophical issues that matter to specific issues raised in the micro-foundationalism literature. Let us begin at a high level of abstraction—we are talking philosophy after all—and then work our way to case studies.

In philosophy, the term "reduction" is used to express a relation between entities, x, and y. When an entity x is said to be reducible to y then y is in some sense "more basic" than x. What does this mean? There are a variety of interpretations, including (this is not an exhaustive list): x fully depends on y, x is constituted by y, x is nothing more than y, x is nothing above and beyond y, x "just is" y . There are two broad philosophical approaches to understanding the reduction relations between entities x and y: *a priori* and *a posteriori* approaches. For *a priori* approaches, the reduction relation is understood from basic metaphysical principles requiring reason alone, not empirical evidence. *A priori* approaches to reduction are central to many philosophical debates. For instance, a philosopher of mind wonders whether a mental state (entity x) is reducible-- "nothing more than"—a physical state of the brain (entity y). Metaphysicians interested in "emergentism" wonder in what context the whole (entity y) is in fact something other than (in an ontological sense) the sum of the parts (the entities x1, x2, …, xn).

For *a posteriori* approaches, the reduction relation is understood on the basis of empirical evidence. "Scientific reductionism" is a species of *a posteriori* approach whereby the reduction is said to be justified on scientific evidence and the success of science. Historically, philosophy of science discussions of reduction were inspired by episodes in physics, such as the reduction of Newtonian mechanics to relativity theory, of chemistry to atomic physics, gas laws to statistical mechanics. In the reduction of gas laws to statistical mechanics, the question is whether the phenomenon being explained in the gas laws is also explained within the theory of statistical mechanics. More recent philosophical discussions have centered on the biological sciences, including ecology and evolutionary biology. Discussions about reductionism in science, much like the discussions in strategic management tend to focus on "levels" of explanation, with "macro-explanation" as the most "upper-level" explanation and "micro-explanation" at the opposite extreme. In Oppenheim and Putnum's (1958) "unity of science" thesis, for instance, each level in biology is reducible to the next lower level. Levels are identified by the phenomena they explain. So, if whole organisms are at the "macro-level", then

each upper level contain the parts of the objects at the next lower level. Below whole organisms there are cells. Cells, in turn, are composed of biochemical molecules, which are composed of atoms.

In strategic management literature the discussions have focused on the best explanations for conceptual entities such as "competitive advantage", "organizational learning", "behavioral strategy", "group cohesion" (Barney and Felin, p. 138), and "organizational routines and capabilities" (Winter 2013, p. 120). Some microfoundation reductionists worry that appeals to higher- or macro-level entities is akin to advocating a sort of metaphysical emergentism or holism, hence taking the study of strategic management out of the realm of the sciences and into the realm *of a priori* analysis. As Kenneth Arrow (1951) wrote: "A full characterization of each individual's behavior logically implies a knowledge of group behavior; there is nothing left out. The rejection of the organisms approach to social problems has been a fairly complete, and to my mind salutary, rejection of mysticism". On the other hand, some anti-reductionists appeal to metaphysical holism or emergentism in the philosophy of mind to maintain that the whole is indeed more than the sum of the parts. Nelson and Winter (1982), for example, appeal to the contemplative writings of John Dewey on the theoretical mental and unconscious processes that make up human deliberation to provide a "fruitful" way forward for the study of organizational routines and capabilities.

The discussion of reduction in strategic management need not be limited to *a priori* justification; research may also appeal to the *a posteriori* approach, as some in fact do. Some anti-reductionists maintain that microfoundationalists rehearse familiar failed arguments about reductionism in science. One such failure is the lack of "guiding principle on where to stop" (Winter 2013, p. 124). Other microfoundationalists counter with a "dispensability argument". The argument begins with a plausible idea that the micro-level constitutes an underlying determinism; for every macro-state there corresponds a micro-state, and for every micro-state there is a micro-explanation. Dispensability is said to follow: macro-explanations are superfluous in light of the existing micro-state explanation. Abel, Felin and Foss' (2008, p. 490) version is as follows: "To be sure, firm-level concepts such as routines and capabilities may be (indeed, are) relevant to the explanation of firm-level outcomes. However, they are relevant because they are useful shorthand for complicated repetitive patterns of individual action and coordinated interaction. Thus, the micro-level (i.e. individual action and interaction) ultimately replaces the macro-level (i.e. the postulated direct link between routines/ capabilities and performance) in the explanation of how routines/capabilities and performance are linked".

In what follows we wish to bring a philosophical focus to the issue of scientific reductionism. That is to say we will take a broadly *a posteriori* stance to understanding the reduction relations between "micro" and "macro" explanations. We will set aside the *a priori* approach; we will invoke no metaphysical principles. Instead, our discussion of reduction will attempt to find justification in scientific evidence and good scientific practices.

Decomposition and Explanation

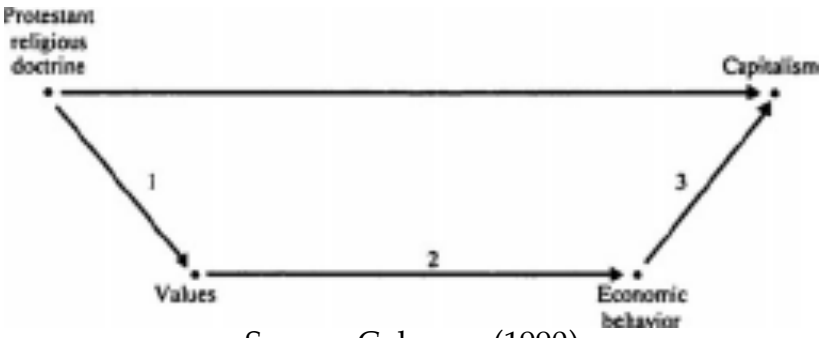
A useful point of departure is to investigate the boundaries of the concept of microfoundations. There appears to be no argument that the opposite end of the conceptual spectrum is the organization, which is the domain of holistic methodology in management, especially strategic management. Most of the papers extolling the virtues of microfoundations begin with the claim of the need to begin at the level of the individual (cf. Felin and Barney 2013; Felin and Foss 2005). The demand for individual-level analyses echoes the decades-old discourse on methodological individualism (Udehn 2001) in that there is some recognition that some collective entities and outcomes ought be analyzed, as there may be some social conditioning of individual behaviors and there may be difficulty separating individual behaviors from visible collective actions. Udehn identifies this position as weak-form individualism. Our reading of the microfoundations literature suggests that no one is wedded to a strong form individualism where only individual behaviors are observed and aggregated directly to represent the whole. What remains is some question about whether separate levels of analysis – individual, team, routine, and firm – are necessary for explanation or whether one can abstract from formal structure and use some implicit aggregation conditioned by shared identity, motivation, goals, or convention (Foss and Lindenberg 2013).

Some authors treat the disaggregation of the whole into routines as a sufficient microfoundation for strategic management. Winter (2013) maintains that demands for individual-level causal explanation for the structure and function of routines (c.f. Abell, Felin, and Foss 2008) are unnecessary reductionism. There have been a couple attempts to make the linkages between individual action and the routine, but questions of explanatory value have been raised.

The bathtub problem

One attempt to establish an explicit microfoundation for organizational routines is Abell, Felin, and Foss (2008). They begin with James Coleman's (1980) meso-theoretical model of social action in which the macro-level causal relation between societal-level antecedents and outcomes (shown in Figure 1 as the Weberian model of Protestant doctrine and capitalism) can be explained by lower level (individual) characteristics and behaviors. Protestant doctrine conditions social values which leads to economic behaviors at the individual level, which in turn cause capitalistic outcomes.

Figure 1. Macro-micro linkages (Coleman's bathtub)

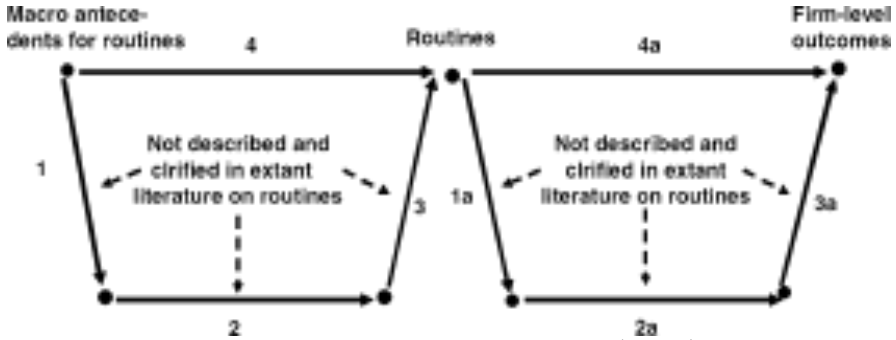


Source: Coleman (1990)

Coleman's approach is an interesting form of methodological individualism that attempts to marry rational choice at the individual level with social structural explanations at the macro level. One may carry the argument forward as to whether the horizontal arrow linking macro-level antecedents and macro-level outcomes becomes redundant and thus, dispensable. Coleman argues that it was not dispensable, but microfundamentalists would disagree. The section of our paper in explanatory reductionism explicitly considers this epistemological stance.

Taking a model by Abell (1996) that requires explicit consideration of micro- and macro-linkages in both *explanatia* and *explananda*, Abell, Felin and Foss (2008) create a double bathtub (Figure 2). In this figure, routines are the macro phenomena. In the left side of the diagram, they are the *explanandum* and in the right side, they are the macro-level *explanans*. The arrows 2 and 2a mark the individual level behaviors that explain the routines (arrow 3) and that explain the firm-level outcomes (arrow 3a). There are macro-level antecedents for routines that condition the individual behaviors (arrow 1) and the routines subsequently condition (arrow 1a) the individual behaviors that explain the firm-level outcomes.

Figure 2. Micro-macro linkages for routines as *explanans* and *explananda*



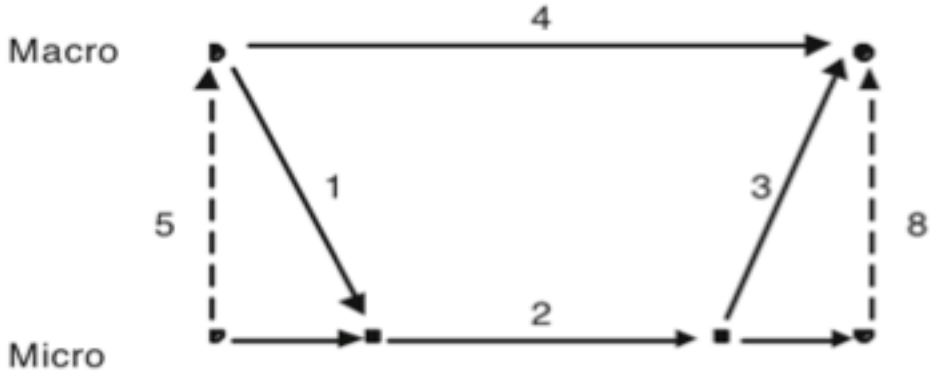
Source: Abell, Felin and Foss (2008)

Abell, Felin, and Foss leave no doubt that they willingly jettison arrows 4 and 4a in favor of an individualist (reductionist) approach to strategic management methodology. We find that the most significant flaw in their model is that they confound the firm-level and the routine-level. Reading across the arrows 4 and 4a, they posit that there are macro-level causes for firm-level outcomes, mediated by routines. Are there no macro-level (firm-level) antecedents to firm-level outcomes that are not mediated by routines? Are routines sufficient to explain firm-level outcomes? Are routines equivalent to the macro-level, in fact? And, of course, if one blithely declares macro-level explanation as redundant and dispensable, then this model insists on routines as a mediating construct between firm-level causes. Moreover, routines are only explained by individual action (arrows 1,2,3) and firm-level outcomes are only explained by individual action (arrows 1a,2a,3a). Coleman would not agree.

Abell, Felin, and Foss are adamant that this model has the advantage of combining methodological individualism (a good thing) with mechanistic explanation (another good thing). As we note at several junctures, the advantage of the former must eventually rest upon the merits of explanatory reductionism, not on an *a priori* preference. We now turn to the value of the latter, mechanistic explanation. We begin with Vromen's (2010) response to Abell, Felin, and Foss (2008). Then we discuss mechanistic explanation within the context of complex systems and whether it is a necessary component of a methodology of microfoundations.

Vromen (2010) argues that the diagonal arrows in the Coleman bathtub are "shorthands" for the true nature of causal explanation in multi-level models. That is, he "squares" the diagram by stating that the micro-level causation occurs in parallel with arrow 4 in Figure 3. The explanation is that the micro-level phenomena are *constituents* of the macro-level phenomena representing the end points of arrow 4. So, if there are macro-level antecedents, they cause the macro-level outcomes. They do not cause (or condition) the micro-level phenomena. There are constituent antecedents (line 5) and constituent outcomes (line 8), which are linked by the causal relationship(s) between the end points of the micro-level.

Figure 3. Squaring Coleman's diagram



Source: Vromen 2010

Vromen bases his analysis upon the definition of mechanistic explanation of Machamer, Darden, and Craver (2000) and Craver and Bechtel (2007), wherein there are no

allowable downward causes and, since macro phenomena are the aggregations of (constituent) micro-phenomena, there are no upward causes. Causal relationships occur only within a given level. This version of mechanistic explanation derives from the physical sciences (hence, mechanistic), wherein the relationships between the whole and the parts in a system are most clearly constituent. An avalanche doesn't cause the individual snowflakes to descend the mountainside, they constitute the avalanche. The "macro" cause of the avalanche affected the whole and the consequence (overrun the village) is explained at the macro level (line 4). But each of the snowflakes responded to the same cause, were part of the phenomenon, and contributed to burying the village.

Vromen's rebuttal to Abell *et al* is valid if all mechanistic explanations have the same characteristic structure as that of Craver and Bechtel (2007) and if Abell *et al* insist that their version of causal explanation is mechanistic.

There is a different perspective on multi-level causes and complex organizations that informs the nature of the methodology of microfoundations in organizations, especially with respect to strategy. The call for elaborating the microfoundations of organizational strategy presupposes that the organization can be decomposed into either discrete units or discrete processes. These discrete components of the strategic whole can be modeled as phenomena for the purposes of explaining organization-level behaviors as outcomes of micro-level behaviors. Many of these phenomena are characterized as mechanisms (see Felin and Foss 2005; Abell, Felin and Foss 2008, 2009; Vromen 2006, 2009, 2011). However, the degree to which the organization can be decomposed constrains the identification of mechanisms and the consequent explanation.

What we should have learned from Herbert Simon

A salient element of Herbert Simon's polymathy was his capacity to see structural and behavioral similarities across a number of complex systems. The scholarship of organization theory, managerial cognition, organizational behavior, and strategic management draws on the so-called Carnegie School for which Simon was a central figure. His 1962 paper on The Architecture of Complexity presented a taxonomy for the degree of complexity (inter-relatedness among constituent parts and processes) in artificial systems (like firms). This taxonomy has been used and elaborated by philosophers of science, notably William Wimsatt and his students (Wimsatt 1972, 1986, 1994; Bechtel and Richardson 2010).

Bechtel and Richardson (2010) identify several levels of decomposability of complex systems.

1. Simply decomposable systems
The components of this system can be disaggregated easily, often because the system is linear. There are minimal interactions among the components. Wimsatt (1986) holds that "[t]here are no cooperative or inhibitory interactions among parts of the system" (p. 269). Such systems are easily modeled because the explanatory mechanisms can be *localized* – identified spatially within a particular component or functionally among linked components (Bechtel and Richardson 2010). These aggregative systems are generally uninteresting.

2. Non-decomposable systems
A complex system may not be decomposable if the mechanisms cannot be localized; that is, the functions of interest in the system are distributed widely, occur synchronously, or have significant interactions such as mutual regulation. The system cannot be characterized as a collection of components that can be considered as independent, even at a first approximation. In such a system we can only observe system-level inputs and outputs.

3. Composite systems
Bechtel and Richardson identify two genres of systems that fall between simply decomposable and non-decomposable poles of a continuum. One genre, *component systems*, falls on the decomposable side of the continuum. Each component's behavior is intrinsically determined, and the system exists to hold the components together. While the components interact, the functions are clearly localizable and the linkages between components are typically input-output relationships. The system may govern the linkages, but not govern significantly the functions within the components. The second genre is *integrated systems*, in which "systemic organization is significantly involved in determining constituent functions" (p.26), including mutual regulation and feedback. Constituent functions/ behaviors are not intrinsically determined; they are regulated by the systemic organization. As such, this genre of composite system is situated between component and non-decomposable systems.

Bechtel and Richardson note that component systems correspond to Simon's (1969) *nearly decomposable* systems. These are not *simply decomposable*, connected linearly and without feedback. For Simon, the interdependence among the components in a nearly decomposable system has the following properties.

"(1) In a nearly decomposable system, the short-run behavior of each of the component subsystems is approximately independent of the short-run behavior of the other components; (2) in the long run the behavior of any one of the components depends only in an aggregate way on the behavior of the other components." (Simon 1969, p. 210)

The boundary between systems that can be decomposed effectively for mechanistic explanations and those that cannot falls along the continuum between *component/nearly decomposable* systems and *integrated* systems. This is precisely the point where organizational strategy is designed and implemented. Thus, the search for microfoundations of strategy may be limited by the complex nature of strategic organizations, made so by design. As we suggest in the first paragraph, the corpus of strategic management scholarship is dominated by whole-firm approaches and strategy implementation that drives decisions made at all levels within the organizational structure is laudable.

If the strategic architecture of the firm cannot meet the requirements of near decomposability, mechanistic explanations of the type described by Vromen (2010) are impossible. The feed-forward and feedback relationships, mutual regulation (by the strategy), and synchrony of behaviors create a complex structure wherein the components are not just constituent parts of a whole. One cannot localize the relevant

structural components and their interactions in a causal mechanistic model. It may be possible, however to have a mixed model in the form of Coleman's bathtub, where macro processes and micro processes may be identifiable. The feed-forward and regulating effects of the firm-level strategy would fulfill the role of Coleman's downward (arrow 1) causation, *pace* Vromen, and there could be localized mechanisms associated with the micro level (arrow 2). And those mechanisms may have upward causal relationships with macro-level outcomes, though (a) they could not be characterized as constituent and (b) they would be highly unlikely to uniquely causal to firm level outcomes, given the complexity of the architecture. Thus, we might argue for some explanatory value of this model, though it would be an empirical issue if the macro-level causes (arrow 4) can be held to be dispensable. It would also be an empirical issue if individual-level behaviors could add any explanatory value to a component-level model in the nearly decomposable system. Abell, Felin, and Foss (2008) may appreciate the putative inclusion of upward and downward causation, but their methodological individualism will not accommodate the non-dispensability of the macro level causal relationships, nor the caution against the localization of behaviors at the individual level.

Debates about reduction among philosophers of science are typically conducted around concepts that are well-confirmed by empirical testing, but have complicated features. While there is a rigorous debate about whether the "classical Mendelian genetics" interpretation of the unit of inheritance is reducible to the molecular conception of the gene, there is little question among philosophers about the ontological status of either a unit of inheritance or a molecular gene. Both are well supported by empirical tests. The constructs discussed above with respect to decomposed systems and with micro-level mechanistic models do not share this ontological status; the *a posteriori* confirmation is missing. We examine this fully in the next section.

Scientific confirmation: the case of routines.

With philosophy of science as our guide we will try to illuminate some of the basic unresolved issues concerning reduction and the value of microfoundationalist research. Let us begin with comparing the reduction literature on "organizational routines" with that of the philosophical literature on the reduction of genetics. There are numerous parallels in the subjects. For instance, routines are, for some strategic management theorists, a unit of inheritance in the evolution of the firm just as genes are the unit of inheritance for systems undergoing evolution by natural selection. Despite the conceptual parallel between genes and routines, there is a troubling disconnect—a feature of the discussion in the philosophy of science that is notably missing in the discussion of strategic management. There is little question among philosophers on both sides of the reduction debate that the "Mendelian gene" and "molecular gene" are concepts that are well-supported by empirical evidence and testing. But, the situation is not so clear-cut for the concept of the routine, as discussed below.

We do not see the same lesson applied in the strategic management discussion on "routines". There have been several reviews of the use of routines in strategy, organization theory, and evolutionary economics, which point to a continuing problem with the construct as an element of the methodology of microfoundations (Parmagiani

and Howard-Grenville 2011; Becker 2004, 2008; Abell, Felin, and Foss 2008; Vromen 2006, 2001). The problem is typically described as an imprecision of the construct. Is it a cognition-saving heuristic (Simon 1947), a repeated, learned behavior that mimics habit at the individual level (Nelson and Winter 1982), loosely coupled processes that allow firms to react to the environment (March and Simon 1958), or a capability that governs the plasticity of the firm's strategy (Gavetti 2005)? Are routines intentional or mindful (Pentland 1995) or are they automatic and habitual (Gersick and Hackman 1990)? Similarly, are they sufficiently fixed to serve as the analogue to the gene in evolutionary models (Nelson and Winter 1982; Hodgson and Knudsen 2010) or the basis for change – "new combinations of existing routines (Nelson and Winter 1982, p. 130)?

It is beyond the scope of this paper to slog through the morass of definitions used in the literature. We recommend the review by Parmigiani and Howard-Grenville (2011) as a yeoman's effort at sorting out the definitions and implications, though they readily admit that incommensurable terminology and rather blithe reliance on Nelson and Winter represent impediments to application of the construct in the management field. They call for abandonment of the definitional quibbling and get on with augmenting the paltry number of empirical studies using the construct. We agree to this last point for a very particular reason in assessing the value of explanatory reduction in the microfoundations project.

As Vromen (2011) notes, much of the current literature on routines is traced back to the seminal work of Nelson and Winter, and, as such, there is a fair bit of interpretive work, to understand the key features of Nelson and Winter's theoretical construct (see Becker 2004). This is not good scientific practice. In science, theoretical concepts are discovered, not interpreted. Discovery involves confirmation, and confirmation involves theory testing against empirical evidence.

Let's illustrate with a specific case drawn from the annals of science. The Monovian monk, Gregor Mendel, is said to be the discoverer of genes; but not because he coined the term. He didn't; Wilhelm Johanssen introduced the term in 1905, two decades after Mendel's death. Nor did Mendel have any direct observation of anything resembling genetic particles. Rather, Mendel postulated a particulate inheritance system and a system of laws on the basis of circumstantial evidence gleaned from breeding experiments on pea plants (*Pisum sativum*). Mendel deserves his title as the father of genetics because he inferred his laws of inheritance through an appropriate method of confirmation—"inference to the best explanation"— based on experimental evidence. Mendel crossed pea plants to test for breeding patterns across several key plant characteristics, including texture of seeds, color of seeds, petals, pods, length of stems, and so on—seven binary characteristics in all. Mendel consistently found 3:1 ratio of characteristics in the grand offspring (after selfing the first generation of offspring). So, for example, he found the proportion of round to wrinkled peas in the grand offspring to be 3:1, and likewise for all seven characters he tested. Mendel used the experimental results to support a series of hypotheses in one of the best textbook examples of "inference to the best explanation" (or "abductive") reasoning. Mendel hypothesized that each parent contributes particles that correspond to a character. (Mendel never said "particle" but never mind, our whiggish history suits the philosophical point about proper confirmation). Mendel further hypothesized that the contribution from each parents to offspring obeys certain laws, the so-called laws of assortment and

segregation. The postulate of particulate inheritance and laws of inheritance best explains the experimental results found in the pea plants. By the use of inference to the best explanation, Mendel used experimental evidence to provide confirmational evidence to support the existence of a theoretical concept (genes) and laws for their behavior.

To see how inference to the best explanation works, compare Mendel's hypotheses with the rivaling "blending hypothesis" that was popular among Mendel's contemporaries—including Darwin. Now, ask the question that is at the heart of inference to the best explanation reasoning: if blending inheritance were true would that explain the 3:1 ratio of characters? The answer is, "no", blending hypothesis predicts that the characteristics would be a mix of characters from the parents, not 3:1 ratios in definite and distinct binary characters. Think of blending inheritance like a paint pot where the offspring's characters come from mixing the qualities of paint contributed by the parents. Now, ask the same question for Mendel's theory: if it were true, would that explain his experimental results? The answer is "yes".

The Mendel example illustrates an important feature of scientific practice that is assumed in much of the philosophical literature on reductionism. When one makes a claim in science that one entity is reducible to another, the entity in question has good empirical evidence that it in fact exists *in rerum natura.* We are not finding the same good practice in the discussion of routines. The status of routines is limited as a construct because of its multifarious instantiations. Unlike the biological concepts "unit of inheritance" and "gene", the requisite empirical confirmation does not exist for routines, as Parmiginai and Howard-Grenville discovered. If philosophy of science is to be a methodological guide for strategic management, we must not build microfoundations upon the shifting sands of routines. The strategic management literature, if they are to utilize the case of routines as their point of discussion for micro foundations, should first determine whether the Nelson and Winter's theory of routines is well confirmed by standard confirmation procedures over empirical data and not rely on *a priori* analysis of what Nelson and Winter might have meant.

The Dispensability Argument

Recall Abell, Felin, and Foss' (AFF) dispensability argument. It begins with a reasonable assumption about deterministic systems; for every macro-explanation there corresponds an underlying micro-explanation. The problem is their hasty conclusion. Accordingly the presence of micro-explanations underlying macro-explanations renders the latter dispensable. That is, when a micro explanation is available—and there always is a micro-explanation available in deterministic systems—we ought to prefer it. There is a vast philosophical literature on the limitations of dispensability arguments like AFF. In what follows, we will highlight the main findings. AFF's error is to neglect the nature of scientific explanations, in particular the object of explanation. Whether macro-explanations or micro-explanations are called for depends on what about the phenomena must be explained.

Let's start with a concrete example, drawn from ecology. Suppose we are interested in the periodic fluctuations in population levels between two species, foxes and rabbits (as in the case of Mendel's genes, this is an overly simplistic example). Foxes are predators; they eat rabbits. Foxes will eat rabbits until there are too few rabbits to sustain the fox population. At that point the foxes die off, and their population numbers decrease rapidly. When that happens rabbits have fewer predators to worry about and start multiplying until there is, once again, enough food for foxes. Then, fox population increases and the cycle repeats. The Lotka-Volterra equation in ecology is a classical expression of the dynamics of predator/prey relations like our fox/rabbit case:

$$\frac{dx}{dt} = x(\alpha - \beta y)$$
$$\frac{dy}{dt} = -y(\gamma - \delta x)$$

where,

- x is the number of prey (rabbits);
- y is the number of some predator (foxes);
- $\frac{dy}{dt}$ and $\frac{dx}{dt}$ represent the growth rates of the two populations over time;
- t represents time; and
- α, β, γ and δ are parameters describing the interaction of the two species.

Let's take a specific situation; the fox population is high. Using our equation as a model we can infer that because the fox population is high, there will be a great "pressure" on the rabbit population. Suppose one rabbit gets caught and eaten. Our model undergirds the following explanation:

"The cause of the death of the rabbit was that the fox population was high".

Now, this is an instance of a micro-level phenomenon—the death of a given rabbit—being explained with a macro-state, an expression of a law that has no reference to causal mechanisms or any other individual level causal events.

Following AFF's dispensability argument, we ought to prefer a micro-level explanation to the macro-level one if one is available. Indeed, in this instance, a micro-level explanation is available. It looks like this: "a rabbit hopping through the field one day passed too close to a tree where a fox was lurking; the fox pounced and ate the rabbit". In fact, we can generalize this sort of micro-level explanation to explain how each individual rabbit gets eaten because the nature of the micro-level explanation is to invoke the complete description of all the interactions between individual foxes and individual rabbits, including physiology and reaction times (Garfinkel 1981). From the complete specification of the various casual interactions between rabbits and foxes we can extract a micro-explanation of the order:

"Rabbit r was eaten because he passed through the capture space of fox f."

But, is the presence of a micro-explanation sufficient to render the macro-explanation involving the Lotka-Volterra equation dispensable? There are many reasons to answer "no". First, consider what does AFF's microfoundationalist requirement amounts to. How does AFF avoid the unfortunate conclusion that any successful explanation of the behavior of complex systems such as predator/prey relations must trace trajectories of each individual organism (Woodward 2003)? Even if such an explanation were possible, there are reasons to prefer the macro-explanation to the micro-explanation. One is purely pragmatic, a full micro-explanation involving large populations might be far too complex to be followed by the human mind.

Another reason is more substantive; the macro-explanation provides information that the micro-explanation does not. Namely, it tells us "what could have been otherwise". That is, causal explanation by use of counterfactuals (see Woodward 2003) can follow from the macro-explanation, but would be impossible with only the information from micro-level models. The macro-explanation is useful information for explaining features of complex system because there are a very large number of different possible life histories of foxes and rabbits in addition to the actual life histories that would produce the same macro-level outcome described by the Lotka-Volterra equation that we might want explained. For instance, when the fox population is high, we can infer from the Lotka-Volterra equation that the chances of a rabbit being eaten is high. Likewise, when the fox population is low, then, we can make the corresponding inference about the chances of rabbits being eaten. From the macro-explanation, we determine what would have happened had circumstances been slightly different than they actually were. In the instance where fox populations are high, had circumstances been different and in fact rabbit r happened not to get eaten by fox f at that particular time we can infer that regardless, rabbit r would have been eaten by some fox or other at a different time. The chances are lower in the instance where the fox populations low. The micro-level explanation is limited to the actual causal interactions that lead particular foxes eating particular rabbits and hence cannot provide this extra information that we might seek of complex systems.

We are not arguing that in this instance one should always prefer macro- to micro-explanations. We are not seeking to dismiss micro-foundational arguments in favor of anti-reduction arguments. The object of explanation determines the appropriate level of explanation – the *explanandum*. We are rejecting the monolithic account of explanation that is explicit in the microfundamentalist stance; and we are pointing out the limitation of AFF's dispensability argument. We favor an ecumenical approach to explanation: If all you care about is the explanation for why a particular rabbit got eaten, then the extra information provided by the Lotka-Volterra equation is unnecessary. But, if what you really want explained is why any rabbit was eaten as opposed to not eaten, then, perhaps the counter-factual or "what could have been otherwise" information that the equation provides is necessary. The *explanandum* in the former is a call for a high level of specificity hence a micro-explanation with all its causal details about what actually happened would be preferable. In contrast, an *explanandum* that seeks generality rather than specificity would be better served with an explanation that trades off information about what actually happen for each member of the population for an explanation that

is more abstract and can tell us what could have happened had the initial conditions been different.

Notice, within our argument against wholesale microfundamentalism is an answer to Nelson's "where to stop" challenge for microfoundationalism. Where we stop depends on the object of explanation. In the foregoing discussion, we chose a simple system from population ecology to illustrate whether we stop at explaining interactive population dynamics at the level of population counts or at the level of individual organism interactions. There is an obvious analogue in the management literature: the population ecology of organizations (Carroll and Hannan 2000). The population ecologist in organization theory is interested in patterns of population changes and ignores intra-firm strategies, structure, and behavior. The stopping rule is implicit in the *explanandum*: dynamics of the population of similar firms over time. What is missing in the micro-foundation literature is attention to nature of scientific explanation which determines the requisite level of explanatory detail.

Let us examine one more phenomenon in biology that highlights an important issue in explanatory reductionism: explanations involving evolution by natural selection. Again, we start with a concrete example. In a fruit fly lab we observe over time a gradual increase of the frequency of flies with fuzzy thoraxes over those without fuzzy thoraxes. The change corresponds with the increasing cold conditions of the lab. Suppose fuzzy thoraxes eventually go to fixation. Ever since Darwin, evolution by natural selection is the immediate macro-explanation. In the modern version of natural selection, if there is heritable variation in trait types and the variation can be ranked and ordered according to a scale of "trait fitness" then, we can predict that over generational time the population will undergo a change in trait frequency according to our trait fitness scale. Don't confuse "trait fitness" with "individual fitness". Individual fitness is a measure of a single organism's chance of reproductive success. Trait fitness is an average over trait types, not individuals. In this specific instance, the trait represents a quantitatively measurable feature (i.e. thorax thickness) in the lab population. Modern natural selection explanations invoke trait fitness, not individual fitness. And, that is to be expected: we are interested in explaining the evolution of fuzzy thoraxes among fruit flies, not why any individual fly developed his. That's what makes natural selection a paradigmatic macro-explanation.

Is there a micro-level explanation available for the evolution of fuzzy thoraxes? Yes. And, because of it, natural selection fits AFF's criterion for macro-explanations: natural selection is a "useful shorthand for complicated repetitive patterns of individual action and coordinated interaction". The micro-level explanation might go something like this. Upon investigation of the fruit flies in the lab, we notice a chromosomal inversion changes frequency in the lab over the course of the observational time. The physical basis for the differences in trait fitness is determined to be the result of the chromsomal inversion causally contributing to the development of fuzzier thoraxes, which helps insulate against an increasingly colder lab (Ariew 2003, Sober 2000, Rosenberg 1978). The micro-explanation just offered refers to the physical characterizations that determine the differences in trait fitness ascribed in the natural selection explanation. So, it is possible to replace all the features of the natural selection explanation with more specific micro-level details. According to AFF's brand of (monolithic) micro-

foundationalism we ought to prefer the physical story over the natural selection explanation. Are they right?

It depends on the object of explanation. If we seek an answer to the question: why did these flies (in this particular lab) evolve to fuzzy thoraxes? Then, the microfoundationalists will prefer the explanation that cites specific details and traces the life histories for each fly lineage including developmental and genetic information. But, natural selection explanations are preferable when we seek general, more unified explanations, perhaps across multiple populations, species, locations. As with Lotka-Volterra equations, natural selection explanations provide information about "what could have been otherwise", had the initial conditions for the fruit flies have been different, or even, if the population and location been different. Macro-explanations offer explanatory information that micro-level explanations cannot. Natural selection provides explanation of what several evolutionary events have in common. It allows us to subsume distinct population events under one description (Ariew 2003, Sober 2000)..

To make the point about the value of generality of macro-explanations stronger, change the populations under considerations. Fruit flies in our lab versus Venus flytraps in the Amazon River basin. The micro-explanation with all its causal detail fails to provide us the generality that we might seek when we try to make sense of the blooming, buzzing confusion of the natural world. What is the charactertistic of predator-prey and natural selection macro-explanations that provides them with this extra explanatory power above the micro-causal details? The answer is that both theories are what philosophers call "multiply realizable" (Shapiro 2008).

The basic idea is that the macro-explanations invoke higher level generalizations that can be instantiated in a variety of specific lower-level micro-states. To illustrate, let us look at the canonical figure from Jerry Fodor (1975).

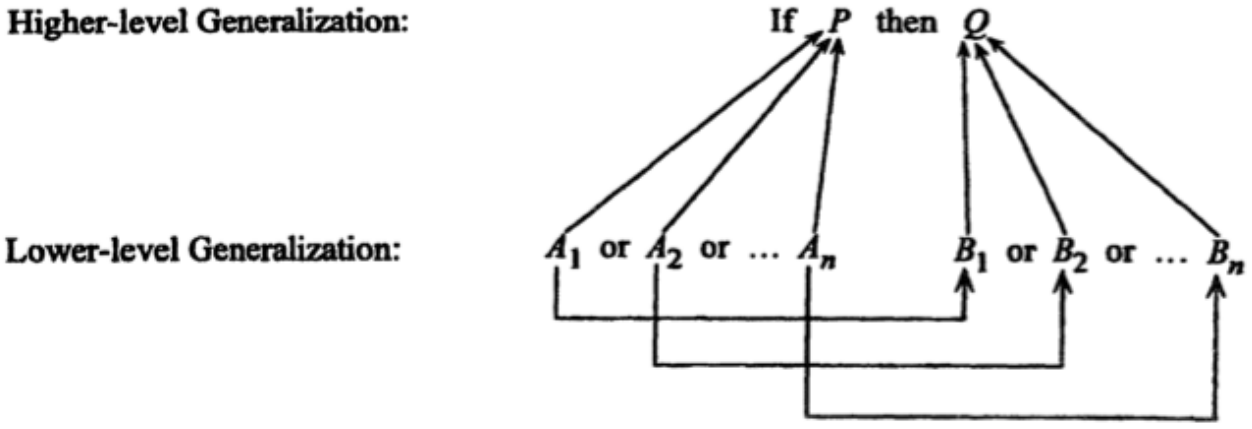


Figure 2. The lower-level properties A_i and B_j provide multiple realizations of the higher-level properties P and Q, respectively. One higher-level law and n lower-level laws are depicted, following Fodor 1975.

In the figure, the higher-level generalizations are expressed as a conditional, If P then Q which entails that everything that has a P also has a Q. The lower-level generalizations range over some number of laws, n, each of which connect an "A" predicate to a "B" predicate. Everything that has an Ai also has Bi (for each i=1, 2, …, n). The connection between the two levels in the figure is such that A1, A2, A3, …, An are different (mutually exclusive and collectively exhaustible) realizations that P might have. Likewise for B1, B2, B3, …, Bn and Q. On this formulation multiple realizably has several components (see Fodor or Sober for a list). The important point is that the relation between the upper and lower generalizations are anti-symmetric: An individual that has P has that property solely in virtue of the fact that it has whichever Ai it possess" (Sober, 545). That is to say the mapping from lower to higher is many-to-one.

The Lotka-Volterra equation is a higher level generalization that is multiply realizable among a variety of fox/rabbit populations or any other biological instance involving the eater and eaten. And, it is because the equation is multiply realizable, the relation of the equation to biological instances is many to one. That allows us to use the same equation to glean information about "what could have been otherwise", regardless of the biological details.

Consider again Coleman's "bathtub" as it is used in strategic management to "clarify the notions of micro/individual level, and macro/collective level, as well as examine the relations between these notions and levels" (Abel, Felin, Foss 2008, p. 491). Many arguments meant to undergird both metaphysical and explanatory versions of reductionism and anti-reductionism use the "bathtub" diagram for illustration. But we wish to point out that the bathtub diagram is limited. In fact, it fails to illustrate the more powerful arguments against monolithic versions of explanatory reductionism involving relations that are multiply realizable. There is no room in the bathtub diagram for the connection between macro-level generalizations and the multiple micro-level generalizations, any one of which plausibly realizes the higher level generalization.

Conclusions

In this paper, we examine the microfoundations project as an instantiation of scientific reductionism. We ask that the project move forward from metaphysical posturing and a self-limiting approach of *a priori* theorizing about reduction from macro- to micro-explanation. To fulfill the promise of the project for improving explanations in management, the methodology must include confirmation of the constructs in the fashion of *a posteriori* approaches to explanatory reduction.

We examine three issues in pursuing the *a posteriori* approach. First, in a complex system, such as multi-unit organization with a strategic plan that touches all units, decomposition of the whole into parts (teams, departments, routines) so that micro-level mechanistic models may be tested is problematic. To decompose the whole and localize behaviors into discrete parts will require that the system meets the standards for disaggregation or require the researcher to control for interactions, feedbacks, and simultaneity among parts and processes, and to account for system-level regulation

(like the strategic plan). Moreover, one has to be specific about the degree to which the mechanistic model permits both micro- and macro-level explanations of phenomena or whether it follows the restrictive design of Vromen (2010) and permits no top-down or bottom-up causation. That is, are the micro-models of organizations strictly constitutive? This cannot be settled by *a priori* reductionism.

Second, we reinforce the need for a program of confirmatory empirical research to establish the value of constructs used in microfoundations research. We focus on the routine as a construct that allegedly represents a researchable level of analysis between the organization and the individual. This construct has a multitude of meanings, is often invoked in the literature, and is rarely confirmed. As such, routines have little to offer in a methodology that is based on an *a posteriori* approach to explanatory reduction. We would caution that individual-level models used in microfoundational research need confirmation as to the behaviors and structures that will permit "aggregation" of individuals (Barney and Felin 2013).

Third, we use examples from explanatory reduction in biology to make the point that micro-level explanations do not necessarily render macro-level explanations dispensable in a sound methodology for microfoundations project in strategic management. One must be concerned with the most efficacious way to build an *explanans* for the *explanandum* that interests us. Micro-level explanations may lack the requisite generalizability to use counterfactuals to explain system-level phenomena, because they are often multiply realizable.

To move forward with a coherent methodology of microfoundations, it will be necessary to leave behind unhelpful metaphysical commitments and get to the serious work of confirming the constructs that will constitute empirical analyses of strategic microfoundations. At the core of our thesis is that when the macro-level causal processes have been confirmed in empirical analyses and one dispenses with them in favor of unconfirmed (and often uncodified) contructs, the result is bad science. Devinney (2013, p.84) makes this point: "…work in microfoundations has, until this point, been a predominantly theoretical and ontological exercise. However, its value will ultimately be empirical, as multiple realizability implies that what structures are dominant can be answered only by a substantive and sophisticated empirical program (Shapiro, 2008)". We look forward to observing, and participating in, this empirical program.

References

Abell, P., Felin, T., & Foss, N. 2008. Building microfoundations for the routines, capabilities, and performance links. *Managerial and Decision Economics*, 29, 489–502.

Ariew, A. 1998. "Are Probabilities Necessary for Evolutionary Explanations?" *Biology and Philosophy*, Vol. 13, No. 2, pp. 245-253.

Ariew, A. 2003. Ernst Mayr's ultimate/proximate distinction reconsidered and reconstructed, *Biology and Philosophy*, pp. 553-565.

Ariew, A., Rice, C., Rohwer, Y. (in press), "Autonomous Statistical Explanations and Natural Selection", *British Journal for the Philosophy of Science*.

Ariew, A. and Lewontin, R. (2004), "The Confusions of Fitness". *British Journal for the Philosophy of Science*, 55, 347-363.

Arrow, K. J. 1951. Mathematical models in the social sciences. The Policy Sciences. reprinted in Brodbeck, M. 1968. *Readings in the Philosophy of the Social Sciences*, New York: Macmillan, pp. 635-668.

Barney, J.B., and Felin, T. 2013. What are microfoundations? Academy of Management Perspectives, 27: 2, pp: 138-155.

Bechtel, W., and Richardson, R. C. 2010. *Discovering Complexity: Decomposition and Localization as Strategies in Scientific Research*, Cambridge: MIT Press.

Becker, M.C. 2004. Organizational routines: a review of the literature. *Industrial and Corporate Change*, 13: 643–678.

Becker, M.C. 2008. The past, present, and future of organizational routines. in M. Becker (Ed.), *Handbook of Organizational Routines.* Cheltenham, UK: Edward Elgar, pp. 3-14.

Coleman, J.S. 1980. *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.

Carroll, G.R., and Hannan, M.T. 2000. *The Demography of Corporations and Industries.* Princeton University Press.

Devinney, T. M. 2013. Is microfoundational thinking critical to management thought and practice? *Academy of Management Perspectives*, 27 (2): 81-84.

Felin, T., & Foss, N. 2005. Strategic organization: A field in search of micro-foundations. *Strategic Organization*, 3, 441–455.

Felin, T., & Foss, N. 2011. The endogenous origins of experience, routines and capabilities: The poverty of stimulus. *Journal of Institutional Economics*, 7, 231–256.

Felin, T., Foss, N., Heimeriks, K., & Madsen, T. 2012. Microfoundations of routines and capabilities: Individuals, processes, and structure. *Journal of Management Studies*, 49, 1351–1374.

Fodor, J. 1975. Special Sciences – Still Autonomous after all these years? in Fodor, J. (ed), *Critical Condition: Polemical Essays in Cognitive Science and the Philosophy of Mind*, Cambridge: MIT Press, pp. 9-24.

Foss, N. J., & Lindenberg, S. 2013. Micro-foundations for strategy: A goal-framing perspective on the drivers of value creation. *Academy of Management Perspectives*, 27(2).

Garfinkel, A. 1990. *Forms of Explanation: Rethinking the Questions in Social Theory*. Yale University Press.

Gavetti, G. 2005. Cognition and hierarchy: Rethinking the microfoundations of capabilities' development. *Organization Science*, 16, 599–617.

Gavetti, G., Greve, H., Levinthal, D., & Ocasio, W. 2012. The behavioral theory of the firm: Assessment and prospects. *Academy of Management Annals*, 6(1), 1–40.

Gersick, C., and Hackman, R. 1990. Habitual Routines in Task-Performing Groups, *Organizational Behavior and Human Decision Processes*, Vol. 47, pp. 65-97.

Glennan, S.S. 1996. Mechanisms and the nature of causation. *Erkenntnis*, 44: 49–71.

Hodgson, G., and Knudsen, T. 2010. *Darwin's Conjecture: The Search for General Principles of Social and Economic Evolution*, University of Chicago Press.

Lewis, D. 1986. Causal Explanation in *Philosophical Papers*, Vol. 2, Oxford University Press, pp. 214-240.

Machamer, P. K., Darden, L. & Craver, C.F. 2000. Thinking about mechanisms. *Philosophy of Science*, 67, pp. 1–25.

Mintzberg, H. 1994. *The Rise and Fall of Strategic Planning*. New York: The Free Press.

Mintzberg,H., Ahlstrand, B. and Lampel, J. 1998. Strategy Safari. New York: The Free Press.

Nelson, R.R. & Winter, S. 1982. *An Evolutionary Theory of Economic Change*. Harvard University Press: Cambridge, MA.

Oppenheim, P., and Putnam, H. 1958. "The unity of science as a working hypothesis", in H. Feigl, M. Scriven and G. Maxwell (eds.), *Concepts, theories, and the mind-body problem (Minnesota Studies in the Philosophy of Science, Vol. 2)*, Minneapolis: University of Minnesota Press, 3–36.

Parmigiani, A., and Howard-Grenville, J. 2011. Routines Revisited: Exploring the Capabilities and Practice Perspectives," *Academy of Management Annals.*

Putnam, H. 1975. The Nature of Mental States in *Mind, Language and Reality.* Cambridge, England: Cambridge University Press.

Rosenberg, A. 1978. Supervenience of Biological Concepts, *Philosophy of Science* 45, pp. 368-386.

Rosenberg, A. 1994. *Instrumental Biology or the Disunity of Science*, Chicago, University of Chicago Press.

Shapiro, L. 2000. Multiple realizations. *Journal of Philosophy*, 97(12), 635–654.

Shapiro, L. 2008. How to test for multiple realization. *Philosophy of Science*, 75(5), 514–525.

Simon, H. A. 1962. The architecture of complexity. *Proceedings of the American Philosophical Society*, Vol. 106, No. 6., pp. 467-482.

Simon, H. A. 1969. *The Sciences of the Artificial*, Cambridge, MA: MIT Press.

Sober, E. 1999. The Multiple Realizability Argument against Reductionism, *Philosophy of Science*, 66:4 pp. 542-564.

Uden, L. 2001. *Methodological Individualism: Background, History, and Meaning*. London: Routledge.

Vromen, J. 2006. Routines, genes, and program-based behavior, *Journal of Evolutionary Economics*, Vol. 16(5), pp 543-560.

Vromen, J. 2010. MICRO-foundations for strategic management: Squaring Coleman's diagram, *Erkenntnis*, vol 39, pp. 365-383.

Vromen, J. 2011. Routines as multi-level mechanisms, *Journal of Institutional Economics*, Vol. 7(2), pp. 175-196.

Waters, C. K. 1990. Why the Anti-Reductionist Consensus Won't Survive: The Case of Classical Mendelian *Proceedings of the Biennial Meeting of the Philosophy of Science Association,* Vol. 1990, pp. 125-139

Wimsatt, W. C. 1972. Complexity and organization, in K. Schaffner and R.S. Cohen (eds.), *PSA 1972: Proceedings of the 1972 Biennial Meeting of the Philosophy of Science Association*, Dordrecht: D. Reidel, pp. 67-86.

Wimsatt, W. C. 1986. Forms of aggregativity, in A. Donagan, A.N. Perovich, and M.V. Wedin (eds.), *Human Nature and Natural Knowledge*, Dordrecht: D. Reidel, pp. 259-291.

Wimsatt, W. C. 1994. The ontology of complex systems: Levels of organization, perspectives, and causal thickets, *Canadian Journal of Philosophy*, Vol. 20 (supp.), pp. 207-274.

Winter, S. G. 2011. 'Problems at the Foundation? Comments on Felin and Foss', *Journal of Institutional Economics*, 7(2): 257–277.

Winter, S. G. 2013. Habit, deliberation and action: Strengthening the microfoundations of routines and capabilities. *Academy of Management Perspectives*, 27(2): 120-137.

Woodward , J. 2003. *Making Things Happen: A Theory of Causal Explanation*. Oxford University Press.