The Many Faces of Unification and Pluralism in Economics: The Case of Paul Samuelson's *Foundations of Economic Analysis**

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Abstract: The history of modern economics abounds with pleas for more *pluralism* as well as pleas for more *unification*. These seem to be contradictory goals, suggesting that pluralism and unification are mutually exclusive, or at least that they involve trade-offs with more of one necessarily being traded off against less of the other. This paper will use the example of Paul Samuelson's *Foundations of Economic Analysis* (1947) to argue that the relationship between pluralism and unification is often more complex than this simple dichotomy suggests. In particular, Samuelson's *Foundations* is invariably presented as a key text in the unification of modern economics during the middle of the twentieth century; and in many ways that is entirely correct. But Samuelson's unification was not at the theoretical (causal and explanatory) level, but rather at the purely mathematical derivational level. Although this fact is recognized in the literature on Samuelson, what seems to be unrecognized is that for Samuelson, much of the motivation for this derivational unification was pluralist in spirit: not to narrow scientific economics into one single theory, but rather to allow for more than one theory to co-exist under a single unified derivational technique. This hidden pluralism will be discussed in detail. The paper concludes with a discussion of the implications for more recent developments in economics.

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One of the most joyful moments of my life was when I was led by listening to E. B. Wilson's exposition of Gibbsian thermodynamics to infer an eternal truth that was independent of its physics or economics exemplification. (A student who studies only one science would be less likely to recognize what belonged to logic rather than to the nature of things). (Samuelson, 1983, xix)

The Keynesian revolution was the most significant event in 20-century economic science. (Samuelson, 1988, p. 618)

I. Introduction

During the 1950s and 1960s economics became a unified science. This was not the case early in the twentieth century, and it is much less unified today, but during the middle of the twentieth century it became widely accepted that modern economic theory had established a degree of unification similar to natural sciences such as Newtonian mechanics and Darwinian evolutionary theory. Of course, unlike those natural sciences, organized opposition existed within economics – heterodox research programs such as Institutionalism, Marxism, and Austrian economics – but even these critics identified standard economics with a single unified theory, although for them it was a negative, rather than a positive feature of the discipline.[1]

It is generally, and to some extent correctly, accepted that one of the most important contributors to the unification of modern economics was Paul Samuelson's *Foundation of Economic Analysis* (1947). This paper will argue that while Samuelson's *Foundations* did in fact play an important role in the unification of modern economics, the unification that it offered was of a very specific type. Using the terms employed below, it was derivational rather than explanatory. This is important for a number of different reasons. For one thing, the nature of this unification is often misunderstood and once this distinction is recognized it becomes easier to understand both the character of the unified period as well as some of its later consequences. Secondly, and more relevant to the philosophy of science, unification is often associated with unification at the

[1] One way to think about this issue is based on the difference between *descriptive* unity or pluralism (the actual degree of unity or pluralism) and *normative* unity or pluralism (what ought to be the degree of unity or pluralism) in economics or any other science (Davis, 2019, p. 287). The point is that there was essentially no debate about the descriptive unity of mainstream economics during most of the second half of the twentieth century.

explanatory level and that was not the case in Samuelson's *Foundations*. It will be shown that *Foundations* exhibited a strong derivational unity at the mathematical level, but not explanatory unity at the causal level, making it a good case study for philosophers like Margaret Morrison (2000, 2002, 2006) who have emphasized the separation of unification and explanation. Finally, the *Foundations* case goes even further than decoupling unification and explanation. Not only does it provide evidence that it is possible to have a tightly unified theoretical science without necessarily having any single explanatory strategy associated with it, the argument will also be made that a single, formal, derivational unification that could support a certain degree of theoretical and causal pluralism, served several purposes that Samuelson considered important. And, at least for most of the second half of the twentieth century, his unification was successful, that is, it was very well-received, among mainstream economists. This is not to suggest that Samuelson himself was thinking self-consciously in terms of what later philosophers of science would call derivational without explanatory unification – he was just trying to improve the foundations of economic science in ways he considered, for a variety of reasons noted below, to be obvious – but simply that the unification that Samuelson offered in *Foundations* provides an excellent example of this form scientific unification.

Since the paper has a number of moving parts – it is a mix of both the history and philosophy of modern economics, as well as various elements of the specific context and motivations of Paul Samuelson – it is useful to state right upfront what I consider to be the main goals of the paper. They are:

- To be clear that Samuelson's *Foundations* did not assert, or even suggest, that optimization was the only causal mechanism at work in economics. Historians of economics who have written specifically about Samuelson in recent years are clear about this, but many other historians of economics, most practicing economists, and the majority of philosophers of economics, often seem to be less clear about it.
- To demonstrate that providing a mathematical framework that would accommodate both optimization-based and non-optimization-based economic theories like Keynesian macroeconomics and Walrasian general equilibrium theory, was one of Samuelson's goals in Foundations – and thus that *Foundations* encouraged a type of theoretical pluralism.
- To show that the derivational unification of twentieth century economics supports Margaret Morrison's long-standing argument that highly successful and unified scientific theories often support multiple causal and explanatory mechanisms.
- Finally, and more speculatively, to suggest that the perception that mainstream economics was unified exclusively by optimization – thus treating it as a constraint on economic theorizing – played some role in the changes that have taken place in economics during the last few decades.

The paper is organized as follows. Section two will discuss explanatory and derivational unification in general and independently of Samuelson's *Foundations*. This section will discuss the philosophy of science literature on unification and explanation, although it will only discuss the arguments of a few specific philosophers who have addressed these issues in ways that are directly relevant to economics. Section three examines the Samuelson's *Foundations* in detail in preparation for the discussion in sections four and five which combines the philosophical material in section two with the discussion of *Foundations* from section three and uses it to defend the claims in the first three bulleted points of the previous paragraph. Section six, as noted above, is more speculative. It proposes some connections between the unifying framework of *Foundations* and some of the major developments that have taken place within economics during the last few decades.

II. Explanatory and Derivational Unification

Ideas about the nature and importance of unification in scientific knowledge can be traced back to at least Immanuel Kant at the end of the eighteenth century and William Whewell's conception of the consilience of inductions from the nineteenth century: "jumping together" of phenomena from various sources (Morrison, 1997, 2000). That said, the account that is most relevant here is Philip Kitcher's work on explanatory unification (Kitcher 1981, 1985, 1989) and certain critical responses to it. Kitcher's account is related to his research on the philosophy of mathematics (Kitcher 1983), but was primarily a result of his attempt to circumvent various problems associated with the empiricist covering law model of explanation (Hempel 1965).

Although the difficulties of the covering law, or deductive-nomological, view of scientific explanation have motivated philosophers of science to turn toward versions of causal explanation, Kitcher resisted this move and continued to think about scientific explanation in broadly empiricist or epistemic – rather than causal, mechanistic, ontological, etc. – terms. In his words:

> … in the wake of logical empiricism, many philosophers of science have made free use of causal concepts, perhaps seeing themselves as shaking off ghostly chains that had seemed to bind their predecessors. Unfortunately, … there are deeper reasons for worrying about causal concepts than a desire to keep one's empiricist conscience pure. (Kitcher, 1989, p. 460)

The core idea of Kitcher's notion of explanatory unification is that good science should explain a lot with a little. More specifically, a particular argument pattern that can be used to deduce – i.e. derive – a large number of empirical statements (descriptions of phenomena) in a very concise and specific way is unifying, and explanation is associated with the best unifier. As Kitcher explained in a classic statement of his position:

> *Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute).* So the criterion of unification I shall try to articulate will be … a set of derivations that makes the best tradeoff between minimizing the number of patterns of derivation employed and maximizing the number of conclusions generated. (Kitcher, 1989, p. 432, italics in original)

Notice how Kitcher's approach tightly links derivational (deductive) unification with explanatory unification. This attitude has motivated at least one commentator to label Kitcher a "deductive chauvinist" (Woodward, 2003, p. 371). Kitcher defends his unificationist approach with examples from Newtonian mechanics and Darwinian evolutionary theory.

> When the view that explanation is unification is initially presented, I think that it strikes many people as invoking a rather ethereal ideal. However, in the examples I have discussed [Newton and Darwin], we do find that a single pattern of derivation (or several closely related patterns of derivation) is (are) used again and again to derive a variety of conclusions. Thus I take the examples to provide prima facie support for the view that unification is important to explanation and that unification works in the way that I have suggested. (Kitcher, 1989, p. 448)

This argument is particularly clear from Kitcher's comments on Newton's *Principia*:

> *Principia* had exhibited how one style of argument, one "kind of reasoning from mechanical principles," could be used in the derivation of descriptions of many, diverse, phenomena. The unifying power of Newton's work consisted in its demonstrations that one *pattern* of argument could be used again and again in the derivation of a wide range of accepted sentences … In searching for force laws analogous to the law of universal gravitation, Newton's successors were trying to generalize the pattern of argument presented in Principia, so that one "kind of reasoning" would suffice to derive all phenomena of motion. (Kitcher, 1981, p. 514)

For Kitcher, like Hempel, the primary goal of science is scientific understanding, but for Hempel an adequate scientific explanation provided scientific understanding. Kitcher adds an additional first step: unification. Derivational unification leads to explanatory unification which in turn leads to scientific understanding. And it does so, Kitcher argues, in a way that insulates it from various well-known criticisms of the covering law account of Hempel and others.[2]

Given this introduction to Kitcher's account of explanatory unification, we can now turn to the critical philosophical literature that will be used in the discussion of *Foundations* below. Although there is a rather extensive critical literature on Kitcher's unificationist account of explanation, the emphasis here will be on two main alternative accounts that are particularly relevant to economics in general and Samuelson's *Foundations* in particular. The first comes from Uskali Mäki. His account is emphasized because of its sharp contrast with Kitcher's position, the useful terminology he employs, and because he focuses directly on explanation and unification in economics (Mäki 2001a, 2009 and Mäki and Marchionni 2009). The second is Margaret Morrison's work on unification and explanation (Morrison 1994, 2000, 2002, 2006, 2015). Her examples come from physics and evolutionary biology, not economics, but the centerpiece of the argument presented in sections four and five will be that Samuelson's unificationist strategy in *Foundations* can be seen as a particular instantiation of Morrison's account of the separation of derivational and explanatory unification. The application of Morrison's arguments to *Foundations* will not be presented until those later sections, but her general account of the unification-explanation relationship will be introduced here.

Mäki recognizes that purely logical or mathematical derivational unification can play and important role in science, but however much practical usefulness it might provide, he argues it does not (alone) provide explanatory unification. Unification can be derivational or ontological and while these two can come together – and in some sense that would be an ideal case – they need not do so. *Ontological unification* is causal "based on the referential and representational capabilities of theories" and it is concerned with redescribing "diverse phenomena as manifestations (outcomes, phases, forms, aspects) of one and the same small number of entities, powers, and processes." (Mäki, 2001a, p. 498). *Derivational unification*, on the other hand, is entirely inferential, based on the derivational capabilities of a formal mathematical structure. Thus for Mäki, unification is only explanatory if it is unified at the ontological level.[3] Given the distinction between

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[2] It is not directly relevant to the main argument in this paper, but it is worth a note in passing, that Kitcher's later work – Kitcher (1993) and Kitcher (2003) in particular – moved in the direction of social epistemology and pragmatism. This is interesting because while Kitcher did not discuss economics in his work on explanatory unification, he did actively employ economic arguments in Kitcher (1993). As he said" "I shall employ an analytic idiom inspired by Bayesian decision theory, microeconomics, and population biology" (Kitcher, 1993, p. 305). It is also interesting that much of the argument in Kitcher (1993) endorses cognitive diversity – a scientific community "that is prepared to hedge its bets when the situation is unclear is likely to do better than a community that moves quickly to a state of uniform opinion" (Kitcher, 1993, p. 344). With respect to the intra-economics debates over unification versus pluralism, this puts Kitcher's later work in the pluralist camp. See Hands (1997) for additional discussion of Kitcher's use of economics.

[3] The problems associated with covering law approaches to explanation has encouraged the development of a number of different causal approaches to explanation within contemporary philosophy of science: causal mechanisms, ontic or ontological, kairetic, difference-making or interventionist, and many others.

(mere) derivational and ontological unification, the type of unification that Kitcher is concerned with is derivational – based on minimizing the number of derivational patterns, where the derivational "patterns are abstract schemes, instantiated in specific applications" (ibid., p. 494). Thus for Kitcher: "Explanation is not a matter of describing causal relations in the world, it is rather the other way around: causal relations are a function of explanatory relations" (ibid., p. 497). Mäki argues that Kitcher's account of explanatory unification need not lead to adequate scientific explanations; it could, but there is no guarantee. It could also lead to unifications that are just ontologically barren derivational exercises.

For Mäki, these distinctions help characterize the proper relationship between unification and scientific realism:

> Scientific realism can be taken to imply a constraint on preferred kinds of unification. If the accomplishment is mere *derivational unification* by way of deriving a large number of *explanandum* sentences from a parsimonious set of *explanans* sentences … this as such is not yet to be celebrated. The realist will hail an accomplishment that makes claims about the real world, not just about logical relationships between sentences. The goal and achievement should be *ontological unification*, whereby an explanatory theory unifies what previously appeared to be different kinds of phenomena by establishing an ontic unity between them, by showing that they are of the same kind after all. (Mäki, 2009, p. 87)

I will focus on Samuelson's economics in the next section, but at this point it is useful to note that Mäki and Marchionni (2009) have provided a detailed case study of unification within a particular subfield of contemporary economics. The subfield is geographical economics (GeoEcon). It involves, like most economic theorizing, derivational unification, but they argue that the forces of unification within the subfield are *not merely* derivational; GeoEcon also involves ontological unification. It involves a single causal mechanism driving the phenomena explained by the theory, in particular, explaining "a variety of kinds of agglomeration phenomena, such as industry clusters, core-periphery patterns among countries and regions, cities and systems of cities, patterns of international trade and specialization, and the causes of economic growth and development" (Mäki and Marchionni, 2009, p. 186). It is a case of explanatory unification in a particular field of economics because it is both derivationally and ontologically unified. GeoEcon thus exhibits both derivational and explanatory

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Since I am not contributing to this general debate there is no reason to pick, and thus privilege, one particular element from this fairly large set of causal explanatory conceptions. For the purposes of this paper it is only necessary to distinguish two broad classes of scientific explanation: those in the covering law tradition (and here only with Kitcher's unificationist account) and those that are causal/ontological/mechanistic. I will thus honor the author's own preferences with respect to the particular version of causal explanation being discussed.

unification in a complementary way: "we might characterize their respective roles by saying that unification has been *motivated* by ontology and *facilitated* by mathematics" (ibid., p. 189).

Morrison's account of the relationship between unification and explanation, like Mäki's, runs counter to Kitcher's account of the relationship between unification and explanation, is committed to a causal account of explanation, and recognizes that derivational explanation can be separated from causal explanation. Although her position differs from Mäki's in at least two respects. First, her argument draws quite heavily on the history of scientific practice in physics and biology, and second, her emphasis is more on what scientific unification does, or does not do (or has, and has not done) rather than on philosophically specifying exactly what does or does not count as a legitimate explanatory unification. As Morrison put it, questions about the character and function of unity in science "involve a philosophical dimension and an empirical dimension; in other words, we must begin by telling an empirical story about unity that is grounded in historical documentation before we can draw philosophical conclusions" (Morrison, 2000, p. 237).

Morrison's case studies of physics and the Darwinian synthesis – examples that Kitcher had used to tie unification to explanation – provide a quite different story about the unification-explanation nexus in these scientific fields. On her account unity comes from mathematical structures that provide derivational unification, but seldom provide a corresponding unification at the level of causality and explanation. As she puts it:

> … the most significant component in the unifying process is not a common explanatory mechanism … but the presence of a mathematical structure … that is a powerful enough to accommodate diverse phenomena within a common framework. (Morrison, 1994, p. 372)
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> Insofar as unity is achieved, for the most part, through the use of certain kinds of mathematical structures, we must look elsewhere for a detailed understanding of how the phenomena behave. The explanatory power simply does not reside in the unifying structure of the theory. (Morrison, 2000, p. 231)

It is important to recognize that derivational unification that does not lead to explanatory unification, does not imply that a unifying mathematical structure necessarily inhibits adequate causal explanations. The argument is simply that we can have derivational unification and causal disunity – that is more than one underlying causal mechanism can exist within a unified scientific theory – and therefore that the "mechanisms that allow you to unify are not necessarily the mechanisms that enable you to explain" (Morison, 1994, p. 372). Morrison argues this is clear from the disunity about the causal details of the selection mechanism in the early Darwinian synthesis:

> … when it came to the specific explanatory details at the foundation of this [Darwinian] unification, my claim is that they had fundamental disagreements about how that story should go; there was no single explanatory account of how selection operated in Mendelian populations. The point is not that there is no explanation in the early synthesis regarding the operation of selection, but rather that there is more than one, and neither can be identified with, nor is a product of, agreements concerning the mathematical aspects of the unification. (Morrison, 2006, p. 234)

We will discover in the next two sections that Morrison's account is an excellent description of the relationship between the diversity of causal mechanisms in economics and the unifying power of the mathematical formalism in Samuelson's *Foundations*.

Finally, Morrison's account means that if we are thinking broadly about the unification of a scientific theory – be it Newtonian or Maxwell's physics, the Darwinian synthesis, or textbook economics in the second half of the twentieth century – then unification, and the unification process, can take many different forms and as a result "the ways in which theory unification takes place and the role it plays in scientific context have little to do with how it has been characterized in traditional philosophical debates" (Morrison, 2000, p. 59). In other words, there are indeed "many faces of unity" (Morrison, 1994, p. 372).[4]

Let us now set the stage for talking about one particular face of unity by examining Paul Samuelson's *Foundations of Economic Analysis.*

III. Comparative Statics in Samuelson's *Foundations*

The decades after World War II were a period of extraordinary stabilization and unification within economics. The interwar period had been characterized by theoretical pluralism and political-economic diversity (Morgan and Rutherford, 1999), but beginning in the 1940s and continuing on through the 1970s, economics became increasingly more homogeneous in its theoretical foundations, standardized in its mathematical and statistical tools, and unified in its political-economic vision. While there remained various heterodox schools of economic thought, the relative size of the mainstream increased and the vast majority of mainstream economists came to see both economics and the economy through a new lens of unified theory and practice. To a previously unprecedented degree, economics became Kuhnian normal science. There is an extensive body of historical scholarship explaining the various social, political, and

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[4] The many faces of unity exhibited in different scientific fields parallels the diversity that Morrison and Mary Morgan emphasized in their work on scientific models (Morrison and Morgan 1999).

technological forces contributing to these changes (Amadae 2003, Bernstein 2001; Erickson et al. 2013; Mirowski 2002 and others), but at the level of pure economic theory, a key instrument of unification was Samuelson's *Foundations of Economic Analysis* (1947), a book that started out as Samuelson's Ph.D. thesis, but quickly became the cornerstone text for economic analysis in both research and graduate education:

> … among economists, Samuelson is Mr. Science. He is widely credited with establishing the scientific ideal in economics at the graduate and professional level with his 1947 *Foundations of Economic Analysis*. (Pearce and Hoover, 1995, p. 184)

Although *Foundations* played a key role in the scientific unification of economics, and it will be the main focus, it should be noted that Samuelson's consolidating impact was actually two-pronged. At roughly the same time that *Foundations* became influential, Samuelson's introductory textbook *Economics* (1948), was introducing the so-called *neoclassical synthesis* of neoclassical microeconomics and Keynesian macroeconomics (Keynes 1936) to the broader audience of U.S. college students.[5]

> Paul Samuelson's greatest contribution … was the neoclassical synthesis, of which he was the principle architect … This *Weltanschauung* reconciled the classical and Keynesian strands of his thinking and that of many of his contemporaries. It became orthodox doctrine for a generation of economists and for many of their students. (Tobin, 1983, p. 197)[6]

So economic science went through a period of unification and standardization, and *Foundations* played a key role in that process, but was it a successful example of the *explanatory unification* discussed in the previous section? Did it increase scientific understanding by providing a reliable blueprint for how "a single pattern of derivation (or several closely related patterns of derivation) is (are) used again and again to derive a variety of conclusions" (Kitcher, 1989, p. 448)?

I will argue that the answer to this question is both "yes" and "no." It is "yes" with respect to derivational unification, but "no" with respect to causal or explanatory unification. Samuelson sought, and provided, a unified mathematical framework for economic analysis, but he did not suggest, or even believe, that the analytical techniques in *Foundations* unified economics with respect to the objective forces at work in economic decision-making, the allocation of resources in a market economy, or with respect to the forces determining the overall level of economic activity. This of course

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[5] Samuelson officially introduced the term neoclassical synthesis in the 3rd 1955 edition of *Economics* and he continued to use it, although with some modification, until the 7th 1967 edition.

[6] There is an extensive literature, and debate, about the neoclassical synthesis going back to the 1950s. For a discussion of the relationship between Samuelson and the neoclassical synthesis that takes advantage of the recent Samuelson archival material, see Backhouse (2015b, pp. 146-150).

has implications for philosophical debates about unification and explanation, but it also has implications for the later development of economic science. These discussions require that we take a fairly close look at Samuelson's *Foundations* and exactly how it was, and was not, an instrument of unification in mid-twentieth century economics.

Foundations was literally a foundational book; Samuelson's goal was to improve the foundations of economic science by providing economists with an analytical framework that would help achieve that goal. Given the positivist spirit of the day, as well as Samuelson's own epistemological commitments, improving the foundations of the field meant increasing the *cognitive significance* of economic theory; in particular, to provide a set of techniques that could be used to derive cognitively *meaningful theorems* about changes in economic variables. As he explained in the introduction to *Foundations*:

> Only after laborious work … did the realization dawn upon me that essentially the same inequalities and theorems appeared again and again, and that I was simply proving the same theorems a wasteful number of times.
>
> I was aware, of course, that each field involved interdependent unknowns determined by presumably efficacious, independent equilibrium conditions … But, … it had not been pointed out to my knowledge that there exist formally identical *meaningful* theorems in these fields, each derived by an essentially analogous method ..." (Samuelson, 1947, pp. 3-4)

So the motivation was clearly unification, but what kind of unification: Derivational? Causal? Something else entirely? What exactly were the tools of economic analysis Samuelson offered in *Foundations* and why did he offer these particular tools?

The main tool of unification in *Foundations*, and why it constituted a general *foundation for economic analysis* rather than simply a particular application of economic analysis, was the method of *comparative statics*. The intuition behind comparative statics analysis is quite simple and remains an important part of economic analysis even today. Such analysis goes like this. Start with a system of equations in equilibrium and allow for one of the initial parameters to change; this will invariably – although through different causal processes in different economic theories – cause a change in the equilibrium values of the variables. As a result, one can *compare* (often just qualitatively) the new equilibrium with the initial equilibrium position. Start out in *static* equilibrium, disturb the equilibrium, and compare the new static position with the old static position: hence *comparative statics.* As Samuelson explained:

> This … is the method of *comparative statics*, meaning by this the investigation of changes in a system from one position of equilibrium to another without regard to the transitional process involved in the

adjustment. By equilibrium is meant here only the values of variables determined by a set of conditions …

This method of comparative statics is but one special application of the more general practice of scientific deduction in which the behavior of a system (possibly through time) is defined in terms of a given set of functional equations and initial conditions. (Samuelson, 1947, p. 8)

Of course in any particular application these equilibrium conditions would always be implied by (i.e. derived from) the relevant economic theory, but, as emphasized below, in *Foundations* Samuelson specified these equilibrium systems abstractly so they could accommodate a wide range of different economic processes. The derivational unification comes about because the mathematical structure and the process used to derive results from that structure is exactly the same for a large number of different underlying economic processes that could have generated the equilibrium equations.

Samuelson introduces the general system of equation at the heart of comparative statics derivations in *Foundations* in the following way:

> All of the above may be stated compactly in mathematical form. Given n variables or unknowns ($x_1, x_2, \ldots, x_n$,) and m, greater or less than n, parameters ($\alpha_1, \alpha_2, \ldots, \alpha_m$), we assume n independent and consistent functional relationships involving our variables and parameters. These may be written most generally in implicit form, each equation involving all variables and parameters. (Samuelson, 1947, p. 10)

The general system thus takes the following form:

$$\begin{aligned} f_1(x_1, x_2, \ldots, x_n,; \alpha_1, \alpha_2, \ldots, \alpha_m) &= 0, \\ f_2(x_1, x_2, \ldots, x_n,; \alpha_1, \alpha_2, \ldots, \alpha_m) &= 0, \\ \vdots \qquad\qquad & \vdots \\ f_n(x_1, x_2, \ldots, x_n,; \alpha_1, \alpha_2, \ldots, \alpha_m) &= 0, \end{aligned} \qquad (1)$$

where the x's are variables and the α's are parameters.

There is no restriction on the relationship between n and m, but the number of equations is necessarily equal to the number of unknowns (n). Such systems of equations appear in a wide range of different sciences and represent a wide range of different theoretical relationships. Economists are actually *more* likely to allow for the possibility of a very large number of variables and parameters than many other scientists, since there is no natural limit on things like the number of goods in an economy, the number of firms in an industry, etc. Economists are also *more* likely to specify the *general* form of such functions (i.e. f_i's) instead of specifying the equations explicitly ($16x_1 + 2.5x_2 - x_3^2 = 0$ or some such) since these equations generally come from theoretical contexts where the relevant explicit functions are unknown.[7] Economists are willing to assume, for example, that an economic agent bases his/her commodity choices on a subjective utility function – and that such a utility function has certain mathematical properties (continuity, transitivity, etc.) – but they generally *will not know* the agent's actual utility function (even approximately). Samuelson implicitly assumed that all of the variables (x's) and parameters (α's) were observable – at least under ideal circumstances – and that correspondence rules existed which could map the observable x's and α's back into the theoretical language of the relevant economic theory.

Given such a system of equations, the *meaningful theorems* that Samuelson sought were comparative statics results.[8] It is important to notice that since the number of variables or parameters is potentially quite large (and in the most general case, an unknown but finite number), and the equations are not explicitly given, it is generally *impossible to solve the system explicitly in order to directly determine the functional relationships between the parameters and the equilibrium values.* It was therefore standard to simply write the equilibrium values as a general system of n implicit functions:

$$x_i^*(\alpha_1, \alpha_2, \dots, \alpha_m) \text{ for } i = 1, 2, \dots, n.^9 \qquad (2)$$

Given this characterization of the equilibrium solutions, the comparative statics impact of a change in one of the parameters, say α_k, would be given by $\partial x_1^*/\partial\alpha_k$, $\partial x_2^*/\partial\alpha_k$, … , $\partial x_n^*/\partial\alpha_k$ in the case of differentiable functions (the majority of the results in *Foundations*). Although in rare cases – more likely in low ($n \leq 3$) dimensions – the model might have enough structure to derive specific expressions for these comparative statics terms; it would of course require a more explicit specification of the functions in (1).

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[7] Perhaps it is more accurate to say that *economists of Samuelson's generation* were more likely to specify general forms than explicit functions, since this seems to be changing in recent decades. There are many reasons for this – the rise of game theory (with its explicit case-by-case approach), experimental and behavioral economics (where the functions often come from laboratory or field data), the ability of computers to actually solve large systems of explicit equations that would have been impossible for Samuelson's generation, and others – but in any case, theoretical practice in economics now seems to be much less committed to the type of general functional specifications that were used in *Foundations* and became the mainstay for most mid-twentieth century mainstream economic theory.

[8] Although Samuelson was generally unwilling to talk about the relationships between the various x's, or the x's and the f_i's, in causal terms, because they were mutually determined by the system of equations, he was willing to say that that the changes in parameters *caused* the resulting change in equilibrium values of the variables: "The only sense in which the use of the term causation is admissible is in respect to changes in external data … it may be said that changes in there *cause* changes in the variables of the system" (Samuelson, 1947, p. 9).

[9] The *existence* of the solutions, the x_i^* functions, is guaranteed by the implicit function theorem (Samuelson, 1947, p. 48, 259). In some cases the relevant condition (a non-vanishing Jacobian matrix) was implied by the structure of the underlying economic theory, but in other cases it was simply assumed. If the conditions of the implicit function theorem are fulfilled – one way or another – the solutions to the system of equations will always exist (and generally inherits the mathematical properties – continuity, differentiability, etc. – of the parent equations), although *actually finding a specific solution was typically impossible* given the very general nature of the equations.

But, *in general* all that can be deduced from any particular comparative statics exercise is *qualitative information*: the signs (+, -, or 0) of the comparative statics terms. As Samuelson said: the technique is "A Calculus of Qualitative Relations" (Samuelson, 1947, p. 23). As a result, the vast majority of comparative statics results simply predicted whether a parameter change would cause the equilibrium values of each of the variables to increase, decrease, or remain the same. Samuelson saw such generality as a strength, not a weakness: "It is precisely because theoretical economics does not confine itself to specific narrow types of functions that it is able to achieve wide generality" (Samuelson, 1947, p. 11).

This comparative statics technique became the derivational backbone for a large portion of economic research as well as graduate education from the early 1950s on through most of the 1970s. Regarding economics education, *Foundations* was frequently used as a graduate textbook, but there were several graduate textbooks – e.g., Henderson and Quandt (1971), Silverberg (1978), and others – that almost exclusively employed Samuelson's technique, but presented it in a more student-friendly way. So the comparative statics technique of *Foundations* helped bring a new unification to economics, but again, exactly what kind of unification was it? To answer this it is necessary to look more closely at his comparative statics technique. These details will be discussed using only minimal mathematics, but a more detailed discussion of the mathematical results – along with two economic examples – is available from the author.[10]

IV. Unification and Pluralism in Samuelson's *Foundations*

The first thing to notice is that even if the system of equations in (1) is mathematically well-behaved, and could be "solved" for the generic equilibrium values as in (2), without any additional structure there is no reason to believe that any of the comparative statics terms could be determined (even with respect to sign). The abstract system (1) is informationally barren; it can only sing – provide qualitative comparative statics information – when the equations are given the additional mathematical structure provided by a particular economic theory or process. Comparative statics information comes from *additional mathematical structure imposed on the equilibrium system by the causal details of the relevant underlying theory.*

The most important additional information for the analysis in *Foundations* concerns the origin of *the equilibrium equations*. In *Foundations* this information came from *two, general, but quite different,* sources. The first type of information came from an *underlying optimization problem* either maximization or minimization. Samuelson called such models *extremum* problems, and for such cases the equilibrium equations were given by the first order (necessary) conditions for the optimization problem (vanishing first

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derivatives). Samuelson dedicated Part I of *Foundations* to optimization-based systems "in a reasonably exhaustive fashion" (Samuelson, 1947, p. 5).

For example, in the case of a profit (π) maximizing firm using two inputs (x_1, x_2) with input prices given (w_1, w_2), the profit function would be $\pi(x_1, x_2; w_1, w_2)$ and the first order conditions are given by:

$$\pi_1(x_1, x_2; w_1, w_2) = 0 \text{ and } \pi_2(x_1, x_2; w_1, w_2) = 0, \text{ where } \pi_i = \partial\pi/\partial x_i \text{ for } i = 1,2.$$

These two equations constitute the equilibrium conditions (1) for this particular maximization problem.

Comparative statics results from such optimization problems only constituted Part I of *Foundations*, but they received a significant amount of attention over the years and were the main focus of Samuelson's Nobel lecture (1972). One reason for the attention is that definitive comparative statics results are relatively easy to obtain for optimization-based models because of the mathematical structure of such models. Optimization not only requires first order conditions, it also requires second order conditions (to guarantee the desired maximum or minimum, rather than some other critical point) and these additional mathematical restrictions provide structure which facilitates signing the comparative statics expressions. These second order conditions were sometimes just assumed – under the guise of the maximization hypothesis – and sometimes they followed from the properties of the underlying economic theory (diminishing marginal returns, etc.).

For example, in the case of the above profit maximizing firm, the second order conditions provide the following additional information:

$$\pi_{11} < 0, \pi_{22} < 0, \text{ and } \pi_{11}\pi_{22} - \pi_{12}\pi_{21} > 0, \text{ where } \pi_{ij} = \partial^2\pi/\partial x_i\partial x_j \text{ for all } i,j = 1,2,$$

which is often sufficient to sign the comparative statics terms such as $\partial x_1^*/\partial w_1$ and $\partial x_2^*/\partial w_1$ where the (*) indicates profit maximizing values. The second order conditions thus provide a substantial increase in the amount of comparative statics information available and such information is not generally available in non-optimizing based models.[11] As Samuelson noted many years later in a commentary on *Foundations* – if he had focused exclusively on the first, optimization-based, applications of comparative statics: "The result would have been a shorter 200-page book with one fully-integrated theme" (Samuelson, 1998, p. 1384).

While Samuelson made it clear that while extremum problems were very important in economics, and they represented an important part of the comparative statics results in *Foundations*, they were only one part of the story; there was much more to *Foundations*

[11] Of course there are particular special cases where such information is available such as in the gradient system Samuelson discusses at the end of ch. 4 of *Foundations* (1947, pp. 52-56)

than optimization. Part II of *Foundations* provided comparative statics results that were derived from a system of equilibrium equations that were *not* based on optimization: systems where "there is no possibility of directly reducing the problem to that of a maximum or minimum" (Samuelson, 1947, p. 5). The causal forces behind certain types of economic phenomena involve a *dynamic process* where the relevant variables change in a systematic way through time (t).[12] The mathematical specification of such models – the source of the equilibrium equations (1) – are not first order conditions, but rather differential or difference equations. It is important to note that even much later in his Nobel lecture which emphasized optimizing systems, Samuelson was clear that such systems did not exhaust crucial economic analysis:

> I must not be to imperialistic in making claims for the applicability of maximum principles in theoretical economics. There are plenty of areas in which they simply do not apply. Take for example my early paper dealing with the interactions of the accelerator and the multiplier … This is an important topic in macroeconomics analysis … (Samuelson, 1972, p. 258)

In such dynamic systems, the additional mathematical structure necessary to derive determinate comparative statics results generally came from the supposition that the underlying dynamic system is *stable*, that is, converged to the equilibrium values as $t \rightarrow \infty$. Samuelson called this relationship between comparative statics and stability the *correspondence principle,* and Part II of *Foundations* contained many examples of determinate comparative statics based on the correspondence principle. For Samuelson, the "relationship between the stability conditions of dynamics and the evaluation of displacements in comparative statics, provides the second great weapon in the arsenal of the economist, interested in deriving definite, meaningful theorems"(Samuelson, 1947, p. 350). He explained the two separate parts and why they were needed early on in *Foundations*:

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12 Of course "time" means many different things to different economists working in various areas of economics and with different practical interests. This has always been, and remains, true, but Samuelson's characterization of *dynamic* as any mathematical specification involving differential or difference equations helped stabilize the language and modeling within mathematical economic theory during the middle of the twentieth century (Weintraub 1991). In particular, many economists during the early twentieth century had thought about optimization in terms of movement through the choice space in the direction of the optimal point (by the way, this is, because of the way computers "find" an optimal value, more like the way that some contemporary economists think about optimization), but in the stabilization that came after *Foundations,* economists, at least those involved in mathematical modeling, generally adopted Samuelson's non-dynamic view of optimization: the optimal is simply characterized by first and second order conditions. Nicholas Georgescu-Roegen (1968, p. 255) provided a very nice way of visualizing the difference between the way the equilibrium is reached in an extremum problem and the way the equilibrium is reached in a dynamic problem; the former is like a bird who surveys from above and then "dives directly at the most preferred spot" while the latter is like a worm which, "from any position, chooses some direction and then moves along it" toward the equilibrium. See Hands (2010) for more discussion.

> In this study I attempt to show that there do exist meaningful theorems in diverse fields of economic affairs … They proceed almost wholly from *two types of very general hypotheses*. The first is that the conditions of equilibrium are equivalent to the maximization (minimization) of some magnitude. Part I deals with this phase of the subject ...
>
> However, when we leave single economic units, the determination of unknowns is found to be unrelated to an extremum position. In even the simplest business cycle theories there is lacking symmetry in the conditions of equilibrium so that there is no possibility of directly reducing the problem to that of a maximum or minimum. Instead the dynamical properties of the system are specified, and the hypothesis is made that the system is in "stable" equilibrium ... By means of what I have called the *Correspondence Principle* between comparative statics and dynamics, definite *operationally meaningful* theorems can be derived from so simple a hypothesis. (Samuelson, 1947, p. 5, emphasis added)

Although Samuelson analyzed many different examples of such dynamic models during the course of his long and very productive career, the two types of non-optimization based models he emphasized specifically in *Foundations* were "various simplified versions of the Keynesian system" and "the *general equilibrium* equations of Walras" (Samuelson, 1947, p. 139).[13] The dynamics of such models are driven by different causal forces, but they do "not arise from an extremum problem and … cannot be converted into this form" (ibid.). These forces are generally modeled in terms differential equations.

It is useful to offer an example of such dynamic models, and although the Keynesian example from *Foundations* (chapter 9, pp. 276-283) – originally in Samuelson (1941) – could be used, it seems helpful to use a Walrasian example instead. There are two reasons for this. One is that while the importance of Samuelson's comparative statics technique to the development of Keynesian macroeconomics is fairly well established, his work on Walrasian multiple market stability receives less attention even though it played a key role in the literature on Walrasian stability that flourished in the late 1950s and 1960s.[14] The second is that it makes it clear that the theoretical diversity

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[13] Perhaps it is just coincidence, but nonetheless interesting that the cover of the 1965 Atheneum paperback edition of *Foundations* had the results of a comparative statics analysis of a Keynesian system on its front cover (not a microeconomic optimization-based model). Since the publication date of 1965 puts it in the heyday of the Keynesian revolution and the 1964 Kennedy-Johnson tax cut, perhaps it was good marketing to emphasize the Keynesian non-optimization Part II side even though *Foundations* was considered to be a microeconomics book.

[14] For example, Arrow and Hurwicz in their early and very influential paper on Walrasian stability say that: "The concept of stability, … in the modern sense, did not receive systematic treatment in the context of economic dynamics until Samuelson's paper of 1941" (Arrow and Hurwicz, 1958, p. 522) and also relate stability to his correspondence principle (p. 530). In fact, Arrow and Hurwicz refer to Samuelson at least a dozen times in the 1958 stability paper and it is always to *Foundations* or Samuelson (1941 and 1942) which became chapters 9 and 10 of *Foundations*. Samuelson's presentation of Walrasian stability in

accommodated by Samuelson's *Foundations* extends beyond the two categories of optimization-based microeconomics and Keynesian macroeconomics.

Although the Walrasian general equilibrium models of the period were generally n-good competitive equilibrium models, a simple two-good version will suffice here. A dynamic two-good Walrasian general equilibrium model typically involved the so-called Walrasian tâtonnement price adjustment mechanism where positive excess demand (demand > supply) would increase the price of a good and negative excess demand (demand < supply) would decrease the price. If the prices of the two goods at time t are given by $[p_1(t), p_2(t)]$, the excess demands by $z_i(p_1, p_2; \alpha)$ for i = 1,2, and α is a common shift parameter, then the tâtonnement price adjustment mechanism can be given by the system of ordinary differential equations:

$$dp_1/dt = z_i[p_1(t), p_2(t); \alpha] \text{ for } i = 1,2. \qquad (3)$$

The equilibrium of this dynamic system – the analog of (1) in this case – would then be the system of equations:

$$z_1[p_1^*, p_2^*; \alpha] = 0,$$
$$z_1[p_1^*, p_2^*; \alpha] = 0,$$

where p_1^* and p_2^* are the equilibrium prices. The comparative statics exercise is to determine – or at least sign – the terms $\partial p_1^*/\partial\alpha$ and $\partial p_2^*/\partial\alpha$, and the assumption that the system of differential equations in (3) is *stable* provides the additional structure needed in the same way that second order conditions provided the additional structure in optimization-based models.

Part II of *Foundations* and the Correspondence Principle are extremely important for the argument here because it makes it clear that Samuelson's unification came at the derivational level, at the level of mathematical structure, and not at the level of the underlying causal forces. *Foundations* was unifying, but not all economic phenomena could be unified by the same causal mechanism because there was more than one account of the origin of equilibrium conditions in mid-twentieth century mainstream economics.

An important aspect of the story is that the two parts of *Foundations* reflect the view of Samuelson and may others of his generation, that what came to be called the neoclassical synthesis was only a synthesis at the formal derivational level and not at the level of underlying causal forces. For many economists the failure of the pre-Keynesian economics to understand or offer policy solutions for the Great Depression was taken as evidence that economic science needed a macro-level theory of real economic forces – that was not reducible to microeconomic optimization – and that the *neoclassical synthesis* meant two different causal levels, micro and macro, and not a

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Foundations was also given attention in the stability sections of advanced textbooks on general equilibrium theory and mathematical economics (for example Takayama, 1974, pp. 314-319).

reduction of the latter to the former.[15] Both could be subsumed at the level of analytical technique under the umbrella of comparative statics, but the relevant causal mechanisms at work in economic life were of (at least) two different types. As Samuelson explained in his re-examination of *Foundations* in 1998:

> I could not resist the temptation to add Part Two on dynamics, even though much of my focus there was on "macroeconomics" (a word not yet coined …). No one associates a Keynesian system with a maximizing single mind or even to an as-if-pretend maximizing system. Yet from considerations of *The General Theory* "stable" dynamics, one could predict that a rise in the propensity to invest would increase, not lower, … equilibrium output and GNP. Why that might be possible needed to be researched in the late 1930s." (Samuelson, 1998, p. 1384)

Although all of the emphasis on optimization-based models (in Part I) and Keynesian macroeconomic models (in Part II) is important to discuss, it is also important to remember that – as the Walrasian case demonstrates – Part II of *Foundations* was concerned with dynamic models that cannot be reduced to optimization and *not* just Keynesian models.

This sentiment is echoed by Roger Backhouse, Samuelson's biographer (Backhouse 2017), who notes that while most economists seem to believe that all of *Foundations* was optimization-based, recognition that there is more to economics than optimization ran quite deep in Samuelson's thinking and is at the core of *Foundations*:

> Central to Samuelson's book was the idea that there were common mathematical structures underlying different problems, both within economics and across disciplines. Operational theorems could be obtained by analyzing the properties of the appropriate equilibrium systems. For problems involving consumers and the firm, this involved maximization and hence, the use of second order conditions. For problems involving aggregates where optimization was not involved, comparative statics results could be derived by assuming that the equilibrium was stable … *Contrary to popular belief* … *Foundations* reflected the view that *there was much more to economics than optimizing behavior*. Macroeconomics required different foundations, for aggregate behavior could not be explained as the outcome of optimizations: hence the need for the correspondence principle … " (Backhouse, 2015a, p. 347, emphasis added)

One additional point about the significance of Part II and the derivational unification of both optimizing and non-optimizing economic systems, has to do with the difficulty of

[15] This is also exhibited clearly in Samuelson's negative attitude about the use of a single representative agent in market or macroeconomic models. See Hands (2016) for more discussion.

doing stability analysis. Given the tools of the day, both the mathematics involved and the paucity of derivational results available from non-optimizing systems, made such models much more difficult to work with than optimizing models.

> I think I have said enough to demonstrate perhaps the hardest part of my 1947 *Foundations of Economic Analysis* had to deal with the statics and dynamics of nonmaximizing systems. (Samuelson, 1972, p. 259)

Why is this important? Because it demonstrates the deep commitment Samuelson had to including non-optimizing economic systems – such as Keynesian and Walrasian economics – into the unification of *Foundations*. Part II was not added as an afterthought, or because it was particularly mathematically tractable (i.e. easy). It was added because Samuelson, like many others, believed that many important parts of economic life could not be adequately modeled as optimizing systems.

So Samuelson's experience of the Great Depression and his commitment to Keynesian theory and non-optimizing economic processes ideas were not one part of Samuelson's economics and the mathematical machinery of *Foundations* another; *they were simply two aspects of the same scientific project of the young Samuelson*. The unity at the mathematical level accommodated the stabilization of the profession in a more unified and normal-science-like way than had previously been the case and yet the unity was erected on a commitment to the idea that a deep and policy-reliable understanding of the forces at work in the modern economy required more than one conception of the way the economic world works. Samuelson's unity is a case where the derivational unity provided by the shared analytical machinery actually accommodated a type of causal an explanatory disunity.[16]

As Samuelson put it, the comparative statics method of analysis can be successfully employed "throughout the whole field of theoretical economics including monetary theory and business cycle theory, … In fact, any sector of economic theory which cannot be cast into the mold of such a system must be regarded with suspicion as suffering from haziness" (Samuelson, 1947, p. 9). This is, in effect, his way of saying: i) Keynesian, Walrasian, and other non-optimization-based economic theories can be a legitimate part

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[16] While I have spoken freely about the "Keynesian" economics of Samuelson and the mainstream economists of the period as representing a kind of pluralism, it is only fair to note that many heterodox economists – primarily, but not exclusively, Post-Keynesian economists – did not and do not see it that way. They have traditionally denied that this version of Keynesian economics, the so-called IS-LM Keynesianism(Hicks 1937), was at all consistent with the ideas in Keynes' *General Theory*. Critics have several different terms for it, including: hydraulic Keynesianism (Coddington, 1976), Keynes' aborted revolutionary theory (Davidson, 2008), and bastard Keynesianism (Robinson, 1975). Pluralism is always, to some extent, in the eye of the beholder. For the young Paul Samuelson, coming from the his undergraduate education at the University of Chicago, a mainstream economics that accommodated non-optimizing models like Keynesian macroeconomics were, from his point of view, pluralist. From the point of view of those Cambridge economists in on the ground floor of Keynes's *General Theory*, and others with similar political economic perspectives, the neoclassical synthesis was not pluralist at all.

of scientific economics, but ii) whatever kind of theory an economist is doing, he/she had better be able to demonstrate its results by means of (cast it in "the mold of") the comparative statics technique in *Foundations*: and if not, perhaps one should be suspicious about it. Although Samuelson himself would have no interest in such a philosophical assessment: this is derivational unification in the form of his comparative statics technique supporting, to an important degree, causal and ontological disunity and the level of the explanatory forces at work within the modern economy and the policy problems it presents. And this makes Samuelson's *Foundations* an excellent example of Morrison's conception of derivational unification without explanatory unification.[17]

V. Derivational Unification without Explanatory Unification

The previous section made the case that Samuelson's *Foundations* fits Morrison's notion of derivational unification sans explanatory unification extremely well. This section will defend the point more explicitly. First, by discussing the influence of certain physicists, particularly E. B. Wilson, on Samuelson's early thinking about the relationship between mathematics and science. And second, by pointing out one minor difference between Morrison's account and Samuelson's *Foundations*, that, strangely enough, seems to highlight their similarity.

The arguments in sections three and four regarding the distinction between derivational and explanatory unification were based primarily on details about the contents of *Foundations*, as well as the impact that the Great Depression and the Keynesian revolution had on Samuelson and many other economists, but since Samuelson's death in 2009 and the establishment of his extensive archives at Duke University, there is now

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[17] In closing this section I would like to note recent research arguing that at least one part of Samuelson's economics – his revealed preference theory starting with Samuelson (1938) – could be better understood, and better protected against some popular criticisms, if defenders adopted a unificationist view built on Kitcher's conception of explanatory unification. The paper is Kate Vredenburgh's "Unificationist Defense of Revealed Preference" (Vredenburgh 2020). She makes it clear that the paper is not trying to argue that revealed preference is better in some way than other approaches to choice behavior; the argument is conditional: "if revealed preference approaches are combined with unificationism about explanation, then these frameworks can escape the explanatory objections" (p. 151). Given this conditional goal, I generally agree with the argument. I might wonder about that particular goal, but that is a topic for another time. The revealed preference theory Vredenburgh is concerned with is the empirical revealed preference theory associated with the generalized axiom of revealed preference (GARP) rather than Samuelson's original 1938 weak axiom of revealed preference (WARP). GARP is clearly the right version to focus on for contemporary economics since it is explicitly empirical and able to accommodate current data and computational techniques. See Diewert (1973) and Varian (1982, 2006) for a discussion of the theoretical developments and Hands (2013) for some of the philosophical issues. Of course this contemporary version did not exist in the 1940s, but even the 1938 version of revealed preference theory really had no role in *Foundations* – it gets only in two pages in the consumer choice chapter (chapter five) and in a few more pages in the section on index numbers (chapter six) – and it does not involve the comparative statics technique that is at the core of *Foundations*. The question of how revealed preference, unification, and explanation all fits together is both interesting and important, it is just outside the scope of the current paper.

a substantial amount of information about Samuelson's early scholarly influences, particularly those of certain physicists, that provide us with a better understanding of what Samuelson was trying to do in *Foundations*. The physicist/mathematician that has rightly received the most attention in this regard is Edwin Bidwell Wilson. Samuelson had a very close relationship with Wilson and his views about the relationship between what would now be called derivational and causal unification had a major influence on Samuelson when he was writing *Foundations* (Backhouse, 2017; Carvajalino 2018, 2019a, 2019b). As Samuelson explained:

> Perhaps most relevant of all for the genesis of *Foundations*, Edwin Bidwell Wilson (1879-1964) was at Harvard. Wilson was the great Willard Gibbs's last (and, essentially, only) protégé at Yale. He was a mathematician, a mathematical physicist, a mathematical statistician, a mathematical economist, a polymath who had done first-class work in may fields of the natural and social sciences. I was perhaps his only disciple … *I was vaccinated early to understand that economics and physics could share the same formal mathematical theorems … while still not resting on the same empirical foundations and certainties*. (Samuelson, 1998, p. 1376, emphasis added)

If economics and physics can "share the same formal mathematical theorems" and yet not be "resting on the same empirical foundations and certainties," then why not take this same approach to different parts of economic science: most significantly optimization-based neoclassical microeconomics and non-optimization based economics like Keynesian macroeconomics? Foundations demonstrated that such an extension to different theoretical and causal processes within economics was entirely possible if one employs the right mathematical tools. That which works successfully between physics and economics can work successfully between different subfields within economic science.

Backhouse has made this case specifically regarding Samuelson's use of the Le Chatelier Principle: an implication the comparative statics for extremum problems (Samuelson, 1947, pp. 36-39, 81, 168):

> The key figure in leading Samuelson to this conception of the unification of economic theory was his mathematical economics teacher, Edwin Bidwell Wilson … In alternate years he taught graduate courses in mathematical statistics and mathematical economics, both of which Samuelson took.
>
> Wilson did not just teach these topics, but would stay on for an hour or more after lectures, talking about anything and everything. One of the subjects he covered was thermodynamics, no doubt inspiring Samuelson to take a course in the subject … One of the lessons Wilson taught Samuelson was that different systems might share a common

> mathematical structure. He introduced Samuelson to the Le Chatelier Principle, governing the way in which chemical equilibrium changes when a system is subject to external changes. It was possible to work out certain results concerning chemical interactions without knowing anything about the substances concerned simply by knowing that the system was in equilibrium. The Le Chatelier principle, though derived in chemistry, could be generalised to apply to any equilibrium system, whether chemical, thermodynamic or economic. Generality lay in the underlying mathematical structure." (Backhouse, 2015b, p. 145)

It seems that both Samuelson's influences from the physical sciences as well as his conception of the difference between the causal forces at work in certain areas of economics contributed to the particular character of the unification that *Foundations* supported.

An additional point can be made about comparative statics and certain mathematical techniques in physics, although it is less direct. Samuelson consistently noted James Clerk Maxwell as a source of inspiration. For example, when Samuelson was discussing integrability condition in his Noel lecture he noted: "In thermodynamics such reciprocity or integrability conditions are known as Maxwell Conditions; in economics they are known as Hotelling conditions in honor of Harold Hotelling's 1932 work" (Samuelson, 1972, p. 253). Why this is relevant is that historians of science have argued that Maxwell himself often saw his own unification of physics in derivational or mathematical terms rather than in terms of the underlying causal forces. For example, Morrison argues that Maxwell's emphasis "was on mathematical rather than physical similarity, a resemblance between mathematical relations rather than phenomena or things related" (Morrison, 2000, p. 65); this was particularly the case with Maxwell's use of the Lagrangian formalism: "What I want to suggest is that the nature of the Lagrangian formalism prohibits any move from what appears to be a reductive unity at the theoretical level to an accompanying ontological unity. The reason is simply that one can achieve a level of theoretical unity using a Lagrange approach *exactly because* one need not take account of the underlying causes that produce the phenomena" (ibid., p. 78). This seems to be the case for Samuelson as well. His comparative statics technique provided an analytical unity precisely because it "need not take account of the underlying causes that produce the phenomena."

One final point about the relationship between Morrison's account of unification and Samuelson's *Foundations* that reveals a difference between the two approaches, but a difference that suggests that Samuelson may have had an even stronger commitment to the "decoupling of unification and explanation" (Morrison, 2000, p. 2) than the natural scientists that Morrison discusses. Morrison's argument is that unification in successful science comes from a unifying mathematical structure and that unifying structure often has little or nothing to do with the causal forces used to explain phenomena in the relevant domain. This is not to say that there are no causal forces, only that they often do not hook up on any systematic way with the unifying mathematical structure and/or that there may be more than one causal force within a particular scientific theory. But this said, it doesn't seem like the scientists that Morrison discussed sought a particular derivational structure necessarily *because* it was able to unify a number of different causal forces. For Samuelson it was the diverse nature of the causal forces at work in economic life that drew him to the comparative statics formalism. The pluralism was built in on the ground floor, just not something that one was willing to live with once the scientific edifice was assembled.

VI. Conclusion

It seems somewhat inappropriate to provide so much detail about *Foundations* and the role it played in the unification of modern economics, and even make a passing comment at the beginning of the paper about economics being much less unified today, and yet conclude without saying anything about what happened between then and now. Although these events in the history of economics are both too recent and too complex to attempt to analyze in any detail here, a few bold features can be sketched quickly. I want to be clear that I consider these remarks to be much more speculative than the arguments presented thus far.

For the period between the early 1980s and the end of the century, it seemed that economics was moving steadily in the direction of explanatory, not just derivational, unification. Keynesian economics was abandoned by macroeconomists and replaced by a series of research programs more grounded in optimization-based microeconomics (so-called microfoundations) including new classical macroeconomics, real business cycle theory, and later dynamic stochastic general equilibrium (DSGE) theory. Walrasian general equilibrium theory also changed, away from the abstract heterogeneous agent versions of the middle of the twentieth century to versions which often assume the economy has a single representative agent (as is the case with many DSGE models). Dynamic economics was very popular but for the most part it employed intertemporal optimization-based models and not the differential and difference equation models in Part II of *Foundations*. Finally, there was a movement away from competitive markets, and certainly dynamic price adjustment in competitive markets, to game theory with strategic interaction by rational individual agents. This is of course painting recent history of economic thought very quickly and with a very broad brush, but all of these things involve a movement away from the theories and associated causal mechanisms of Part II of *Foundations* and a movement toward the theories and causal mechanisms of Part I. Moving toward the end of the twentieth century optimization and individual rational choice steadily became the only explanatory game in mainstream economic theory.

But by the end of the twentieth century, the momentum in economics appears to have begun moving in the opposite, more pluralist, direction. Behavioral economics and its many empirical anomalies to rational choice theory challenged the *homo economicus* view of individual decision-making that had dominated the discipline since early in the twentieth century.[18] Economics has become much more applied and empirical during the last few decades.[19] Some of this is certainly the big data and fast computers that have had such a profound impact on almost every aspect of our lives, but that is not the entire story. Some of the motivation seems to stem from a general dissatisfaction with exclusively optimization-based economic theory that has moved some economists in new theoretical directions, and moved still others in the direction of an almost a-theoretical type of empirical research. New theoretical frameworks and new tools of analysis, some unknown and some just previously unused, have flourished and become part of standard economics: including agent-based models, simulation, randomized controlled trials, survey data, happiness studies, neuroeconomics, evolutionary economics, as well as a number of different versions of experimental and behavioral economics (including behavioral welfare economics). There has been talk about a substantive turn in economics (Colander, Holt, and Rosser, 2008; Davis, 2007) for over a decade:

> We believe that this process is important because economics is currently at a turning point; it is moving away from a strict adherence to the holy trinity – rationality, greed, and equilibrium – to a more eclectic trinity of purposeful behavior, enlightened self-interest, and sustainability … What is new is that we are now arriving at the point where the changes are recognizable to individuals outside the profession. Thus, we are seeing more and more articles in the popular press on aspects of the new economics – behavioral economics, agent-based modeling, evolutionary game theory, and experimental economics. (Colander, Holt, and Rosser, 2004, p. 1)

So suppose we accept the description I have offered in the previous two paragraphs: both the narrowing toward explanatory unification toward the end of the century and a more pluralist tendency in recent years. How might one explain, or even begin to understand, these changes through the prism of *Foundations* and the general arguments that have been made in this paper? There are probably many possible stories, but let me close with just one.

Recall that Mäki was critical of derivational unification without ontological unification. But one additional part of his argument was that successful explanatory unification involves not treating a particular type of unification as a constraint, but rather discovering, or at least providing reliable evidence that, the unification is in fact how the world works (Mäki 2001b). If a unification is treated as a constraint then explanations that rely on other causal mechanisms may be dismissed as ad hoc (Mäki,

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[18] Behavioral economics is sufficiently well-known that standard references are probably not necessary, but I would note Dhami (2016): a 1700+ page advanced textbook in behavioral economics, titled, perhaps not coincidentally *The Foundations of Behavioral Economic Analysis*.

[19] See for example, Backhouse and Cherrier (2017), Biddle and Hamermsh (2017), and Davis (2019).

2001a, p. 503) independently of whether they have been examined and tested to see if they are relevant or not. When it comes to explanatory unification, the unifying power of a theory will "have limits that are based *on the degree of ontic unity of its domain*" (Mäki, 2001b, p. 504, italics added). In other words: "unification is contingent upon factual discoveries about causal structures in the world rather than being constitutive of explanation" (ibid.).

Perhaps this applies to the developments sketched above. First, mainstream economics gradually adopted the idea that the *only* causal mechanisms worthy of attention were optimization-based. As such, optimization became an ad hoc constraint on acceptable economic theorizing, rather than a source of discovery about the way the economic world works. Now suppose that Samuelson was actually right and the *economic world really is causally disunified* and requires different economic theories that are based on fundamentally different underlying causal mechanisms. Perhaps the narrow focus on optimization proved to be inadequate to the various tasks of predication, explanation, and policy advice that are expected of economic science and that helped precipitate the broad-based pushback against rationality and optimization represented by the more pluralist turn during the last few decades.

Certainly the situation in macroeconomics after the 1980s would suggest optimization was treated as a constraint rather than as a discovery of an important causal mechanism at work in the macroeconomy. Even macroeconomists defending models that were based on a single maximizing representative agent tend to do so in a relatively ad hoc way by praising the simplicity, consistency with microeconomic foundations, and derivational advantages of such models, rather than the discovery of such a representative agent (or even the discovery of additional predictive power from an as-if version of such an agent). For example as Jaakko Kuorikoski and Aki Lehtinen note in their philosophical investigation of ad hocness in DSGE macro-modeling, such a defense seems to be ubiquitous among contemporary macroeconomists:

> The response to criticisms targeted at the shared theoretical core of the DSGE model is swift and unambiguous: it is completely useless to criticize the model platform itself. DSGE is taken as the only reasonable choice, the best available tool for reasoning about interrelated multiple markets in a quasi-quantitative way. The common blueprint is seen as a necessary condition for a meaningful and theoretically rigorous macroeconomic discussion. (Kuorikoski and Lehtinen, 2018, p. 258)

Using Mäki's terminology, this is treating microfoundations and optimization as an ad hoc constraint rather than as an instrument for discovery of the way the world works. Given this, the pushback against DSGE modeling seems unsurprising.[20]

[20] See Duarte (2012), Hoover (2012, 2015), and Kuorikoski and Lehtinen (2018) and the literature cited therein.

Granted, this is, as admitted, only a brief sketch of a part of the recent history of modern economics. There is no claim that it is definitive, complete, or anything else one might look for in a serious history of a specific science. Nonetheless it seems to be something worth considering as a hypothesis for additional research.

Turning back to the main argument of this paper and away from these speculations about the evolution of economics in the post-unification period, it does seem be somewhat ironic that now that we appear to be moving into a less unified and more pluralist phase of scientific development in economics, that we are just starting to understand the nature of the dramatic unification that modern economics experienced during the second half of the twentieth century and the role that *Foundations* played. I will close with just two remarks about this irony. First, we should not be surprised; as Hegel said, the owl of Minerva flies only at dusk. Second, this paper is just the *Foundations* tip of the Samuelsonian iceberg; there is much more to that particular iceberg, as well as many more icebergs, to explore.

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