HYPOTHETICAL PATTERN EXPLANATIONS IN ECONOMIC SCIENCE: HAYEK'S EXPLANATION OF THE PRINCIPLE AND PATTERN PREDICTION MEETS CONTEMPORARY PHILOSOPHY OF SCIENCE☆

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ABSTRACT

During the last decade or so, philosophers of science have shown increasing interest in scientific models and modeling. The primary impetus seems to have come from the philosophy of biology, but increasingly the philosophy of economics has been drawn into the discussion. This paper will focus on the particular subset of this literature that emphasizes the difference between a scientific model being explanatory *and one that provides* explanations of specific events. *The main differences are in the structure of the models and*

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the characteristics of the explanatory target. Traditionally, scientific explanations have been framed in terms of explaining particular events, but many scientific models have targets that are hypothetical patterns: "patterns of macroscopic behavior across systems that are heterogeneous at smaller scales" (Batterman & Rice, 2014, p. 349). The models with this characteristic are often highly idealized, and have complex and heterogeneous targets; such models are "central to a kind of modeling that is widely used in biology and economics" (Rohwer & Rice, 2013, p. 335). This paper has three main goals: (i) to discuss the literature on such models in the philosophy of biology, (ii) to show that certain economic phenomena possess the same degree of heterogeneity and complexity often encountered in biology (and thus, that hypothetical pattern explanations may be appropriate in certain areas of economics), and (iii) to demonstrate that Hayek's arguments about "pattern predictions" and "explanations of the principle" are essentially arguments for the importance of this type of modeling in economics.

Keywords: Hayek; idealization; explanation; positivism

> Probably the best illustration of a theory of complex phenomena which is of great value, although it describes merely a general pattern whose detail we can never fill in, is the Darwinian theory of evolution by natural selection. It is significant that this theory has always been something of a stumbling block for the dominant conception of scientific method. (Hayek, 1967 [1964], p. 31)

INTRODUCTION

Bruce Caldwell's *Beyond Positivism* (1982, 1994) broke new ground in presenting the literature on economic methodology through the lens of contemporary debates within the philosophy of natural science. It differed from Mark Blaug's *Economic Methodology* (1980), which was published a few years earlier and also discussed the philosophy of science, in that *Beyond Positivism* approached the philosophical literature in a balanced and even-handed way: not picking one favored philosophical position and examining economics through that lens, but rather surveying the major philosophical ideas of the time with an eye toward those which had been, and could be, useful in helping us understand economic science. Caldwell's scope was wide-ranging. He surveyed the literature of Logical Positivism and Logical Empiricism, moved beyond positivism to detractors from within the philosophy of science – Karl Popper (1965, 1968) in particular – as well as the influential work of Thomas Kuhn (1970), Imre Lakatos (1970), and others associated with what came to be called the growth of knowledge literature, and finally surveyed the dominant methodological writers from modern economics: Lionel Robbins, Terence Hutchison, Ludwig von

Mises, Milton Friedman, and others. It was a masterful presentation of an extraordinarily wide range of philosophical and economic ideas by a (then) young economist. The Caldwell book played a key role in the revival of methodological discussion within economics during the late 1980s and 1990s. The fact that the philosophy of science and economic methodology were both indeed beyond positivism, was, by the late 1980s, a given, but without much consensus about what should replace the positivist tradition in either field. Given all this, Caldwell's plea for methodological pluralism (or critical pluralism, Caldwell, 1989) was quite appropriate and in many ways still is.

The two decades that followed the publication of *Beyond Positivism* brought even more sweeping changes to economic methodology, which moved it not only beyond positivism, but beyond traditional philosophy of science and epistemology, as the field began to look to areas like science studies, the rhetoric of science, and a host of other resources for theorizing about economic knowledge. The philosophy of natural science was changing as well during this period, and while it was certainly beyond positivism, it continued to emphasize questions of epistemic and cognitive significance, but the *way* these topics were typically addressed changed significantly. There were many changes, but two in particular are relevant here. First, the philosophy of natural science became more naturalistic, in the sense that philosophical investigations tended to be more grounded in contemporary scientific practice and less in *a priori* arm-chair philosophizing, and second, it became more close-focused in its scientific targets; increasingly the emphasis was on smaller units of scientific endeavor: subprograms, local scientific communities with shared goals and constraints, and specific sites of scientific practice. This naturalism and localism also spilled over into work in the philosophy of economics and economic methodology, with research being less likely to focus on broad topics like neoclassical economics and more likely to focus on specific subprograms within economics (particularly areas like game theory, experimental economics, and behavioral economics). These inquires, like previous philosophical inquires, were generally normative – they assessed, appraised, and offered accounts of, epistemic achievement – but the domain of assessment was more local and more sensitive to the goals and constraints of the relevant community of economic practitioners (Hands, 2001).

One of the subjects that began to receive more attention with the changes that were taking place within the philosophy of natural science was scientific models. Although scientific theories still mattered, for many philosophical studies, the main theme became the character, structure, and epistemic role of scientific models. This was driven in part by an increased interest in biology – particularly the applied, policy-relevant areas of biology – but also by the emphasis on close-focused investigation of scientific practice: practice where epistemic questions generally concerned which model rather than which general scientific theory. But given that economics is fundamentally a modeling science, and that many models in economics look quite similar to those in areas of biology, this increased the interest in economic models by philosophers of natural science. However, it was a two-way street, and philosophers of economics have also turned to questions about models and modeling.[1] Finally, in addition to these philosophical developments, both historians of economic thought (e.g., Morgan, 2012) and practicing economists (e.g., Katzner, 2017, Rodrik, 2015) have also exhibited increased interest in the role and character of economic models. This turn is many-faceted, but this paper will focus on just one particular topic within the literature on economic models. Keeping with the theme of Caldwell's work and interests, it is a body of literature that not only exhibits *Beyond Positivism*-type contact between developments within contemporary philosophy of science and economic science, but it also connects up with the methodological insights of the economist who has been the primary subject of Caldwell's research over the years: Friedrich Hayek.

EXPLANATORY IDEALIZED MODELS

Many economic models are both abstract and idealized. There is some disagreement about how to define these two terms, but the most common way to distinguish them is that abstraction involves the omission or subtraction of certain properties or factors, while idealization involves distorting or exaggerating certain properties or factors. Both involve the introduction of fictions/falsehoods into the model, but since idealizations are more a matter of degree − one can reduce the distortion (de-idealize) by a little or a lot − the majority of the philosophical discussion has concerned idealization, and in particular, the impact that idealization has on the degree to which the model represents (or could represent, or is intended to represent) relevant real-world targets. Since most accounts of scientific explanation require accurate representation of the relevant causal processes or mechanisms − the ones that make a difference − such scientific models raise the problem of explanation in idealized models: How can extremely idealized, and thus false, models provide adequate scientific explanations, or for that matter do any other type of epistemically valuable work?[2] "How can a model that really looks nothing like any system it is supposed to "represent" play a role in allowing us to understand and explain the behavior of that system?" (Batterman & Rice, 2014, p. 350).

This issue has become one of the main drivers of debates about modeling within the recent literature in both the philosophy of biology and the philosophy of economics:

> Highly abstract and simplified theoretical models have an important role in many sciences, for example, in evolutionary biology and economics. Although both scientists and philosophers have expressed doubts about the epistemic import of these idealized models, many scientists believe that they provide explanatory insight into real-world phenomena. Understanding the epistemic value of these abstract representations is one of the key

> challenges for philosophers of science who attempt to make sense of scientific modeling. (Ylikoski & Aydinonat, 2014, p. 19)

The relationship between idealization and explanation is behind what Julian Reiss calls the explanation paradox in economics:

> We have now reached an impasse of the kind philosophers call a paradox: a set of statements, all of which seem individually acceptable or even unquestionable but which, when taken together, are jointly contradictory. These are the statements:
>
> (1) Economic models are false.
>
> (2) Economic models are nevertheless explanatory.
>
> (3) Only true accounts can explain. (Reiss, 2012, p. 49)

Although the relationships between idealization, representation, and explanation are interesting and important problems that have driven a substantial portion of the philosophical literature about economics in recent years, this paper makes no attempt to address such questions in general. The emphasis here will be much narrower and focus on just one particular set of explanatory problems raised by a particular type of idealized economic models: the literature on hypothetical pattern explanations. Although before introducing the concept of hypothetical pattern explanations it is useful to briefly discuss the role of mathematics in idealized economic models and the idea of a minimal model.

The problem of explanation in idealized models often raises the question of the role of mathematics in such economic models; although idealized models need not be mathematical, in modern economics they almost always are. The mathematical structure of the model may introduce falsities of a purely mathematical sort – continuity, differentiability, boundary conditions, etc. – that are generally not features of the target. A simple example is that while real-world prices and quantities are finite, the vast majority of economic theorizing is conducted using models where variables are defined over the real numbers (some subset of R^n). Although these mathematical restrictions are tractability assumptions that may not be problematic – for example, if it can be shown that the results of the model are robust over various specifications of such assumptions (Hands, 2016; Kuorikoski, Lehtinen, & Marchionni, 2010) – such assumptions can also introduce features into the model that are seriously at odds with the causal forces at work within the target system.

One account of how such idealized mathematical models can still explain and have epistemic content is to focus on minimalist models and the associated concept of a minimalist idealization:

> Minimalist idealization is the practice of constructing and studying theoretical models that include only the core causal factors which give rise to a phenomenon. Such a representation is often called a minimal model of the phenomenon. Put more explicitly, a minimalist model contains only those factors that make a difference to the occurrence and essential character of the phenomenon in question. (Weisberg, 2007, p. 642, emphasis added)[3]

Even though such models are highly idealized and employ a variety of false assumptions, it is argued that they can still tell us something causally relevant about the target system – and thus provide scientific explanations – because the models identify what really matters: the core causal factors that make a difference. Although many economic models are minimalist in this sense – and thus can provide adequate explanations of particular real-world targets – it is also reasonable to argue that many economic models do not meet even the relatively weak requirements of being minimalist models and thus for these models the problem of explanation in idealized mathematical models remains.

There may be many ways around this difficulty – many ways to defend the explanatory adequacy and epistemic value of idealized economic models that cannot satisfy even the relatively weak cognitive standards of minimalist models – but the rest of this paper will focus on just one account: the so-called hypothetical pattern explanation account of Rohwer and Rice (2013).[4] This account of idealized scientific models will be discussed in detail in the next section and will be compared to Hayek's account of economic models in the following section.

HYPOTHETICAL PATTERN EXPLANATION

Rohwer and Rice (2013) are concerned with certain types of models – scientifically respectable models – in biology. These models are highly idealized and do not necessarily identify the core causal mechanisms at work in any particular instance of the relevant biological phenomena. The problem is that the biological phenomena the models are concerned with are both complex and heterogeneous. Given the complexity and heterogeneity of the relevant biological domain, quite different causal factors will be at work in each particular instance, and since highly idealized models fail to take account of these various individual factors, they will not explain the relevant biological phenomenon. This said, the models are explanatory in the sense that they can provide an explanation of the general pattern present in this class (and sometimes other classes) of biological phenomena; they are explanatory with respect to generic patterns – "patterns of macroscopic behavior across systems that are heterogeneous at smaller scales" (Batterman & Rice, 2014, p. 349) – but do not explain any specific real-world events or features.

The extended biological example Rohwer and Rice employ is the Hawk-Dove game which models members of the same animal species competing for an important resource. The implication of the Hawk-Dove model is that – contrary to what one would expect under such circumstances – the animals "often exercise restraint in combat instead of fighting to the death" (Rohwer & Rice, 2013, p. 339). The model "idealizes away many features of real-world populations, such as differences between animals' abilities to fight, their ability to play alternative strategies, or cases in which animals occupy different roles" (*ibid.*, p. 340), and thus does not provide an explanation of any actual example of such restraint. Since "a necessary condition for a model to be an explanation is that it veridically represent the explanatorily relevant features of its target system(s), then this highly idealized model cannot be an explanation" (*ibid.*, p. 348), it does not provide an explanation of any actual case of such animal restraint, and yet is explanatory with respect to the general pattern in the behavior of same-species competition.

> The Hawk-Dove model … does not aim to provide an accurate representation of any actual causal factors that could be used to provide an explanation (or a partial explanation). Instead, the central aim … of the Hawk-Dove model is to show that individual selection is compatible with the observed biological pattern. … Yet, the model is able to provide this insight without accurately representing any causes within real-world systems—and this remains true even if we consider causal processes at the level of types. (Rohwer & Rice, 2013, pp. 343–344)

Summarizing their argument:

> We call this kind of idealization *hypothetical pattern idealization*. These idealizations are used to construct models of hypothetical scenarios that need not be instantiated by any real-world system—indeed sometimes they will present impossible scenarios. In addition, we call this kind of idealization hypothetical *pattern* idealization because it is most likely to be fruitful when the phenomenon of interest is a general pattern that ranges over extremely heterogeneous and complex systems […] Consequently, the motivation behind hypothetical pattern idealization is to construct models of hypothetical scenarios that, even though they may not accurately describe any core causal factors of a real-world system(s), are able to aid in the investigation of general patterns across extremely heterogeneous and complex systems." (Rohwer & Rice, 2013, p. 344)[5]

Rohwer and Rice close their discussion of hypothetical pattern explanations in biology with a brief discussion of economics, concluding that "it seems likely that many of the idealizations used within economics modeling will also be instances of hypothetical pattern idealization" (Rohwer & Rice, 2013, p. 352).[6]

It is also interesting to note that Batterman and Rice (2014) use the term "caricature" to discuss the way such models are used in biology, since caricature has been used to characterize certain types of economic models (e.g., Gibbard & Varian, 1978 and Morgan, 2012):

> In a diverse array of applications, models that are only *caricatures* of real populations are employed to explain actual biological populations. In such cases, the systems whose behavior we would like to explain typically have very few features in common. However, despite the extreme complexity and heterogeneity of biological systems, biological modelers have been extraordinarily successful at building relatively simple mathematical models to explain the patterns we observe in nature. Such biological models typically make no reference to the details of particular populations whose behavior they purport to explain. Yet, despite their lack of attention to such details, these models can be used to explain many of the patterns we observe across a diverse range of populations. (Batterman & Rice, 2014, p. 365, emphasis added)

AN ECONOMIC EXAMPLE

Rather than discussing the hypothetical pattern idealization account of biological models in more detail, let us consider an economic example that seems to fit this account. Although highly idealized economic models are often mathematical, this example will come from elementary economics and does not require mathematical representation.

Suppose we observe a decrease in the price of a product produced by a particular real-world firm. One standard explanation of such a price reduction is the following:

It is a competitive market and entry reduced the price of the firm's product. (E)

Since this is a microeconomic explanation, (E) could only explain a decrease in the price of this product relative to the overall price level in the economy (i.e., it does not explain the price falling as the result of a general deflation) and both the entry and competitive conditions would need to be met. So, the first matters of detail to examine to determine whether (E) is an adequate explanation of a particular price reduction are to determine if (i) there has been a general deflation (a matter which involves idealized macroeconomic models, a theory of index numbers, and a number of statistical details), (ii) whether entry has occurred (a very complex question having to do with the characteristics of the industry and the details about the way in which the entry actually occurred), and (iii) the degree to which the industry is competitive (which can mean a variety of things and come in a various degrees).

But supposing that there is information that the industry is competitive and that adequate entry actually occurred, then the standard idealized model behind (E) would be the model of a perfectly competitive industry and firm. Fig. 1 provides the diagrammatic representation of the model used to derive (E) in elementary economics.

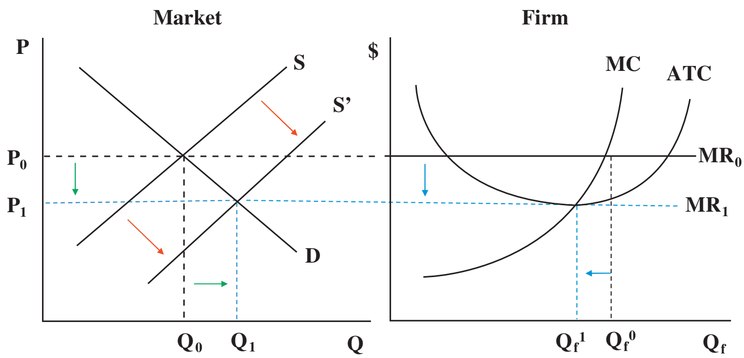

Fig. 1. Perfectly Competitive Market and Firm.

The model characterizes a perfectly competitive market for a particular good and a firm in that market/industry. The model says that when there is entry into the industry/market, the supply of the good will increase (S shifts to S') and that will cause the price in the market to fall (from P_0 to P_1). Since the market determines the price of the output of a perfectly competitive firm, the firm will face a lower price and thus marginal revenue (MR), which will reduce its profit-maximizing output. Each firm in the industry produces a lower output but the total market output increases because there are more firms in the industry. This explanation (without the diagrams) was in Adam Smith in 1776 (Book I, Ch. VII) and Ricardo in 1817 (Book I, Ch. VI) and it is in essentially every elementary economics textbook used today (with the diagrams).

Note this is an extremely idealized model. First of all, the model involves many tractability assumptions (prices and quantities are real numbers, functions are continuous, etc.) and it would be very difficult, and perhaps even impossible, to determine whether each of these various tractability assumptions are problematic for this particular case. Second, the model also involves various *ceteris paribus* assumptions that may also be troublesome. These include assumptions such as no offsetting increase in demand, no market interventions such as price controls, no increase in the costs of production that would push the supply curve in the other direction offsetting the entry, and many others. While it may be possible to assess some of these *ceteris paribus* assumptions, it is also very likely that some will be impossible to evaluate; this is automatically the case when they are infinite in number, but even if they are finite, they may not be observable and/or the necessary information may not be available. But there are idealizations that strike deeper into the core causal mechanisms of the model. It is perfect competition – no firm has any impact on the price of the good it produces (the output of the firm has measure zero in the industry output) – a condition that may not hold exactly in any real-word industry and holds approximately only in very few. Entry must cause the supply curve to shift out, which only occurs under idealized conditions (no price ceilings or floors, no wholesaler storing excess production so as not to lower the price, and other such conditions). The market mechanism of supply and demand must cause the price to fall, but the exact mechanisms that make prices fall when there is excess supply at the given price has long been contested. The standard Walrasian price adjustment mechanism (tâtonnement) is not only highly idealized, it is an impossible mechanism for price changes; there are a number of reasons that it cannot be the actual mechanism that drives real price movements in (even) competitive markets (Hands, 2016). And there are of course many more potentially problematic idealizing assumptions.

The bottom line is that the perfectly competitive model does not provide an adequate explanation of why the price of any real-world firm actually fell, even if there was entry into the market/industry for the firm's product (and or course prices fall for many reasons other than entry). The obvious question is: Why would one even try to explain a particular price reduction in this way, given the layers of idealization and abstraction involved, the complexity and heterogeneity of the firm and industry in question, and the contingency (and in some cases impossibility) of the particular causal mechanisms at work in this specific case? The model behind (E) seems to be far too idealized and rests on far too many false assumptions to provide a useful explanation of any particular price reduction. It seems like a much more obvious way to explain the target event would be to get detailed information about how and why prices in fact fell in this particular case (Who did it, how, and why?).

But (E) is one of the theoretical backbones of economic theorizing about price reduction and has been since Adam Smith. It is not only the historical standard, it remains the current standard in economic textbooks at every level as well as in the applied work of practicing economists. In addition, not only is it long- and well-established in economics, it is employed in the daily activities of various business and traders around the world and it seems unlikely they would be using it if it did not reliably track the broad pattern of entry and price reductions they are interested in. As such, it seems to provide reliable scientific information about, and valuable explanations of, a number of general pattern phenomena that both economists and members of the business community are concerned with. And, given the general acceptance and use of such idealized models, it seems safe to assume that these users are satisfied with the explanatory power such models provide, since they do not seek more narrowly focused explanations (either because explanations of more specific events are not, given their goals and constraints, as valuable to them, or they are simply not available). So this model seems to be explanatorily successful even though it does not provide an adequate explanation of any individual case.

Although this seems paradoxical, the paradox seems to fade if one thinks about it less in terms of the success or failure of the competitive model, and more in terms of the differences in the relevant target. Perhaps it is simply that those who produce and use such models are not interested in explaining why the price fell for the output of a specific firm, but broader patterns of price reductions. Maybe they are looking for a hypothetical pattern prediction about the generic relationship between entry and price reduction, rather than the explanation of a specific price reduction by a particular firm. If so, it may be that despite all the issues about idealization raised above regarding the explanation of a particular price reduction, the standard model behind (E) is explanatory in the sense of Rohwer and Rice's hypothetical pattern explanation; it is able to "aid in the investigation of general patterns across extremely heterogeneous and complex systems"(Rohwer & Rice, 2013, p. 344) and "provide insight without accurately representing any causes within real-world systems" (*ibid*., p. 343). This is of course just one particular idealized economic model, but it seems representative of a large class of, and perhaps a large portion of, the models in modern economics. But having discussed an economic example, it is time to turn to Hayek's discussion of such economic models.

HAYEK ON EXPLANATION OF THE PRINCIPLE AND PATTERN PREDICTIONS

Friedrich Hayek (1899−1992) was concerned with the relationship between scientific knowledge and economics throughout his professional life. Although this concern is reflected in much of his research, the two works that will be most directly relevant here are "Degrees of Explanation" (1967 [1955]) and "The Theory of Complex Phenomena" (1967 [1964]).[7] In these works, Hayek made the case that while social sciences like economics provided a kind of scientific knowledge, it was a different kind of knowledge than provided by natural sciences such a physics. The main source of these differences was that the social sciences studied complex phenomena and complex phenomena are not amenable to the approach that physicists have traditionally taken to investigate physical phenomena.

In the case of complex phenomena, scientific inquiry does not allow us to identify the particular cause of any explanandum phenomena. Rather than having specific causal detail, the best that can be obtained is an explanation of the principle behind the phenomena to be explained. Such principles allow for a scientific understanding of phenomena of a particular type or pattern, but the complexity prevents access to the specific causes behind any individual explanandum event. In Hayek's words, the causal processes "refer only to some properties of the resulting phenomenon, in other words, to a *kind* of phenomenon rather than to a particular event" (Hayek, 1967 [1955], p. 15).

As Caldwell explains, the argument for explanation of the principle in the study of complex phenomena was not restricted to the social sciences and in fact Hayek identified it directly with evolutionary biology:

> "Degrees of Explanation" is [...] significant because, when Hayek illustrates his claims about sciences that study complex phenomena, he chooses, not economics, but the *theory of evolution* as his exemplar. In doing so, he links evolutionary theory directly to his earlier ideas about "explanation of the principle": "The most familiar instance in the natural sciences of this sort of mere 'explanations of the principle' is probably provided by the theory of evolution by natural selection of different organisms" (Hayek, 1967 [1955], pp. 11−12). (Caldwell, 2004, p. 302)

Notice how much Hayek's explanation of the principle sounds like hypothetical pattern explanations. The explanandum phenomena are complex and explanations of the actual causes at work in any particular case are not available, and yet "despite their lack of attention to such details, these models can be used to explain many of the patterns we observe across a diverse range of populations" (Batterman & Rice, 2014, p. 365).

Notice that Hayek is making a case for the limits of explanatory power in economics, but it is not limited because of the models used; it is limited because of the complex character of the phenomenal domain. As Catherine Herfeld explains:

> Because a detailed specification of the variables […] is frequently not feasible, any attempt to causally explain the actually unfolding patterns by specifying the actual mechanism at work is bound to encounter immense computational limitations. Those limitations pose […] epistemological constraints upon economic models and limit their explanatory power. Yet, those limitations do not primarily originate in the construction of the model. Rather, it is a characteristic inherent in the object of study of economists – the phenomena to be explained. (Herfeld, 2018, p. 17)[8]

But Hayek is not only concerned with how complex phenomena affect scientific explanations, he is also, and perhaps is even more, concerned about the impact on prediction. If the model only addresses higher-level patterns and not the details of specific targets, it will not produce the kind of specific predictions necessary either to test the theory or to guide forward-looking control objectives in engineering or policy.[9] As Hayek explains, the lack of specific predictions is sometimes a problem in the physical sciences, but it is always present in the social sciences and other sciences of complex phenomena.

> The distinction between a prediction of the appearance of a pattern of a certain class and a prediction of the appearance of a particular instance of this class is sometimes important even in the physical sciences. The mineralogist who states that the crystals of a certain mineral are hexagonal, or the astronomer who assumes that the course of a celestial body in the field of gravity of another will correspond to one of the conic sections, make significant predictions which can be refuted. But in general the physical sciences tend to assume that it will in principle always be possible to specify their predictions to any degree desired. The distinction assumes, however, much greater importance when we turn from the […] phenomena with which the natural sciences deal, to the more complex phenomena of life, of mind, and of society, where such specifications may not always be possible. (Hayek, 1967 [1964], pp. 24–25)[10]

This is of course particularly the case in economics, since "economic theory is confined to describing kinds of patterns which will appear if certain general conditions are satisfied, but can rarely if ever derive from this knowledge any predictions of specific phenomena" (Hayek, 1967 [1964], p. 35).

Caldwell explains how this limitation on prediction in economics fits into Hayek's overall framework for thinking about the relationship between economics and scientific knowledge:

> Finally, Hayek's new categorization scheme implied that *many other sciences confronted the very same limitations regarding prediction as economics did* and the same necessity of resorting to "explanations of the principle." This was the reason that Hayek was so keen to establish the ubiquity of complex phenomena. And that the scheme allowed him to preserve the scientific status of economics is whey he would decide at just this moment to abandon the old 'natural science – social science' distinction and replace it with that of 'simple-complex phenomena.' With this new distinction, it is clear that the sciences are fully *scientific*; it is just that they are among those sciences that study complex phenomena. (Caldwell, 2004, p. 305)

Given all this, it would be useful to have a clear discussion of prediction, as well as explanation, in the hypothetical pattern account of idealized models to serve as a reference point for the comparison with Hayek's extensive discussion of prediction. Unfortunately, neither Rohwer and Rice (2013) nor the other research that has been employed thus far addresses prediction in a detailed way. However, there is research in the philosophy of biology that, while not referring specifically to the hypothetical pattern idealization account, characterizes highly idealized models in certain areas of biology in a way that is quite similar to the hypothetical pattern account and focuses specifically on the question of prediction in such models. The work that will be discussed here is Elliott-Graves (2016) research on invasive species (invasion biology).

The biological models used in invasion biology are highly idealized and are, like the economic models discussed above, explanatory with respect to broad generic patterns, but for policy purposes – actually taking action to prevent invasions in a particular place at a particular time – such knowledge is seldom very useful. For practical purposes, invasion biology needs to be able to make accurate predictions "which can determine precisely whether or not a particular organism will succeed in invading a particular environment" (Elliott-Graves, 2016, p. 376). The problem, as in the other models discussed here, is that both the specific phenomena and local causal mechanisms are complex and heterogeneous; it is thus causal heterogeneity which is "an important source of difficulty for making predictions that are both specific and accurate" (*ibid.*, p. 375).

This seemed to be the conclusion in the economic example (E) as well. If one really needs an explanation of why the price of a particular real-world firm's output fell, it is probably best to ignore the highly idealized model in economics textbooks and study the details – the objectives, constraints, and causal factors – relevant to the particular firm and the industry. But of course the success of the idealized model supporting (E) suggests that mere "explanation of the principle" and "pattern predictions" are what economists and elements of the business community have traditionally desired (or been satisfied with).

It is also useful to note one way in which the accounts of both Rohwer & Rice and Elliot-Graves might provide an enrichment of Hayek's account of the explanation and prediction of complex phenomena. Hayek's domain restriction is complex phenomena, and while the complexity of the subject matter is certainly important to the appropriate modeling strategy for the other authors as well, they stress both the complexity and the heterogeneity of the relevant domain: where "complexity refers to the number and type of interactions between parts of a system, heterogeneity refers to the diversity of the parts themselves" (*ibid.*, p. 376). It is not just that certain biological and economic phenomena are complex, they are also heterogeneous, and it is the combination – heterogeneous complexity – that matters to the type of predictions and explanations that are available within a given domain. And, as John Matthewson explains, complexity alone does not imply heterogeneity:

> Airbus A380 airlines are complex entities, but it is possible to model their properties precisely, realistically and in a way that generalizes across all of them. This is because they are very similar to one another. They are homogeneous, and so in spite of their complexity, these entities can be represented in a general way. (Matthewson, 2011, p. 331)

This is not to suggest that Hayek paid no attention to the heterogeneity of economic agents, industries, or other economic subjects – he certainly did – it is just to note that his core argument about what makes economics different from certain natural sciences, and which in turn conditions the type of predictions and explanations that are possible within economics, is complexity, and it might be useful to explore the specific role that heterogeneity plays as well.

Finally, the invasive biology case puts an important spin on both the hypothetical pattern idealization account and Hayek's account of explanation and prediction of complex phenomena. It seems that Rohwer & Rice, Hayek, and Elliott-Graves are all talking about roughly the same class of highly idealized models in biology and economics, but they are emphasizing very different things about such models. For Rohwer & Rice and Hayek, the emphasis is generally positive: focusing on how explanatory – how, if you will, scientific – such idealized pattern models are, even though they do not predict specific real-world events. However, for Elliott-Graves, the emphasis is fairly negative: how such models do not deliver the kind of explanations and predictions needed for policy application in the field of invasion biology. All of these authors suggest that these highly idealized models deliver something of cognitive value, but for Rohwer & Rice and Hayek, it is something important that would be missing if we were only concerned with scientific explanation of specific real-world phenomena (as has been the traditional focus of the philosophy of science), while for Elliot-Graves it is something that we are missing because we are not sufficiently concerned with specific predictions and explanations.

This critical aspect of the Elliot-Graves argument is particularly clear where she is discussing a particular example of hypothetical pattern idealization-based approach to invasion biology: Synthetic Invasion Metaframework (SIM) which "is aimed at subsuming all instances of invasion" (*ibid*., p. 381). Here it is clear that the type of broad explanatory power emphasized by Rohwer & Rice and Hayek is not the right kind of explanatory power for invasion biology:

> It could be argued that the SIM provides us with a *general* explanatory framework for invasion biology, even though it does not do a very good job of explaining particular invasions. I would cautiously agree with this characterization, as the SIM does provide us with a set of all possible causes of invasions, hence it explains invasions in a weak sense. However, I would also argue that as far as general explanations go, the SIM explanation is neither particularly informative, nor particularly useful [...] The second important limitation of the SIM is that it does not make the right sort of predictions. [...] it seems to me that because of the way the SIM is constructed, by abstracting causal detail, it is more likely that the SIM's predictions will be ambiguous, inaccurate or misleading. (Elliott-Graves, 2016, p. 383)

This criticism helps clarify the differences between hypothetical pattern idealization-based models and models that provide specific scientific explanations – as well as how these differences impact prediction – but it also demonstrates that the value of various types of explanations and predictions depends on the pragmatic purposes at work in the relevant field of scientific inquiry. Pragmatic constraints on modeling – based on purpose and audience – have recently been emphasized even by realist-oriented philosophers of science (Mäki, 2009, 2011) and this is a good example. Assuming that Rohwer and Rice's account of the explanatory character of such models is adequate, the question of whether such generic explanations do, or do not, satisfy the pragmatic constraints imposed by various fields of inquiry is dependent on the specific, and contingent, features of those fields. For Rohwer and Rice, it seems that for the biological areas they are concerned with, the pragmatic constraints are well-satisfied – as is the case for Hayek[11] – but for Elliot-Graves, that is not the case.

FINAL THOUGHTS

At this point, it is useful to review the main points of the above discussion. I will list these points in order of what seems to me, at least, to be the firmness of their anchoring: starting with those that seem to be most well-anchored, moving on to those that are moderately well-anchored but more difficult to defend, and finally to the one that seems to be the least well-anchored: issues involving philosophical justification (in particular, those relating to what does, and does not, count as a scientific explanation).

In the category of most firmly anchored, I would put (i) the description of the hypothetical pattern idealization account of scientific explanation and prediction: both what Rohwer and Rice (2013) said about such idealizations/explanations and the arguments they use to defend such pattern explanations, (ii) the description of Hayek's account of complex phenomena and his explication of the impact such phenomena have on scientific prediction and explanation, and (iii) the claim that the hypothetical pattern idealization and Hayek's own account are quite similar.

Moving to some parts of the discussion that may be a little less firmly anchored is the argument that there are in fact idealized economic models, perhaps a fairly large portion of economic models, that predict and explain in a way that is consistent with both the hypothetical pattern idealization account and Hayek's account of complex phenomena. I hope my example (E), and the idealized competitive model that undergirds it, contributes to the persuasiveness of this descriptive claim about economic models, but the defense of the claim does not stand or fall on that one example (there seem to be many more such examples).

Finally, and probably the least anchored, is the argument that being explanatory without providing any actual explanations in the way discussed in here is a philosophically justified account of (one type of) explanatory adequacy. Although philosophers of science have generally grown less austere in their demands regarding the adequacy conditions of scientific explanations during the decades following the heyday of the Deductive-Nomological model, there may be some who will find the hypothetical pattern account less than adequate.

All I can say about this is that I did not make, nor did I intend to make, any original contribution on the general philosophical question of the justification or adequacy of this type of pattern explanations. I did (i) discuss the arguments of several philosophers defending such adequacy (at least in certain fields and with respect to certain phenomena), (ii) present arguments (my own and from others) for the effective use of such models in economics, and (iii) make the case that Hayek's account of explanation of the principle and pattern prediction is consistent with the hypothetical pattern account. And these three things seem to be sufficient to move the discussion of these topics forward.

Returning to economics, I would suggest that the most important take-away from the discussion is not whether hypothetical pattern explanations exist in economics (it seems pretty clear they do), or whether they provide scientific explanations (they seem to do so, although perhaps in a weaker way than scientific explanations of particular real-world events), but rather it is the argument that pragmatic purposes – goals and constraints – are the real drivers for the kinds of successful idealized models that are at work in any particular subfield or research program within economics. Returning to the themes of Beyond Positivism and Bruce Caldwell's methodological writings, the importance of such pragmatic purposes – and the context-dependency of those pragmatic purposes on the specific features of the particular subfield or research program within economics – is an excellent argument for methodological/critical pluralism. The pragmatic context matters and in economics that means that explanatory adequacy will change with the economy, institutions, epistemic values, technology, and the kinds of tasks economists are assigned and assign to themselves. These things underwrite methodological, yet critical, pluralism.

NOTES

1. For examples, see Morgan and Knuuttila (2012), the 2009 *Erkenntnis* symposium on credibility and idealization in economics (see Grüne-Yanoff, 2009a for an introduction), and the 2013 *Journal of Economic Methodology* symposium on idealization and explanation in economics (see Reiss, 2012 for background).

2. There is an extensive recent literature on nonrepresentational scientific models, but an explicit discussion of it is beyond the scope of the current paper. I will note in passing that Bokulich (2012), Grüne-Yanoff (2009b, 2013), and Knuuttila (2011), as well as some of the papers in Frigg and Hunter (2010), provide useful discussions of the relevant issues.

3. Only tractability and minimalist assumptions have been discussed here, but there are many other categories of idealizing assumptions in the recent literature. For example, in addition to minimalist, Weisberg (2007) also discusses Galilean and multiple-model idealizations, while Kuorikoski et al. (2010) discuss substantial and Galilean, in addition to tractability. Of course, the literature on economic methodology has a long history of debates about the classification of assumptions in economic models: for example, the assumptions debate sparked by Friedman (1953) – see Hindricks (2006), Mäki (2000),

Musgrave (1981), and the surrounding literature – as well as the Hutchison-Machlup debate over assumptions and economic models (see Caldwell, 1982, Chapters 6 and 7).

4. The main reference will be Rohwer and Rice (2013), but arguments from Batterman and Rice (2014), Elliott-Graves (2016), and Rice (2015, 2016) will also be employed at various points.

5. On first reading, one might get the idea that all Rohwer and Rice are talking about is the distinction between a type-level explanation and a token-level explanation, where the former involves a causal mechanism at a general type level (smoking causes lung cancer) and the latter involves causal mechanisms at the specific or individual level (smoking caused Fred's lung cancer). Although it is clear that when Rohwer and Rice refer to explanations of specific events, they do mean token-level explanations, these two quotes also make it clear that they mean something different than simply type-level explanations when they refer to theories or models being explanatory without providing any specific explanations. Notice in the first quote they say hypothetical pattern idealizations may not represent "even if we consider causal processes at the level of types" and in the second quote that such idealizations may represent "impossible scenarios." Although they could have done a better job clarifying exactly how being explanatory without providing any specific explanations differs from providing explanations at the type level, it is clear that they believe there is a difference.

While the exact nature of this difference is not clear from the discussion in Rohwer and Rice (2013), they do seem to provide additional insight into Rohwer and Rice (2016). In the later paper, they differentiate between models that are "metaphysically related to an explanation" and those that are only "epistemologically related to an explanation" (p. 1132), where the former provide scientific explanations, and the latter provide the "cognitive achievement" of "scientific understanding" (p. 1139) without providing explanations. In the 2016 paper, they provide a detailed discussion of the features of, and the differences between, these two classes of models and provide numerous examples of both types (including some from economics). Based on the discussion in these two papers, one certainly gets the idea that the latter category from the 2016 paper – providing scientific understanding without providing explanations – is what they meant in the 2013 paper by being explanatory without providing explanations. One gets that idea, although regrettably, they never explicitly make this connection. Fortunately, it is not necessary to sort all this out for the purposes of this paper.

6. Hypothetical pattern idealizations and explanations are often associated with optimality models and the associated equilibrium-based explanations (Rice, 2015, 2016). But given the longstanding debates over the relationship between optimization, dynamics, and equilibrium within biology and economics, and also given the lack of consensus regarding optimality and equilibrium explanations among philosophers of science (Kuorikoski, 2007; Potochnik, 2007, 2009; Rice, 2012, 2015, 2016; Sober, 1983, and others), these topics are best set aside here. The position taken here is that if there are theoretical models in economics that provide the kind of hypothetical pattern explanations discussed here, then most, but not all, will involve optimization. But since a model's, or modeler's, use of optimization seems to be neither necessary nor sufficient for hypothetical pattern explanations, the focus will be exclusively on the latter and the question of relationship with optimality-based models and explanations will be left for another time.

7. Other key works on the relationship between scientific knowledge and economics include Hayek's "Economics and Knowledge" (1937), "The Use of Knowledge in Society," (1945), and *The Sensory Order* (1952). The definitive study on the topic is Caldwell (2004).

8. Herfeld (2018) uses Hayek's account of complex phenomena as a point of departure for a more general argument for economics as a (complex) science of social interaction rather than a science of individual action or behavior. This issue is beyond the scope of the current discussion.

9. One of the methodological costs of complex phenomena not accommodating the prediction of specific events is that it creates tensions between Hayek's view of social and biological science and Karl Popper's falsificationist philosophy of science. Not being able to make specific predictions means scientific hypotheses are not easily tested because there "can be no crucial experiments which decide between them" (Hayek, 1967 [1955], p. 19) and it is not consistent with falsificationism since "the possibility of a conclusive falsification requires that the scientist of complex phenomena possess knowledge that she cannot possess according to Hayek's methodology" (Scheall, 2015, p. 55). See Caldwell (1991, 1992a, 1992b, 2004, 2006, 2013) on the complex relationship between Hayek and Popper on methodological issues.

10. Scheall (2015) explains this in terms of the notion of predictive degree: "Summarizing, a bit more formally, a precise prediction of particular events specified to the extent desired in the given scientific context requires both knowledge of a (positive and 'large') number p of theoretical parameters and knowledge of the particular value that each of these parameters assumes at the time relevant to the prediction. If (and only if) the scientist's knowledge satisfies both conditions, then her prediction will be of degree 1. However, if the first condition is satisfied, i.e., if the scientist possesses knowledge of p theoretical parameters, but she does not know the value of any of the parameters at the time relevant to the prediction, then she will only be able to make a mere pattern prediction, the degree of which will be greater than 0 and less than 1, but which will approach 1 as she acquires more knowledge of the relevant values of each of the p parameters." (Scheall, 2015, p. 46)

11. The argument for Hayek is subtle. Hayek argues that most economists think about the purposes of economics in terms of prediction and control of specific economic behaviors, interactions, and institutions, but given Hayek's own political economic orientation, this is the incorrect, in some cases a dangerous, way to think about the purpose of economics. His arguments about prediction and explanation in the context of complex phenomena lead to the conclusion that such specific prediction and control is impossible in areas like economics anyway, and since what one ought to do is constrained by what one can do, economists ought not to focus on predicting and explaining particular economic events, but rather focus on that which is possible: pattern predictions and explanations of the principle.

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