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University of Vermont

The Direction of Justification Problem:
Understanding the Strengths and Shortcomings of Philosophies of Economics

Liam O'Brien

HON 243: Honors Thesis in Philosophy

Advised by Professor Randall Harp

30 April 2021

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Introduction

It is a fairly recent development in both economics and the philosophy of science that there has been a dedicated and sustained discussion around the philosophical justifications for our approach to economics. This discussion aims to bring value to economics in two ways. The first way philosophical analysis of economics aims to bring value to economics is to make sense of underlying principles which are being relied on by economic programs, and show the relationship that these underlying principles have to claims economists make about the world. Oftentimes, the philosophical principles which play a justificatory role in the claims that economists make about the world are not themselves the subject of economic research. Disagreements between defenders of competing economic theories can sometimes stem from the result of differences in these less visible commitments. Thus, philosophy of economics can help make sense of disagreements between defenders of competing economic theories by filling this gap. The second way philosophy of economics aims to bring value to economics is to enable us not only to better understand the disagreements between economic theories based in competing philosophical principles, but actually weigh in on these disagreements to help decide which theory is *better*.

In this paper, I argue that philosophy of economics already provides us with tools for accomplishing our first aim quite effectively but suffers from a crucial shortcoming when trying to accomplish the second aim. In trying to accomplish this second aim, philosophy of economics is confronted with what I refer to as the *direction of justification problem*. In order to make prescriptive claims about which economic theory is based on better philosophical principles, philosophers must make some commitment about what economics ought to achieve and how best to achieve it. For the purposes of my paper, I refer to this necessary commitment as a *philosophy*

of economics. The direction of justification problem occurs when the prescriptions of a philosophy of economics contradict the actual practices of some economists and it is unclear whether said contradiction ought to serve as evidence against our philosophy of economics or as an indictment of the economists who violate it. I argue that thus far, philosophers of economics have not provided a principled solution to the direction of justification problem, and because of this, end up making opposing prescriptions about economics which stem from differences in intuition about how to answer this problem. In order to cross the gap from descriptive to prescriptive power, philosophers of economics must acknowledge this problem and confront it head on.

This paper is divided into four parts. In part one, I establish a set of conceptual tools which are instrumental in creating a descriptive analysis of the kinds of underlying philosophical principles relied on in economics and the relationships that these principles have to economic theories. This toolset is built both from drawing on work in the general philosophy of science which has not yet been applied in detail to economics, and extending this work using some original concepts. In part two, I first use the conceptual tools developed in part one to give a demonstration of analyzing a disagreement in economics which is at first difficult to make sense of but becomes clear once analyzed with our philosophical tools. Then, I show how this analysis falls short of giving us prescriptive power because of the direction of justification problem. In part three, I survey three views in the philosophy of economics which I argue diverge from one another due to differing intuitions about the direction of justification problem. In part four, I consider two strategies for answering the direction of justification problem which are insufficient on their own, but each provide important insights into how we can develop an answer. Finally, I

propose a framework for how to answer the direction of justification problem which draws from the insights of these two insufficient solutions.

1. Building Our Conceptual Toolset

In order to establish that there exists an underdetermination problem in deciding between philosophies of economics and the economic programs whose prescriptions they contradict, I want to first carefully explain what I mean by a philosophy of economics and an economic program. To do this, I will first present what I call *metascientific commitments* and the necessary role that these commitments play in science. Then, drawing on work from Larry Laudan, I will explicate the relationship between metascientific commitments and scientific practice using a three-part model of scientific rationality. This three-part model was originally created as a way of understanding science in general but has not yet been applied in detail to economics. To demonstrate that this model is in fact a fruitful tool for understanding economics, I apply the three-part model to notable examples in economics throughout part one. Finally, I will then define exactly what I mean by a *philosophy of economics* and an *economic program* in terms of the concepts I lay out beforehand. With this conceptual toolset in hand, I can then begin to demonstrate the explanatory value of philosophy to economics and articulate the problem which prevents us from making prescriptions about economic programs.

1.1 Metascientific Commitments

Broadly speaking, philosophers of science seem to agree that in order to generate and justify scientific theories, scientists must rely on a set of background commitments which govern the rules of scientific inquiry. The exact nature of these background commitments and the extent

to which they influence the objectivity of scientific theories is certainly up for debate, but every philosopher of science must recognize that there are some commitments which play a role in science which are not themselves typically the subject of scientific study. After all, what value does the philosopher of science bring to science if not the study of principles which scientists themselves do not usually study but nevertheless play a role in science?¹ Typically, the role that background commitments play in science is to prescribe certain norms of scientific practice so that scientists are able to sufficiently generate, justify, and rationally endorse scientific theories. Some examples of these commitments are as follows:

1. Scientific theories ought to entail testable predictions.
2. If two theories explain the same phenomena equally well, then pick the one which posits fewer unobservable entities (Ockham's Razor).
3. The justification for a scientific theory should be objective, rather than relative to particular agents who choose to endorse it.

I do not mean to claim that every practicing scientist endorses these three claims in particular, but rather that all practicing scientists rely on some set of background commitments which entail a general rule set for how scientists are to generate scientific theories and determine which scientific theories they ought to endorse. These commitments may conceivably be different depending on the scientific discipline in question, but all scientific disciplines have in common the fact that they require a commitment to some set of background principles which entail or constitute the norms of scientific practice.

¹ One might argue that philosophy of science brings *no* value to science, it only helps non-scientists better understand what scientists already understand. This view would be consistent with the belief that there are no background commitments in science that scientists themselves aren't already studying or haven't already taken care of, but I suspect that this is not an understanding of the role of philosophy of science that many would find adequate.

Because these commitments, which I will broadly refer to as *metascientific commitments*, typically fall outside the domain of the work with which a scientist is directly engaged, scientists usually commit themselves to a set of metascientific commitments implicitly when they choose to engage in certain scientific practices and endorse certain theories among multiple possible contenders. While some scientists who are interested in methodological research might take the time to explicitly formulate and follow a set of metascientific commitments, these commitments are typically committed to implicitly when scientists make the choice of doing science a certain way. Because the role of these commitments often go unrecognized, it has largely been the interest of philosophers of science to examine these metascientific commitments, to identify them where they may be relied on implicitly, to categorize them and determine the interrelationships that these commitments hold with one another, and most importantly, to determine the degree to which scientists are justified in holding particular commitments.²

There are two other things to say about metascientific commitments for the sake of clarity. First, the reason I am using the terms “commitment” and “endorsement” rather than using belief language is because there are likely many cognitive attitudes besides belief that a scientist could hold in relation to a metascientific commitment or a scientific theory.³ A scientist might, for example, say that she never *believes* a metascientific commitment to be true. Instead she might hold a particular commitment to be most plausible among all contenders, or most useful

² It is difficult to know just how important discourse about metascientific commitments matters to scientists themselves *qua* scientist. Some may passionately defend the stance that scientists must be acutely aware of all the underlying principles on which they rely in order to be truly justified in their scientific pursuits. Others will instead argue that because metascientific commitments rarely play an explicit role in scientific discourse, it's best to let scientists focus on what they do best. It is not my intention to settle this debate in this paper, but I submit that at the very least, if a scientist need not have knowledge of her metascientific commitments *qua* scientist, it would at least benefit her *qua* general critical thinker to have a more holistic understanding of her scientific practices. Furthermore, it benefits the non-scientist public to better understand what principles they are committing to when they endorse a particular scientific theory or field of research.

³ Other possible cognitive attitudes might include *relying on* (Alonso 163-164), *accepting*, or *acting according to the assumption that X*.

for the purpose of achieving some scientific goal, like the goal of producing predictively accurate theories. For example, scientists often hold the metascientific commitment that we ought to form our theories according to the assumption that the world follows deterministic causal laws.

Research in quantum physics suggests that this deterministic view is, strictly speaking, false.

Nevertheless, above a certain scale it is perfectly sensible to act on the assumption that the world is deterministic for the purposes of achieving some scientific goal.

Likewise, a scientist might endorse a scientific theory despite not, strictly speaking, believing it to be making true claims. Consider the case of a scientific anti-realist who endorses atomic theory despite having serious doubts about the possibility of having any knowledge of the ontological status of atoms. This scientist may accept atomic theory because it is the best theory for achieving her scientific goals despite not believing that atoms exist. Thus, to hold a background commitment and to endorse a scientific theory merely entails that a scientist is choosing said commitments and theories to be the best among all available choices according to whatever standard that scientist uses to judge the merit of a commitment or theory. Typically, this standard of scientific merit is based on the desire to achieve some scientific goal. However, I will have to return later to exactly what I mean by “scientific goal” once I have established some more conceptual architecture.

Secondly, not all of a scientist's background commitments are metascientific commitments on my definition of the term. A scientist's background commitment is only a metascientific commitment if it plays some role in the scientific justification of the theories or scientific practices being promoted. A scientist who holds, for example, claim (1) discussed earlier is going to justify her endorsement of a scientific claim at least partially in virtue of its consistency with the prescriptions of claim (1). That is to say that she will treat the testability of

a scientific theory as a necessary condition for that theory's status as a scientifically justified claim. In contrast to this, a scientist who holds the commitment that a scientist ought to be kind to her colleagues might still feel that this is an important condition for producing good science. A scientist who is unkind might be difficult to work with thus making it difficult to produce good results. However, whether a scientist was kind to her colleagues in the lab will not ultimately play any role in the scientific justification of any theories or practices which are being endorsed by this scientist. Likewise, ethical considerations, like a prohibition on human experimentation, might be a condition for "good science" in a moral or humanistic sense. But whether human experimentation was conducted in producing a scientific theory will not play a role in whether that theory or the practices which led to the development of that theory have scientific merit.

1.2 The Three-Part Model of Scientific Rationality

I have tried to suggest thus far that there is a special set of commitments which play a justificatory role in any scientific claim, which I call *metascientific commitments*. Metascientific commitments are unique in that they play a justificatory role in scientific claims despite not typically being the object of scientific inquiry themselves. I do not think that this claim is controversial amongst philosophers of science. Larry Laudan, in his historical work on the philosophy of science, has tried to document the ways that philosophers have tried to make sense of these commitments in science. Laudan's work has dealt extensively with identifying the nature of different kinds of metascientific commitments, and I will draw from this work in order to better identify how these commitments come to influence scientific discourse while extending Laudan's insights into the domain of economics.

In Laudan's work on explicating the history of the debate surrounding the relationship between metascientific commitments and scientific theories, Laudan presents the hierarchical model of justification⁴ as the most widely known attempt at explaining this relationship. Due to its popularity in the history of the philosophy of science, the hierarchical model has largely set the terms for the debate around metascientific commitments (*Science and Values* 23). The hierarchical model posits that scientists hold commitments at three levels: *the factual level*, *the methodological level*, and *the axiological level* and posits a particular relationship among the three levels (*Science and Values* 27). According to the hierarchical model, factual commitments are generated and justified by appealing to methodological commitments which determine the rules for evaluating factual claims, and methodological commitments are generated and justified by their ability to achieve the scientific goals which are entailed by the axiological commitments.

While the particular relationship that these three types of commitments hold with one another is controversial—Laudan himself argues that there are more interrelationships between these kinds of commitments than the hierarchical model acknowledges—the debate following the development of the hierarchical model has seemed to rather uncontroversially accept that whatever the relationship between these three types of commitments may be, this three-part distinction captures important features of the different types of commitments scientists hold. Thus, for example, while Thomas Kuhn rejects the view that methodological commitments could ever unambiguously pick out one set of factual claims or that axiological commitments could ever unambiguously pick out one methodology, he nevertheless accepts that scientists hold commitments of all three types.⁵ While Alexander Rosenberg argues for an additional conceptual

⁴ Also referred to as the theory of instrumental rationality.

⁵ When, for example, Kuhn argues that methodological rules always underdetermine theory choice, he is implicitly committing to the existence of methodological rules and their distinction from scientific theories (*Science and Values* 30-31). Likewise, when Kuhn argues that interparadigmatic conflict necessarily involves scientists coming to

distinction within the axiological level between proximate and ultimate axiological commitments, he still accepts the distinction between factual, methodological, and axiological commitments (Rosenberg 12-13). And while Laudan rejects the view that any of the three types of commitments is more fundamental than any other in terms of its justification, he nevertheless still accepts that scientists make commitments of each type and endorses the conceptual distinction among the three types of commitments. Laudan's updated version of the hierarchical model, which he refers to as the reticulated model of scientific rationality, also makes the three-part distinction but holds that no type of commitment is more foundational in its justification (*Science and Values* 63).

Because the relationship between the three types of commitments is disputed, I refer to the model I am presenting as the three-part model of scientific rationality. The three-part model of scientific rationality is not committed to whether or not the types of commitments that scientists make are better suited to a foundationalist model of justification or a coherentist model. For the purposes of my argument, resolving the debate surrounding the exact relationship that these three levels of commitment hold with one another is not necessary. Instead, I merely claim that these types of commitments are conceptually distinct and that at the very least, the relationships that the hierarchical model posits between the types of commitments are among the relationships that these commitments hold with one another; I am not committed to the claim that the relationships posited by the hierarchical model are the *only* relationships that these commitments share. It may be that these types of commitments are related to one another in

the table with different cognitive goals, he is committing to the existence of axiological commitments (*Science and Values* 47). For a more in-depth treatment of how Kuhn's views fit with the distinctions posited by the hierarchical model, see chapters 2 and 3 of Laudan's *Science and Values* (1984).

additional ways which make them more suited to a coherentist model than a foundationalist one.⁶ But whether or not this is the case, simply by recognizing that there is a meaningful distinction between these three kinds of commitments, and that they hold at least the kinds of relationships to one another that the hierarchical model posits, we can begin to better understand some difficult problems in economics. Since these three types of commitments play an important role in my argument, and they have yet to be systematically applied to economics in a detailed manner, I will now explain each type of commitment individually and the role each plays in economic theories.

1.2.1 Factual Commitments

Firstly, *factual commitments* are the commitments which are most transparent to a practicing scientist. Laudan defines factual commitments as “all manner of claims about what there is in the world, including claims about theoretical or unobservable entities,” (*Science and Values* 23).⁷ Factual commitments are distinguished from the other levels in the three-part model by their being about the world, rather than about scientific practice itself. While Laudan does not go on to define factual commitments more precisely, I think we can broadly distinguish them into two categories. *Basic* factual commitments are the basic building blocks which serve as a foundation for developing scientific theories. These commitments posit the fundamental objects which constitute the domain of phenomena being researched and lay out the rules by which these fundamental objects are able to causally interact. This conception of basic factual commitments

⁶ For example, we might argue as Laudan does that while methods are judged according to whether they achieve the axiological commitments, axiological commitments can also be judged according to whether they can be achieved by our best available methods. (*Science and Values* 63).

⁷ Factual claims are distinct from facts; whether it is a fact that nothing travels faster than the speed of light is dependent on the way the world is structured, whereas whether Newtonian physics holds this as a factual commitment is dependent on whether the theories posited by Newtonian physics assert this to be the case or rely on this commitment as part of their justification.

mostly coincides with what Laudan refers to in his earlier book, *Progress and its Problems* (1977), as a research tradition's basic ontology (*Progress and its Problems* 79-80) although with one important distinction being that basic factual commitments need not necessarily entail ontological commitments.⁸ What I will call *complex* factual commitments are the most common objects of scientific research and debate. Complex factual commitments are the theories which purport to explain or predict phenomena in the world by reducing these phenomena down to the building blocks constituted by our basic factual commitments.⁹

Consider Richard D. Wolff and Stephen A. Resnick's account of the basic building blocks of neoclassical economic theory as an example of basic and complex factual commitments in economics. Wolff and Resnick write:

Neoclassical theory attaches basic importance to three economic acts that are attributed to all individuals: owning, buying, and selling. It assumes that all goods and services are privately owned by individuals and that all individuals seek to maximize their satisfaction from consuming goods and services. Neoclassical economists proceed to analyze what such rationally motivated individuals will do with their property as they maximize their satisfaction. (7)

We can make sense of what Wolff and Resnick are up to here using our distinction between basic and complex factual commitments. According to Wolff and Resnick, the basic objects of neoclassical economic theory are individuals who try to maximize their utility according to their preferences. We might also add that they do so according to a well-ordered preference function

⁸ One can conceivably, for example, be a strict scientific anti-realist and still believe that atomic theory entails the proper set of basic factual commitments for achieving the purposes of physics.

⁹ We might also add to this division something like *brute* or *trivial* factual commitments. These would be factual commitments whose justification are so widely accepted that they do not play an interesting role in the scientific discourse. For example, the assumption that there is an external world might technically be required for a complete justification of a scientific claim, but it does not play any interesting role in scientific discourse.

with transitive preferences. The basic causal forces by which these entities interact is through buying and selling goods and services in the marketplace. This collection of basic objects and basic causal rules by which the objects can interact constitute a neoclassical economist's basic factual commitments. An economist who then goes on to explain a particular phenomena or event by reducing it down to these basic commitments as much as possible—for example, explaining the wage rate in a particular region in terms of individuals and firms in labor markets with particular conditions—is engaged in developing complex factual commitments.

1.2.2 Methodological Commitments

Methodological commitments constitute the instrumental rules scientists follow when generating factual commitments, and the rules that determine which claims a scientist ought to endorse when faced with competing theories that posit distinct factual commitments (*Science and Values* 24). Theories are evaluated at least in part according to how well they maximize the features which methodological commitments tell us that a theory ought to have and their ability to minimize or avoid the features which methodological commitments identify as shortcomings in a theory. Methodological commitments are distinct from factual commitments because they are claims about *scientific practice*, rather than claims about the world. Thus, commitments like claim (1), which states that scientific claims ought to be testable, constitute methodological commitments because they give us prescriptive rules for whether a factual commitment ought to be accepted or rejected.

Scientific programs will often, though not always, hold a different set of methodological commitments for justifying their basic factual commitments than the methodological commitments which govern their complex factual commitments. For example, consider

economist Lionel Robbins's proposed account of economic methodology. On Robbins's view, the basic propositions of economic theory are justified in virtue of their being deductions from universal truths of human experience with which we are immediately acquainted. Robbins writes:

The scarcity of goods and services, which is the fundamental assumption of the system of deductive [economic] generalizations ... is a known fact both of introspection and of observation.... And, on the basis of this knowledge, we may assert the applicability of the abstract deductions from the concept of scarcity to the actual condition of the world in which we live. Any suggestion that this is not so rests upon the most palpable failure to observe elementary facts. (96-97)

Thus, Robbins lays out the methodological rules for generating and justifying the basic factual commitments of economic theory: we do so by deducing them from the indisputable truths about the relationships between desired ends and scarce means.

Beyond these truths of which we are immediately acquainted, the methodological rules change. Robbins writes:

...there is nothing in this conception of scarcity which warrants us in attaching it to any particular commodity. Our *a priori* deductions do not provide any justification for saying that caviare is an economic good and carries a disutility. Still less do they inform us concerning the intensity of the demand for caviare or the demand to be rid of carrion. From the point of view of pure Economics [sic] these things are conditioned on the one side by individual valuations, and on the other by the technical facts of the given situation. (98)

Robbins is using the term *a priori* a bit loosely here, since he earlier claims that our basic commitments are known by “both introspection and observation.” It might be better to substitute “obvious” or “immediate” for *a priori*. However, the importance of this passage is that Robbins establishes a distinction between the methods for establishing the basic factual commitments of economics, and the methods for establishing its complex factual commitments. Once we aim to generate and justify claims about particular economic phenomena or events, we must look beyond our immediate intuitions about human behavior and account for the “technical facts” of a given situation. Thus, Robbins sets up different standards for first establishing the basic factual claims of an economic program before going on to develop methodological standards for creating complex factual claims about the economy.

A methodological commitment can, though need not, be an epistemic claim. In the case of Robbins’s account of economic methodology, his justification for the principle of scarcity is an epistemic claim. Robbins is committed to the claim that we can have some legitimate knowledge which comes from introspection and reflection upon universal human experiences. An economic program which denied this would need some alternative claim about human knowledge to replace it. Terence Hutchinson, for example, who is committed to a positivist view of economics, argues that the basic postulates of economics which make claims about human behaviors need to be themselves empirically justified by experimental testing (Caldwell, *Beyond Positivism* 106-111). However, a methodological commitment need not be a direct claim about what kinds of propositions are knowable, but instead could be about what kinds of propositions are to be considered properly scientific. Hutchinson, for example, could conceivably admit that we can have knowledge about humans through reflection on our universal experiences, but argue that this knowledge is not precise or reliable enough to serve as a basis for an economic program.

Or that, even if it is reliable, its nature is such that it will not be as fruitful in building a foundation for generating a body of economic knowledge as a commitment to experimental verifiability.

An example of a methodological commitment that does not make an epistemic claim would be neoclassical economists' general commitment to the view that economic theories should be expressed mathematically. Austrian economists reject the view that claims about the economy which happen to be expressible mathematically are more epistemically valuable than claims about the economy which cannot be easily expressed mathematically.¹⁰ However, the neoclassical economist need not defend her commitment to expressing economic claims mathematically on the grounds that propositions which can be expressed mathematically constitute a superior kind of knowledge. Instead, the neoclassicist could respond with the claim that a methodological commitment to expressing economic claims mathematically has generally led to more fruitful research which can be more easily expanded and developed. It is not that mathematical economic claims are a higher form of knowledge, but that they are generally more useful when trying to build a body of economic theories and are thus preferable to claims which cannot be expressed mathematically.

1.2.3 Axiological Commitments

Methodological commitments, then, constitute the rules by which factual commitments can be generated and evaluated. But, if methodological commitments are not necessarily claims about what we can *know*, then what determines our methodological commitments? This is where

¹⁰ See Scott Scheall's "Slaves of the Defunct: The Epistemic Intractability of the Hayek-Keynes Debate" (2013) for an example of a disagreement between Austrian and neoclassical economics that is influenced in part by differences in the value each school of thought places on mathematical expressions of economic theory.

axiological commitments play their role in science. Axiological commitments are commitments about the cognitive aims or goals of science or of a particular scientific discipline (*Science and Values* 26-27). They tell us what it is our scientific pursuit is trying to achieve. On the traditional hierarchical view, methodological commitments are justified based on their ability to achieve the goals set out by our axiological commitments. Objectors to the traditional model like Laudan extend this to a two-way relationship, in which methodological commitments can be justified in virtue of their ability to achieve our goals at the axiological level, but our axiological commitments can also be justified in virtue of the possibility that they can be realized by our best methods (*Science and Values* 63). However, this more robust position still contains within it the original relationship posited by the hierarchical model and only adds an additional relationship to the picture. Thus, although the relationships between aims and methods might not be sufficiently explicated by the fact that aims are appealed to to justify methods, this is at least a necessary component of the relationship between axiological and methodological commitments.

Axiological commitments are often posited as stances on the kinds of knowledge science should aim to produce. For example, Milton Friedman argues in his famous essay on economic methodology that the aim of positive economics “is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances,” (4). Friedman’s axiological commitment which drives his assessment of economic methodology is the claim that the primary aim of economics is to make correct predictions. Following the prescriptions of this axiological commitment, Friedman makes the methodological commitment that an economic theory’s “performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields” (4). Thus, we can

begin to see how positing the aim of economics at the axiological level provides us with the standards of evaluation we can use to judge any proposed economic methodology.

On Friedman's view, economists' commitments at the methodological level are to be judged by their ability to produce economic theories which are predictive, and it follows that one methodological approach to economics can be determined to be superior to another if it is able to generate theories which have greater predictive power. As a result of these commitments, Friedman is happy to endorse economic theories which we have no reason to believe are making true claims, as long as these claims entail accurate predictions. Thus, the common criticism that economic theories make 'unrealistic assumptions' (such as perfectly competitive markets or rational utility-maximizing agents) is rendered moot unless we have reason to believe that Friedman's axiological position is wrong.

An alternative to Friedman's view can be found in the work of Ludwig von Mises, who held that the goal of economics was not to make true predictions but was instead to produce a body of synthetic *a priori* knowledge. Someone such as Ludwig von Mises, who proposes an alternative axiology, holds an alternative set of methodological commitments which includes the rule that we ought to reject an economic theory if it contradicts our synthetic *a priori* knowledge of human agency (*Reflections Without Rules* 41). Thus Mises, unlike Friedman, may reject an economic theory on the grounds that it makes 'unrealistic assumptions' because his axiology entails that economic theories ought to conform to our *a priori* beliefs about human action.¹¹ It is not my intent to resolve this dispute—nor, as I will argue later, do I think we can do so without confronting the direction of justification problem—but I hope that an overview of this

¹¹ We might, as Alexander Rosenberg argues, say that Friedman and Mises are still united at a deeper level by the same *ultimate* axiological commitments: to produce knowledge. But this goal is itself too abstract to entail methodological rules and thus requires that we create proximate axiological commitments (Rosenberg 15). On this account, it is on the level of proximate axiological commitments where Mises and Friedman disagree.

disagreement helps establish the distinct role of axiological commitments and their role in shaping methodological commitments.

1.3 Philosophies of Economics and Economic Programs

We are now ready to specify what it is I mean by *philosophies of economics* and *economic programs* as well as demonstrate how, with these concepts, we can point out a problem in the existing philosophy of economics literature. I use the term *scientific program* to refer to any actually practiced or once practiced tradition of scientific research which holds a general set of commitments at each of the three levels of scientific commitment, typically across generations of scientists. These commitments can be either explicit or implicit but are typically implicit at the axiological level and at least a portion of the methodological level. What makes a commitment partly constitutive of a scientific program is the *role that this commitment plays in the justification of the theories and practices endorsed by said scientific program*. Thus, what determines whether a commitment is part of a scientific program is solely whether said commitment is necessary for the justification of the theories endorsed by the program and the practices in which scientists in the program engage. It may be that scientists who generally practice within this scientific program claim to support other principles than those which play a role in the theories and practices of the program, but if this is the case it would not count as a part of the scientific program. Likewise, some commitments will be part of a scientific program despite scientists in the program not explicitly endorsing them because they nevertheless play a role in the justification of the theories and practices defended by the members of this scientific program. I use the term *economic program* to refer specifically to scientific programs which claim to be within the domain of economics. For example, on this definition, neoclassical

economics and Marxian economics constitute distinct economic programs because they hold different commitments on, in this case, all three levels of commitment. They differ on their commitments to what economics ought to aim for at the axiological level, what rules it ought to employ at the methodological level, and what factual commitments constitute our best economic theories.¹²

There may be some ambiguity as to how different economists' commitments at each level must be in order to distinguish them as working within distinct economic programs. Two economists need not agree on all of the same factual commitments to still be considered part of the same economic program. Two economists who hold generally the same commitments regarding the basic causal mechanisms which govern the economy, the proper methods for identifying these causal mechanisms, and the epistemic aims of economic research, but hold different views on, say, the degree to which primary education improves economic growth in developing nations, would still be considered within the same economic program on this definition.

Nor do two economists need to disagree at every level of commitment to be considered working within different economic programs. For example, Mises and Friedman likely share many factual commitments, such as the law of diminishing marginal utility and the transitivity of preferences. However, Mises rejects the axiological view that the aim of economics is predictive accuracy and as such subscribes to a completely distinct set of methodological commitments. Despite some of their factual agreements, these two economists are generally considered part of distinct economic traditions and on this definition would be considered members of distinct

¹² For an explanation of where these two programs diverge at each level, see section 2.

economic programs.¹³ While we may not be able to identify the exact amount of divergence two economists' commitments must have in order for them to be considered practicing in different economic programs, there are many unambiguous in which economists are either clearly within the same program or clearly in different programs.

What I call a *theory of science* is any explicit systematic view about what science *ought* to be committed to at all three levels of commitment. For example, Popperian falsificationism would constitute a theory of science on this view. Falsificationism explicitly contains claims about what science ought to look like at all three levels. It claims that the aim of science at the axiological level is to produce hypotheses with a high degree of empirical content which entail predictions that can survive genuine attempts at falsification. As such, it tells us that at the methodological level, the scientific merit of a theory should be determined based on whether it entails predictions which can, in principle, be falsified by empirical testing. Finally, falsificationism entails endorsements of some factual commitments over others based on their ability to meet the standards of the methodological commitments. A *philosophy of economics* is a particular kind of theory of science which is specifically concerned with what economics ought to be committed to at all three levels of commitment. Theories of science and philosophies of economics are distinguished from scientific and economic programs in that they are independent from what principles actually play a justificatory role in the theories and practices that economists defend. Economists practicing within an economic program may endorse a philosophy of economics which is not identical to the economic program within which they practice. For example, if in practice Friedman did not actually select which theories to endorse based on the standard of predictive power when he was doing economic research, but only

¹³ That is, assuming that the principles Friedman and Mises implicitly commit to in virtue of the theories they endorse and the practices they engage in align with the views they explicitly promote.

promoted that we ought to do so when he was talking *about* economic research, then his philosophy of economics would not align with his economic program.

Using the conceptual tools of economic programs and philosophies of economics, we can begin to untangle debates in economics between different schools of thought which are otherwise difficult to make sense of. However, our ability to make prescriptions about which economic programs are better than others is complicated by an issue I call the *direction of justification problem*. The direction of justification problem occurs when our philosophy of economics contradicts our economic program, and it is unclear whether this contradiction ought to serve as evidence that our economic program is flawed for not following the prescriptions of our philosophy of economics, or as evidence that our philosophy of economics is flawed for not adequately accounting for our economic program. In the following part, I will demonstrate how our conceptual tools developed thus far can be useful in making sense of disagreements between rival economic camps which may otherwise be incommensurable. Then, I will show how the direction of justification problem arises when we try to advance beyond an explanatory analysis and make prescriptive claims about economics.

2. Demonstrating the Uses and Limits of our Conceptual Tools

2.1 Applying the Three-Part Model to Disputes in Economics

When we account for the many levels at which economists form commitments, we can start to better explain some disagreements between economic programs which are otherwise perplexing. One kind of disagreement between economic programs which seems strange on its face occurs when different economic programs posit competing theories which appeal to different sets of basic facts, but neither program believes the other's basic facts are untrue. Take,

for example, the competing basic models of the economy proposed by neoclassical economics and Marxian economics. Martin Hollis and Edward Nell, in their criticism of neoclassical economics, try to distinguish the basic neoclassical model of the economy and the basic Marxian model:

The basic constituents of the neo-Classical [sic] vision are consumers and firms, whose optimising behaviour can be described with the aid of equations ... The alternative [Marxian] vision, by contrast, begins by examining what neo-Classicism takes for granted. It is concerned with structure, with dependencies between institutions, with what makes for continuance or disintegration. Its basic constituents are industries, sectors, processes and activities, defined in technological terms. The constituents do not normally coincide with the neo-Classical 'decision making' agencies. (17-18)

What is immediately perplexing about the disagreement between the basic models of the neoclassical and Marxian program is that neither is appealing to basic objects whose existence the other denies. The Marxian economist does not deny the existence of individual households and firms, nor does she deny that the behaviors of social groups are ultimately constituted by the behavior of individuals. Likewise, the neoclassical economist does not deny the existence of social groups like industries and sectors or the causal impact of technological processes on the economy. Nevertheless, the neoclassical economist does deny that these objects ought to make up the basic model of the economy, and the Marxian denies that the basic model of the economy should be reduced to the behaviors of individuals.

If the only axiological commitment underlying economics were the claim that economic theories ought to make true claims—that is, if our only goal at the axiological level were *truth*—

then the only reason we would have to reject an economic theory would be on the grounds that it relies on claims which are untrue. It is not until we account for the fact that there can be many different aims at the axiological level of an economic program, such as in Friedman's case predictive accuracy, that we can start to make sense of this disagreement between the neoclassical and Marxian program. Because we now understand that the source of disagreement between competing theories could arise at any of the three levels, we can understand why the two programs hold different basic factual commitments. The source of the difference traces back to neoclassical economists' general methodological commitment to *methodological individualism* in contrast to Marxian economists' general commitment to *methodological holism*. Individualists hold that economic phenomena ought to ultimately be explained by appeal to individual behavior and the greater causal implications of these behaviors. Thus, a supply and demand model is logically deduced from the theories of the individual consumer and individual firm. Holists instead argue that economic theories can appeal to classes or groups and ought not to (or perhaps cannot) be reduced to an explanation that appeals to individual behavior, such as in the Marxian case, social classes, industries, and sectors.

Disputes over methodological commitments, like that of the individualist and the holist, can be resolved in principle if the disputing parties hold the same axiological commitments. However, Hollis and Nell seem to deny the claim that neoclassicists and Marxians hold the same axiological commitments, writing that for neoclassical economics, "The object is to predict [optimizing] behaviour and its consequences, taking for granted the circumstances in which the behaviour occurs," whereas for Marxian economics, "Instead the object is to arrive at a blueprint of the economic system which explains how the system responds to institutional changes. The blueprint is essentially an analysis of the nature of production and of the social relations

surrounding production” (16-17). With our conceptual tools from the three-part model, we can now see that Hollis and Nell’s analysis hinges on a distinction which goes deeper than a factual or methodological dispute to a disagreement at the axiological level about the purpose of economic theory. According to the neoclassicists, the aim of economics is to predict the consequences of individuals changing their behavior in response to some change in the economy, whereas for Marxians, the goal of economics is to analyze and understand the social relations between groups which form around modes of production and how these social relations evolve in response to changing material conditions. The ultimate root of the difference in basic factual commitments is thus driven by a difference in goals. This is made most clear when Hollis and Nell go on to write:

... in order to conduct a comparative test [between the basic neoclassical and basic Marxian model], an umpire would have to decide whether the payment of profits (rent, interest, etc.) is an exchange. ... He would have to decide whether there is a basic social division into producers and consumers or whether the key concepts are those of hierarchy and class. Observation will not settle the point...each vision can accommodate the findings of the other, by assigning them a subordinate role. Many neo-Classical models include ‘structural’ relationships and many Classical-Marxian models include behavioural functions. But there remains a difference in emphasis, whose correctness cannot be judged ... without making assumptions about the underlying correctness of one of the rival visions.

(19)

Thus, the disagreements between economists in the neoclassical program and the Marxian program are ultimately rooted in “rival visions” of the aims of economic theory, which are not themselves determinable by scientific testing.

An account of economic programs using the hierarchical model allows us to better make sense of what is otherwise a fairly perplexing problem. We observe that neoclassicists and Marxians are engaged in separate economic programs and are producing distinct complex factual commitments. The distinction between these complex factual commitments results from the fact that economists in the two competing programs are explaining economic phenomena by reducing those phenomena down to their respective basic factual commitments, which also differ from one another. In order to settle this difference in basic factual commitments, we can appeal to methodological commitments. However, we see that neoclassical economists’ basic factual commitments are justified by their methodological commitment to methodological individualism, while Marxians’ basic factual commitments are justified by their commitment to methodological holism. These two methodological commitments are mutually exclusive as they entail mutually exclusive rules for generating basic factual commitments. Thus, we look to the axiological level to determine which methodological approach better achieves the cognitive goals of economics, but we find that the two economic programs are committed to different axiological principles. Neoclassical economics holds that the aim of economics is to generate theories which predict the consequences of certain events based on appeals to the ways in which individuals will change their behavior. Marxian economics holds that the aim of economics is to analyze how social relations between groups are formed around economic modes of production. Thus, with our three-part approach we can identify the source of the differences in neoclassicists’

and Marxians' factual claims by identifying how they arise from, at bottom, different axiological commitments.

2.2 *The Shortcoming of the Analysis*

If the goal of philosophy of science were merely to explain and identify the source of disagreements between camps of scientists, then we would find ourselves with a sufficient, or at least highly effective toolset that is generated merely by our ability to conceptually organize the different commitments of economic schools of thought at distinct levels. This conceptual ordering is valuable in itself because it brings awareness to the ways in which disagreements between competing economic programs can be a result of deeper commitments which are not themselves typically the object of economic research. By understanding exactly what the economic programs we analyze are committing themselves to at each level, we can better understand why economic programs diverge from one another in their views and practices. However, philosophers of science typically want to go beyond this. Ideally, we would like, after explicating the distinct programs of neoclassical economics and Marxian economics, to identify which program is *the better economic program*. But what are we to appeal to in order to do this?

One option for making a prescriptive judgment is to find agreement at some level and reason outwardly from there. For example, if two economic programs were committed to the same set of axiological commitments, we could in principle determine which program's methodology better achieved the shared goals at the axiological level. However, on the traditional hierarchical version of the three-part model, it is not possible for us to do this when we reach a disagreement at the axiological level, as this is the most foundational level of justification for a scientific program. In order to surpass this problem, Laudan proposes his

coherentist version of the three-part model which I briefly discussed in part one. Because according to Laudan, axiological commitments are not any more foundational than the other levels of commitment, we could settle axiological differences by appealing to shared methodological commitments. If two programs had the same views about which methods are the best for doing economics, we could in principle determine which program's axiological commitments are more realizable by our best methods. However, as we already saw, this difference at the axiological level between neoclassicists and Marxians is paired with a methodological disagreement, so we cannot hold fixed the methodological level of commitment as a shared standard for commensurability either.

In the case of wholesale disagreements between competing economic programs at every level, we are left with two options: concede that we will not be able to make prescriptive claims about competing economic programs, or develop a principled view of what an economic program should be committed to at each level which is prior to the facts about what existing economic programs happen to commit themselves to. That is to say, if we are to make prescriptions about economic programs in cases like the one above between the neoclassicists and the Marxians, we need a *philosophy of economics*. But if a philosophy of economics is necessary for us to make judgments about economic programs, what are we going to judge our philosophy of economics against? If the standards set out by our philosophy of economics failed to conform with our most widely-cherished economic programs, would this serve as an indictment of our best economic programs, or a sign that we've erred in the development of our philosophy of economics? This is where the direction of justification problem arises: when our philosophy of economics contradicts a particular economic program, it is unclear whether this ought to serve as a sign that our philosophy of economics is mistaken, or that the economists

who have violated our philosophy of economics are mistaken. To see how this problem arises more acutely, let us turn to an example from the natural sciences.

2.3 The Direction of Justification Problem in the Natural Sciences

To see where the direction of justification problem arises in the general philosophy of science, I want to first analyze a widely known theory of science, Karl Popper's falsificationism. Recall from part one that Popper's falsificationism offers a prescription for scientific programs at all three levels. It tells us that scientific programs ought to aim to produce theories with high levels of empirical content, that the method by which they should do this is by developing falsifiable theories and subjecting them to rigorous attempts at falsification, and that some existing theories, like Einstein's theory of relativity, are the best exemplars of science because they meet these conditions. If Popper's theory of science is correct, he has given philosophers of science a way to use their conceptual tools to, in principle, make prescriptions about scientific programs. The role of the falsificationist philosopher of science is, then, to identify the implicit commitments of an existing scientific program and judge the merit of this program based on whether these commitments are consistent with the prescriptions of falsificationism. If we held fixed that falsificationism was the correct philosophy of economics, then we could settle the dispute between neoclassicists and Marxians by determining which program is committed to the principles which are more aligned with the prescriptions of falsificationism.

Unfortunately, the philosopher of science's job is not that simple. Many philosophers reject falsificationism and posit competing theories of science instead. One problem that prevents philosophers of science from agreeing on a theory of science is that when a theory of science contradicts the actual practices of a scientific program, it is unclear whether the theory of science

should serve as evidence against the viability of the scientific program, or whether the scientific program should serve as evidence that philosophers have an inaccurate theory of science. For example, one might hold that evolutionary biology is not a justified scientific program because it can't live up to the goals of science entailed by falsificationism. Since evolutionary theories cannot easily be subject to an experimental test, it is hard to see how a theory of evolution could entail falsifiable predictions. Popper himself notoriously called evolutionary theory into question for its failure to produce falsifiable theories, although he would later recant this view ("Natural Selection" 343-349). However, one might just as sensibly hold the view that Popperian falsificationism is a poor theory of science because it fails to explain why evolutionary biology *is* a successful scientific program. Thus, it is unclear in this case whether Popper's theory of science holds prescriptive power over the scientific program of evolutionary biology, or if the scientific program of evolutionary biology serves as evidence against Popper's theory of science. This uncertainty about which of the two objects in question—the program of evolutionary theory and the falsificationist theory of science—should serve as evidence against the other is an example of the *direction of justification problem*. In order to see just how problematic this can be when left unaddressed, let's take a closer look at how Popper tries to justify falsificationism as a comprehensive theory of science.

In *Conjectures and Refutations* (1962), Popper explains the process by which he came to develop and justify his theory of falsificationism. Notably, he discusses pre-theoretical intuitions he had about science, which he used to develop his view. He writes that the theories which first interested him were Einstein's theory of relativity, Marx's theory of history, Freudian psychoanalysis, and Alfred Adler's "individual psychology," (*Conjectures and Refutations* 34). However, he quickly became disillusioned with the latter three of the four theories and this

disillusionment served as evidence in the development of falsificationism. He writes that in the beginning of developing his view, “My problem perhaps first took the simple form, ‘what is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton’s theory, and especially from the theory of relativity?’” (*Conjectures and Refutations* 34). Thus, in developing a theory of science, Popper begins by appealing to our intuitions about already existing scientific programs.

Popper assumes that we have good *pre-philosophical* reasons for presuming that Einstein’s theory of relativity constitutes a good scientific program and the latter three theories constitute programs which are inferior to Einstein’s program in some way. From this initial intuition, Popper reasons that the best explanation for *why* Einstein’s theory is more scientific than the latter theories is that relativity entails falsifiable predictions while Marxism, psychoanalysis, and individual psychology do not. Popper draws from this that, at the axiological level, the aim of science is to produce bold falsifiable conjectures, the method for evaluating these bold conjectures is attempts at falsification, and finally that the factual commitments which we are left with after following the falsificationist method are therefore the best of our scientific claims. What’s notable about this method that Popper employs is that he is first appealing to our existing intuitions about scientific programs, and then using the distinctions between these already existing scientific programs as evidence for developing a theory of science. In this case, Popper seems to endorse the view that philosophers of science should be able to appeal to our beliefs about existing scientific programs as evidence for the plausibility of our theory of science. However, Popper *also* wants to use his theory of science as a prescriptive tool for evaluating existing scientific programs and determining their scientific status.

This methodology should strike us as *prima facie* suspect, as it carries with it a threat of circularity. If we evaluate the plausibility of a theory of science based on its ability to explain our beliefs about scientific programs, but we evaluate our beliefs about scientific programs based on their compliance with the prescriptions of our theory of science, we've caught ourselves in a circle. Thus, it cannot be the case that Popper thinks we can both justify falsificationism by its compliance with our beliefs about scientific programs and justify our beliefs about scientific programs based on its compliance with falsificationism. It would be uncharitable to assume Popper would catch himself in such an apparent trap. Instead, we might more charitably conclude that Popper thinks we can justify our beliefs about our theory of science based on its ability to explain *some*, but not *all*, of our beliefs about scientific programs. If we had a principled reason for determining in which cases our theory of science is to be judged against our beliefs about scientific programs, and in which cases our beliefs about scientific programs are to be judged against our theory of science, we could avoid the circular position of having the two mutually justify one another. But, how are we to determine which of these beliefs about scientific programs make it into this privileged class of beliefs which we use as evidence for our theory of science, and which of these beliefs about scientific programs should themselves be evaluated by our theory of science? We have escaped the problem of logical circularity only to end up in another problem: the problem of determining *in which cases* we can use our beliefs about scientific programs as evidence to inform our theory of science, and in which cases we can use our theory of science as evidence to inform our beliefs about scientific programs. Without a way of distinguishing these cases from one another, in any possible contradiction between a theory of science and a scientific program it will always be indeterminate as to which of the two

beliefs counts as evidence against the other. Thus, any comprehensive theory of science, or philosophy of economics, must have an answer to this problem if it wishes to be prescriptive.

2.4 Is the Direction of Justification Problem a Live Problem?

One immediate response to this problem is that it does not follow from the fact that two different conclusions are both logically possible that those two conclusions are equally plausible. It might be the case that the direction of justification problem only shows us that there are two different logically possible conclusions in the case of a contradiction between a theory of science and a scientific program, but not that this will actually pose a practical problem in making a decision between the two choices. There seem to be some cases in which this response to the direction of justification problem is sufficient. For example, consider the Duhem-Quine problem as a response to falsificationism. According to the Duhem-Quine problem, the falsificationist view that theories can only be scientific if they can be tested by attempts at deductively falsifying their entailments is untenable because, in practice, it is impossible to isolate a theory's entailments such that they can be deductively falsified in a test. This is because all scientific theories are necessarily tested alongside a collection of auxiliary assumptions (Ladyman 77-80). If, for example, I am testing the claim that the speed of gravity on earth is 9.8 meters per second, I must make auxiliary assumptions to control for things like wind resistance or other interferences. Furthermore, I'll have to assume that whatever tools I'm using to measure the speed of a falling object are calibrated properly, I'll have to assume that I am reading the results correctly, as so forth. Thus, in practice, according to the Duhem-Quine problem, we could never isolate one entailment of a scientific theory such that it can be deductively falsified in an

experimental setting since we will always rely on induction to make inferences about auxiliary conditions.

The logical structure of the Duhem-Quine problem as a response to falsificationism can be formalized in the following way:

1. According to naive falsificationism, a theory is scientific only if it entails claims which can be deductively falsified in an experimental setting
2. No theory entails a claim which can be deductively falsified in an experimental setting because all theories are tested in conjunction with auxiliary assumptions.
3. Because (1) and (2), according to naive falsificationism, there are no scientific theories.

This is obviously meant to be a *reductio ad absurdum* of naive falsificationism. The Duhem-Quine problem is meant to bring our attention to the fact that a view of science which is committed to naive falsificationism is untenable because it entails claim (3), which is unacceptable. However, it is still logically possible for us to instead maintain our commitment to naive falsificationism, and simply accept claim (3). Of course, just because this conclusion is logically possible does not mean that it is plausible. No one would accept the conclusion that there are no scientific theories. Thus, we instead conclude that naive falsificationism must be rejected or amended as a theory of science in order to avoid the need to accept claim (3).

From this analysis, we can say the following about the direction of justification problem: in cases in which a theory of science contradicts a scientific program, the direction of justification problem only poses a practical problem to our coming to an agreement on a conclusion when more than one of the possible conclusions is plausible to us. Therefore, if we analyze the philosophy of science and find that in all cases in which a contradiction is identified between a theory of science and a scientific program, all but one of the possible responses to this

contradiction are unacceptable in the way that claim (3) is unacceptable, then there should be no practical reason to be concerned about the direction of justification problem. However, as I will argue for in the following section, at least in the case of economics, there are substantial instances in which not only could reasonable agents form different conclusions in response to the contradiction between a philosophy of economics and an economic program, but that the philosophy of economics literature contains substantial instances in which it is actually the case that philosophers have formed opposite intuitions in these cases. Thus, at least in the case of economics, the direction of justification problem forms a practical and not just logical problem for the philosophy of economics.

3 The Direction of Justification Problem in Economics

While in the natural sciences, we may be able to avoid the direction of justification problem on the grounds that the correct position will be obvious to us when a scientific program contradicts a theory of science, this is not the case in economics. In fact, not only are multiple responses to the contradiction between philosophies of economics and economic programs plausible, the philosophy of economics literature contains examples of philosophers whose disagreement seems to arise from having opposite intuitions about this matter without realizing it. In this section, I will discuss three opposing views in the philosophy of economics literature which seem to diverge as a result of each party having different intuitions about the direction of justification problem. My hope is that once it is clear that the philosophy of economics has a cleavage due to philosophers having different implicit intuitions about the solution to this problem, it will become clear why establishing a solution to this problem is a necessary step in the advancement of the philosophy of economics.

3.1 Hollis and Nell

Hollis and Nell offer their own view for how to solve this dispute between the neoclassical and Marxian economic programs using the tools of philosophy, which ultimately suffers because of their failure to recognize the role of the direction of justification problem. According to Hollis and Nell, “economic theories are to be judged partly by whether they are backed by a suitable scientific method which is itself backed by a sound theory of knowledge” (13). They hold that neoclassical economics is “backed by” a philosophy of science rooted in positivism, which they aim to show is unsound. In rejecting the positivist philosophy of science, Hollis and Nell aim to “leave neo-Classicists [sic] without any coherent methodology or criteria of scientific merit” (20). The main strategy for Hollis and Nell to settle the dispute between neoclassicists and Marxians is to show that neoclassicism depends on an unsound philosophy of science while Marxian economics does not.

How are Hollis and Nell able to establish that neoclassical economics is “backed by” a particular philosophy of science? Rather than demonstrating some kind of entailment relationship between a positivist philosophy of science and the neoclassical program, they appeal to the kinds of rhetoric that prominent neoclassicists wield. For example, they draw upon the following passage from economist Paul Samuelson to establish the connection between positivist principles and neoclassicism. Samuelson writes:

All sciences have the common task of describing and summarizing reality.

Economics is no exception. There are no separate methodological problems that face the social scientist different in kind from those that face any other scientist...Finally it is clear that no *a priori* empirical truths can exist in any field.

If a thing has *a priori* irrefutable truth, it must lack factual content. It must be regarded as a meaningless proposition in the technical sense of modern philosophy. (qtd. in Hollis and Nell 10)

Hollis and Nell claim this passage implies that the neoclassical economic program is committed to a positivist philosophy of science. They draw out ten principles they claim constitutes this positivist philosophy of science which in turn serve as the basis for the neoclassical program (9-10). Among these principles, the most important for their argument against neoclassical economics are (1) that all cognitively meaningful statements are either synthetic or analytic but not both, (2) that no synthetic statements can be known *a priori*, (3) that analytic statements have no factual content and (4) that the test of a theory is the success of its predictions. It follows from these statements that the only justifiable economic theories which contain factual content are constituted by synthetic *a posteriori* claims which are empirically testable.

After establishing the commitments which constitute the philosophy of science which backs neoclassical economists, Hollis and Nell go on to argue that this philosophy of science makes neoclassical economics untestable by its own standards. Their argument for this position is as follows: In order to be testable, an economic claim must specify a set of *ceteris paribus* clauses which exclude non-economic or otherwise irrelevant influences. However, in order to justify using a certain set of *ceteris paribus* clauses, we need some already existing theory which can attribute the interfering influences to some non-economic cause and thus establish that these influences can be controlled for. But, if we want to appeal to another theory in order to establish these *ceteris paribus* clauses, that theory will have to be *a posteriori* justified by its own empirical test. Thus, we will need an additional set of *ceteris paribus* clauses to test this theory, which of course will require appealing to another theory that justifies these *ceteris paribus*

clauses and so on. This process repeats infinitely if we remain committed to the principle that the only kind of knowledge we can have about the world is empirical. The only way to stop this regression, according to Hollis and Nell, would be to justify our claims, at the most fundamental level, in *a priori* truths. But, because neoclassicists' positivist philosophy of economics denies the existence of synthetic *a priori* truths, and claims analytic truths are devoid of factual content, they are forbidden from making this move without violating their philosophy of economics. Thus, neoclassical economic theories are untestable as long as neoclassicists remain committed to a philosophy of economics which denies synthetic *a priori* truths.

Hollis and Nell are right in identifying a contradiction between a positivist philosophy of science and the neoclassical economic program. However, they have misunderstood the relationship that these two hold with one another. As Alexander Rosenberg points out in his criticism of Hollis and Nell, rather than showing that neoclassical economics is "backed by" positivist philosophy of science which is itself unsound, they have tried to show that positivism is unsound because it is incompatible with neoclassical economics. However, this undercuts their own starting premise that the neoclassical program is backed by a positivist philosophy of science (Rosenberg 24-27). Rosenberg suggests that Hollis and Nell have made this crucial mistake because they have been misled by the empty lip service that some prominent economists like Milton Friedman and Paul Samuelson have played toward positivist rhetoric. However, I think there is a relationship between neoclassicism and positivism that goes deeper than just empty rhetoric which Hollis and Nell have uncovered with their analysis, despite their framing it incorrectly.

What Hollis and Nell mistake as the discovery of an internal contradiction in one economic program is actually a perfect demonstration of the difference between economic

programs and philosophies of economics. Recall that a philosophy of economics is constituted by the axiological, methodological, and factual commitments we explicitly claim to support, an economic program is constituted by the axiological, methodological, and factual commitments that the theories we endorse and practices we engage in *actually commit us to*. While it may be the case that the implicit principles we commit ourselves to when endorsing a set of economic theories perfectly aligns with the philosophy of economics we explicitly endorse, oftentimes the principles our theories and practices commit us to are not the same as the ones we explicitly endorse. What Hollis and Nell have uncovered in their analysis is that among neoclassical economists who endorse a positivist philosophy of science, the neoclassical economic program they endorse implicitly commits them to principles which are incompatible with the positivist philosophy of economics they explicitly endorse. Rather than uncovering a contradiction in the neoclassical program itself, Hollis and Nell have uncovered that neoclassical economists like Samuelson who endorse a positivist philosophy of economics are contradicting the economic program to which they are implicitly committed.

Once we are able to untangle what Hollis and Nell saw as one internally contradictory economic program into two distinct collections of claims—one an economic program and the other a philosophy of economics—Hollis and Nell's conclusion that neoclassical economics is unjustified no longer follows. This is because there is no *necessary* connection between the neoclassical economic program and a positivist philosophy of economics, there is merely a contingent connection between the two which comes from the fact that prominent neoclassical economists like Samuelson endorse both of them. Because of their confusion of these two distinct sets of claims as making up one program, Hollis and Nell are led to a particularly absurd answer to the direction of justification problem: the contradiction between the neoclassical

economic program and the positivist philosophy of economics is used as evidence to reject *both the economic program and the philosophy of economics*.

This can't be right. Since the relationship between these two sets of principles are purely contingent and not necessary, we cannot reject them both on the grounds that they contradict one another. If we are to reject neoclassical economics as unsound based on the claim that it presupposes unsound epistemic principles, then we must do so by criticizing the principles that the neoclassical program actually commits itself to implicitly. But to do this, we must judge these principles against the principles that we think make up the correct philosophy of economics. If we are to make a comparative judgment between a positivist philosophy of economics and the neoclassical economic program, we are only justified in using the incompatibility of the two to *either* reject the neoclassical economic program on the grounds that it doesn't align with the prescriptions of our positivist philosophy of economics, *or* reject the positivist philosophy of economics on the grounds that it doesn't account for the successes of the neoclassical economic program. Thus, while we can progress beyond Hollis and Nell by realizing that the relationship between a positivist philosophy of economics and the neoclassical program are purely contingent, we are still left with the direction of justification problem; Hollis and Nell provide us no reason for why we ought to accept one and reject the other.

3.2 Rosenberg

While Rosenberg understates the value Hollis and Nell bring to the discussion by their identifying (however imperfectly) the contradiction between prominent neoclassicists' philosophy of economics and their economic program, he does reach the same conclusion about what Hollis and Nell are actually entitled to conclude. He writes, "The moral of the story should

be either (a) positivism is discredited as an account of the actual character of science...or (b) neoclassical economics is discredited as failing to satisfy positivist strictures" (27-28). While he doesn't frame it in terms of the direction of justification problem, Rosenberg recognizes that the contradiction Hollis and Nell identify can't disprove *both* positivism and neoclassicism. But, if we are only entitled to conclude one or the other, how do we determine which of the two to pick?

Rosenberg tries to answer this question along the same lines as Laudan's coherentist response to the problem of determining how we get prescriptive power from a philosophical analysis of a scientific program. Rosenberg writes, "Different philosophies of science reconcile [scientific] goals, theories, and methods by, so to speak, holding one or two of these three variables constant and adjusting the others to suit" (13). Thus, like Laudan, Rosenberg does not think that we need to approach an analysis of a scientific program with an independent theory of science. Instead, we can choose which of the levels in the three parts of our scientific program we find plausible and hold this level fixed while we adjust the others to be consistent with our preferred assumptions. Just like Laudan, Rosenberg's method does nothing to help us when confronted with two economic programs which disagree at every level, such as the neoclassical versus Marxian example discussed in part two, since there is no level of commitment which we can hold fixed as a shared standard for both programs.

For his analysis, Rosenberg holds fixed the axiological principle that "a scientific discipline should be expected to show a long-term pattern of improvements in the proportion of correct predictions and their precision" (18). He then goes on to argue that we have good reason to believe economics has not improved in its predictive power over time and therefore does not live up to its own axiological commitment (56). As for why we are to hold this axiological principle fixed while adjusting the rest according to their ability to square with this principle,

Rosenberg offers a few independent reasons, which he admits will be largely unconvincing to someone already entrenched in an alternative axiology (19). His main reason for holding this principle fixed however, is that this is the axiological principle to which most economists themselves are committed (56). But here, Rosenberg seems to make the same mistake as Hollis and Nell. Hollis and Nell mistakenly took neoclassical economics to be internally contradictory because they believed economics was backed by a positivist philosophy of science that it could not live up to. But as we saw during our analysis of Hollis and Nell, they did not in fact identify an internal contradiction within economics, but merely showed that a positivist philosophy of economics was incompatible with the neoclassical economic program and thus the contingent connection between the two made by economists who endorsed positivism was unsound.

Likewise, in holding the axiological commitment of predictive power fixed *because economists endorse it*, Rosenberg is not actually showing that neoclassical economics does not live up to its own principles, but is instead showing that economists who endorse improvement in predictive power as an axiological commitment of their philosophy of economics are not acting in a way which is consistent with the economic program they endorse .

Thus, Rosenberg, like Hollis and Nell, is discovering a contradiction between a philosophy of economics which is typically endorsed by neoclassical economists and the implicit principles which make up the neoclassical economic program. Unlike Hollis and Nell, Rosenberg does not make the mistake of thinking that this contradiction proves *both* the economic program and the philosophy of economics wrong. But, like Hollis and Nell, Rosenberg takes a stance on what this contradiction entails without good reason. Rosenberg decides to endorse the philosophy of economics which holds improvement in predictive power as its key axiological commitment and rejects neoclassical economics for failing to live up to this standard. Thus, he

chooses a direction of justification which starts with a philosophy of economics and judges economic programs according to this standard. But the other possibility is still open. Why ought we not instead conclude that Rosenberg's (and many economists') philosophy of economics is flawed because it fails to square with our economic programs?

It may be that this alternative position which uses economic programs as a standard to judge philosophies of economics is widely considered unacceptable, as was the case with the example from part two where we considered whether we ought to reject all of science in order to preserve our commitment to falsificationism. If this is the case, then we would have good reason to side with Rosenberg and not consider the direction of justification problem to be a live problem. However, this is not the case. Other notable philosophers of science, such as D. Wade Hands, have the *exact opposite* intuition about how to answer the direction of justification problem.

3.3 Hands

Hands finds himself confronted with the same problem as Rosenberg, as well as Hollis and Nell, but offers yet a third intuition about how this problem is to be solved. In his paper "Second thoughts on Lakatos" (1985), Hands assesses the applicability of a Lakatosian philosophy of economics to the neoclassical economic program. Lakatos's methodology of scientific research programs (MSRP) aims to assess the degree to which a scientific research program is progressing over time. To clear up some potential terminological confusion, according to the distinctions I made in section one, Lakatos' MSRP falls under the category of a *theory of science* (or *philosophy of economics* when applied to economics). This is because, according to the three-part model, methodological commitments tell us how a science is to

achieve the goals expressed by the axiological commitments but do not tell us the goals themselves. The MSRP tells us what a scientific program ought to achieve and which methodologies best achieve this, meaning that it expresses not only methodological but also axiological commitments. Therefore, despite Hands referring to the MSRP as a methodology, I will be referring to it as either a theory of science or a philosophy of economics.

Hands aims to assess the MSRP by seeing if it can explain why our most successful economic programs are successful. While the MSRP aims to be a prescriptive theory of science that is able to assess the relative merits of competing scientific programs, it is itself tested by whether it is able to explain why our most successful scientific programs are successful. As for how this is applied to economics, Hands writes:

If we are to follow this metamethodology in appraising the MSRP as a methodology of *economic* science, it appears that we should proceed by examining the best gambits of economics through Lakatosian spectacles. If the acknowledged best economic theories appear rational in light of the MSRP—that is, if the profession's acceptance of these theories can be explained internally by the standards set forth in the MSRP—then Lakatos' methodology should be given positive marks with respect to economic science. (*Testing, Rationality, and Progress* 40-41)

Thus, Hands presupposes the exact opposite answer to the direction of justification problem as Rosenberg at the very beginning of his analysis. According to Hands, a philosophy of economics should be assessed based on how well it coincides with our most successful economic programs. If our philosophy of economics contradicts our most highly endorsed economic programs, then this gives us reason to reject our philosophy of economics as inadequate.

According to the MSRP, the standard according to which a scientific program ought to be judged as successful or unsuccessful is based on whether it is progressing or degenerating over time. A scientific program is progressing over time if and only if its successive theories are able to explain or predict novel facts which its predecessors could not (*Testing, Rationality, and Progress* 42). Using this standard, Hands assesses what he believes are the two “best gambits” in economics: the Keynesian program and the neo-Walrasian program. In both cases, Hands concludes that these two programs have not progressed according to the MSRP and are thus unsuccessful programs according to a Lakatosian philosophy of economics. For the Keynesian program, Hands writes, “There can be little doubt that Keynesian economics is a best gambit in economics if anything is ... few, if any, economists would doubt its progressiveness in the 1930s” (*Testing, Rationality, and Progress* 46). Nevertheless, by the strict standards of the MSRP, it had even less empirical content than its predecessors and as such was degenerative (*Testing, Rationality, and Progress* 46). Hands concludes that the MSRP’s inability to explain the Keynesian program’s clear successes must count against the plausibility of the MSRP (*Testing, Rationality, and Progress* 46). He goes on to make the same claim about the neo-Walrasian program.

Rather than suggesting we change the way we do economics so as to meet the standards set out by the Lakatosian philosophy of economics, Hands concludes his paper by suggesting ways that we can change our philosophy of economics so that it better explains the successes of our best economic programs. Specifically, Hands argues that our philosophy of economics must create room for explaining the progress of an economic program purely in terms of its *theoretical* rather than empirical advancements if our philosophy of economics is going to explain why the neo-Walrasian program has been so successful (*Testing, Rationality, and Progress* 48). Hands

thus identifies the same contradiction as Rosenberg: a philosophy of economics which stipulates that improvement in empirical power over time is a necessary condition for a successful economic program is inconsistent with the reality of our best neoclassical economic programs. Nevertheless, Hands draws from this same observation the opposite conclusion: rather than rejecting our best economic programs on the grounds that they do not live up to the standards set by our philosophy of economics, as Rosenberg does, we ought to reject any philosophy of economics which can't explain our best economic programs as successful.

3.4 The Common Theme

All three of the works addressed in section 3 diverge from one another on their prescriptions about economics, despite the fact that all three works identify the same phenomena: a contradiction between the philosophy of economics to which neoclassical economists often subscribe and the economic programs to which those economists subscribe. Yet, all three works draw different conclusions; Hollis and Nell conclude that the solution is to reject both the philosophy of economics and the economic program, Rosenberg concludes that we ought to maintain the philosophy of economics and reject the economic program, and Hands concludes that we ought to maintain the economic program and reject the philosophy of economics. If philosophers of economics hope to obtain some amount of prescriptive power over economics, then we must settle on a principled answer to the direction of justification problem. Otherwise, whenever a philosopher of economics astutely makes the observation that a philosophy of economics contradicts an economic program, we will have no principled way of determining what this contradiction entails.

4. Moving Towards an Answer to the Direction of Justification Problem

In part four, I survey two possible answers to the direction of justification problem, one from Larry Laudan and the other from Bruce Caldwell, and show that they are both insufficient answers, at least within the domain of economics. However, I argue that both proposed answers provide us with valuable insights into what a sufficient answer to the direction of justification problem would have to look like. Based on these insights, I try to create an image of what an answer to the direction of justification problem will have to look like. While I do not aim to provide an answer to the problem, I hope to show that these insights provide a valuable framework for future research in the philosophy of economics for those who recognize the need to develop a response to the direction of justification problem.

4.1 Using Laudan's Insights to Help Solve the Direction of Justification Problem

In attempting to develop his own comprehensive theory of science, Laudan anticipates the potential for a problem like the direction of justification problem to be levied toward philosophers of science. He only briefly considers this potential as part of a much larger work and frames it specifically as a way of addressing the relationship the philosophy of science should have with the history of science. Nevertheless, he presents a valuable, if incomplete, insight into how we can solve the direction of justification problem in economics. In this section, I will first explain Laudan's position and how it could be applied to the direction of justification problem in economics. I argue that his answer on its own, while perhaps sufficient for the natural sciences, is not applicable to economics. Then, I draw on some elements of Laudan's position to begin articulating what a sufficient answer to the direction of justification problem will need.

In *Progress and Its Problems* (1977) Laudan expresses similar concerns to the direction of justification problem in the general philosophy of science. He acknowledges that despite our desire to build a purely *a priori* theory of science, there will inevitably be some cases in which our theory of science coming into contradiction with the realities of scientific practice should serve as evidence against our theory of science rather than as an indictment of science itself. Most notably, he writes that if a theory of science “entailed that the whole of the history of science was irrational, we would tend to view that as a *reductio ad absurdum* of the theory of [scientific] rationality rather than as a demonstration that science itself had been a sequence of entirely irrational preferences,” (*Progress and Its Problems* 157). Laudan recognizes that in some cases our theory of science allows us to make negative appraisals about scientific programs when they contradict the prescriptions of our theory of science, but in other cases this contradiction should count against our theory of science rather than as an indictment of science, then there must be “mutual dependence between history of science and philosophy of science,” (*Progress and Its Problems* 157).

If we are to admit to this mutual dependence, how are we to do so without falling into a viciously circular position? Laudan is concerned that if we allow the history and philosophy of science to be dependent upon one another without clearly determining which of the two is more fundamental in our hierarchy of justification, then we risk begging the question. He writes:

If the writing of history of science presupposes a philosophy of science and if a philosophy of science is then to be authenticated by its capacity to lay bare the rationality held to be implicit in the history of science, how can we avoid automatic self-authentication, since the history we write will presuppose the very

philosophy which the written history will allegedly test? (*Progress and Its Problems* 157)

Here, Laudan is expressing a concern which parallels that of the direction of justification problem. If the philosophy of science is authenticated by its ability to square with the history of science, but our writing of the history of science presupposes a philosophy of science which tells us which moments in the history of science were scientifically justified, then we've caught ourselves in a vicious circle. Likewise, as in the direction of justification problem, if our philosophy of economics is evaluated by its ability to explain why we think certain economic programs are successful, but we also use our philosophy of economics as the standard against which we determine whether an economic program is successful, we've caught ourselves in a circle much like the one Laudan is concerned about. Both of these problems require that we have some way of determining in which cases science as it exists (or once existed) in practice ought to be evaluated against our philosophical commitments, and in which cases our philosophical commitments ought to be evaluated against science as it exists in practice.

Laudan's solution for preventing a descent into circularity is to appeal to our "prephilosophical hunches about which theories are rational and which are not," (*Progress and Its Problems* 159). Laudan argues that to avoid circularity, we must hold a handful of moments in the history of science in place as intuitively rational, such that any "scientifically educated" person would have to endorse them as examples of good science (*Progress and Its Problems* 159-160). Once we have collected a small set of moments in the history of science that we "prephilosophically" suppose must be rational—Laudan refers to these beliefs as our "preferred pre-analytic intuitions about scientific rationality"—we can use these cases as a standard upon which we can judge the strength of any theory of science (*Progress and Its Problems* 161-162).

Thus, any theory of science must be judged on its ability to explain the set of moments in the history of science captured by our pre-analytic intuitions about scientific rationality, and any indictments the theory has on the history of science beyond this should be considered a shortcoming of the sciences and not of our theory of science (*Progress and Its Problems* 162).

Laudan therefore gives us the following answer to the direction of justification problem in science in general: we must use our intuitions about science to determine, in advance of coming across a contradiction between a scientific program and a theory of science, which will serve as the more foundational commitment that should be maintained at the expense of the other. If the scientific program in question is one which is captured by our intuitions, then its being in contradiction with our theory of science is evidence against our theory of science. In all other cases, the contradiction between a scientific program and a theory of science is evidence against the scientific program.

Provided we can agree upon a set of moments in the history of science we will hold as intuitive, Laudan's solution seems to offer us a formula which provides us an answer to the direction of justification problem in every case. Of course, Laudan's proposed solution is only useful to us if it is actually possible for us to identify a shared set of pre-analytic intuitions. While Laudan does not claim to have identified what claims make up this set of intuitions, he does provide some examples to illustrate what this set of claims would look like. For example, one of our intuitions might be that "it was rational to accept Newtonian mechanics and to reject Aristotelian mechanics by, say, 1800" (*Progress and Its Problems* 160). Notably, all of the intuitions he appeals to are exclusive to the *natural sciences*. He offers us no examples of intuitions from the history of economics or any other social science. This could imply that either (1) Laudan is not considering the potential for the direction of justification problem to arise in

the social sciences or (2) Laudan thinks the social sciences should be held to the same standard as whatever view we settle on to evaluate the natural sciences.

Both of these answers are insufficient for our purposes; the first because it does not answer our questions about the direction of justification problem in economics, and the second because it leaves too much on the table. If we commit ourselves to the second claim, that economics ought to be evaluated according to whatever theory of science we use to evaluate the natural sciences, we push some of the most interesting philosophical debates in economics aside and risk begging the question. In many of the disputes about what philosophy of economics we ought to commit ourselves to, the question of whether economics should be held to the same standards as the natural sciences is itself part of the dispute. For example, Austrian economists are often distinguished from their neoclassical counterparts by their commitment to a dualistic view according to which the correct theory of natural science is not the same as the correct theory of social science. Austrian economists explicitly hold as part of their philosophy of economics at the level of axiological commitment that the social sciences are fundamentally aiming at producing different kinds of knowledge than the natural sciences are trying to produce. If acceptance of Laudan's view requires that we presuppose that economics can be evaluated by the same theory of science as the natural sciences, then his framework requires us to presuppose too much that is up for debate.

Thus, it is unlikely that using the intuitions from the natural sciences that Laudan provides for us will be sufficient for our purposes in economics. However, the strategy he uses is a valuable one and worth emulating in the philosophy of economics. In establishing a small set of intuitions from the history of science, Laudan is trying to make the least amount of

assumptions necessary for creating a prescriptive philosophy of science. Laudan is well aware of the costs of the assumptions he's making, writing:

We should be very explicit about what we are committing ourselves to in taking this approach: (1) *that at least certain specified developments in the history of science were rational*; and (2) *that the test of any putative model of rational [theory] choice is whether it can explicate the rationality assumed to be inherent in these developments*. Claim (1), modest though it is, remains entirely a matter of faith since there is, in principle, no way we could prove these cases were rational, for our criterion of rationality itself will take their rationality for granted.

(Progress and Its Problems 161)

Laudan is very clear about the costs of his approach here: his method of appealing to pre-analytic intuitions requires that we take on faith that there have been at least some moments in the history of science which were rational. By rational, he means that we had good epistemic reasons for accepting this development as a progression in science. Thus, we might rewrite his assumption as (1) *that at least certain specified developments in the history of science were for good epistemic reasons*.

The goal of Laudan's strategy of appealing to pre-analytic intuitions is to allow for the development of a prescriptive theory of science while starting with a set of assumptions that are as modest as possible, such that any scientifically educated person would reasonably agree to them as starting principles. I think this is a valuable strategy that we might hope to emulate in the philosophy of economics. A Laudanian approach to establishing a strategy for developing a prescriptive philosophy of economics might look as follows: first, we identify a set of intuitions about economics, or perhaps the social sciences in general, based on the most modest set of

assumptions possible. Once we have established this set of intuitions, we can then judge our philosophy of economics by how consistent it is with the assumption that our selected intuitions are indeed true. In all other cases, when we apply our philosophy of economics to economic programs and find the two to be in contradiction, we must conclude that this counts as evidence against the economic program in question and not against our philosophy of economics.

What are we to use as our set of intuitions for economics? I am not sure. But I think we can make some assertions about what this set of intuitions would have to be like. A set of intuitions will have to (1) be robust enough for us to use them as a standard against any proposed philosophy of economics and (2) be as modest as possible while still achieving the goal of (1). Furthermore, I think the best way to measure the modesty of our assumption is based on how well our assumption minimizes the number of reasonable people with informed beliefs about economics whose positions are excluded. An assumption analogous to Laudan's but applied to economics, (1') *that at least certain specified developments in the history of economics were for good epistemic reasons*, does not seem modest enough for economics. This is because prominent skeptical philosophers of economics like Rosenberg who are prepared to reject all of the progress of economics thus far would be locked out of the philosophy of economics on this assumption. I am not sure what a better assumption would be, but it would have to be one which didn't exclude prominent philosophers of economics like Rosenberg on faith alone. A potential alternative assumption could be as follows: (1'') *that at least certain specified developments in the history of economics were such that reasonable, educated thinkers thought these developments were for good epistemic reasons*. This assumption essentially requires us to assume something even more modest than the assumption that economics has progressed: that economists are smart, and that because of this, any philosophy of economics should at least be able to explain why smart

economists saw progress even if there in fact was none. I think Rosenberg would agree to this more modest principle, but I am not sure if it is robust enough to use as a standard for developing a philosophy of economics. I hope that philosophers and economists who read this may be able to think of a better assumption, or find a way to build a robust philosophy of economics according to the assumption I've provided.

I have tried to lay out what the first step to solving the direction of justification problem will have to look like. It must begin by evaluating our philosophy of economics according to a set of intuitions which are developed from the most modest set of assumptions possible, where modesty is measured by the ability to minimize the number of reasonable agents educated in economics who reject the assumption. This first step only gets us part of the way there, however. I argued in part three that a fatal mistake many philosophers of economics make is to judge an economic program based on the explicitly stated philosophy of economics of that program's members. Instead, we ought to judge economic programs based on their own implicit commitments and not the explicit commitments of their members. But how do we determine what these implicit commitments are if we can't simply ask the members of the economic program in question? In the next section, I will try to answer this question.

4.2 Using Caldwell's Insights to Help Solve the Direction of Justification Problem

Bruce Caldwell offers a strategy for assessing competing economic programs he calls *methodological pluralism*. Methodological pluralism asserts that the best way to criticize economic programs is on their own terms. According to Caldwell, methodological pluralism "takes as a starting assumption that no universally applicable, logically compelling method of theory appraisal exists. (Or, more correctly, even if it exists, we can never be sure that we have

found it, even if we have.)” (*Beyond Positivism* 245). Rather than analyze economic programs according to the degree to which they align with our philosophy of economics, we are to try our hardest to criticize economic programs according to their own principles. Caldwell writes, “Such [economic] programs should be criticized either on their own terms, or for failing to show how they can be compared to other programs,” (*Beyond Positivism* 248). But, what exactly does it mean to criticize an economic program *on its own terms*? It’s not made extremely clear by Caldwell, but there are some important clues he leaves for us.

Caldwell claims that the pluralist approach is most useful “when the methodologies of opposing camps are founded on rival epistemological systems,” (*Beyond Positivism* 248). The idea here is a valuable one, but it is an ambiguous one. Caldwell rightly wants to avoid the tendency for economists with rival axiological views to reject alternative economic programs simply because they presuppose their own axiology to be correct. For example, he cites the frustrating tendency for neoclassical economists to reject the Austrian program on the grounds that Austrian theories cannot be subject to an empirical test, which is to presuppose the neoclassical economist’s axiological commitment to testability (*Beyond Positivism* 249-250). However, despite correctly rejecting the approach which presupposes one’s own axiology, he leaves us with a fairly vague idea for his alternative. Caldwell suffers from the same problem as Hollis and Nell when he uses the ambiguous relation of an economic program being “founded on” an epistemological system. Without a clearer understanding of what this relation is, we might worry that Caldwell’s “founded on” relation is just as contingent as Hollis and Nell’s “backed by” relation between neoclassical economics and positivism, where this “backing” relation turned out to be no more than the contingent connection formed by neoclassical economists who have tended to endorse a positivist philosophy of economics. If Caldwell’s idea

of a “founding” relation is like this, then Caldwell’s position would collapse into Hollis and Nell’s position. Thus, I propose two possible ways of interpreting what Caldwell means here. Under the first interpretation, I will argue, Caldwell’s view collapses into the same untenable position as Hollis and Nell’s view. However, under the second interpretation, Caldwell provides us with a promising component to a solution for the direction of justification problem.

One way we could go about criticizing an economic program “on its own terms” is to criticize the theories produced by an economic program according to whether they live up to the explicit philosophy of economics that economists in that program endorse. So, if most neoclassicists endorse positivism as Hollis and Nell suggest, we criticize theories produced in the neoclassical tradition based on whether they are consistent with the prescriptions of positivism. This interpretation would leave Caldwell guilty of the same misunderstandings as Hollis and Nell’s position. Since the relationship between an economist’s philosophy of economics and that economist’s economic program is a purely contingent one (it is not necessarily the case that the axiological, methodological, and factual commitments that economists explicitly endorse are the commitments which she implicitly commits to when engaging in her research) we have not demonstrated anything about whether the economic programs under scrutiny themselves have merit. We’ve only shown that these programs are or aren’t consistent with the philosophy of economics those economists tend to endorse. It might be interesting to us whether the economists we are criticizing are being consistent between their explicit and implicit commitments, but this fact is irrelevant to whether the programs under scrutiny are themselves worthwhile. Since Caldwell elsewhere cites his criticism of Austrian economics as an example of the kind of internal criticism he is promoting, (“The Case for Pluralism” 240), and this criticism of Austrian economics is based in whether Austrian’s theories live up to their own explicit endorsement of

praxeology, this first interpretation is probably what Caldwell has in mind.¹⁴ And I should note that for Caldwell's purposes this may be perfectly adequate. Caldwell himself is a pragmatist to a tee, and so the question of whether an economic theory is true or justified is secondary to his interests ("The Case for Pluralism" 241-243). But, for those of us who are interested in what kinds of epistemic attitudes we should take toward economic programs, this won't do.

I propose an alternative interpretation of what it means to criticize an economic program "on its own terms" which I will argue is much more relevant to the epistemic merit of economic programs. Instead of criticizing an economic program according to the philosophy of economics explicitly promoted by members of that program, we should criticize economic programs according to the best possible set of axiological, methodological, and factual commitments we could construct that, if true, would make the economic program we are scrutinizing justified. Let me explicate this position a bit more to show why it better fits with the understanding of economic programs I have presented in this paper. Recall that what distinguishes philosophies of economics from economic programs is that a philosophy of economics is constituted by the axiological, methodological, and factual commitments we explicitly endorse, whereas an economic program is made up of the commitments that the theories we support and the practices we engage in actually commit us to. For example, we can imagine an economist who claims to be a strict falsificationist, but nevertheless she supports economic theories which are unfalsifiable or when doing research does not actually adhere to the practice of rejecting a theory as soon as it is falsified. If we criticize this economist because her theories don't live up to the falsificationist standards to which she supposedly endorses, we may have learned something interesting about this economist, but we've learned *nothing* about whether the theories and

¹⁴ See *Beyond Positivism* (1982) pp. 128-135 for Caldwell's criticism of Austrian economics.

practices she supports are *themselves justified*. What matters to the merit of an economic program is not the explicit principles which the economist endorses, but the implicit ones which come from her practices.

Unfortunately, it is no simple endeavour to figure out what principles an economist is implicitly committing herself to when she supports a theory or a certain way of doing economics. Caldwell himself is aware of this difficulty, writing:

It is perhaps appropriate to note at this point that figuring out what economists are up to is not particularly easy. One can learn how to *do economics*, of course, by simply training to be an economist. But as Thomas Kuhn points out, this process is anything but a self-conscious one. It occurs through a sort of mental osmosis as one learns the paradigmatic solutions to various well-established normal science puzzles. (*Beyond Positivism* 232)

The difficulty of figuring out exactly what implicit principles make up an economic program is further complicated by the fact that our answer will likely be underdetermined. For any collection of theories or practices in economics, there will likely be many possible collections of principles which could serve as the implicit commitments of the economists who engage in these practices and produce these theories. Therefore, I suggest that if we are to approach an economic program *on its own terms*, we ought to judge the theory according to *the most plausible set of commitments that, if true, would be sufficient for the economic program in question to be justified*. We then choose to endorse or reject the program in question based on whether this most charitable reconstruction is possible for us to accept.

The way this process could work is as follows. Suppose we observe a group of economists who all seem to be more or less working within the same program. They tend to

endorse the same theories, perform the same practices when doing research, and typically come to the same conclusions when considering the merits of newly proposed theories. This group of economists is practicing within an economic program, but because their commitments are implicit, we cannot simply ask them about their commitments. To ask these economists about the principles they endorse would be to ask them about their *philosophy of economics* but not their economic program. Thus, we must ourselves reconstruct what underlying principles the economists in question are committing themselves to. But we could likely posit multiple different accounts of this economic program which would be consistent with the actions of the economists in question. In this case, we ought to construct an account of this economic program in question which is constituted by the principles that are as plausible to us as possible which would justify the theories and practices of the economists in question if they were true. Then, we can judge the economic program in question according to whether this best possible reconstruction of the axiological, methodological, and factual commitments in question constitutes a set of commitments we are comfortable endorsing.

4.3 Proposing a Framework for Solving the Direction of Justification Problem

Neither Laudan nor Caldwell on their own solve the direction of justification problem. However, when we combine the strategies proposed by Laudan and Caldwell, we can create a framework for a robust methodology. Using the insights of Laudan and Caldwell I will now formulate a method for answering the direction of justification problem using a hypothetical example.

Suppose we take neoclassical economics as an economic program we are interested in criticizing using the tools of philosophy. This is how we could do so while answering the

direction of justification problem: first, we must come to the table with a philosophy of economics. We develop this philosophy of economics according to its ability to square with the set of intuitions we have decided to hold fixed. The intuitions we choose to hold fixed will be based on whatever intuitions require the most modest assumptions while also being robust enough to serve as an evaluative standard for our philosophy of economics. Once we have explicated a philosophy of economics by determining which set of principles best square with our intuitions, we now have the tools to evaluate the neoclassical economic program. To perform this analysis, we must analyze the practices neoclassical economists are engaging in and the theories they endorse so that we may propose a set of axiological, methodological, and factual commitments which are as plausible to us as possible while being sufficient for justifying the neoclassical program if the commitments were true. Then, we determine whether this most charitable reconstruction of the neoclassical program is consistent with our philosophy of economics we have developed from our intuitions. If the reconstructed neoclassical program is consistent with our philosophy of economics, we accept the neoclassical program as an example of *good* economics. If the reconstructed version of the neoclassical program is inconsistent with our philosophy of economics, we reject the neoclassical program as *bad* economics pending future changes in the neoclassical program which would warrant a reevaluation.

A full solution to the direction of justification problem will require that we fill out each step along the way; we must establish a set of intuitions, develop a philosophy of economics according to these intuitions, reconstruct the economic program in question as charitably as possible, and finally determine whether our philosophy of economics is consistent with this program. While I have not filled out these steps by providing what I think to be the best set of intuitions, the best philosophy of economics, and the best reconstruction of the neoclassical

program, I nevertheless hope that this strategy I've proposed will allow us to make these next steps and develop a prescriptive philosophy of economics.

Conclusion

In this paper, I have tried to establish two major claims about the philosophy of economics. First, that the tools from the philosophy of science, like the three-part model, are worth applying to economics if only for the explanatory role they can play in untangling disagreements between economic schools of thought whose divergence arises from differences in underlying principles that economists may not even be aware of. I have tried to defend this position by drawing from numerous examples in economics, including demonstrating an exercise in analyzing a divergence between neoclassical and Marxian economics. The second claim I have tried to establish in this paper is that the ability for philosophers to weigh in on these disputes between competing economic programs is currently blocked by a hitherto unrecognized problem: our inability to know when our philosophy of economics ought to influence economic programs and when economic programs ought to influence our philosophy of economics, which I have called the direction of justification problem. Finally, I have tried to argue for what a solution to the direction of justification problem would have to look like.

My intent is not to suggest that we should abandon the pursuit of using philosophy to settle some of these disagreements in economics. On the contrary, I have tried to demonstrate that the fit between the tools of philosophy of science and economics make this avenue worth pursuing. However, if we are to pursue it, I urge that philosophers of economics recognize this problem which has currently gone unrecognized in the philosophy of economics, and understand the importance that a principled solution to the direction of justification problem will have if the

philosophy of economics is to achieve its pursuit of prescriptive power. I hope that the strategy I have explicated in the final section of this paper will provide a framework for this further pursuit.

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