

**WHEN THE “REALISM OF ASSUMPTIONS” MATTERED:
MILTON FRIEDMAN’S CRITIQUE OF THE PHILLIPS CURVE**

Marcos Picchio (picchio@wisc.edu)

Forthcoming in *Studies in History and Philosophy of Science*

Please cite published version when available

Abstract: In this paper I challenge the pernicious aspects of Milton Friedman’s methodological outlook that continues to hold sway over mainstream neoclassical economists. I do this by showing how Friedman’s own methodological dicta could have been used against him when he famously advanced the expectations critique of the Phillips curve at his presidential address to the American Economic Association. I use this case study to further suggest that psychological and neurophysiological data should not be deemed irrelevant to economic science.

Keywords: Milton Friedman; Phillips curve; expectations critique; scientific instrumentalism; economic methodology; neuroeconomics

Acknowledgements: I would like to thank Frank Cabrera, Hayley Clatterbuck, Megan Fritts, Elliott Sober, and especially Dan Hausman for helpful comments and suggestions. I would also like to thank all the organizers and participants of the 2019 INEM Summer School on Economic Behaviours, where I first presented an early version of this paper.

1. INTRODUCTION

Daniel M. Hausman has diagnosed contemporary economics as suffering from a *methodological schizophrenia*, “whereby methodological doctrine and practice regularly contradict one another” (Hausman, 1992a, p. 152). As this paper reveals, a prime example of an economist suffering from this affliction is none other than Milton Friedman, one of the titans of 20th century economics. Friedman’s (1953) methodological tract, in which he advances an off-brand account of scientific instrumentalism, has proven to be incredibly influential.¹ For Friedman, the ultimate goal of science is (in Hausman’s (1992b) terminology) “narrow predictive success,” i.e., correct prediction for “the class of phenomena the hypothesis is designed to explain” (Friedman, 1953, p. 14). The popularity of Friedman’s instrumentalist methodology among economists is undoubtedly due to how effectively it deals with the charge that economic theory is at fault for its reliance on unrealistic assumptions since, for Friedman, the only pertinent question to ask about the assumptions of a theory is whether they lead to accurate observations about the phenomena the theory is intended to make predictions about.

The aim of this paper is *not* to challenge what Friedman takes the ultimate goal of science to be; this position stems from a respectable tradition in the philosophy of science that continues to this day.² Rather, the aim is to challenge the *implications* of what Friedman takes the ultimate goal of science to be *for* scientific methodology, and in turn, economic methodology. The main culprit, discussed in more detail below, is the insistence that a theory should never be assessed by the “realism of its assumptions.” This position ties into what I refer to as the *relevance criterion*,

¹ “It is the only essay on methodology that a large number, perhaps the majority, of economists have ever read” (Hausman, 1992a, p. 162).

² The tradition I am referring to is the American pragmatist tradition represented by the likes of Charles Sanders Peirce, William James, and John Dewey. Commentators such as Abraham Hirsch and Neil de Marchi (1990) make an extensive case for the conclusion that Friedman’s methodological outlook was heavily influenced by Dewey in particular.

which suggests that the only evidence relevant for theory and model assessment must be about “the class of phenomena the hypothesis is designed to explain.”

Instead of arguing against Friedman’s methodological stance on *a priori* conceptual grounds, I adopt a strategy informed by the actual scientific practices of economists. For this reason, contemporary economists should find my line of reasoning especially persuasive. As I argue below, rejecting Friedman’s methodological proclamations has led to progress in economics by none other than Friedman himself. I base my case around Friedman’s (1968) even more influential presidential address to the American Economic Association—his third most cited work and the most directly bearing on economic science (Mankiw and Reis, 2018, p. 81). It was here that Friedman criticized the existing macroeconomic framework that employed the Phillips curve and established the methodological importance of microfoundations. As I argue below, Friedman’s criticism of the Phillips curve not only flouts his own methodological prescriptions, but more crucially, makes plain why one should reject these methodological prescriptions.

The structure of the rest of the paper is as follows. Section 2 provides an overview of the most common, instrumentalist interpretation of Friedman’s views on methodology. Section 3 explicates the central argument implicit in Friedman’s essay. Section 4 sets up the historical backdrop to Friedman’s presidential address, which is necessary for understanding the expectations critique outlined in section 5. Section 6 argues that if Friedman had internalized his own methodological proclamations, he would not have bothered to advance the expectations critique. Section 7 identifies a methodological dilemma for economists who accept the relevance criterion and then argues that rejection of the relevance criterion is the only way to resolve the dilemma. Section 8 concludes with some remarks about how Friedman’s criticism of the Phillips

curve is pertinent to current methodological controversies surrounding the behavioral turn in economics.

2. FRIEDMAN'S METHODOLOGICAL STATEMENT

Friedman's methodological tract, "The Methodology of Positive Economics," is not uncontroversial; it has a host of detractors and interpretive difficulties.³ One particular source of interpretive difficulty is section IV of the essay, which frustrates any attempt at rendering the whole of Friedman's essay consistent.⁴ Throughout this paper, I only discuss what Uskali Mäki (2009a, p. 57) calls a "consumptionist" reading of Friedman's essay, i.e., a reading focused on the essay's influence and reception rather than on Friedman's true beliefs and intentions—whatever they may have been. Consequently, barring one exception at a later stage, I will focus on the parts of Friedman's essay that have proven most influential among economists: sections II and III. These sections lend the most credibility to the instrumentalist reading of Friedman's essay.⁵ The origins of this instrumentalist reading stem from Ernest Nagel's (1963) commentary at an American Economic Association panel session on Friedman's essay (Mäki, 2009a, p. 62).⁶ Economists have subsequently come to accept the instrumentalist label (e.g., Boland, 1979, p. 503). Such a reading is quite natural. After all, section II begins with the assertion that "The

³ For a recent and concise overview, see Mäki (2009a). Also see Hausman (1992a, p. 163, fn. 17) for an extensive list of references.

⁴ Section IV reverses course and suggests a more moderate position than the one I will discuss here. Recent archival work by J. Daniel Hammond (2009) has revealed an explanation for the discrepancy. There are two earlier drafts of the essay; the first written around summer 1948 and the second written around fall 1952. The second draft shows considerable revisions to the second half of the essay, with section IV being completely new. Hammond attributes the more moderate position in the second draft to criticisms of the first draft by Arthur Burns and George Stigler.

⁵ Philosophers of science may be confused as to why Friedman is being labelled an instrumentalist since instrumentalism is oftentimes considered a semantic commitment regarding descriptions of unobservable entities in scientific practice. The problem of unobservables does not play any role in Friedman's essay, and as Hausman (1998) has argued, the problem of unobservables is not of much philosophical interest in economics since the unobservables economists theorize about are folk psychological cognates (beliefs and desires), which raise no special epistemological difficulties (cf. Mäki, 2000).

⁶ This instrumentalist interpretation of Friedman's essay has been challenged in more recent times by Mäki (2009b), who advances a realist interpretation. See Reiss (2010) for a critical evaluation.

ultimate goal of a positive science is the development of a ‘theory’ or ‘hypothesis’ that yields valid and meaningful (i.e., not truistic) predictions not yet observed” (1953, p. 7).

Perhaps the biggest idiosyncrasy one will find in Friedman’s essay is the frequent use of scare quotes around the word “explain” (though he does at times omit the scare quotes). The standard interpretation of this practice is that Friedman does not think explanation is a cognitive goal that science can achieve. Perhaps to the dismay of philosophers, there is no explicit argument in favor of this anti-explanatory position. It is typically thought that Friedman takes his rejection of explanation as supported by his “no-nonsense” approach to science (Hausman, 2001, 314). This is not to suggest that an anti-explanatory position necessarily follows from instrumentalism alone. Friedman’s position is extreme, and as we will see, it is also not supported by his version of instrumentalism.⁷

While it is true that there is no explicit argument for his anti-explanatory position, a careful reading reveals that Friedman does provide some support in favor of this controversial view. This is a consideration that will be important at a later stage, so it is worth mentioning now. Early in his essay Friedman writes:

The validity of a hypothesis ... is not by itself a sufficient criterion for choosing among alternative hypotheses. Observed facts are necessarily finite in number; possible hypotheses, infinite. If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are (1953, p. 9).

Philosophers of science will be quick to recognize that the problem Friedman is describing is a familiar one: the underdetermination of theory by data.⁸ Of course, the availability of additional empirical evidence may rule out hypotheses no longer consistent with the data; Friedman does

⁷ See Reiss (2012a) for a contemporary defense of instrumentalism as a methodological position in economics. Also see Reiss (2012b) for an overview of the difficulty associated with maintaining that economic models provide genuine scientific explanations.

⁸ Mäki points out that Friedman displays an “admirable awareness of the underdetermination issue” (Mäki, 2009b, p. 108). Note that W.V.O. Quine’s (1951) classic “Two Dogmas of Empiricism” was published only two years earlier.

not deny this. But as we will see, Friedman will place a restriction on what evidence is “relevant” for this purpose.

What is important to note regarding underdetermination is that Friedman may have invoked it to support his anti-explanatory position in a way unappreciated by past commentators. According to the version of the underdetermination thesis Friedman is invoking, any phenomenon can be explained by a multiplicity of hypotheses. Further, if there is no non-arbitrary way of deciding between explanatory hypotheses, then there is no reason to believe we can ever know if we have genuinely explained some phenomena. To support this reading of Friedman, note that he writes that “The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary” (1953, p. 10). For this argument to work, Friedman would have to insist that the *observational equivalence* of multiple hypotheses implies their *epistemic equivalence* (Mäki, 2009b, p. 108).⁹ In fact, he implicitly does just this when he discusses the example of an excise tax on a commodity leading to a price raise equal to the amount of the tax (1953, p. 9).¹⁰ What I take Friedman as suggesting from these brief remarks is that underdetermination prevents scientists from ever rationally determining if they have genuinely explained some phenomena, and therefore, scientists should simply focus on prediction.¹¹

⁹ Note that Friedman does not consider that observational equivalence not only requires consistency between competing hypotheses and the current data, but also that the competing hypotheses make identical predictions about the future.

¹⁰ He considers three hypotheses: “This is consistent with competitive conditions, a stable demand curve, and a horizontal and stable supply curve. But it also consistent with competitive conditions and positively or negatively sloping supply curve with the required compensating shift in the demand curve or the supply curve; with monopolistic conditions, constant marginal costs, and stable demand curve, of the particular shape required to produce this result; and so on indefinitely” (1953, p. 9.). Though the example illustrates the underdetermination problem nicely, it is not a particularly strong example. Some basic testing would likely reveal which explanation is correct in this case.

¹¹ Interestingly, while Friedman concedes that theoretical virtues such as simplicity and fruitfulness may also play a role in narrowing down candidate hypotheses, he anticipates Thomas Kuhn (1977) in suggesting that such criteria “defy completely objective specification” (1953, p. 10).

At this stage it is clear Friedman does not think explanation is even a subsidiary goal of science. Accurate prediction, and only accurate prediction, is what matters. This would suggest that Friedman is a run-of-the-mill instrumentalist, though he does hint at commitment to Karl Popper's (1963) falsifiability criterion early on when he writes that "Factual evidence can never prove a hypothesis; it can only fail to disprove it" (1953, p. 7). What makes Friedman's instrumentalism off-brand is that he restricts the set of relevant predictions a theory yields to those concerning the observable phenomena the theory is being *intentionally* used to predict. Standard instrumentalists, on the other hand, care about *all* observable predictions a theory yields and would not place any such restriction (Hausman, 1992b). The uniqueness of Friedman's view comes from his relevance criterion for assessing empirical evidence. For a test of a theory to be "relevant," "the deduced facts must be about the class of phenomena the hypothesis is designed to explain" (1953, p. 13). This, I take is Friedman's most significant, yet ultimately misguided, contribution to methodology.

In order to understand the motivations behind the relevance criterion, we need to look at the distinction Friedman makes between the assumptions of a theory and its implications (1953, p.14). Both assumptions and implications are consequences deduced from a theory when conjoined with initial conditions and background assumptions. To complicate matters, the word "assumption" has multiple meanings in Friedman's essay.¹² If the assumptions of a theory are axioms of the theory itself, which we will see is what Friedman really means in the case of economics, then they are trivially deducible from the theory itself. It is worth emphasizing that while implications of a theory are necessarily observable consequences, the assumptions need not be. What matters for theory assessment in Friedman's view is solely the conformity of the

¹² See Brunner (1969) for an extensive analysis of the multiple senses of "assumption" Friedman employs. See also Nagel (1963) for analysis of the different senses of an "unrealistic assumption."

theory's implications with empirical evidence (or data). The way one distinguishes between an implication and an assumption is by appeal to the relevance criterion. The proper implications of a theory *only* concern the phenomena, or class of phenomena, of interest. As we will see, for Friedman the conformity of a theory's axioms with empirical evidence is irrelevant to the assessment of the theory in question. In many parts of the text, it is this purportedly scientific practice that Friedman is proscribing. Per Friedman, inspecting the empirical accuracy of a theory's axioms is to mistakenly "test" the theory by the "realism of its assumptions."

Friedman truly reveals his hand in section III; it is here that we learn that the pertinent sense of "assumption" is a general statement that serves as an axiom of a theory. Economic theory is peculiar in that its axioms are not *just* general statements. The axioms are, strictly speaking, *false* general statements. Examples are not hard to come by: while consumer goods are divisible, consumer choice theory assumes goods are infinitely divisible; and while business owners do obviously seek profits, producer theory assumes they narrow-mindedly attempt to *maximize* profits. Hence, the charge that economics relies on unrealistic assumptions.

Because Friedman believes it is inappropriate to assess a theory by testing its assumptions, he goes onto suggest that a hypothesis that "explains" (read: predicts) the distribution of leaves on a tree by assuming that the leaves are consciously doing complex mathematical calculations in order to maximize sunlight intake would be "plausible" insofar as the hypothesis yields correct predictions confirmable by observation (1953, pp. 19-20). He similarly suggests that we can be confident in a hypothesis that makes accurate predictions about the shots an expert billiard player will make even if the hypothesis posits that the expert billiard players acts *as if* she "knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of

the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas” (1953, p. 21).

The leaves-on-a-tree and expert-billiard-player examples reveal that the assumptions Friedman is after are not akin to what philosophers and natural scientists nowadays regard as an idealization, i.e., an aspect of reality abstracted away for the purposes of testing a scientific theory. For Friedman, a scientific theory may have substantive assumptions that are completely false statements about some feature of reality, e.g., “all leaves calculate maximum sunlight intake.” Yet it should be clear now that this is not a worry for Friedman since he restricts the relevant set of predictions that a theory implies to those concerning the theory’s target phenomena. For example, in his leaves-on-a-tree example, the target phenomenon is the distribution of leaf density on a particular tree. This is why Friedman regards proving that leaves do not make complex mathematical calculations as irrelevant to our assessment of his toy theory. Hence, Friedman’s insistence that “none of these contradictions of the hypothesis is vitally relevant; the phenomena involved are not within the ‘class of phenomena the hypothesis is designed to explain’”¹³ (1953, p. 20).

It is not difficult to see how Friedman’s views on the nature of science can easily be invoked by economists eager to discharge the all-too-common criticism that economic theory relies on unrealistic assumptions. Contrary to what some take economic theory to suggest, ordinary people do not use complex and mathematical decision-making rules in deciding what to do, what to buy, and how hard to work. If the goal of an economic theory is narrow predictive success instead of explanation, which presumably requires truth, one can maintain, as Friedman

¹³ Friedman adds important qualifications to this position. For example, in the event that two theories both achieve narrow predictive success to the same degree, Friedman suggests that generality and wideness of scope be used as a tiebreaker. This is how he can maintain that the standard “explanation” for the distribution of leaves on a tree is preferable to his toy theory (1953, p. 20).

does, that what matters is that the theory yields correct predictions about some target market phenomena (e.g., the hourly wage rate in some industry) instead of addressing what he deems as the “largely irrelevant question of whether businessmen do or do not in fact reach their decisions by consulting schedules, or curves, or multivariable functions showing marginal cost and marginal revenue” (1953, p. 15).

From these remarks, we can see how the relevance criterion is Friedman’s solution to the philosophical difficulties raised by economic methodology—particularly its assumptions about individual economic behavior. However, as Alexander Rosenberg (1992, pp. 57-62) argues, Friedman both attacks a strawman and begs the question in his methodological essay. Per Rosenberg, questions about unrealistic assumptions in economics do not stem from any concern with the philosophy of science. Rather, the concern with unrealistic assumptions stems from “dissatisfaction with what Friedman assumes to be beyond question,” i.e., “the predictive success of neoclassical economic theory” (Rosenberg, 1992, p. 60). Rosenberg (1992) notably goes on to argue that economics is not empirical science due its historical inability to make improvements in the accuracy of its predictions. But as a preview of what is to come below, what is noteworthy for present purposes is that Friedman cared about the “realism of assumptions” in a scenario where an economic model *appeared to be* making successful predictions—at least in the short run.

3. FRIEDMAN’S CENTRAL ARGUMENT

It would be unreasonable to fault Friedman for not meeting the argumentative standards demanded by philosophers. Regardless, as a notable figure in his scientific field, Friedman’s methodological prescriptions should be of interest to philosophers of science, and it is the job of the philosopher of science to assess the merits of such prescriptions. The explicit arguments

avored by contemporary philosophers are nowhere to be found in Friedman's essay, yet he is certainly advancing an argument. Hausman (1992b) has performed a service to philosophers and economists alike by extracting the central argument implicit in Friedman's essay. As Hausman suggests, Friedman attempts to show that the relevance criterion follows from the ultimate goal of science, i.e., narrow predictive success. Since what scientists (and particularly economists) ultimately care about is correct prediction of some target phenomena, it supposedly follows that the only evidence scientists should consider when assessing a theory or model is evidence concerning the class of phenomena that interests them. It would then further follow that any other aspects of a theory or model, such as whether it has realistic assumptions or whether it provides a genuine explanation of the target phenomena, is irrelevant to its scientific assessment.

Hausman (1992b) shows this line of reasoning to be suspect by way of an analogy. When purchasing a used car, one's goal is typically to find a car that drives safely, economically, and comfortably. But it does not follow that the only way to determine whether a used car meets these criteria is by way of a road test; this would imply that there is nothing to learn by opening the hood of the car and inspecting its individual components. Just as looking under the hood of a used car may very well reveal important information about how it will perform, which a road test will not, so examining the "realism" (read: plausibility) of a theory's assumptions helps us better achieve narrow predictive success—a point Rosenberg (1992) also makes in response to Friedman, and which he cites as the real motivation behind why critics fault economics for relying on unrealistic assumptions.

Hausman (2001) expands on this point in a follow-up article on the role of explanation in economics. Given Friedman's insistence on narrow predictive success as the ultimate goal of science, it is perplexing that he would not even grant that explanation is a subsidiary goal of

science. Here, specifying the relevant sense of “explanation” is important. As Hausman argues, if what we ultimately care about is predicting *and controlling* economic events, this requires us to be able to uncover *causal* truths of events and states of affairs. In other words, we need to be able to give causal explanations of economic phenomena (to at least some degree) to understand why a particular theory or model will or will not fail to “get it right” in the future or in new circumstances. Hausman calls this practice *diagnosis*. Skeptics of causal explanation in economics (e.g., Reiss, 2012a) will remain unconvinced by these general remarks. But as we will see, as a practicing economist, Friedman was very much concerned with diagnosis during a time which his contemporaries were not. To use Hausman’s analogy, as a practicing economist Friedman took a look under the hood, even if he would have insisted that he did not have to.

To help illustrate the problems with Friedman’s methodological prescriptions, as well as the points made by Hausman above, I will work with the following reconstruction of Friedman’s central argument:

- (1) The goal of constructing a theory or model is to make correct predictions concerning the class of phenomena it is intended to explain or predict.
- (2) The only data that are relevant to the acceptance or rejection of a theory or model concern the class of phenomena the theory or model is intended to explain or predict (Interim conclusion – from 1).
- (3) Therefore, any other data not concerning the class of phenomena that the theory or model is intended to explain or predict is irrelevant to its scientific assessment (From 2).¹⁴

Note that, in the argument above, premise 2 (an interim conclusion) supposedly follows from premise 1. Hausman’s used car analogy aims to show why the inference from premise 1 to premise 2 is invalid. While I do not deny that the inference from premise 1 to premise 2 is fallacious, economists (and perhaps other scientists) might be better persuaded to abandon the

¹⁴ The reconstruction of Friedman’s argument that I am presenting is a synthesis of the reconstructions presented in Hausman (1992b) and Hausman (2008).

chain of reasoning above if presented with a case study illustrating exactly *why* the inference is fallacious. They might also be better persuaded by seeing that doing good economics requires that one reject the relevance criterion (premise 2) all together. And what better way to do that than to show that a landmark work by Friedman himself depends crucially on rejecting the relevance criterion?

4. FRIEDMAN’S PRESIDENTIAL ADDRESS

Friedman’s (1968) presidential address to the American Economic Association was both controversial and groundbreaking for its time—at least, this is what the economics profession collectively believes. Historically minded economists such as James Forder (2014) have recently pushed back against the narrative I am about to briefly recount.¹⁵ For my purposes, the accuracy of historical record is beside the point. What matters is that, to this day, the economics profession takes Friedman’s presidential address to be an exemplar of their discipline. It may not surprise some readers that this narrative has been propagated by Friedman himself starting with his 1977 Nobel Prize lecture. The irony is that in his Nobel Prize lecture, Friedman uses the most famous aspects of his presidential address as further evidence of the scientific credentials of economics, which he takes himself to have already established in his methodological essay.¹⁶ This would suggest that Friedman did not abandon his views on methodology in the twenty-four-year period between publishing his writings on methodology and winning a Nobel Prize. While I completely agree with Friedman that his 1968 presidential address can establish the scientific legitimacy of some aspects of economic methodology, as we will see the claim that Friedman’s actual work on

¹⁵ Just as some philosophers of economics complain about the profession’s methodological schizophrenia, it appears as if some historians of economics complain about the profession’s collective amnesia.

¹⁶ The exact quotation is as follows: “Rather than pursue these ideas in the abstract (I have discussed the methodological issues more fully in Friedman [1953]), I shall illustrate the positive scientific character of economics by discussing a particular economic issue that has been a major concern of the economics profession throughout the postwar period, namely, the relation between inflation and unemployment” (Friedman, 1977, p. 453).

methodology also establishes this conclusion is dubious at best. One can only speculate as to why economists have routinely overlooked the discrepancy between Friedman’s thought and practice which I am about to highlight. What is more important, however, is that going forward economists take heed: following what Friedman said instead of what he did can prevent economics from advancing as a science.

It was in his 1968 presidential address that Friedman famously argued that monetary policy has no long run effects on the real economy. Friedman had two arguments to support this “monetary-policy invariance hypothesis” (Hall and Sargent, 2018). The first is that monetary policy cannot peg interest rates for more than very limited periods; the second is that monetary policy cannot peg the rate of unemployment for more than very limited periods (Friedman, 1968, p. 5). Central to this second argument is what is now known as the “expectations critique,” which will be my focus going forward.¹⁷ As I show below, Friedman concerned himself with more than narrow predictive success when he argued that monetary policy cannot be used to change the rate of unemployment in the long run.

A brief detour through the recent history of economic thought is necessary to set the stage. Friedman was responding to the dominant macroeconomic framework employing the Phillips curve, a single-equation model which accounts for an inverse statistical relationship between inflation and unemployment (figure 1).¹⁸

¹⁷ The expectations critique is in Friedman (1966), but it is his presidential address that popularized it. Edmund Phelps (1967) is sometimes also credited with advancing the expectations critique.

¹⁸ The history of the Phillips curve is itself a disputed matter and there is a case to be made that the discovery of the relationship should be attributed to Irving Fisher (1926). The name of the model derives from A.W. Phillips (1958), yet in his influential paper Phillips only fits a curve to data showing a relationship between unemployment and the rate of change of money wage rates. The interpretation of the Phillips curve presented above, and its subsequent popularity, is attributable to Paul Samuelson and Robert Solow (1960). Forder writes that “[Samuelson and Solow] are often, rightly, credited with bringing the curve to the attention of a wide audience, and, with a rather murkier justification, debited with using it to advocate inflations as a means of reducing employment” (Forder, 2010, p. 496). While it is true that Samuelson and Solow’s analysis contains nuance and suggests they were aware of the limitations of the Phillips curve, their presentation of the Phillips curve graph does include the following caption:

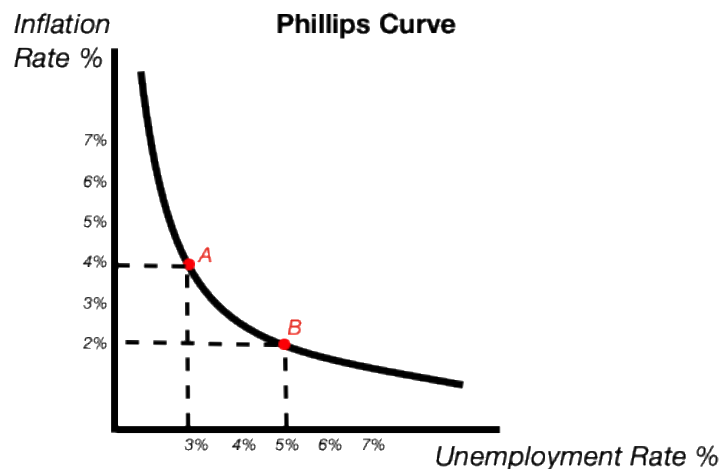


Figure 1. The Phillips curve showing a hypothetical trade-off between high inflation and low unemployment (point A) and low inflation and high unemployment (point B).

It is worth emphasizing that the Phillips curve was not part of John Maynard Keynes’s (1936) *General Theory*, but it was one of the central features of Keynesian economics which Friedman and his followers sought to challenge. Here, Friedman’s perspective is revealing:

The hypothesis that there is a stable relation between the level of unemployment and the rate of inflation was adopted by the economics profession with alacrity. It filled a gap in Keynes’s theoretical structure. It seemed to be the “one equation” that Keynes himself had said “we are . . . short” (1936, p. 276). In addition, it seemed to provide a reliable tool for economic policy, enabling the economist to inform the policymaker about the alternatives available to him (Friedman, 1977, p. 469).

This narrative is echoed in recent reflections on the legacy of Friedman’s presidential address.

For example, Robert Hall and Thomas Sargent write that:

In 1968, the idea of a Phillips curve was ascendant: expansionary monetary policy could drive down the unemployment rate, but at the cost of higher inflation. A tradeoff was thought to exist, even in the longer run. Economies willing to accept more inflation could have tighter labor markets with high employment and lower unemployment (Hall and Sargent, 2018, p. 123).

“This shows the menu of choice between different degrees of unemployment and price stability, as roughly estimated from the last twenty-five years of American data” (Samuelson and Solow, 1960, p. 192). Mankiw and Reis (2018, pp. 83-84) suggest that this “menu of choice” was the main takeaway for many policy-inclined economists of the time.

Hall and Sargent further claim that by 1968, macroeconomists “had adopted the basic Phillips curve as the favored model of correlations between inflation and unemployment” (Hall and Sargent, 2018, p. 121).

The basis for the widespread endorsement of the Phillips curve was the stable correlation between lower unemployment and higher inflation—particularly the correlation observed in the 1950s, 60s, and into the early 70s. One can say that the Phillips curve achieved narrow predictive success: a change in unemployment could be used to correctly predict a change in price inflation, and vice versa. Relying on a correlation to make a prediction is not in itself a serious transgression. After all, one may innocently rely on the established correlation between barometer readings and thunderstorms to decide when to go for a walk in the park. Friedman’s real concern with regard to the Phillips curve was twofold. First, he took issue with what he regarded as an a-theoretical attitude towards the relationship among his contemporaries (Friedman, 1977, p. 455). This a-theoretical attitude led to a more alarming, second worry, with practical consequences, namely, that economists and policymakers of the time believed that the relationship in question could be exploited to lower unemployment by intervening on variables that would increase inflation.

The most obvious way in which policymakers might think that they could exploit the Phillips curve for desirable ends is by increasing the money supply, which would then increase inflation and lower unemployment. Yet there are multiple causal pathways one could specify in order to account for how this intervention would work, and Friedman’s Keynesian contemporaries appeared to express little interest in identifying any causal pathway before making policy recommendations. For example, one possibility is that increasing the money supply increases aggregate demand, which then serves as a common cause of both lower

unemployment and higher inflation. Another is that increases in the money supply increase aggregate demand, which causes lower unemployment, which then causes wages to rise, and in turn, causes higher inflation. For what it is worth, A.W. Phillips, for whom the model is named after, did conclude his influential paper by suggesting that “There is need for much more detailed research into the relations between unemployment, wage rates, prices and productivity” (Phillips, 1958, p. 299). But even *if* a causal story along the lines of the ones above were posited, notice that they all involve relations among macrolevel phenomena and make no reference to individual economic agents. As we see below, what made Friedman’s criticism of the Phillips curve so effective is that he looked at how microlevel phenomena underpinned macrolevel phenomena.

5. THE EXPECTATIONS CRITIQUE OF THE PHILLIPS CURVE

Regardless of the causal story, and per Friedman’s narrative, economists and policymakers in the 1960s believed that the Phillips curve could be used to peg the rate of unemployment via increases or decreases in the money supply. As far as they were concerned, increasing the money supply would lead to lower unemployment and higher inflation; decreasing it would lead to higher unemployment and lower inflation. Friedman famously established that to lower unemployment by increasing the money supply amounted to a fool’s errand: policymakers would find themselves where they started in due time, except with more inflation. His ultimate conclusion was that “there is always a temporary trade-off between inflation and unemployment; there is no permanent trade-off. The temporary trade-off comes not from inflation per se, but from *unanticipated inflation*” [my emphasis] (Friedman, 1968, p. 11). Unanticipated inflation is a variable that his contemporaries had not considered, perhaps because it is not a variable that is easily measured and aggregated. To account for unanticipated inflation, one must refer to the beliefs of economic agents, which is exactly how Friedman was able to give a crucial diagnosis:

the relationship between inflation and unemployment posited by the Phillips curve breaks down in the long run.

What kind of beliefs does one need to make reference to? In this case, Friedman relied on economic agent's beliefs about the economy, in particular its price levels. These beliefs are nowadays called "expectations." It is ultimately the expectations of individual agents that underpin Friedman's diagnosis, and in turn his explanation, of how various macrolevel phenomena relate, or fail to relate, to one another. Friedman's explanation is best left said in his own words. First, he begins by accounting for why the Phillips curve has led to short run predictive success. He asks us to consider a scenario in which prices have been stable and an authority decides to increase the money supply in response to high unemployment, which then leads to an increase in income and spending:

To begin with, much or most of the rise in income will take the form of an increase in output and employment rather than in prices. People have been expecting prices to be stable, and prices and wages have been set for some time in the future on that basis. It takes time for people to adjust to a new state of demand. Producers will tend to react to the initial expansion in aggregate demand by increasing output, employees by working longer hours, and the unemployed, by taking jobs now offered at former nominal wages. This much is pretty standard doctrine (Friedman, 1968, p. 10).

But as he notes, this only describes the initial effects of this expansionary monetary policy. The failure of the Phillips curve is accounted for below:

Because selling prices of products typically respond to an unanticipated rise in nominal demand faster than prices of factors of production, real wages received have gone down—though real wages anticipated by employees went up, since employees implicitly evaluated the wages offered at the earlier price level. Indeed, the simultaneous fall ex post in real wages to employers and rise ex ante in real wages to employees is what enabled employment to increase. But the decline ex post in real wages will soon come to affect anticipations. Employees will start to reckon on rising prices of the things they buy and to demand higher nominal wages for the future. "Market" unemployment is below the "natural" level. There is an excess demand for labor so real wages will tend to rise toward their initial level.

Even though the higher rate of monetary growth continues, the rise in real wages will reverse the decline in unemployment, and then lead to a rise, which will tend to return unemployment to its former level (Friedman, 1968, p. 10).

To put it in simpler terms, the idea is that the workers perceive the effects of an expansionary monetary policy on their nominal wages before they perceive the effects of an expansionary monetary policy on the bundles of goods and services they can purchase with their wages. In the short run workers are, in a sense, fooled into supplying more labor based on their mistaken beliefs about their real wages. The key insight is that workers cannot be fooled forever, and so their expectations are *adaptive*. In the long run workers catch onto the fact that their supposedly higher wages cannot buy the bundles of goods and services they had expected, and so they will continually demand higher wages up until unemployment eventually returns to its former level.

Using this insight, Friedman predicted that increasing inflation via the money supply would not reduce unemployment in the long run. What's more, Friedman was able to predict that both inflation and unemployment would rise (stagflation) during the economic recession of 1973-1975. As Gregory Mankiw and Ricardo Reis point out, this is "one of the greatest successes of out-of-sample forecasting by a macroeconomist" (Mankiw and Reis, 2018, p. 88). What would account for such a success? In this instance, it appears as if looking under the hood paid off.

6. WHEN THE "REALISM OF ASSUMPTIONS" MATTERED

As I now argue, if Friedman had internalized the methodological views he is typically associated with, he would not have bothered to criticize the Phillips curve, which up until that point had been successful in making accurate predictions about the macrolevel phenomena it was intended to predict (the relationship between inflation and unemployment). Not only that, but per his own admission, the Phillips curve appeared to be a reliable tool for guiding economic policy. Given

the pragmatic attitude expressed in Friedman's methodological essay, it is not a stretch to suggest that reliably guiding policy is exactly the kind of goal he envisioned for economics qua scientific enterprise.

The first obvious thing to note is that in his presidential address Friedman abandoned his anti-explanatory stance. Though he is not explicit about this, there is no denying that Friedman gives an explanation of *why* there has been an observed relationship between inflation and unemployment. Here, Friedman employs classical economic methodology by relying on common-sense psychology to give an explanation of an economic phenomena grounded in aggregate human action.¹⁹ It is expectations presumably combined with preferences that *cause* the behavior that generates the relationship in the short run. It is this understanding of the causal basis for the observed relationship between inflation and unemployment that in the end allowed Friedman to diagnose why intervening on variables such as the money supply would only temporarily lower unemployment, thereby leading to predictive success in the long run. When the rubber hit the road, Friedman was in the business of providing causal explanations.²⁰

The most unfortunate aspect of Friedman's 1953 essay is the fact that his own methodological views could have been turned against him at his presidential address. To establish this point, it is helpful to imagine how a contemporary of Friedman, convinced of

¹⁹ This is a complication for Hirsch and De Marchi's (1990) interpretation of Friedman's methodology as a reaction to the "introspective" methodology associated with classical economics.

²⁰ I am not the first to make this point. Kevin Hoover (2009) argues that Friedman and Schwatz's (1963) *A Monetary History of the United States, 1867-1960* employed a causal realist methodology instead of the instrumentalist methodology Friedman is known for. Hoover uses this analysis to interpret Friedman's methodological essay as a causal realist statement despite Friedman's reluctance to directly invoke casual language—a point Hoover does address. I agree with Hoover that as a practicing economist Friedman sought to "identify the true mechanisms underlying observed phenomena" (Hoover, 2009, p. 314). The main conclusion of this paper could certainly support Hoover's interpretation, though I am hesitant to endorse Hoover's reading due to the interpretive difficulties noted earlier, and that my real concern is how economists interpret Friedman to this day. A key distinction between my contribution and Hoover's is that I am emphasizing the importance common-sense psychology played in Friedman's theoretical toolbox, whereas Hoover is more concerned with realistic causal relations among macrolevel phenomena in advancing his interpretation of Friedman's methodology.

Friedman's methodological views, would have responded in defense of the Phillips curve. We can stipulate that our imaginary interlocutor was of the Keynesian persuasion for added measure. The point is not that actual Keynesians did, in fact, appeal to Friedman's off-brand instrumentalism to defend themselves but rather that they could have very well defended themselves by doing so. Let us consider what such a hypothetical response would have looked like.

First and foremost, our hypothetical Keynesian would have been dismissive of Friedman's attempt to explain the relationship posited by the Phillips curve in the first place. Friedman's very own invocation of the underdetermination problem fifteen years earlier supports the a-theoretical attitude which he associated with Keynesianism. The Phillips curve is a clear example of how the correct theory may be underdetermined by the available data. As noted earlier, there are multiple observationally equivalent—and therefore epistemically equivalent—hypotheses (causal stories) one could give about how macrolevel phenomena interact to produce the Phillips curve. Hence our hypothetical interlocutor could have retorted: "your concern with 'explaining' the Phillips curve is a hopeless endeavor. What matters is that the model help guide policy, which to date it has done so reliably."

Of course, the appeal to common-sense psychological reasoning lends a lot of plausibility to Friedman's explanation. It also lends plausibility to Friedman's warning that intervening on the money supply to take advantage of the Phillips curve would eventually backfire. This virtue of Friedman's explanation is what perhaps leads to the most embarrassing objection someone could have raised. Faced with the complications that the role of expectations spells out for the Phillips curve, our hypothetical interlocutor could have dismissed Friedman's expectations critique as an instance of trying to test a theory (or in this case a model) by the "realism of its

assumptions.” Our interlocutor could simply accept the added realism of Friedman’s account is descriptively accurate but simply suggest that the Phillips curve assumes that expectations do not adjust to the reality of inflation; this could simply be stipulated as one of the axioms of the Phillips curve model. She could say: “your concern with the realism of this assumption is irrelevant to the assessment of the Phillips curve. Assuming expectations do not adjust to inflation works for the purpose at hand.”

At this stage it is worth considering what Friedman says in section IV of his methodological tract, a section in which he reverses course. Here, Friedman discusses the “significance and role of the ‘assumptions’ of a theory,” (1953, p. 23) though he notes he is not confident in his remarks on the matter. Of particular interest is the second role he acknowledges: “the use of ‘assumptions’ as an indirect test of a theory” (1953, p. 26). In response to our hypothetical interlocutor’s claim that the Phillips curve assumes that expectations do not adjust to inflation Friedman could respond that assumptions can sometimes be a source of indirect evidence as he does in the passage below:

what are called the assumptions of a hypothesis can be used to get some indirect evidence on the acceptability of the hypothesis in so far as the assumptions themselves can be regarded as implications of the hypothesis, and hence their conformity with reality as a failure of some implications to be contradicted, or in so far as the assumptions may call to mind our implications of the hypothesis susceptible to casual empirical observation (1953, p. 26).

In this part of the essay, Friedman is making concessions to classical economic methodology by suggesting that plausible assumptions about human behavior *can* indirectly support a theory (or model). Similarly, one can take Friedman’s presidential address as suggesting that plausible assumptions about human behavior can indirectly discredit theories and models in certain circumstances.

Friedman's hypothetical interlocutor could grant the point quoted above, namely, that plausible assumptions can indirectly support a theory. But she would not grant that plausible assumptions can ever discredit a theory. She could easily remind Friedman of the supposed plausibility of his leaves-on-a-tree hypothesis or billiard-player example, which casual observation clearly provides indirect evidence against. She would ultimately maintain that indirect evidence does not cut both ways. In other words, indirect evidence can only support a theory; it can never be used against a theory. Providing indirect evidence against a theory amounts to nothing more than a test of a theory by the "realism of its assumptions."

Without a radical reinterpretation of his methodological essay, the best one can do in Friedman's defense is point out that, from his own methodological point of view, the monetary-policy invariance hypothesis would come to be assessed as superior after the Phillips curve decisively broke down as he correctly anticipated. Yet considering that Friedman acknowledged that it would take somewhere between two to five years before the initial effects of a new inflationary regime would wear out (Friedman, 1968, p. 11), one must wonder whether he would have been willing to bite his tongue until the confirming evidence trickled in. It would have been strange for Friedman to have refrained from advising policymakers to pursue his preferred monetary policy. Of course, supposing policymakers did not also hold idiosyncratic views on the philosophy of science, he perhaps could have appealed to the realistic assumptions his hypothesis rested on, but that would have clearly involved compromising his own methodological tenets.

7. MICROFOUNDATIONS AND THE RELEVANCE CRITERION

Recall that the relevance criterion is the true source of Friedman's prohibition on appealing to plausible assumptions—a prohibition he nonetheless disregarded as a practicing economist. Having established this, as well as the importance of diagnosis and explanation for achieving

narrow predictive success, I now turn to the relevance criterion itself along with the question of what kind of data is relevant for theory and model assessment. While economists nowadays may be more amenable to granting a positive role to explanation, the relevance criterion restricts what kind of data is “relevant” for this purpose. Friedman’s central argument is once again reproduced below:

- (1) The goal of constructing a theory or model is to make correct predictions concerning the class of phenomena it is intended to explain or predict.
- (2) The only data that are relevant to the acceptance or rejection of a theory or model concern the class of phenomena the theory or model is intended to explain or predict (Interim conclusion – from 1)
- (3) Therefore, any other data not concerning the class of phenomena that the theory or model is intended to explain or predict is irrelevant to its scientific assessment. (trivially from 2)

Unlike before, we are now in a position to see why this argument is unsound. Not only is premise 2 (the relevance criterion) arrived at through a specious inference, but no serious economist should accept it when its full implications are drawn out.

Let us consider once again a hypothetical interlocutor of the Keynesian persuasion. Recall that whatever the causal story Keynesian economists endorsed (if any) in order to account for the Phillips curve, it relied upon generalizations about macrolevel phenomena, e.g., aggregate demand, the money supply, the rate of unemployment, the rate of inflation, etc. To predict and control a macrolevel phenomenon such as a decrease in unemployment, it appeared as if the relevant data one needed to look at concerned the rate of inflation and the size of the money supply (among other things) as Phillips suggests at this end of his influential paper. Skeptical of Friedman’s pronouncements in his presidential address, an economist fifty years ago could have argued:

- (1) The goal of constructing a theory or model is to make correct predictions concerning the class of phenomena it is intended to explain or predict.

(2) The only data that are relevant to the acceptance or rejection of a theory or model concern the class of phenomena the theory or model is intended to explain or predict (Interim conclusion – from 1).

(3*) Macroeconomic models are intended to explain or predict macroeconomic phenomena (e.g., inflation rates, unemployment rates).

(4*) Therefore, only data concerning macroeconomic phenomena are relevant to the acceptance or rejection of macroeconomic models (From 2 and 3*).

One may object that Keynesians would not have endorsed this argument; they after all placed emphasis on the irrational “animal spirits” that underpinned macrolevel phenomena. But this is beside the point. We can imagine how an argument such as the one above (and indeed Friedman’s own methodological position) could have been used to reject Friedman’s expectations critique and his monetary-policy invariance hypothesis. Macroeconomic models, almost by definition, seek to predict or explain macroeconomic phenomena. Appealing to microlevel phenomena like expectations to criticize the Phillips curve would therefore be inappropriate. Expectations are an entirely different class of phenomena than unemployment rates. Not only are the former microlevel, but more crucially, expectations are *intentional* phenomena; the latter are non-intentional macrolevel statistical aggregates (Hoover, 2008). According to our hypothetical economist convinced of Friedman’s methodological views, expectations are not relevant to our acceptance or rejection of any macroeconomic model, including the Phillips curve.

Contemporary economists will—and rightfully should—find the conclusion of this argument objectionable. There is no denying that nowadays “expectations remain at the forefront of macroeconomic analysis” (Mankiw and Reis, 2018, p. 87). As the story surrounding Friedman’s presidential address goes, one of the more immediate consequences of the presidential address was that it stressed the importance of microfoundations for macroeconomic modeling in both the new classical and the New-Keynesian schools that emerged immediately

after. The importance of microfoundations in contemporary macroeconomics is perhaps best enshrined in the work of Robert Lucas Jr., who extended Friedman’s insight regarding the Phillips curve and emphasized caution when using aggregated macrolevel data for policymaking. The central lesson of the influential *Lucas critique* (Lucas, 1976) is that policy changes based on large-scale macroeconometric forecasting models will fail to be effective if the policy relies on a model that does not consider how individual utility-maximizing agents will respond to the decision problem they face in light of the proposed policy changes. The subsequent challenge for macroeconomists has been to build models that specify microfoundations to get around the Lucas critique, and macroeconomists including Lucas have attempted to do so by modeling the optimization problems individual economic agents face in light of new constraints.²¹

Given the considerations above, contemporary economists are left with only one option if they wish to block the reasoning of our hypothetical interlocutor and maintain the methodological importance of specifying microfoundations: reject the premise that only data that are relevant to the acceptance or rejection of a model concern the class of phenomena the model is intended to explain or predict. Otherwise, the importance of microfoundations in macroeconomics—perhaps one of the most important recent developments in economic methodology—cannot be adequately justified.

8. CONCLUSION

²¹ Of course, one cannot model every single agent in the economy, and so there has been recourse to modeling the decision problem of a single representative-agent. What is noteworthy about these so-called representative-agents is that in addition to being utility-maximizers, they are also modeled as having *rational* expectations, i.e., detailed and sophisticated knowledge of how the proposed policy changes will affect their economic choices, thereby by allowing them to adapt instantly. John Muth (1961), who first proposed rational expectations in a microeconomic context, is often credited with this modeling strategy. However, since a single representative-agent obviously has no interpersonal interactions, one may reasonably question whether these representative-agent models provide *genuine* microfoundations. Indeed, the emphasis on rational expectations in macroeconomic modeling is perhaps the methodological equivalent of taking one step forward, two steps back. See Hoover (2001, Ch. 6) and Hoover (2008) for criticisms of the current representative-agent approach to microfoundations.

In closing, I turn now to some brief remarks concerning the contemporary controversies surrounding the behavioral turn in economics. Such remarks may at first appear to be completely unrelated to the preceding discussion. But as noted at the outset, Friedman's methodological stance is still popular among economists. Faruk Gul and Wolfgang Pesendorfer (2008) have recently invoked Friedman's methodological views to defend mainstream economic theory from the encroachments of psychologists and neuroscientists.²² Echoing Friedman's insistence that one should not test a theory by the "realism of its assumptions," Gul and Pesendorfer claim that their central argument is simple: "neuroscience evidence cannot refute economic models because the latter make no assumptions or draw no conclusions about the physiology of the brain" (Gul and Pesendorfer, 2008, p. 22). Space does not permit a full assessment of the claim that "psychological and physiological evidence are directly relevant to economic theories" and that this evidence "can be used to support or reject economic models or even economic methodology" (Gul and Pesendorfer, 2008, p. 3).²³ Call this *The Neuroeconomic Thesis* for short. Despite the lack of space, I take the considerations above to bear directly on the plausibility of the Neuroeconomic Thesis. I should emphasize that by rejecting the relevance criterion I have not established the Neuroeconomic Thesis but rather have only left the door open for it. I *do* take the argument in the previous section to show is how misguided it is for economists to insist that the only data that is relevant to the acceptance or rejection of a model concern the class of phenomena the model is intended to explain or predict. Rejecting such methodological claims has allowed the science of economics to progress in the past. So, it is surprising to see it invoked in the present day.

²² Gul and Pesendorfer's writings on economic methodology draw on other influences besides Friedman. See Hausman (2008) for a philosophical analysis.

²³ See Camerer (2008) for an introduction to the burgeoning field of neuroeconomics.

The lessons from Friedman’s presidential address can be put into perspective with an analogy: just as macroeconomics has not been successfully reduced to microeconomics, but instead is informed by microeconomics and taken into consideration, so too can neuroeconomics be evidentially relevant to microeconomics and macroeconomics.²⁴ Perhaps it would be important to distinguish a *Strong Neuroeconomic Thesis* from a *Weak Neuroeconomic Thesis*. The weak version would suggest that psychological and physiological data *may be* directly relevant to the assessment of an economic model just as microeconomic data may very well be relevant to assessing macroeconomic models. The stronger version would call for something like the reducibility and replacement of economics and its concepts to the language of psychology and neuroscience. At times Gul and Pesendorfer address something like this stronger thesis—in part because their opponents also appear to espouse it—and their arguments (which I do not survey here) are worthy of consideration on this front. Yet even if one is an instrumentalist about economic science, and one that is at most concerned with narrow predictive success, then one should welcome something like a Weak Neuroeconomic Thesis.²⁵ Psychological and neurophysiological data may be essential for diagnosing when a theory or model will fail in the future or in new circumstances. I have attempted to make economists more amenable to such a view, not by appealing to hairsplitting philosophical argument, but rather by appealing to the actual methodological practices of one of their own.

²⁴ Rosenberg (1992, pp. 129-130) also makes a similar analogy in passing. However, Rosenberg appears to also endorse a much more reductionistic program when he writes that the “failure of microeconomic theory to uncover laws of human behavior is due to its wrongly assuming that these laws will trade in desires, beliefs, or their cognates” (Rosenberg, 1992, p. 236)

²⁵ In fact, Raj Chetty, one of the most celebrated economists alive today, has recently emphasized this exact point in his (2015) Richard T. Ely lecture. Erik Angner (2019) takes Chetty’s views as representative of economists changing attitude towards behavioral themes and suggests a *behavioral synthesis* is on the horizon.

REFERENCES

- Angner, E. (2019). We're All Behavioral Economists Now. *Journal of Economic Methodology*, 26, 195-207.
- Boland, L. (1979). A Critique of Friedman's Critics. *Journal of Economic Literature*, 17, 503-522.
- Brunner, K. (1969). 'Assumptions' and the Cognitive Quality of Theories. *Synthese*, 20, 501-525.
- Camerer, C. (2008). Neuroeconomics: Using Neuroscience to Make Economic Predictions. In D. Hausman (Ed.), *The Philosophy of Economics: An Anthology* (pp. 356-377). Cambridge: Cambridge University Press.
- Chetty, R. (2015). Behavioral Economics and Public Policy: A Pragmatic Perspective. *American Economic Review*, 105, 1-33.
- Fisher, I. (1926). A Statistical Relation Between Unemployment and Price Changes. *International Labour Review*, 13, 785-792.
- Forder, J. (2010). The Historical Place of the 'Friedman-Phelps' Expectations Critique. *The European Journal of the History of Economic Thought*, 17, 493-511.
- Forder, J. (2014). *Macroeconomics and the Phillips Curve Myth*. Oxford: Oxford University Press.
- Friedman, M. (1953). The Methodology of Positive Economics. In *Essays in Positive Economics* (pp. 3-43). Chicago: Chicago University Press.
- Friedman, M. (1966). What Price Guideposts? In G. Schultz and R. Aliber (Eds.), *Guidelines, Informal Controls, and the Market Place* (pp. 17-39). Chicago: Chicago University Press.
- Friedman, M. (1968). The Role of Monetary Policy. *The American Economic Review*, 58, 1-17.

- Friedman, M. (1977). Nobel Lecture: Inflation and Unemployment. *Journal of Political Economy*, 85, 451-472.
- Friedman, M. and Schwartz, A. (1963). *A Monetary History of the United States, 1867-1960*. Princeton: Princeton University Press.
- Gul, F. and Pesendorfer, P. (2008). The Case for Mindless Economics. In A. Caplin and A. Schotter (Eds.), *The Foundations of Positive and Normative Economics: A Handbook* (pp. 3-42). Oxford: Oxford University Press.
- Hall, R. and Sargent, T. (2018). Short-run and Long-run Effects of Milton Friedman's Presidential Address. *Journal of Economic Perspectives*, 32, 121-134.
- Hammond, J. (2009). Early Drafts of Friedman's Methodology. In U. Mäki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 68-89). Cambridge: Cambridge University Press.
- Hausman, D. (1992a). *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.
- Hausman, D. (1992b). Why Look Under the Hood? In *Essays on Philosophy and Economic Methodology* (pp. 70-73). Cambridge: Cambridge University Press.
- Hausman, D. (1998). Problems with Realism in Economics. *Economics and Philosophy*, 14, 185-213.
- Hausman, D. (2001). Explanation and Diagnosis in Economics. *Revue Internationale de Philosophie*, 3, 311-326.
- Hausman, D. (2008). Mindless or Mindful: A Methodological Evaluation. In A. Caplin and A. Schotter (Eds.), *The Foundations of Positive and Normative Economics: A Handbook* (pp. 125-155). Oxford: Oxford University Press

- Hirsch, A. and De Marchi, N. (1990). *Milton Friedman: Economics in Theory and Practice*. Herfordshire: Harvester Wheatsheaf.
- Hoover, K. (2001). *Causality in Macroeconomics*. Cambridge: Cambridge University Press.
- Hoover, K. (2008). Does Macroeconomics Need Microfoundations? In D. Hausman (Ed.), *The Philosophy of Economics: An Anthology* (pp. 315-333). Cambridge: Cambridge University Press.
- Hoover, K. (2009). Milton Friedman's Stance: The Methodology of Causal Realism. In U. Mäki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 303-320). Cambridge: Cambridge University Press.
- Keynes, J.M. (1936). *General Theory of Employment, Interest, and Money*. London: Macmillan.
- Kuhn, T. (1977). Objectivity, Value Judgment, and Theory Choice. In *The Essential Tension* (pp. 74-86). Chicago: Chicago University Press.
- Lucas, R. (1976). Econometric Policy Evaluation: A Critique. In K. Brunner. and A. Meltzer (Eds.), *Theory, Policy, Institutions: Papers from the Carnegie-Rochester Conference Series on Public Policy* (Vol. 1, pp. 19-46). Elsevier.
- Mäki, U. (2000). Reclaiming Relevant Realism. *Journal of Economic Methodology*, 7, 109-125.
- Mäki, U. (2009a). Reading *the* Methodological Essay in Twentieth-Century Economics: Map of Multiple Perspectives. In U. Mäki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 47-67). Cambridge: Cambridge University Press.
- Mäki, U. (2009b). Rereading and Rewriting F53 as a Realist Statement. In U. Mäki (Ed.), *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy* (pp. 90-116). Cambridge: Cambridge University Press.

- Mankiw, G. and Reis, R. (2018). Friedman's Presidential Address in the Evolution of Macroeconomic Thought. *Journal of Economic Perspectives*, 32, 81-96
- Muth, J. (1961). Rational Expectations and the Theory of Price Movements. *Econometrica*, 29, 315-333.
- Nagel, E. (1963). Assumptions in Economic Theory. *American Economic Review*, 53, 211-219.
- Phelps, E. (1967). Phillips Curve, Expectations of Inflation and Optimal Unemployment Over Time. *Economica*, 34, 254-281.
- Phillips, A.W. (1958). The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861-1957. *Economica*, 25, 283-299.
- Popper, K. (1963). *Conjectures and Refutations*. London: Routledge.
- Quine, W.V.O. (1951). Two Dogmas of Empiricism. *Philosophical Review*, 60, 20-43.
- Reiss, J. (2010). Review of *The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy*. *Erasmus Journal for Philosophy and Economics*, 3, 103-110.
- Reiss, J. (2012a). Idealization and the Aims of Economics: Three Cheers for Instrumentalism. *Economics and Philosophy*, 28, 363-383.
- Reiss, J. (2012b). The Explanation Paradox. *Journal of Economic Methodology* 19, 43-62.
- Rosenberg, A. (1992). *Economics—Mathematical Politics or Science of Diminishing Returns?* Chicago: The Chicago University Press.
- Samuelson, P. and Solow, R. (1960). Analytical Aspects of Anti-Inflation Policy. *American Economic Review*, 50, 177-194.