

Slaves of the Defunct:
The Epistemic Intractability of the Hayek-Keynes Debate

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ABSTRACT

The present essay addresses the epistemic difficulties involved in achieving consensus with respect to the Hayek-Keynes debate. In particular, it is argued that the debate cannot be settled on the basis of the observable evidence; or, more precisely, that the empirical implications of the theories of Hayek and Keynes are such that, regardless of what is observed, both of the theories can be interpreted as true, or at least, not falsified. Regardless of the evidence, both Hayek and Keynes can be interpreted as right.

The underdetermination of theories by evidence is an old and ubiquitous problem in science. The present essay makes explicit the respects in which the empirical evidence underdetermines the choice between the theories of Hayek and Keynes. In particular, it is argued both that there are convenient responses one can offer that protect each theory from what appears to be threatening evidence (i.e., that the choice between the two theories is underdetermined in the holist sense) and that, for particular kinds of evidence, the two theories are empirically equivalent (i.e., with respect to certain kinds of evidence, the choice between the two theories is underdetermined in the contrastive sense).

DEDICATION

To my family: Bonnie, John, Judy, and Cindy. This would not have been possible without your support—emotional, financial, and otherwise.

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PREFACE

“The great debate [in business cycle theory] is still Keynes versus Hayek. All else is footnote.”—NYU economist Mario Rizzo, July 1, 2010 *Christian Science Monitor Online*

A casual perusal of the relevant literature makes it seem that every contemporary author who dares write the names of F.A. Hayek and John Maynard Keynes on the same page is obligated to the ghost of the late Sir John Hicks to open with a quote from Hicks’ 1967 discussion of Hayek’s business cycle theory. The reader should consider my duty to the late Professor Hicks hereby discharged:

When the definitive history of economic analysis during the nineteen-thirties comes to be written, a leading character in the drama (and it was quite a drama) will be Professor Hayek. Hayek’s economic...writings are almost unknown to the modern student; it is hardly remembered that there was a time when the new theories of Hayek were the principal rival of the theories of Keynes. Which was right? Keynes or Hayek?¹

If there is any substance to the epigraph that precedes this essay – if it is true that everything in business theory is mere footnote to the Hayek-Keynes debate² – then answering Hicks’ question decisively is one of the more pressing issues confronting economic science, and indeed, given the depressing (not to say *depressed*)

¹ Hicks (1967, 203); Hicks wrote this critique at the height of the Keynesian ascendancy. The subsequent descent of the Keynesian orthodoxy and Hayek’s slight professional rebound belie Hicks’ assessment of Hayek’s lack of influence; nonetheless, the quote is a perfect indication of the rout that Austrian capital-based economics suffered at the expense of Keynes’ approach in the short- to intermediate-term wake of the 1936 publication of Keynes’ *General Theory*.

² And for the purposes of the current essay, we will assume that Rizzo’s assertion is correct.

economic conditions of the last several years, the world. Unfortunately, nearly 75 years on from the publication of Keynes' *General Theory of Employment, Interest, and Money* (1936) and over 80 years on from the publication of Hayek's *Prices and Production* (1931), there is little substantive agreement across the economics profession with respect to the correct answer to Hicks' question. Of course, there are cliques within the profession in which one theory or the other is defended against all comers: there are Keynesians and there are Hayekians (AKA Austrians).³ Why do the members of these respective camps believe as they do? On what grounds have they made their particular choices? Why do they disagree? Can't we all just get along?

The present essay addresses the epistemic difficulties involved in achieving consensus with regard to the answer to Hicks' question. In particular, it is argued that the question cannot be decided on the basis of the observable evidence; or, more precisely, that the empirical implications of the two theories are such that, regardless of what is observed, both of the theories can be interpreted as true, or, at

³ And, of course, there are economists who reject both approaches in favor of some third way or another. Some readers may wonder why the positions associated with the University of Chicago are not considered here. Despite the fact that they famously draw policy conclusions opposed to those of Keynes and his intellectual descendants, the business cycle theories of the Chicago School, such as they are, are all based on a Keynesian theoretical framework, and largely borrow Keynes' methodology (i.e., the theoretical focus is on explaining relationships between aggregative and composite variables). For the purposes of the present paper, the relevant theories of the Chicago School count as Keynesian-related, or mere "footnote" in the sense of the epigraph above. Indeed, in general, whatever the unique details of some proposed third way, it is assumed in the present essay that they amount to no more than footnotes on the Hayek-Keynes debate. As can be easily shown – though no attempt will be made to show it here – there is no extant theory of the business cycle that dodges the epistemic difficulties raised in the later chapters of the present essay for the theories of Hayek and Keynes.

least, not falsified. Regardless of the evidence, *both* Hayek and Keynes can be interpreted as right.⁴

The underdetermination of theories by evidence is an old and ubiquitous problem in science. The present essay makes explicit the respects in which evidence underdetermines the choice between the theories of Hayek and Keynes. In particular, it is argued both that there are convenient responses one can offer that protect each theory from what appears to be threatening evidence (i.e., that the choice between the two theories is underdetermined in the *holist* sense) and that, for particular kinds of evidence, the two theories are empirically equivalent (i.e., with respect to certain kinds of evidence, the choice between the two theories is underdetermined in the *contrastive* sense).⁵

⁴ This problem vis à vis business cycle theory has been recognized before. See, e.g., Rothbard (2008, xxxix-xl):

Suppose a theory asserts that a certain policy will cure a depression. The government, obedient to the theory, puts the policy into effect. The depression is not cured. The critics and advocates of the theory now leap to the fore with interpretations. The critics say that failure proves the theory incorrect. The advocates say that the government erred in not pursuing the theory boldly enough, and that what is needed is stronger measures in the same direction. Now the point is that *empirically there is no possible way of deciding between them*. Where is the empirical “test” to resolve the debate?

Rothbard answers this question to the effect that, “Clearly, the only possible way of resolving the issue is in the realm of pure theory—by examining the conflicting premises and chains of reasoning” (*Ibid*; italics in the original). This latter thesis is not the business of the present essay. Suffice it to say that I see little reason to think that epistemic difficulties similar to those raised here will not arise in deciding the status of the “conflicting premises.” It seems to me that Rothbard’s solution solves little.

⁵ For more on the distinction between holist and contrastive forms of underdetermination, see Stanford (2009). In particular, note that

The first chapter of the present essay encompasses a history of the debate between the two principals. The objective is to explicate the fundamental methodological and theoretical issues that differentiate the positions of Hayek and Keynes and their respective followers. If it is true that everything in business cycle theory is mere footnote to the Hayek-Keynes debate, then what is the subject of this marginalia? Unfortunately, the need for relative brevity prevents extensive discussion in this chapter of either the fascinating biographical and personal details of the two principals or the historical development of their respective theoretical perspectives. In any case, these matters have been dealt with adroitly by other authors.⁶ The discussion of the history of the Hayek-Keynes debate in the first chapter is slave to the argument of the second chapter concerning the epistemic difficulties involved in arriving at a consensus with respect to Hicks' question. So, the first chapter seeks to say no more and no less than is necessary and sufficient to substantiate the argument in the sequel.

Contrastive underdetermination is so-called because it questions the ability of the evidence to confirm any given hypothesis *against alternatives*, and the central focus in this connection...concerns the character of the supposed alternatives. Of course, the two problems are not *entirely* disconnected, because it is open to us to consider alternative possible modifications of the web of beliefs as alternative theories or theoretical "systems" between which the empirical evidence alone is powerless to decide. But...one *need* not think of the alternative responses to recalcitrant experience as competing theoretical alternatives to appreciate the character of the holist's challenge, and...one need not embrace any version of holism about confirmation to appreciate the quite distinct problem that the available evidence might support more than one theoretical alternative...[I]f we give up such extreme holist views of evidence, meaning, and/or confirmation, the two problems take on very different identities, with very different considerations in favor of taking them seriously, very different consequences, and very different candidate solutions.

⁶ For a recent treatment, see Wapshott (2011)

The second chapter addresses the difficulties involved in arriving at a consensus regarding the proper response to Hicks' question. The observable implications of each theory are described, or, more carefully, the aspects of each theory that would have to be observable in order for empirical evidence to guide a choice between them are considered. In the process, the respects in which the choice between the theories of Hayek and Keynes is underdetermined both holistically and contrastively are revealed.

The third chapter considers the relevance for the debate of various alternative strategies for choice between underdetermined theories. The implications of both theories are meant to work only under particular conditions; however, these conditions are underspecified by both Keynes and Hayek. In *The Separate and Inexact Science of Economics*,⁷ his canonical book on (micro)economic methodology, Dan Hausman advances four necessary criteria for belief in such implicitly qualified generalizations as scientific laws. It is argued that Hausman's criteria, which are fine as far as they go, don't go far toward resolving the absence of consensus with regard to the correct answer to Hicks' question.

In his seminal paper *Demystifying Underdetermination*,⁸ Larry Laudan suggests that, though it may be reasonable to believe that the resources of deductive logic underdetermine the choice of scientific theories, the addition of inductive (or "ampliative") criteria may suffice to settle the choice between two or more theories. However, it is argued that, whatever the case may be with regard to science, or even

⁷ Hausman (1992)

⁸ Laudan (1990)

just economics outside of business cycle theory, the appeal to inductive criteria is not sufficient to settle the case between Keynes and Hayek. Keynesians and Austrians adopt unique and, in many cases, competing ampliative criteria, and there is no non-question-begging perspective from which we can judge the rationality of adopting different ampliative methodological principles.

In the concluding chapter, I consider the consequences of the argument both for the future of business cycle research and for policies aimed at ameliorating the effects of the cycle. I defend a sort of pluralism as the proper methodological response and caution as the proper political one. I also briefly consider the question of whether, in his later years, Hayek may have found the consequences of the present paper agreeable, despite (or perhaps even because of) the pessimistic conclusions it draws with respect to the evaluation of his trade cycle theory.

CHAPTER 1

A (RELATIVELY) BRIEF HISTORY OF THE HAYEK-KEYNES DEBATE

1 Hayek's *Monetary Theory and the Trade Cycle* (1929)

Hayek's first book, and the opening argument in his business cycle project, is primarily methodological in its focus. In order to understand Hayek's argument in *Monetary Theory and the Trade Cycle* (hereafter *MTTC*) it helps to know something of the context in which it was written. The book, originally published in German in 1929 and aimed at an audience of German-language business cycle theorists, was intended to counter the widespread resistance in German economics at the time to *monetary* theories of the trade cycle, which were typically associated with naïve versions of the quantity theory of money. Like his intended audience, Hayek was also critical of crude treatments of the quantity theory, yet his own account of the trade cycle was a monetary one. *MTTC* was Hayek's attempt to convince skeptical German economists both that an acceptable monetary account of the trade cycle could be constructed that did not rely upon a simplistic understanding of the quantity theory, and moreover, that – in the prevailing state of economic-theoretical knowledge at the time – such an approach was the only legitimate one.

Hayek's argument begins with a theme that would have been familiar to anyone raised in the traditions of German-language economics (as it was a point that the founder of the Austrian school, Carl Menger, had pushed in the famous *Methodenstreit* against the German historical school economists of Menger's own generation⁹), namely, that an explanation of the business cycle cannot start from

⁹ For more on the *Methodenstreit* see chapter three of Caldwell (2004)

presupposition-less observation of the relevant phenomena; some kind of *theory* is required. Hayek develops this point as part of an argument to the effect that statistical and empirical inquiries have a limited and always secondary role (to theory) to play in economic analysis. Such studies cannot “provide new insight into the causes or the necessity of the trade cycle.”¹⁰

The unique task of trade cycle theory is to explain how particular prices are determined and to indicate their influence on other phenomena of production and consumption; therefore, such an explanation requires a theoretical account of price formation and of the influence of price changes on production and consumption such as is provided by static equilibrium theory: “the determining conditions of these phenomena are already given by elementary theory.”¹¹ Any attempt to explain the trade cycle on the basis of observation unassisted by theory would have to reinvent the wheel, starting with a general explanation of all of the price phenomena that are already explained by static equilibrium theory; it could never proceed immediately to an explanation of the particular phenomena of the cycle.¹²

Trade cycle phenomena can be integrated into existing theory only by adding new assumptions to the existing skeleton of static equilibrium theory. However, Hayek argues, these new assumptions cannot be discovered by statistical inquiry, because statistics cannot establish cause and effect: “Empirically established relations between various economic phenomena continue to present a problem to theory until

¹⁰ Hayek ([1933] 2008, 9)

¹¹ *Ibid.*

¹² *Ibid.*, 10

the necessity of their interconnections can be demonstrated independently of any statistical evidence.”¹³ Statistical correlations have no known causal significance until the interconnections they indicate are independently established by deduction from some set of theoretical assumptions.

Hayek also assigns the statistical method little role with respect to the “verification” of trade cycle theories. Assuming that such a theory is logically sound, and that it explains the observed phenomena as a deductive consequence of the given theoretical assumptions, “the best statistical investigation can do is to show that there still remains an unexplained residue of processes. It could never prove that the determining relationships are of a different character from those maintained by the theory.”¹⁴ Thus, according to Hayek, the statistical method has a purely *negative* role to play with respect to trade cycle theory and economic theory more generally; the statistical method can indicate phenomena that a theory fails to explain, but it cannot alone show that a theory’s explanation of some observed phenomena is false.

However, Hayek’s attitude toward statistical research is not entirely negative. Statistics can provide information about the events posited by a theory, and thereby “enable us not only to connect two consecutive events as cause and effect, *a posteriori*, but to grasp existing conditions completely enough for forecasts of the future and,

¹³ *Ibid.*, 11

¹⁴ *Ibid.*, 12-13

eventually, appropriate action, to become possible. It is only through this possibility of forecasts of systematic action that theory gains practical importance.”¹⁵

Hayek considers what he takes to be the methodological strengths and weaknesses of existing explanations of the business cycle, non-monetary and monetary, in turn. He argues, in essence, that non-monetary theories have the correct explanandum, but adopt an inconsistent technique of explanation; while monetary theories adopt the appropriate explanatory technique, but seek to explain the wrong phenomenon. Hayek’s explicit goal in *MTTC* is theoretical unification, i.e., the “bridging of the gulf that divides monetary from non-monetary theories.”¹⁶

Non-monetary theories of the trade cycle all “regard the emergence of a *disproportionality* among the various productive groups, and in particular the excessive production of capital goods, as the first and main thing to be explained.”¹⁷ This, Hayek believes, is all to the good: “The development of theory owes a real debt to statistical research in that, today, there is at least no substantial disagreement as to the thing to be explained.”¹⁸ However, an immediate problem arises for non-monetary theories: none of the various devices posited to explain observed divergences from equilibrium can overcome the logic of the theoretical framework upon which these theories are founded. Static equilibrium theory permits only one sort of reaction to a change in the economic data, i.e., the establishment of a new equilibrium. The

¹⁵ *Ibid.*, 14

¹⁶ *Ibid.*, 17-18

¹⁷ *Ibid.*, 25

¹⁸ *Ibid.*

disproportionalities that non-monetary theorists seek to explain are inconsistent with the assumptions of the schema adopted to explain them. In Hayek's words,

There is a fundamental difficulty inherent in all trade cycle theories that take as their starting point an empirically ascertained disturbance of the equilibrium of the various branches of production. This difficulty arises because, in stating the effects of that disturbance, they have to make use of the logic of equilibrium theory. Yet this logic, properly followed through, can do no more than demonstrate that such disturbances of equilibrium can come only from outside—i.e., that they represent a change in the economic data—and that the economic system always reacts to such changes by its well-known methods of adaptation, i.e., by the formation of a new equilibrium. No tendency toward the special expansion of certain branches of production, however plausibly adduced, no shift in demand, in distribution or in production, could adequately explain, within the framework of this theoretical system, why a general “disproportionality” between supply and demand should arise. For the essential means of explanation in static theory—which is, at the same time, the indispensable assumption for the explanation of particular price variations—is the assumption that prices supply an automatic mechanism for equilibrating supply and demand... The problem before us cannot be solved by examining the effect of a certain cause within the framework, and by the methods, of equilibrium theory. Any theory that limits itself to the explanation of empirically observed

interconnections by the methods of elementary theory necessarily contains a self-contradiction.¹⁹

Hayek considers three distinct kinds of non-monetary explanations. There are those theories according to which the relevant disproportionalities are a consequence of changes in *techniques of production*; there are accounts which attribute disequilibria to discrepancies between *savings and investment*; and there are explanations according to which it is considerations of *human psychology* – ignorance, uncertainty, and error – that account for the cycle. The problem with such non-monetary explanations, Hayek argues, is not a lack of empirical evidence for the relevant factors they adduce, but that these causal factors are fundamentally inconsistent with the assumptions of the static equilibrium framework upon which such theories are based. It is simply inconsistent to attribute the cycle to human ignorance while assuming (as the equilibrium construct does) that all market participants possess perfect knowledge; nor can changes in production techniques be a convincing explanation in an explanatory framework that takes these as given; similarly, savings and investment are necessarily equal in the equilibrium framework, so no discrepancy between the two is theoretically possible in an equilibrium-based explanation of the cycle.

Hayek does not deny the possibility of building an account of the trade cycle on some theoretical basis other than that of static equilibrium. However, anyone who might opt out of the equilibrium framework would lose the only then-accepted explanation of price formation and of the effects of price changes on production and distribution. Given the aforementioned point that cyclical phenomena are price

¹⁹ *Ibid.*, 18-19

phenomena, anyone who eschews theorizing in terms of the equilibrium framework would have to rebuild the entirety of the economic-theoretical edifice. Thus, it seems that not only is one required to base an explanation of the trade cycle on some theory, but that this theory must be (or, at least, must have been in 1929 given the history of the discipline to that date) the equilibrium theory.

Far from denying a role in an explanation of the business cycle to those factors that non-monetary theorists typically postulate, Hayek seeks a theory that accounts for these factors as secondary phenomena. The trick is to extend the equilibrium framework in such a way that all of the empirically-ascertained phenomena of the cycle appear as deductive consequences of the augmented set of assumptions:

The obvious, and (to my mind) the only possible way out of this dilemma, is to explain the difference between the course of events described by static theory (which only permits movements toward an equilibrium, and which is deduced by directly contrasting the supply of and demand for goods) and the actual course of events, by the fact that, with the introduction of money (or strictly speaking with the introduction of indirect exchange), a new determining cause is introduced. Money being a commodity that, unlike all others, is incapable of finally satisfying demand, its introduction does away with the rigid interdependence and self-sufficiency of the “closed” system of

equilibrium, and makes possible movements that would be excluded from the latter.²⁰

In other words, the introduction of money into the static system of equilibrium introduces a unique kind of good – a good the demand for which can never be fully satisfied – that makes it possible to demonstrate the appearance of the relevant cyclical phenomena as deductive consequences of the expanded set of assumptions. Moreover, the monetary starting point makes the factors that figure as explanans in non-monetary theories implications of the relevant assumptions:

The existence of most of the interconnections elaborated by the various trade cycle theories can hardly be denied...our task is rather their coordination in a unified logical structure. When...the question is answered on different lines, viz., by reference to *monetary* circumstances, it can be shown that the elements of explanation adduced by different theories lose their independent importance and fall into a subordinate position as necessary consequences of the monetary cause.²¹

Whereas Hayek argues against non-monetary theories on the grounds that, though they have the correct explanandum, they adopt an inconsistent technique of explanation; his argument against monetary theories is the reverse of this: monetary theories adopt the appropriate explanatory technique, but seek to explain the wrong phenomenon. Monetary theories generally identify fluctuations in trade with changes in the general price level. However, Hayek argues that an explanation of changes in

²⁰ *Ibid.*, 20

²¹ *Ibid.*, 23-24

the general price level is neither necessary nor sufficient to account for the disproportionalities in need of explanation:

What we expect from a monetary trade cycle theory differs considerably from what most of the monetary trade cycle theorists regard as the essential aim of their explanation. We are in no way concerned to explain the effect of the monetary factor on trade fluctuations through changes in the value of money and variations in the price level—subjects that form the basis of current monetary theories. We expect such an explanation to emerge rather from a study of all the changes originating in the monetary field—more especially, variations in its quantity—changes that are bound to disturb the equilibrium interrelationships in the natural economy, *whether the disturbance shows itself in a change in the so-called “general value of money” or not.*²² [Italics in the original]

Those theorists who identify changes in the volume of money with changes in its value and treat the latter as necessary and sufficient to explain the cycle ignore both the disequilibrating effects of changes in the volume of money that do not impact the general price level and that the general price level can change in the absence of any changes in the volume of money.²³ An explanation of the business cycle should

²² *Ibid.*, 52

²³ Hayek notes another problem with existing monetary theories of the trade cycle, namely, that they are typically based on the illegitimate assumption that the price level is “constant,” which itself is sufficient to break down the equilibrium relationships. This assumption “forces us to assume variations in the effective quantity of money as given.” Hayek continues, “Such variations, however, always dissolve the equilibrium interrelationships described by static theory; but they must necessarily be assumed if the value of money is to remain constant despite changes in

be sought in changes in the volume of money alone without regard for general price level effects.

Hayek begins his analysis of monetary theories with a discussion of the work of the influential Swedish economist, Knut Wicksell.²⁴ With respect to trade cycle theory, Wicksell's key contribution was the introduction of the concept of the "natural" rate of interest, i.e., "that rate which exactly balances the demand for loan capital and the supply of savings."²⁵ Wicksell, who, like other monetary theorists, assumed that the key phenomena to be explained concern the general price level, argued that money is neutral with respect to the price level if and only if the money (or bank or loan) rate of interest coincides with the natural rate. However, according to Hayek, Wicksell's important contribution was not his analysis of the effects of deviations between bank rates and the natural rate on the general level of prices, but Wicksell's discussion of the distortive effects of such deviations on incentives to produce and consume. That the latter are the important phenomena in need of theoretical explanation and not general price level effects, Hayek argues, follows from the fact that changes in supply and demand relationships always follow (on the assumptions of static theory) from such deviations in the relevant interest rates, but that general price level effects do not:

data; and therefore they cannot be used to explain deviations from the course of events which static theory lays down" (*Ibid.*, 54-55).

²⁴ Wicksell (1898)

²⁵ Or, in other words, the rate that would prevail in the absence of money, i.e., the *equilibrium* interest rate.

If one were to make a systematic attempt to coordinate these ideas into an explanation of the trade cycle...a curious contradiction would arise. On the one hand, we are told [by Wicksell] that *the price level remains unaltered when the money rate of interest is the same as the natural rate*; and, on the other, that *the production of capital goods is, at the same time, kept within the limits imposed by the supply of real savings*.²⁶ One need say no more to show that there are cases—certainly all cases of an expanding economy, which are those most relevant to trade cycle theory—in which the rate of interest that equilibrates the supply of real savings and the demand for capital cannot be the rate of interest that also prevents changes in the price level. In this case, stability of the price level presupposes changes in the volume of money; but these changes must always lead to a discrepancy between the amount of real savings and the volume of investment. *The rate of interest at which, in an expanding economy, the amount of new money entering circulation is just sufficient to keep the price level stable, is always lower than the rate that would keep the amount of available loan capital equal to the amount simultaneously saved by the public*; and thus, despite the stability of the price level, it makes possible a development leading away from the equilibrium position.²⁷ [Italics in the original]

In short, changes in the volume of money may or may not influence the general level of prices, but such changes *always* (again, given the assumptions of static equilibrium

²⁶ That is, the supply of savings that would be brought forth under the natural rate of interest.

²⁷ Hayek ([1933] 2008, 58-59)

theory) lead the economic system away from equilibrium; price level effects are neither necessary nor sufficient for such disequilibria to appear. This means that an adequate monetary theory of the trade cycle must focus on changes in *relative prices* – i.e., changes in the relations between prices of individual goods or classes of goods – and not on changes in the average level of prices.

Hayek then considers Ludwig von Mises' extension of Wicksell's account of deviations between the natural rate and money rate. Mises'

exposition already contains an account of practically all those effects of a rate of interest altered through monetary influences, which are important for an explanation of the course of the trade cycle...he describes the disproportionate development of various branches of production and the resulting changes in the income structure.²⁸

This said, Mises' account is "dangerous," according to Hayek, because, like Wicksell's theory, it ignores the effects of discrepancies between the relevant rates of interest that do not influence the general price level, but do – as all such discrepancies must given the assumptions of static equilibrium theory – give rise to disproportionalities between supply and demand: "The effects of an artificially lowered rate of interest, pointed out by Wicksell and Mises, exist whether this same circumstance does or does not eventually react on the general value of money...they must be dealt with independently if they are to be properly understood."²⁹

²⁸ *Ibid.*, 61-62

²⁹ *Ibid.*, 62

Hayek refers again to the case of an expanding economy in which price stability requires an increase in the volume of money that is sufficient to discombobulate the relation between the natural and loan rates of interest, and notes further that “This case is particularly important, because under contemporary currency systems, the automatic adjustment of the value of money in the form of a flow of precious metals will regularly make available new supplies of purchasing power that will depress the money rate of interest below its natural level.”³⁰ In other words, the presence of a deviation between the relevant rates of interest – more exactly, a money rate below the natural rate – is the normal state of an expanding economy under a commodity money standard; moreover, such deviations do not necessarily reveal themselves in effects on the general level of prices. Under the influence of the view that stable prices are necessary and sufficient to avoid trade cycle effects “Economists have overlooked the fact that the changes in the volume of money, which, in an expanding economy, are necessary to maintain price stability, lead to a new state of affairs foreign to static analysis, so that the development that occurs under a stable price level cannot be regarded as consonant with static laws.”³¹

Hayek makes plain the significance of the foregoing for the methodology of business cycle theory:

General price changes are no essential feature of a monetary theory of the trade cycle; *they are not only unessential, but they would be completely irrelevant if only they were completely “general”—that is, if they affected all prices at the same time and in*

³⁰ *Ibid.*, 62-63

³¹ *Ibid.*, 63

the same proportion. The point of real interest to trade cycle theory is the existence of certain deviations in individual price relations occurring because changes in the volume of money appear at certain individual points; deviations, that is, away from the position that is necessary to maintain the whole system in equilibrium...the nature of changes in the composition of the existing stock of goods, which are effected through such monetary changes, depends of course on the point at which the money is injected into the economic system.³² [Italics in the original]

In other words, unlike the quantity theory of money, the then-modern Austrian theory of money – due in large part to Mises’ development of Wicksell’s ideas in the former’s *The Theory of Money and Credit*³³ – is not satisfied with comparing end-states, but attempts to trace the consequences of an influx of new money on the successive changes in particular prices. Unlike the older (and later) quantity theories, which assume that new money enters the economic system all at once and is spread evenly across the economy, the fact that new money always enters the economic system at specific points plays a central role in the Austrian theory of money.

The first recipients of new money can expand their demand, thereby pushing up the prices of the goods demanded, at the expense of others, who suffer the price effects of this increased demand without new money to compensate. The latter must curtail their consumption, i.e., they are “forced” to “save,” as a consequence. The price effects of new money spread out from the initial injection point and, *contra*

³² *Ibid.*, 64-65

³³ Mises ([1912] 1981)

quantity theories, effect distinct sectors of the economy unevenly. The exact price effects of a new influx of money are contingent upon the point at which the money enters the economic system and related considerations. This means that, as Austrians picture the operations of modern economies, accurate predictions of the effects of increases in the volume of money require knowledge about the location within the economic system where new money enters and the uses to which it is put. Hayek thereby solidifies the fourth and final methodological principle with respect to trade cycle theory that he originally set out to establish, namely, that it is *relative price changes* not changes in the general price level that are relevant to the cycle.

In summary, Hayek's argument in *Monetary Theory and the Trade Cycle* proceeds 1) from the recognition that presupposition-less observation alone cannot explain the cycle to the claim that some theory is required for an explanation; 2) from this latter claim and the fact that only the static equilibrium framework is – or *was* in the then-prevailing state of economic-theoretical knowledge – the only sufficient explanation of price phenomena, Hayek draws the conclusion that an explanation of the trade cycle must be built on the foundations of equilibrium theory; 3) from this latter claim and the recognition that only money is capable of dislodging the rigid interrelationships of the equilibrium system, Hayek draws the conclusion that an explanation of the trade cycle must be built on monetary assumptions; finally, 4) from this latter claim and the recognition both that disequilibria need not result from changes in the general price level and that price level stability is not sufficient to avoid disequilibria, Hayek draws the conclusion that relative price changes rather than changes in the general price level are the truly important phenomena of the

cycle. As Hayek's biographer, Bruce Caldwell, points out, "by a process of elimination," Hayek's account "is the only contender that remains."³⁴

Hayek offers a sketch of his positive explanation of the cycle. He considers the question "why, under the existing organization of the economic system, [do] we constantly find those deviations of the money rate of interest from the equilibrium rate which, as we have seen, must be regarded as the cause of the periodically recurring disproportionalities in the structure of production[?]"³⁵ The answer to this question, Hayek argues, will explain why certain changes lead the economic system away from equilibrium, and "are, actually, the cause of recurrent shifts in economic activity that subsequently have to be reversed before a new equilibrium can be established."³⁶ In other words, we will have explained the cycle when we have shown why certain changes in the data lead to a "boom" that must later be reversed in the form of a "bust" before a new equilibrium can obtain.

The foregoing analysis pinpoints the elasticity of the currency as the fundamental cause of the cycle, which is another way of saying that the cycle is a consequence of the fact that the loan rate of interest can deviate from the natural rate, i.e., that banks can and do throw onto the credit market more funds than the supply of voluntary savings alone would permit. The question that Hayek considers is whether the elasticity of the currency is a contingent fact or "an immanent

³⁴ Caldwell (2004, 157)

³⁵ Hayek ([1933] 2008., 73)

³⁶ *Ibid.*

characteristic of our present money and credit system.”³⁷ More to the point, Hayek asks whether deviations between the relevant rates of interest are due to “arbitrary interferences by the authorities responsible for the regulation of the volume of currency media,” or whether such deviations are a fact of the economic system against which all of the knowledge in the world and the best of political intentions are impotent.

Yet another way of posing the same question is to consider whether the correct theory of the cycle is an *exogenous* or an *endogenous* one. Endogenous theories, “in the course of their proof, avoid making use of assumptions that cannot either be decided by purely economic considerations, or regarded as general characteristics of our economic system,”³⁸ and therefore, can claim general validity. Exogenous theories, on the other hand, because they ascribe causal influence to factors external to the economic system, have “to be proved separately in each individual case.”³⁹ Mises’ account of the cycle is an exogenous one: it attributes deviations between the natural and bank rates of interest always and everywhere to external interferences on the part of bankers. This, Hayek notes, is “perhaps, one of the main reasons for the prevailing skepticism concerning the value of such theories. A theory that has to call upon the *deus ex machina* of a false step by bankers, in order to reach its conclusions is, perhaps, inevitably suspect.”⁴⁰ Hayek argues that it is not necessary to adopt this

³⁷ *Ibid.*, 74

³⁸ *Ibid.*, 76

³⁹ *Ibid.*

⁴⁰ *Ibid.*

contrivance in order to generate the cycle. Mises erred in treating “a single especially striking case...as the normal[.]”⁴¹ In fact, intentional interference on the part of banks, while sufficient to cause the cycle, is by no means necessary:

The situation in which the money rate of interest is below the natural rate need not...originate in a *deliberate lowering* of the rate of interest by the banks. The same effect is obviously produced by an improvement in the expectations of profit or a diminution in the rate of saving, which may drive the “natural rate” (at which the demand for, and the supply of, savings are equal) above its previous level; while the banks refrain from raising their rate of interest to a proportionate extent, but continue to lend at the previous rate, and thus enable a greater demand for loans to be satisfied than would be possible by the exclusive use of the available supply of savings.⁴² [Italics in the original]

The latter case is important, not mainly due to the fact that it is “probably the commonest in practice, but to the fact that it *must inevitably recur* under the existing credit organization”⁴³ [italics in the original]. That is, Hayek’s account of the trade cycle is an endogenous theory that attributes the trade cycle to the operations of the economic system itself, and not to some external force.

The volume of money in an economic system on a commodity money standard is governed by three interrelated factors: “changes in the volume of cash,

⁴¹ *Ibid.*, 77

⁴² *Ibid.*, 78

⁴³ *Ibid.*

caused by inflows and outflows of gold; changes in the note circulation of the central banks: and last, and in many ways most important the often-disputed ‘creation’ of deposits by other banks.”⁴⁴ The question to be answered concerns whether expansion of the money supply by the banking system “may not take place automatically under certain conditions—without the necessity for any special assumption of the inadequate functioning of any part of the system...this certainly appears to be true as regards the third factor of money expansion—the ‘credit creation’ of the commercial banks.”⁴⁵

Hayek argues that it is impossible for bankers to know whether they are at any time creating additional credit or lending on the basis of accumulated savings: “As credit created on the basis of additional deposits does not normally appear in the accounts of the same bank that granted the credit, it is fundamentally impossible to distinguish, in individual cases, between” deposits based on savings and those that result from the extension of credit.⁴⁶ In other words, with respect to any particular loan, it is typically the case that the lending institution is not the deposit institution (either because the lendee deposits the loan in another bank or because the lendee spends the money with a vendor who then deposits the loaned funds in another bank), and because new deposits don’t arrive dog-eared either “savings-based” or “credit-based,” it is impossible for the deposit institution to know whether they are receiving (and subsequently lending on the basis of) savings or credit.

⁴⁴ *Ibid.*

⁴⁵ *Ibid.*, 79

⁴⁶ *Ibid.*, 87

But this consideration rules out, *a priori*, the possibility of bankers limiting the amount of credit granted by them to the amount of “real” accumulated deposits...Once the impetus has been given to any part of the banking system, mere adherence to the routine of banking technique will lead to the creation of additional deposits without the possibility arising, at any point, of determining whether any particular credit should properly be regarded as “additional.” Every time money that has been deposited is re-lent...this process is to be regarded as the creation of additional purchasing power.⁴⁷

In other words, there is no reason to believe that banks engaged in credit operations necessarily act inappropriately or out-of-order with standard banking techniques.

To this point in the analysis, the relevant assumption has been that banks receive newly-deposited funds which then serve as the basis for further loans. The next issue that Hayek considers is the banking system’s response to an increased demand for loans in the absence of new deposits. On the assumption that this increased demand is not a consequence of the banks lowering their own interest rates, “this additional demand is always a sign that the natural rate of interest has risen—that is, that a given amount of money can now find more profitable employment than hitherto.”⁴⁸ Of course, were banks to respond to this increased

⁴⁷ *Ibid.*, 87-88

⁴⁸ *Ibid.*, 89-90:

The reasons for this can be of very different kinds: new inventions or discoveries, the opening up of new markets, or even bad harvests, the appearance of entrepreneurs of genius who originate “new combinations” (Schumpeter), a fall in wage rates due to heavy immigration, the destruction of great blocks of capital by a natural catastrophe, or many others” (*Ibid.*, 90).

demand by raising their lending rates in line with the increase in the natural rate, no discrepancy between the relevant rates could arise, and – assuming the soundness of Hayek’s explanation – no cycle could be set in motion. The question is why banks do not respond in this manner: is it a matter of malfeasance or a rational response to a change in economic circumstances? Furthermore, how it is even possible for banks to extend credit in the absence of an influx of new funds? Answers to these questions are to be found, according to Hayek,

in the fact that the ratio of reserves to deposits does not represent a constant magnitude, but, as experience shows, is itself variable. But we shall achieve a satisfactory solution only by showing that the reason for this variability in the reserve is not based on arbitrary decisions of the bankers, but is itself conditioned by the general economic situation.⁴⁹

Hayek considers the response of a single bank manager to an increased demand for loans “in consequence of an all-round improvement in the business situation,” and further assumes that this bank is the first to experience the increased demand, “let us say, its customers are drawn from just those industries that first feel the effects of the new recovery.”⁵⁰ This means that only one of the factors that

Hayek’s point here is a subtle one and is important to keep in mind in much of what follows in the present essay. The initial deviation between the natural and bank rates of interest may occur for any number of non-monetary reasons; what must be attributed to the activities of the banks is not the initial disequilibrating change, but the failure of the economic system to re-equilibrate following the change. Many things might cause a disequilibrium situation, but, on Hayek’s account, only money can maintain one.

⁴⁹ *Ibid.*, 91

⁵⁰ *Ibid.*

determine the bank's lending decisions has changed: "whereas previously, at the same rate of interest and with the same security, no new borrowers came forward, now, under the same conditions of borrowing, more loans can be placed. On the other hand, the cash holdings of the bank remain unchanged."⁵¹ Hayek argues that it is wrong to think that the same considerations of liquidity that guide the bank's decisions under prior conditions continue to influence its decisions under the new conditions assumed. If the bank recognizes that its cash requirements can be met only by raising interest charges, then considerations of profit will lead the bank to dip into its cash reserves, i.e., "to a policy that involves diminishing the size of this non-earning asset"⁵²; they do this, furthermore, in the knowledge that the very conditions that call forth additional loans protect them to some degree against the standard risks of illiquidity. That is, when there is an increased demand for loans due to improved profit expectations "the risks of borrowing are less; and therefore a smaller cash reserve may suffice to provide the same degree of security."⁵³ All of this said, it is, in the final analysis, the forces of competition that ultimately lead such a bank to diminish its cash reserves:

The bank that first feels the effect of an increased demand for credit cannot afford to reply by putting up its interest charges; for it would risk losing its best customers to other banks that had not yet experienced a similarly increased demand for credit. There can be little doubt, therefore, that the

⁵¹ *Ibid.*, 91-92

⁵² *Ibid.*, 92

⁵³ *Ibid.*

bank or banks that are the first to feel the effects of new credit requirements will be forced to satisfy these even at the cost of reducing their liquidity.⁵⁴

Once a single bank or group of banks – be it the one that first experiences the amplified demand or a competitor – satisfies the increased demand for loans, the process described above whereby new deposits, which cannot be identified as savings or credit, lead to further loans is set in motion:

Once one bank or group of banks has started the expansion, then all the other banks receive, as already described, a flow of cash that at first enables them to expand credit on their own account without impairing their liquidity. They make use of this possibility the more readily since they, in turn, soon feel the increased demand for credit. Once the process of expansion has become general, however, the banks soon realize that, for a moment at any rate, they can safely modify their ideas of liquidity.⁵⁵

This is due to the more or less simultaneous and equal settlement of claims at the clearinghouse. Any bank that opts out of the initial expansion of credit will, “sooner or later” be persuaded to join, “since it will continue to receive cash at the clearinghouse as long as it does not adjust itself to the new standard of liquidity.”⁵⁶ And, once a bank has joined the expansion, it is impossible for it to “apply the only control by which the demand for credit can, in the long run, be successfully kept

⁵⁴ *Ibid.*, 92-93

⁵⁵ *Ibid.*, 93

⁵⁶ *Ibid.*

within bounds; that is, an increase in its interest charges,” lest it lose customers to competitor banks.⁵⁷ For reasons of competition,

concerted action in this direction, which...is the only action possible, will ensue only when the increased cash requirements of business compel the banks to protect their cash balances by checking further credit expansion, or when the central bank has preceded them by raising its discount rate. This, again, will only happen, as a rule, when the banks have been induced by the growing drain on their cash to increase their rediscount.⁵⁸

The crisis (“bust”) commences once banks stop expanding the volume of credit, and this must happen “sooner or later”: “Only so long as the volume of circulating media is increasing can the money rate of interest be kept below the equilibrium rate; once it has ceased to increase, the money rate must, despite the increased total volume in circulation, rise again to its natural level.”⁵⁹ Many of the investments based upon lent credit are thereby rendered unprofitable by the increase in interest charges—in short, the bust has arrived.

In sum, credit expansion provides a “means for enterprises for which no provision could be found if the choices of the different economic subjects were strictly followed[.]”⁶⁰ By creating additional credit beyond what is available on the

⁵⁷ *Ibid.*

⁵⁸ *Ibid.*

⁵⁹ *Ibid.*, 94

⁶⁰ *Ibid.*, 95

basis of the supply of voluntary savings alone, the banks ensure that any tendency toward expansion of production will not be curbed by a rise in interest. Credit expansion prevents the equilibrating mechanism from operating as it does in static equilibrium theory; an economy with an elastic currency must react differently than the sort of economy described by the latter.

Hayek considers whether his theory settles the relevant disputes between monetary and non-monetary theorists. He reiterates the point that there's no necessary reason why the change that initially increases profitability must be of a monetary origin; there are many different factors that can increase the profitability of certain businesses. What is important is that – because of the forces unleashed by credit expansion – the economy responds to these changes not by adjusting to a new equilibrium, but by setting in motion a boom “that contains within itself the seeds of an inevitable reaction. This phenomenon...should undoubtedly be ascribed to monetary factors, and in particular to ‘additional credit’ that also necessarily determine the extent and duration of the cyclical fluctuation.”⁶¹ It is the failure to adjust to a new equilibrium, not necessarily the originating change in the data, which must be attributed to the influence of money. Moreover, the theory as presented – whether it should ultimately be treated as a monetary theory because it attributes the failure of the equilibrating mechanism to monetary factors or as a non-monetary theory because it leaves open the possibility that the initiating change in the economic data may not be of a monetary origin – has one important advantage over others:

⁶¹ *Ibid.*, 98-99

*It deals with problems that must, in any case, be dealt with, for they are necessarily given when the central apparatus of economic analysis is applied to the explanation of the existing organization of exchange. Even if we had never noticed cyclical fluctuations, even if all the actual fluctuations of history were accepted as consequences of natural events, a consequential analysis of the effects that follow from the peculiar working of our existing credit organization would be bound to demonstrate that fluctuations caused by monetary factors are unavoidable.*⁶² [Italics in the original]

Hayek then considers the policy implications of the theory. The most important political consequence of the theory is that, given prevailing monetary and credit institutions, *we don't know how to prevent the cycle*; the theory “implies that no measure that can be conceived in practice would be able entirely to suppress these fluctuations.”⁶³ It is a consequence of the theory that securing a stable price level is neither necessary nor sufficient to prevent disproportionalities between supply and demand. Moreover, it is nonsensical to “blame” the banks—they have no way of knowing whether any of the particular deposit and lending activities in which they engage in the normal course of business constitute credit expansion or not: “Nobody has ever asked them to pursue a policy other than that which, as we have seen, gives rise to cyclical fluctuations; and it is not within their power to do away with such fluctuations, seeing that the latter originate not from their policy but from the very nature of the modern organization of credit.”⁶⁴

⁶² *Ibid.*, 99

⁶³ *Ibid.*, 101

⁶⁴ *Ibid.*, 102

Economic cycles are the price paid for the possibility opened up by the availability of bank credit, i.e., “a speed of development exceeding that which people would voluntarily make possible through their savings[.]”⁶⁵ The appearance of cycles makes it obvious that the use of bank credit does not resolve all of the problems of economic progress, but “it is at least conceivable that the non-economic factors of progress, are thereby benefited in a way we should be reluctant to forgo.”⁶⁶ Bank credit allows economies to grow more rapidly than they would in its absence, but it also necessitates the appearance of alternating periods of boom and bust. Everything in life is a tradeoff; but Hayek emphasizes the complexity of this particular tradeoff. The only conceivable course of action for the elimination of the cycle is the annihilation of bank credit, but this is “purely utopian.” Eliminating the credit operations of banks “would necessitate the complete abolition of all bank money—i.e., notes and checks—and the reduction of the banks to the role of brokers, traders in savings.”⁶⁷ Even if this course of action is possible, it is by no means obvious that it is advisable given its consequences, for it would mean that

The stability of the economic system would be obtained at the price of curbing economic progress. The rate of interest would be constantly above the level maintained under the existing system...The utilization of new inventions and the “realization of new combinations” would be made more

⁶⁵ *Ibid.*

⁶⁶ *Ibid.*

⁶⁷ *Ibid.*

difficult, and thus there would disappear a psychological incentive toward progress, whose importance cannot be judged on purely economic grounds.⁶⁸

The final chapter of *MTTC* considers certain unresolved problems in cycle theory. Hayek's discussion of the contingency of predictions about cyclical phenomena is particularly relevant to our purposes in the present essay. Hayek argues that accurate predictions require sufficient information about the "genesis" of the change in the volume of money and "the part of the economic system where it took place."⁶⁹ Substantive predictions cannot be made about, e.g., changes in the volume of money that result from the discovery of new troves of the commodity money in the absence of additional information about the recipients of the new money and their various uses of it. Similarly, in the absence of information about the uses to which it will be put, little can be predicted about the effects of credit granted to the state.

The situation is somewhat different with respect to credit granted to industry:

This credit is only given when and where its utilization is profitable, or at least appears to be so. Profitability is determined, however, by the ratio of the interest paid on this credit to the profits earned by their use... The uses to which the additional money can be put are thus determined by the rate of interest, and the amount that can be said about those uses will therefore

⁶⁸ *Ibid.*, 103

⁶⁹ *Ibid.*, 107

depend, in turn, on how much is known about the importance and the effects of interest.⁷⁰

In other words, propositions about the effects of credit granted to industry require not only knowledge of the industries to which it is granted and the uses to which it is put, but also a thorough understanding of interest phenomena.

2 Hayek's Criticisms of Pre-Keynesians: "The 'Paradox' of Savings" (1929)

Hayek's next work on the business cycle, the lengthy essay "The 'Paradox' of Savings" also published in 1929, is an important part of the story of the debate with Keynes. Hayek uses the article to integrate the capital theory that he inherited from Knut Wicksell and Eugen Böhm-Bawerk with Ludwig von Mises' explanation of the business cycle, thereby providing the latter with an explicit capital-theoretic foundation. Moreover, Hayek's specific criticisms of the underconsumptionist theory of William Trufant Foster and Waddill Catchings anticipate many of his later objections to Keynes' various attempts – first in 1930's *Treatise on Money* and later in *The General Theory* – to articulate an underconsumptionist explanation of unemployment and related cyclical phenomena.

In general, Hayek objects to underconsumptionist theories on the grounds that they ignore the effects of changes in the effective quantity of money on the capital structure of the economy, and that it is only on the assumption that these effects can be safely ignored that these theories generate their particular results and policy implications. In "The 'Paradox' of Savings," Hayek argues that Foster and

⁷⁰ *Ibid.*

Catchings misapprehend the role of *capital* in a money-using economy, and that according to Foster and Catchings account a proper theory of capital, in particular, the Austrian one, results in something that looks like Hayek's own explanation of the business cycle. As we will see, these same objections are mirrored in the complaints Hayek later levels against Keynes' underconsumptionism.

The strange personal tale of Messrs. Foster and Catchings is entertainingly rehearsed in the introductory sections of "The 'Paradox' of Savings." Suffice it to say here that the pair – the first a former college president, and the second a successful lawyer and banker – fancied themselves amateur economists, and via a series of publicity stunts, including a \$5,000 essay prize for the best criticism of their view (Hayek did not submit an entry), managed to create a bit of a stir in American economics in the mid-1920s.⁷¹

Hayek argues that Foster and Catchings' theory is just another in a long and (theretofore) unsuccessful series of attempts to prove the old ("almost as old as the science of political economy itself"⁷²) proposition that the effects of a *ceteris paribus* increase in savings, though they are generally beneficial to the individual saver, are deleterious with respect to society as a whole; in other words, that, as the title of one of Foster and Catchings' more popular works put it, there is a *Dilemma of Thrift*.⁷³ According to Foster and Catchings' account, an increase in the volume of saving makes it impossible to clear the market of the whole of current output, thereby

⁷¹ Hayek ([1929, 1931] 2008, 150-151)

⁷² *Ibid.*, 133

⁷³ Foster and Catchings (1926)

leading to entrepreneurial losses, shrinking production, unemployment, etc. Hayek argues that this same idea, which appeared in essentially unaltered form in the economic writings of the Earl of Lauderdale and Thomas Malthus, was refuted by James Mill and J.B. Say, only to be resuscitated in the nineteenth century by Thorstein Veblen and J.A. Hobson.⁷⁴

Foster and Catchings state their theory of the cycle clearly:

Money spent in the consumption of commodities is the force that moves all the wheels of industry. When this force remains in the right relation to the volume of commodities offered for sale, business proceeds steadily. When money is spent faster than the commodities reach the retail markets, business booms forward. When commodities continue to reach the retail markets faster than money is spent, business slackens. To move commodities year after year without disturbing business, enough money must be spent by consumers, and no more than enough, to match all the commodities dollar for dollar.⁷⁵

The practical remedy associated with this theory is laid out in Foster and Catchings' *Profits*: "The one thing that is needed above all others to sustain a forward movement of business is enough money in the hands of consumers."⁷⁶ The relevant theoretical

⁷⁴ Hayek ([1929, 1931] 2008, 133)

⁷⁵ Foster and Catchings ([1923] 1928, 277); quoted in Hayek ([1929, 1931] 2008, 136)

⁷⁶ Foster and Catchings (1925, 11); quoted in Hayek ([1929, 1931] 2008, 137)

question is what circumstances make it the case that the funds in the pockets of consumers fail to suffice to sell the whole of output at cost-covering prices?

Foster and Catchings' answer is that, provided everyone in the community – consumers and the one enterprise alike – spends all that they earn, no difficulties arise in clearing the market of the relevant output: “But as soon as the company retains part of the profits in the business...in order to improve ‘capital facilities’, which puts it in the position to increase the volume of production, this happy state of affairs changes.”⁷⁷ Foster and Catchings rule out by assumption the possibility of a fall in the prices per unit of this increased product, so it is that “As soon as the increased volume of products reaches the market, it is inevitable that the means of payment in the hands of the consumer should prove insufficient to take up the product at remunerative prices...a proportion of the enlarged product must therefore remain unsold”⁷⁸—that is, of course, “unless the deficiency...is made up from outside sources.”⁷⁹ In a subsequent analysis under a different assumption Foster and Catchings show that, if prices are permitted to fall, then the market clears, including the enlarged product due to the increase in investment; unfortunately, such investment is rendered unprofitable in virtue of the fall in prices and the incentive to continue production at the increased level disappears.

Foster and Catchings argue that augmenting the supply of money is insufficient to ensure the clearing of the market at prices adequately remunerative to

⁷⁷ Hayek ([1929, 1931] 2008, 139)

⁷⁸ *Ibid.*

⁷⁹ Foster and Catchings (1925, 281); quoted in Hayek ([1929, 1931] 2008, 140)

sustain the incentive to continue production at the increased level. This, according to the authors, is for two reasons. In the first place, under the existing monetary system, credit is typically extended to the wrong people – i.e., to producers – a circumstance that only exacerbates the mismatch between the incomes and expenditures of consumers. Secondly, the system ensures that credit is extended at the wrong time, as a response to improved demand conditions, and restricted just as demand shrinks: “In this way...every advance toward higher standards would be promptly checked; for whenever it appeared that consumer income was too small, it would be made smaller still through wage reductions, and under-production would follow promptly.”⁸⁰ What is needed, Foster and Catchings argue, is a system that provides credit to consumers at the time that the increased output arrives on the market for which the extant funds of consumers are insufficient:

If any safe and practicable means could be devised, in connection with increased public works and decreased taxes, or in any other connection, of issuing just enough money to consumers to provide for individual savings and to enable them to buy an enlarged output, and business men were confident that issues to consumers would continue at this rate and at no other rate, there would be no drop in the price-level and no reason for curtailing production, but, on the contrary, the most powerful incentive for increasing production.⁸¹

⁸⁰ Foster and Catchings (1925, 324); quoted in Hayek (2008, 143)

⁸¹ Foster and Catchings (1925, 330-331); quoted in Hayek (2008, 143)

With respect to concrete policy proposals, Foster and Catchings argue that, on the basis of a comprehensive system of economic statistics, all of the financial activities of the government, including the planning of public works, should be directed toward evening out fluctuations in consumption demand. Also, there should be a “Federal Budget Board” charged with collecting and publishing these statistics, and with designing and implementing a plan for consumption, and ensuring that consumption not lag behind production:

Progress requires a constant flow of new money to consumers. If, therefore, business indexes show the need for a reinforced consumer demand which cannot be met without additional Government expenditure, the Board should bring about such expenditure, not only out of funds previously accumulated for that purpose, but at times out of loans which involve an expansion of bank credit. This feature of the plan is essential. It follows that the Government should borrow and spend the money whenever the indexes show that the needed flow of money will not come from other sources.⁸²

According to Hayek, none of the essay award criticisms⁸³ successfully expose the main flaw in Foster and Catchings’ system. Most of the published objections attempt to resolve the purported dilemma of savings with the proof that the existing organization of money and credit “suffices to increase the supply of money in the course of an extension of production so as to avoid a fall in the price level.” However, “the alleged necessity to ease the sale of the enlarged product by an

⁸² Foster and Catchings (1928, 22)

⁸³ Foster and Catchings (1927)

increase in the money supply is, in general, allowed to pass unquestioned.”⁸⁴ These critics resort to various expedencies to meet Foster and Catchings’ objection that extension of credit to productive enterprises tends to exacerbate the consumer’s deficient purchasing power, which, according to Hayek, “Correct as these objections may be, they miss the point. The main thesis remains untouched.”⁸⁵

Hayek argues that the flaw in Foster and Catchings’ theory arises from the pair’s tendency to “overlook the phenomenon of changes to more or less capitalistic methods of production”⁸⁶ That is, Foster and Catchings ignore the possibility that production may increase (decrease) by shifting to more (less) capital-intensive methods of production and need not involve (as Foster and Catchings implicitly assume) employing capital in the same proportion relative to the “original” factors of production – land and labor – as before. In short, Hayek argues, the American authors fail to recognize the possibility that an increase in the volume of saving may be absorbed in an extension of the production process.

Hayek’s criticism of Foster and Catchings and his own explanation of the trade cycle depend crucially on the Austrian theory of capital⁸⁷ and the role it assigns to the interest rate in coordinating production and consumption decisions across time. According to this theory, and *contra* the suppositions of Foster and Catchings,

⁸⁴ Hayek ([1929, 1931] 2008, 147)

⁸⁵ *Ibid.*

⁸⁶ *Ibid.*, 154

⁸⁷ A theory which, it should be noted, has always had its critics within the Austrian camp. Indeed, Carl Menger, the founder of the Austrian School, considered it “one of the greatest errors ever committed” (Schumpeter 1954, 847n).

the production of consumers' goods is a time-consuming process that proceeds in multiple stages. On one end of the production process (or "structure") are those stages of production that are most temporally remote from final consumption, such as research and development, geological extraction, and the production of the most durable capital goods (e.g., plant equipment, commercial and residential buildings). On the other end of the structure of production are those stages that are nearer in time to the end-consumer, e.g., wholesale and retail operations. Goods typically pass through a number of stages before emerging from the process ready for consumption.

Under normal conditions, the interest rate on bank loans plays a vital role in coordinating intertemporal decision-making: it functions to balance the savings and consumption decisions of income-earners with the decisions of producers with respect to the production of goods on the far end of the structure (i.e., in industries producing "higher-order" or producers' goods) versus the production of goods on the near end of the structure (i.e., in industries producing "lower-order" or consumers' goods).⁸⁸ That (other things equal) investors prefer longer-term investments – i.e., in Austrian-speak, investments in higher-order goods – is an implication of the standard model of cash-flow discounting. A relatively low interest rate increases the comparative value of any future cash flow – it is indication to

⁸⁸ It should be noted that the distinction between producers' goods and consumers' goods is one of degree rather than of kind. There are some producers' goods that are specific to particular stages of production and others that can be employed in multiple phases of the production process. Indeed, one and the same good might be a consumers' good in certain uses (consider, e.g., a computer as used to access pornography for personal entertainment purposes) and a production good in other uses (e.g., the same computer as used to design a new building or a new machine).

investors that their future cash flow will suffice to cover their more immediate expenses – and, thus, *ceteris paribus*, lower interest rates encourage relatively greater investments in goods on the far end of the structure of production. Put another way, according to Hayek’s Wicksellian interest theory, the rate of interest on bank loans “governs not only the level of investment but also the allocation of resources *within* the investment sector...As implied by standard calculations of discounted factor values, interest rate sensitivity increases with the temporal distance of the...stage of production from final consumption.”⁸⁹

Other things equal, a higher interest rate is an indication that consumers prefer more goods in the near-term; that is, it is a sign that consumers are relatively unwilling to put off present consumption in order to save for the future, which is the same thing as saying that a higher rate of interest is a sign that consumers are relatively unwilling to make loans to producers. It is at the same time an indication to producers that, if they are to satisfy the relatively stronger demand for consumer goods in the near-term, then production must shift in the direction of methods capable of satisfying this demand, i.e., that production should shift to comparably less time-consuming (and, for this reason, less productive) methods. Conversely, *ceteris paribus*, a lower interest rate is a sign that consumers are relatively more willing to save in the present and wait for consumers’ goods; and it is a sign to producers that they can afford to wait longer for the sale of a final product and that production can be extended further into the future. In Austrian-speak, a lower interest rate is an indication to producers that they can afford to *extend (or lengthen) the structure of production* further out in time.

⁸⁹ Garrison (1996, 101)

Under normal conditions, the loanable funds market operates reasonably smoothly. Consumers increase (decrease) their voluntary savings, producers lengthen (shorten) the structure of production, and the supply of consumer goods conforms to consumers' demands. Of course, individual producers are human beings, and so, they occasionally make errors – that is, sometimes profit expectations for certain investments fail to meet reality – but, in the absence of some intervening factor, there is no reason why investors should make errors *en masse* and all in the same direction as they do during a typical business cycle.

In order to understand the sources of such errors, Austrians argue, it is vital to recognize that the structure of production can be lengthened in two different ways, i.e., either, as above, via an increase in the volume of voluntary saving, or through an increase in the supply of loanable funds *in the absence of a corresponding shift in consumers' preferences for future consumption (saving)*, i.e., via an expansion of bank credit. A structure of production extended due to an expansion of credit is not sustainable in perpetuity; according to Hayek's account of the business cycle, a point inevitably arises where consumers demand more consumers' goods than the lengthened structure of production can provide; when this happens, the structure of production must shrink in order to again be compatible with the demands of consumers; but this means that the entrepreneurs who started new investment projects in virtue of the eased access to credit cannot all complete their projects, and the crisis appears. In short, given the Austrian theory of capital at its core, Hayek's account of the business cycle explains economic disequilibrium in terms of the discombobulating effect that

credit expansion has on the delicate links between the interest rate, the supply of and demand for loanable funds, and consumers' preferences and producers' decisions.

Hayek's criticism of Foster and Catchings essentially amounts to the demonstration that their theoretical results depend on the absence of any such theoretical conception of the structure of production, and moreover, that appending the relevant capital theory to the authors' other assumptions leads to results that mirror those of Hayek's explanation of the business cycle. Moreover, echoing his discussion in *Monetary Theory and the Trade Cycle* and anticipating another of his objections to Keynes', Hayek argues that Foster and Catchings' theory starts from the assumption of the presence of the very disequilibrium phenomena that it purports to explain. That is, according to Hayek,

Messrs. Foster and Catchings seem to avail themselves of the assumption of an "industrial reserve army"—a notion much favored in trade cycle theory—from which the labor power necessary for a proportional extension of production can always be obtained at will. Quite apart from the incompatibility of this assumption with the known facts, it is theoretically inadmissible as a starting point for a theory that attempts...to show the causes of crises, and thus of unemployment, on the basis of the modern "equilibrium theory" of price determination.⁹⁰

⁹⁰ Hayek ([1929, 1931] 2008, 152-153)

3 Hayek's *Prices and Production* (1931)

It was “The ‘Paradox’ of Savings” that brought Hayek to the attention of Lionel Robbins, who in 1929, at 30 years of age, was appointed chair of the economics department at the London School of Economics. Robbins took it as his first task as chair to hire a highly-rated economic theorist who could both improve the L.S.E. department’s theoretical *bona fides* and provide a counterweight to the mighty Cambridge theoretical tradition associated with Alfred Marshall, A.C. Pigou, and the Keyneses (both junior and senior, Maynard Keynes’ father John Neville having himself been a respected Cambridge economist and methodologist). It was in this context that Hayek – like Robbins, just barely in his thirties – was invited to the L.S.E. in early 1931 to give the four lectures that would be published later that same year as *Prices and Production*. Hayek’s lectures, according to his biographer Bruce Caldwell, “had been hurriedly prepared. His English was little less than awful. (He was later told that he was all but incomprehensible whenever he was reading, but became intelligible when he paused to answer a question.) Despite all that, the lectures caused a huge stir.”⁹¹ Hayek was offered a one-year visiting professorship at the L.S.E., and in the following year, the Tooke Chair in Economic Science and Statistics.⁹²

⁹¹ Caldwell (1995, 21)

⁹² It was during this same foray to England that Hayek gave a condensed version of the *Prices and Production* lectures to certain members of the so-called Cambridge “Circus,” (Keynes himself was not present). The lectures did not go over as well in Cambridge as they did in London and it seems that Hayek was rather mockingly treated. The story of Hayek’s visit to Cambridge and his reception there is recounted in Caldwell (2004).

The first lecture of *Prices and Production* encompasses a history of the development of monetary theory and its relation to contemporary accounts of the business cycle. Hayek identifies three distinct strands of thought, each developed mostly independently of the others, two of which were finally brought together in the groundbreaking work of the Swedish economist, Knut Wicksell, with the third added by Ludwig von Mises.

The development of the first line of thought began with John Locke's statement of the naïve quantity theory of money, according to which, changes in the quantity of money bear no influence on "real" economic activity. The subsequent contribution of Richard Cantillon consisted in tracing the chain of cause and effect between changes in the supply of money and concomitant changes in the relative prices of different goods. Cantillon concluded, *contra* Locke and the naïve quantity theory, that "those persons are benefited by the increase of money whose incomes rise early, while to persons whose incomes rise later the increase of money is harmful."⁹³ More famously, David Hume later argued that an increase in the quantity of money in circulation is a boon to industry only during the interval between the appearance of new money and its influence on prices. Here the development of monetary theory stagnated.

It was only after the mid-19th century discoveries of gold in Australia and California that British economist John Cairnes was compelled to refine and precisify the arguments of Cantillon and Hume. Later, it was Ludwig von Mises' who, early in the 20th century, developed and extended the analysis upon the more secure value-

⁹³ Hayek ([1931, 1935] 2008, 203)

theoretical basis of marginal utility theory, and, moreover, made some progress toward a full integration of monetary theory with Wicksell's capital theory.

Distinct from the story of the advancement of monetary theory proper are the coincident developments of two lines of thought that eventually came together in Wicksell's capital theory, in particular, the explanation of the relationship between the quantity of money and the interest rate, and the related (but independently developed) theory of the influence of changes in the rate of interest upon the relative demand for capital (i.e., producers') goods and consumers' goods.

The recognition of some relationship between the interest rate and the quantity of money was initially made by Locke, but, according to Hayek, the first author to offer anything like a theory of the relationship was Henry Thornton, who – as a major participant in the British Banking and Currency school debates of the early 1800s – argued that the extent of banks' ability to place new loans is limited only by the difference between the prevailing rate of interest and the profit to be made on the use of credit, and so long as the latter exceeds the former, there is no limit upon banks' ability to expand the supply of money in circulation. This same argument appears in the work of David Ricardo, who “gave it a still more modern ring by speaking of the rate of interest falling below its *natural level* in the interval between the issues of the bank and their effects on prices.”⁹⁴

Thomas Joplin, one of the more prominent members of the Currency school, extended the argument to show that the rate of interest functions so as to bring the supply of and demand for capital into (what more modern economists

⁹⁴ *Ibid.*, 207

would describe as) an equilibrium relation. Joplin added the further point that banks that both deal in capital and issue currency cannot distinguish between demand for one or the other: “the demand for currency and the demand for capital are so mingled together that all knowledge of either is totally confounded.”⁹⁵ Similar arguments appeared in the work of Thomas Tooke (the namesake of the chair that Hayek occupied at the L.S.E.), Nassau Senior, and, “in a somewhat emasculated form,”⁹⁶ in John Stuart Mill’s famous *Principles of Political Economy*.

Another line of thought became bound up with the latter and eventually came to constitute capital theory as Hayek received it from his predecessors. This line concerns the effects of changes in the quantity of money on the production of capital goods and extends back to the economic works of Jeremy Bentham. However, the first author to clearly state the doctrine that came to be known as “forced saving” was Thomas Malthus, who wrote that

Whenever...a fresh issue of notes comes into the hands of those who mean to employ them in the prosecution and extension of profitable business, a difference in the distribution of the circulating medium takes place...in altering the proportion between capital and revenue in favour of the former. The new notes go into the market as so much additional capital, to purchase what is necessary for the conduct of the concern. But, before the produce of the country has been increased, it is impossible for one person to have more of it, without diminishing the shares of some others. This diminution is

⁹⁵ Joplin (1832, 109)

⁹⁶ Hayek ([1931, 1935] 2008, 210)

affected by the rise of prices, occasioned by the competition of the new notes, which puts it out of the power of those who are only buyers, and not sellers, to purchase as much of the annual produce as before.⁹⁷

Although the concept of forced saving appeared in the economic works of Dugald Stewart, and was later alluded to by Joplin, Mill, and Robert Torrens, the doctrine of forced savings received by Hayek was little changed from the one propounded by Malthus. It was only the later development and “perhaps independent rediscovery”⁹⁸ of the principle of forced savings by Léon Walras that is important to subsequent developments and this only

because it is probably through Walras that this doctrine reached Knut Wicksell. And it was only this great Swedish economist who at the end of the century finally succeeded in definitely welding the two, up to then, separate strands into one. His success in this regard is explained by the fact that his attempt was based on a modern and highly developed theory of interest: that of Böhm-Bawerk⁹⁹

Unfortunately, from Hayek’s perspective, it was not this momentous synthesis for which Wicksell earned his reputation, but “for the one point in which he definitely erred: namely, for his attempt to establish a rigid connection between

⁹⁷ Malthus (1811)

⁹⁸ Hayek ([1931, 1935] 2008, 214)

⁹⁹ *Ibid.*

the rate of interest and the changes in the general price level.”¹⁰⁰ Hayek’s description of Wicksell’s theory is worth quoting at length:

If it were not for monetary disturbances, the rate of interest would be determined so as to equalize the demand for and the supply of savings. This equilibrium rate, as I prefer to call it, he christens the natural rate of interest. In a money economy, the actual or money rate of interest may differ from the equilibrium or natural rate, because the demand for and the supply of capital do not meet in their natural form but in the form of money, the quantity of which available for capital purposes may be arbitrarily changed by the banks. Now, so long as the money rate of interest coincides with the equilibrium rate, the rate of interest remains “neutral” in its effects on the prices of goods, tending neither to raise nor lower them. When the banks, however, lower the money rate of interest below the equilibrium rate, which they can do by lending more than has been entrusted to them, i.e., by adding to the circulation, this must tend to raise prices; if they raise the money rate above the equilibrium rate—a case of less practical importance—they exert a depressing influence on prices. From this correct statement, however, which does not imply that the price level would remain unchanged if the money rate corresponds to the equilibrium rate, but only that, in such conditions, there are no *monetary* causes tending to produce a change in the price level, Wicksell jumps to the conclusion that, so long as the two rates agree, the price level must always remain steady...It is worth observing a further

¹⁰⁰ *Ibid.*

development of the theory. The rise of the price level, which is supposed to be the necessary effect of the money rate remaining below the equilibrium rate, is in the first instance brought about by the entrepreneurs spending on production the increased amount of money loaned by the banks. This process, as Malthus had already shown, involves what Wicksell now called enforced or compulsory saving.¹⁰¹

On Hayek's telling, in the early 1930s, monetary theory was both better developed than ever before, and yet, wholly inadequate. The former was evidenced by the widespread acceptance of Wicksell's argument that, contrary to simplistic versions of the quantity theory, monetary changes could indeed influence "real" economic factors, while the latter inadequacy was, from Hayek's perspective, manifested in the common insistence among economists – also responding to Wicksell's arguments – that maintenance of a constant average value of money was both necessary and sufficient for economic stability.

The next progressive development of monetary theory, Hayek says, tentatively began with his own *Monetary Theory and the Trade Cycle* and "The 'Paradox' of Saving," and involves both the rejection of the Wicksellian claim that stability of the general price level is both necessary and sufficient for economic equilibrium and the concomitant development of a theory that does not set out

to explain any "general value" of money but only how and when money influences the relative values of goods and under what conditions it leaves these relative values undisturbed, or, to use a happy phrase of Wicksell, when

¹⁰¹ *Ibid.*, 215-216

money remains *neutral* relatively to goods. Not a money which is *stable* in value but a *neutral* money must therefore form the starting point for the theoretical analysis of monetary influences on production, and the first object of monetary theory should be to clear up the conditions under which money might be considered neutral in this sense.¹⁰²

Hayek considers three explanations of variations in industrial output. The first, the view that “variations of industrial output are to be found in changes in the willingness of individuals to expand effort,”¹⁰³ is rejected on the grounds that it is a “highly artificial assumption” that is not “at all justified by our common experience.” Hayek is prepared to resort to such an explanation only if all others fail; its adequacy is “a question of fact” that Hayek makes no effort to explicitly undermine, arguing instead that there are other “less artificial”¹⁰⁴ explanations available. The second explanation accounts for variations in industrial output in terms of changes in the amount of factors of production used. Hayek argues – as he did in his earlier *Monetary Theory and the Trade Cycle* – that all such theories start from the assumption that unused resources exist, i.e., they assume the very disequilibrium that they purport to explain:

If we want to explain fluctuations of production, we have to give a complete explanation. Of course this does not mean that we have to start for that purpose *ab ovo* with an explanation of the whole economic process. But it

¹⁰² *Ibid.*, 221

¹⁰³ *Ibid.*, 223

¹⁰⁴ *Ibid.*, 224

does mean that we have to start where general economic theory stops; that is to say, at a condition of equilibrium when no unused resources exist. The existence of such unused resources is itself a fact which needs explanation.¹⁰⁵

The third account – the one that Hayek prefers – explains industrial fluctuations in terms of changes in the methods of using existing resources. More exactly, Hayek’s explanation does not invoke technological improvements as an explanation of increased productivity, but

the increase of output made possible by a transition to more capitalistic methods of production, or, what is the same thing, by organizing production so that, at any given moment, the available resources are employed for the satisfaction of the needs of a future more distant than before. It is to this effect of a transition to more or less “roundabout” methods of production that I wish particularly to direct your attention. For, in my opinion, it is only by an analysis of this phenomenon that in the end we can show how a situation can be created in which it is temporarily impossible to employ all available resources.¹⁰⁶

Hayek begins by specifying his stipulations and assumptions. Most important here is the assumption that a shift to temporally lengthier (more “roundabout”) methods of production is more productive, i.e., the premise that “within practical limits we may increase the output of consumers’ goods from a given quantity of

¹⁰⁵ *Ibid.*

¹⁰⁶ *Ibid.*, 226

original means of production [land and labor] indefinitely, provided we are willing to wait long enough for the product.”¹⁰⁷

The notion that the extent of productivity (in *value*, but not necessarily in *physical*, terms) is directly related with the roundaboutness of prevailing production methods has perplexed many economists since it was first expounded by Böhm-Bawerk¹⁰⁸ There is, of course, nothing intrinsic to more time-consuming production methods that necessarily make them comparatively more productive. However, Austrian capital theory is predicated on the assumption that a shift to a more roundabout method will be undertaken only on the expectation that – because the possibilities for increasing production via temporally-shorter methods have all been exhausted (*ceteris paribus* of course; some technological innovation might increase the productivity of less time-consuming processes) – longer methods are the only means of increasing output. In other words, humans typically attempt to realize their respective individual ends via the shortest path; a longer path will be chosen only in the expectation that it will generate more value than the alternate route. Other things equal, a producer will adopt a longer process of production only if she expects it to be more productive than the existing process.

To see this point better, consider Crusoe on his island, his waking hours entirely occupied with attempting to catch enough fish by hand to sustain him through the next morning. He imagines that he might be able to catch more fish per unit of time were he to fashion a net from sticks and kelp. Yet, he realizes that constructing the net will consume time that might otherwise be expended catching

¹⁰⁷ *Ibid.*

¹⁰⁸ Related notions appear in Ricardo and A.R.J. Turgot. See Garrison (1990)

fish by hand. Obviously, Crusoe will invest in constructing the net only if he believes that he will ultimately be compensated for the expense of its construction by an increased yield of fish, which, of course, might mean catching the same number of fish in a shorter period of time. Moreover, in order to construct the net at all, he will first have to *save* enough fish to sustain him through the period of its construction, a period during which he will catch no fish.¹⁰⁹ Of course, Crusoe's profit expectations might be disappointed: a rogue typhoon might drag his half-constructed net out to sea. But (to extend the analogy to the business cycle case), if we imagine a multiplicity of Crusoes scattered on disparate islands across the globe, then, absent some intervening force that deludes them all in the same way, there's no reason why all their investments should suffer the same fate. In other words, given such a plurality of Crusoes and the absence of any comprehensively-deluding force, we should expect the typical result of each extension of the respective production processes to be an increased yield of fish.

In *Prices and Production*, Hayek attempts to establish the direct relation between the extent of production and the roundaboutness of production methods with the assumption that the proportion between the amount of "intermediate goods" (which is to say producers' goods that are not land or labor, the latter being the so-called "original factors of production") that is required to generate a

¹⁰⁹ From the Classical, Austrian, and Neo-Classical perspectives, one major function of an increase in capital is to maintain industry long enough for the increased output to reach market, or, as Hayek put it in "The 'Paradox' of Saving," the function of capital is that of a "carrying agent" (Hayek [1929, 1931] 2008, 152) which allows industry to bridge the time gap separating the sale of goods of one period and the sale of the increased output resulting from a shift to more temporally-extended production methods.

continuous output of consumers' goods and the extent of that output increases with the length of production processes: "As the average time interval between the application of the original means of production and the completion of the consumers' goods increases, production becomes more capitalistic, and *vice versa*."¹¹⁰

Hayek's analysis begins from the simplifying assumption that the process of production is divided into distinct periods or stages, and that goods move from one stage to the other in equal temporal intervals. He further assumes that each stage represents a distinct enterprise, so that "goods moving toward consumption...change hands against money in equal intervals which correspond to our unit production periods." These two assumptions together imply that

the proportion of money spent for consumers' goods and money spent for intermediate products is equal to the proportion between the total demand for consumers' goods and the total demand for the intermediate products necessary for their continuous production; and this, in turn, must correspond, in a state of equilibrium, to the proportion between the output of consumers' goods during a period of time and the output of intermediate products of all earlier stages during the same period.¹¹¹

A shift to more (less) capital-intensive methods of production occurs when money demand for production goods increases (decreases) relative to money demand for consumers' goods. Such a shift occurs when there is a change in the proportion between the quantity of money available to producers for the purchase of

¹¹⁰ Hayek ([1931, 1935] 2008, 231)

¹¹¹ *Ibid.*, 234

factors of production and the quantity of money available for the purchase of consumers' goods. The latter sort of shift occurs either as a result of changes in the voluntary saving decisions of the individual members (including firms) of society, or in the absence of such a change, when the lending activities of the banks alter the extent of credit available to producers for the purchase of capital goods. The effects of these distinct means of altering the structure of production are different in important ways.

Hayek first considers the case of a *ceteris paribus* increase in the volume of voluntary saving (and its concomitants, i.e., a fall in demand for consumers' goods relative to producers' goods – an increase in saving being identical with a decrease in consumption – and an increase in the supply of money available for lending, and therefore, a fall in the interest rate.)¹¹² Such an increase in the funds available for production indicates to producers that they can afford to wait longer for an increased production, and so, they respond by extending the structure of production further out in time; under Hayek's assumptions, this means that additional, earlier stages are added to the existing structure of production.¹¹³ In terms of prices and profits, an increase in voluntary savings means that, the prices of consumers' goods fall relative to those of producers' goods, or, what is the same thing, that production in the earlier stages becomes relatively more profitable than production in the later stages of production. If they are not to suffer losses in the short-run, producers of

¹¹² With a few exceptions that we will not consider here, the effects of a *ceteris paribus* fall in the volume of voluntary saving are “exactly the reverse” of those described for an increase in saving (*Ibid.*, 260)

¹¹³ *Ibid.*, 238

consumers' goods must restrict the supply of their goods that reach the market, i.e., they must build up inventories, which serve the dual purpose of protecting these entrepreneurs against loss and ensuring that a quantity of goods is available that suffices to bridge the temporal gap required to complete the new, extended process of production. Those factors of production that can be used in multiple stages, i.e., most kinds of land, labor, and "nonspecific" capital goods, shift toward the earlier stages of production; the prices of those "specific" factors that can only be used in earlier stages rise (and therefore, are produced in greater quantities), while the prices (and production) of capital goods specific to later stages suffer the opposite result.¹¹⁴

It is important to note that such an increase in the volume of voluntary saving leads to no particular disequilibrium effects except perhaps in the (presumably brief) period of transition from one equilibrium to another. Under Hayek's assumptions – in particular, given the Austrian theory of capital – those factors thrown out of employment in the later stages of production due to the fall in the funds available for consumption find employment in the now more profitable earlier stages of production. To put the point another way, from Hayek's perspective, that underconsumptionist explanations of the cycle lead to the result that a *ceteris paribus* increase in the volume of voluntary savings must cause unemployment is due to the failure to build such analyses upon a capital-theoretic foundation like that provided by the Austrian account; underconsumptionists thus fail to recognize the possibility (indeed, necessity, on Hayek's assumptions) that such an increase in saving merely alters the composition of employment across the structure of production – i.e.,

¹¹⁴ *Ibid.*, 258-260

causes a relative shift in employment of factors from the later to the earlier stages of production – without manifesting persistent unemployment. Moreover, Hayek argues, the absence of such a capital-theoretic foundation blinds the authors of such explanations to the true effects of their common remedy, namely, increases in the quantity of money in circulation.

Hayek next considers the effects of such a *ceteris paribus* increase in the amount of the circulating medium in the form of additional credit granted to producers. Like an increase in voluntary saving, this too represents an increase in the proportion between the quantity of money available to producers for the purchase of capital goods and the quantity of money available for the purchase of consumers' goods, and producers respond as in the first instance, by extending the structure of production further out in time. However,

When a change in the structure of production was brought about by saving, we were justified in assuming that the changed distribution of demand between consumers' goods and producers' goods would remain permanent, since it was the effect of voluntary decisions on the part of individuals. Only because a number of individuals had decided to spend a smaller share of their total money receipts on consumption and a larger share on production was there any change in the structure of production. And since, after the change had been completed, these persons would get a greater proportion of the increased total real income, they would have no reason again to increase the

proportion of their money receipts spent for consumption. There would accordingly exist no inherent cause for a return to the old proportions.¹¹⁵

On the other hand, when the proportion between the respective demands for capital goods and consumers' goods shifts in favor of the former in the absence of an increase in the volume of voluntary saving, a situation of forced saving arises, that is, consumers *maintain* the proportion of their money income allotted to saving and consumption, but, because factors are bid away from the consumption end of the structure of production in virtue of the additional credit available to producers, fewer consumption goods reach market and their prices begin to rise; those individuals who have yet to experience a rise in their money incomes (i.e., those who earn their income in stages of production closer to consumption) are forced to abstain from consuming as much as they'd like—i.e., given that a decrease in consumption is nothing but an increase in saving, such individuals are forced to save: “There can be no doubt that, if their money receipts should rise again, they would immediately attempt to expand consumption to the usual proportion.”¹¹⁶

Hayek argues that, as the wage increases that are necessary to attract original means of production toward the far end of the structure of production spread out from the point at which increased credit is injected, money incomes *do* rise and consumers *do* reassert their old proportions between saving and consumption. That is, Hayek assumes that the proportion of their incomes that individuals allot to consumption and saving respectively are both relatively stable and not altered by the

¹¹⁵ *Ibid.*, 241-243

¹¹⁶ *Ibid.*, 243

credit activities of banks: in the case of an increase in the volume of voluntary saving, the new proportions are maintained and, in the case of forced saving, once their incomes rise again, individuals reassert their prior proportions. Thus, Hayek argues, it happens that the proportions between demand for producers' goods and consumers' goods shift again, this time in favor of the latter, and the structure of production shrinks.

The prices of consumers' goods will therefore rise relatively to the prices of producers' goods. And this rise of the prices of consumers' goods will be the more marked because it is the consequence not only of an increased demand for consumers' goods but an increase in the demand as measured in money. All this must mean a return to shorter or less roundabout methods of production if the increase in the demand for consumers' goods is not compensated by a further proportional injection of money by new bank loans granted to producers. And at first this is probable. The rise of the prices of consumers' goods will offer prospects of temporary extra profits to entrepreneurs. They will be all the more ready to borrow at the prevailing rate of interest. And, so long as the banks go on progressively increasing their loans it will, therefore, be possible to continue the prolonged methods of production or perhaps even to extend them still further. But for obvious reasons the banks cannot continue indefinitely to extend credit; and even if they could, the other effects of a rapid and continuous rise of prices would, after a while, make it necessary to stop this process of inflation.¹¹⁷

¹¹⁷ *Ibid.*, 268-269

In other words, the banks can prevent a contraction of the structure of production so long as they keep the lending rate below the profit expectations of entrepreneurs, which – because each (*ceteris paribus*) fall in the interest rate means that an increasing number of investments appear profitable – actually means “progressively increasing their loans.” However, “for obvious reasons” connected with either the depletion of their reserves, the inflationary price effects of prior credit expansions, or the simple fact that there is an absolute floor (i.e., zero) to the extent that they can lower the lending rate, a time inevitably arises when banks can no longer prevent the contraction of the production structure by further expanding their lending activities.

The consequences of a shift to a shorter structure of production in virtue of the end of a credit expansion are similar to the effects of a *ceteris paribus* increase in credit granted to consumers, so Hayek considers the two cases together. The relative shift in favor of demand for consumers’ goods is first reflected in increased price margins on the later end of the structure of production. Entrepreneurs are tempted by the increased profitability of these stages to bid nonspecific factors of production away from earlier stages.

Very soon the relative rise of the prices of the original factors and the more mobile intermediate products will make the longer processes unprofitable.

The first effect on these processes will be that the producers’ goods of a more specific character, which have become relatively abundant by reason of the withdrawal of the complementary nonspecific goods, will fall in price.

The fall of the prices of these goods will make their production unprofitable; it will in consequence be discontinued. Although goods in later stages of

production will generally be of a highly specific character, it may still pay to employ original factors to complete those that are nearly finished. But the fall in the price of intermediate products will be cumulative; and this will mean a fairly sudden stoppage of work in at least all the earlier stages of the longer processes.

What's more, Hayek argues, it is not the case that those factors thrown out of work in the earlier stages of production are rapidly absorbed in the expanded later stages, "Quite the contrary; the shorter processes will have to be started at the very beginning and will only *gradually* absorb all the available producers' goods as the product progresses toward consumption and as the necessary intermediate products come forward."¹¹⁸ The unemployed factors remain unemployable, in other words, so long as it takes to build the intermediate products specifically suited to the later stages of a shortened structure of production. The increased demand for consumers' goods that follows either at the end of a credit expansion financed by producers' credits or the beginning of one financed by consumers' credits removes some of the nonspecific capital goods required for the completion of the longer structure of production, the remaining nonspecific goods do not suffice, and the specific goods required to employ all of the nonspecific factors have not all been produced:

The situation would be similar to that of a people of an isolated island, if, after having partially constructed an enormous machine which was to provide them with all necessities, they found out that they had exhausted all their savings and available free capital before the new machine could turn out

¹¹⁸ *Ibid.*, 271

its product. They would then have no choice but to abandon temporarily the work on the new process and to devote all their labor to producing their daily food without any capital. Only after they had put themselves in a position in which new supplies of food were available could they proceed to attempt to get the new machinery into operation.¹¹⁹

In other words, according to Hayek, the crisis phase of the cycle arises with the realization that the products of investments encouraged by credit expansion cannot be brought to market profitably because the demand envisioned will not ultimately materialize as originally anticipated when the investments were undertaken: “The spending decisions of income earners clashes with the production decisions that generated their income. An intertemporal mismatch between earning and spending patterns eventually turns boom into bust.”¹²⁰

Hayek proceeds to consider the effects of a change in the amount of money in circulation under the assumption that resources are not fully employed. Hayek immediately dispenses with the notion that such a circumstance could be rectified by an increase in consumers’ credits:

If the foregoing analysis is correct, it should be fairly clear that the granting of credit to consumers, which has recently been so strongly advocated as a cure for depression, would in fact have quite the contrary effect; a relative

¹¹⁹ *Ibid.*, 272

¹²⁰ Garrison (1996, 102)

increase of the demand for consumers' goods could only make matters worse.¹²¹

On the other hand, Hayek allows that it is possible “in theory” that granting producers' credits during the crisis stage, when the structure of production shrinks beyond what is necessary for the restoration of equilibrium, may produce positive effects; however, if such a measure is not to be more harmful than beneficial, precision and prudence are required with respect to both the quantity and recipients of such credits: “Frankly, I do not see how the banks can ever be in a position to keep credit within these limits.”¹²² Recall the difficulties indicated by Joplin¹²³ and emphasized by Hayek in *Monetary Theory and the Trade Cycle*¹²⁴ concerning the impossibility of distinguishing incoming deposits as either saving-based or credit-based and the concomitant impossibility of knowing whether any particular loan is made on the basis of savings or credit. In essence, bankers are never in a position to know whether the current loan rate is below, above, or at par with the (natural) rate of interest that would obtain in a possible world, distinct from our own, in which the entire supply of loanable funds is the result of voluntary decisions to save rather than consume. In short, Hayek argues that any theoretical arguments that might be advanced for credit expansion during the crisis phase are undermined by the practical

¹²¹ Hayek ([1931, 1935] 2008, 274)

¹²² *Ibid.*, 274-275

¹²³ See pages 42-43 above.

¹²⁴ See pages 19-20 above.

– more specifically, *epistemic* – difficulties involved in preventing credit expansion from causing harm.

Turning from the crisis stage to the actual depression phase, Hayek argues that “it is still more difficult to see what lasting good effects can come from credit expansion.”¹²⁵ Equilibrium will be restored only when the structure of production is once again adapted to the proportion between the demand for consumers’ goods and the demand for producers’ goods as determined by the voluntary savings and consumption decisions of the individual members of society. Far from remedying economic depression, credit expansion can only foster the relevant disproportionalities.

Hayek infers an important methodological conclusion from his theoretical considerations, namely, that any theoretical emphasis on the effects of movements of the general price level is entirely misplaced:

The average movements of general prices show us nothing of the really relevant facts; indeed, the index numbers generally used will, as a general rule, fail even to attain their general object because, being for practical reasons almost exclusively based on prices of goods of a nonspecific character, the data used are never random samples in the sense required by statistical method, but always a biased selection which can only give a picture of the peculiar movements of prices of goods of this class...for similar reasons every attempt to find a statistical measure in the form of a general average of the total volume of production, or the total volume of trade, or general

¹²⁵ *Ibid.*, 275

business activity, or whatever one may call it, will only result in veiling the really significant phenomena, the changes in the structure of production.¹²⁶

In the last lecture of *Prices and Production* Hayek considers “The Case For and Against an ‘Elastic’ Currency.” If Hayek’s theoretical considerations are sound, then the common view that the money supply should vary as production increases or decreases is mistaken. The fall in prices that typically accompanies a *ceteris paribus* increase in productivity is “not only entirely harmless but is in fact the only means of avoiding misdirections of production.”¹²⁷ The contrary view is a symptom of the belief that stability in the general level of prices is both necessary and sufficient for economic equilibrium. Hayek’s theoretical considerations support instead the view that, in order to neutralize the effects of money on prices, the supply of the circulating medium must be invariable.

Hayek adopts the oft-used metaphor of an inverted pyramid to represent a country’s credit structure. The bottom of the pyramid represents the structure’s cash basis; the next level corresponds to central bank credit, the level above it represents commercial bank credit, and above that is the level corresponding to business credits external to the banking system. It is

only in regard to the two lower parts, cash and central bank credit, that an immediate control can be exercised by the central monetary authority. So far as the third part, the credits of the commercial banks, are concerned, it is at

¹²⁶ *Ibid.*, 276

¹²⁷ *Ibid.*, 283

least conceivable that a similar control could be exercised. But the uppermost section of the pyramid—private credits—can be controlled only indirectly through a change of the magnitude of their basis, i.e., in the magnitude of bank credit.¹²⁸

During the boom phase of a cycle, it often happens that the angle of the apex of the triangle increases, i.e., that relatively more central bank credit is granted on a certain cash basis, and relatively more commercial bank credit on a particular amount of central bank credit, and so on, so that

even if central banks should succeed in keeping the basis of the credit structure unchanged during an upward swing of the cycle, there can be no doubt that the total quantity of the circulating medium would nonetheless increase. To prevent expansion, therefore, it would not be sufficient if central banks, contrary to their present practice, refrained from *expanding* their own credits...it would be necessary for them actually to *contract* credit proportionately.¹²⁹

Hayek finds it “entirely utopian” to believe that such an experiment could be conducted given the state of opinion with respect to monetary affairs. In his characteristically pessimistic fashion, as he proceeds to consider the theoretical case for changes in the quantity of money, Hayek further emphasizes the “enormous”

¹²⁸ *Ibid.*

¹²⁹ *Ibid.*, 292

practical difficulties confronting the rationalization of monetary policy, “difficulties which monetary reformers are always so inclined to underrate.”¹³⁰

Heretofore, Hayek has assumed the *rate* at which money changes hands, the so-called “velocity of circulation,” to be constant. However, Hayek’s argument applies not to the amount of money as such, but to the “*amount of payments* made during a period of time,” which means that the invariability that must be maintained in order to secure monetary neutrality is not of the money supply narrowly understood but of the *flow* of the supply of money through the economy, the “money stream”: the maintenance of neutral money requires that monetary authorities compensate changes in the velocity of circulation¹³¹ such that the money stream remains constant. However, the practical difficulties of monetary neutrality do not end at discovering and quantitatively compensating for shifts in the velocity of circulation, “it would be necessary also to see that it came into the hands of those who actually require it, i.e., to that part of the system where that change in...the habits of payment had taken place.”¹³²

Regardless of whether such a problem could be managed in practice, it is evident, Hayek says, that “only to satisfy the legitimate demand for money in this sense, and otherwise to leave the amount of the circulation unchanged, can never be a practical maxim of currency policy.” This is due to the difficulty raised by Joplin

¹³⁰ *Ibid.*

¹³¹ Such changes occur for many reasons, one which Hayek considers is the division of a single firm representing multiple stages of the structure of production into distinct enterprises (and the contrary case of merger), which results in the addition of payments to (subtraction of payments from) the money stream.

¹³² *Ibid.*

above, i.e., the fact that banks that both deal in capital and issue currency cannot distinguish between demand for one and demand for the other, or, as Hayek expresses it in *Prices and Production*, the difficulty of determining “the demand for money as money which is justifiable, and the demand for money as capital which is not justifiable.”¹³³ It is stating the same problem in another way to say, as Hayek does, that “The ‘natural’ or equilibrium rate of interest which would exclude all demands for capital which exceed the real supply of capital [i.e., saving], is incapable of ascertainment.”¹³⁴

The only substantive policy advice that Hayek believes to follow from his theoretical considerations is

probably the negative one that the simple fact of an increase of production and trade forms no justification for an expansion of credit, and that—save in an acute crisis—bankers need not be afraid to harm production by overcaution. Under existing conditions, to go beyond this is out of the question...It is probably an illusion to suppose that we can ever be able entirely to eliminate industrial fluctuations by means of monetary policy.¹³⁵

The best Hayek is willing to hope for in this respect is the gradual dampening of public resistance to the policies necessary for monetary neutrality by some method of economic education.

¹³³ *Ibid.*

¹³⁴ *Ibid.*, 298

¹³⁵ *Ibid.*

However, Hayek offers the further warning that monetary neutrality provides no actionable criterion of rational policy. Strict monetary neutrality requires the establishment of all of the conditions that the theory says are necessary, but it is “very probable that this is practically impossible.”¹³⁶ In particular, securing a constant flow of the money stream requires the establishment of the conditions assumed by equilibrium theory, i.e., complete price and wage flexibility (the more or less immediate adjustment of prices and wages to changes in the economic data), and, relatedly, given that contracts are always written in terms of some medium of exchange, the basing of all intermediate- and long-term contracts on correct foresight with respect to future changes in the value of the exchange medium. Where these conditions do not obtain, “frictions” prevent the smooth and rapid adaptation of the price system to changes in the economic data that is assumed by equilibrium theory, and which are necessary for the effectiveness of policy aimed at monetary neutrality: “it may be necessary to seek for a compromise between two aims which can be realized only alternatively: the greatest possible realization of the forces working toward a state of equilibrium, and the avoidance of excessive frictional resistances.”¹³⁷ But, if such a compromise is practically mandated,

the elimination of the active influences of money has ceased to be the only, or even a fully realizable, purpose of monetary policy; and it could only cause confusion to describe this practical aim of monetary policy by the same

¹³⁶ *Ibid.*, 303

¹³⁷ *Ibid.*, 303-304

name, which is used to designate the theoretically conceivable situation, in which one of the two competing aims was fully obtained.¹³⁸

In other words, if the conditions required for the perfect adaptation of the price system to changed circumstances are not secured, the ideal of monetary neutrality “could not be realized by any kind of monetary policy.”¹³⁹

4 The Hayek-Keynes Debate Narrowly Construed (1930-1932)

The published debate between Hayek and Keynes, such as it is, concerns the latter’s (1930) *A Treatise on Money* and not his better-known and more influential (1936) *The General Theory of Employment, Interest, and Money*. The *Treatise* is a *long* (running to nearly seven-hundred pages in *The Collected Writings of John Maynard Keynes*, spread across two volumes) and challenging work. Thankfully, a detailed discussion of its contents is not necessary for our purposes.

A brief discussion of the theory of the *Treatise* is offered before proceeding to an analysis of the published debate – which consists of Hayek’s review of the *Treatise* (published in two parts in the L.S.E.’s house journal *Economica* in August 1931 and February 1932, respectively), Keynes’ reply and Hayek’s rejoinder (both published in *Economica* in November 1931, i.e., after the public airing of the first part of Hayek’s review, but before the appearance of the second part) – as well as a brief correspondence between the two principals written between December 1931 and the following February. We then consider the March 1932 review of *Prices and Production*

¹³⁸ *Ibid.*, 304

¹³⁹ *Ibid.*, 304

written for the *Economic Journal* (Cambridge's house journal) by Keynes' close friend and follower Piero Sraffa, as well as Hayek's June 1932 response to the latter.

The aforementioned publications and correspondence constitute what I call the Hayek-Keynes debate "narrowly construed." The debate more broadly construed both precedes Hayek and Keynes and, indeed, continues on to the present day; it is essentially identical to the continuing struggle over the methodological and theoretical issues raised by Hayek and Keynes.

The writing of the *Treatise* engaged Keynes during the latter part of the 1920s, at a time when he was considerably occupied with non-academic work in finance and government (and government finance). One can discern from the book's preface that its author is less than fully satisfied with the results of such a long intellectual endeavor and recognizes it as a work representing a transition to ideas that he has yet to articulate:

As I read through the page proofs of this book I am acutely conscious of its defects. It has occupied me for several years, not free from other occupations, during which my ideas have been developing and changing, with the result that its parts are not all entirely harmonious with one another. The ideas with which I have finished up are widely different from those with which I began. The result is, I am afraid, that there is a good deal in this book which represents the process of getting rid of the ideas which I used to have. There are many skins which I have sloughed still littering these pages. It follows that I could do it better and much shorter if I were to start over again. I feel like someone who has been forcing his way through a confused

jungle. Now that I have emerged from it, I see that I might have taken a more direct route and that many problems and perplexities which beset me during the journey had not precisely the significance which I supposed at the time. Nevertheless, I expect that I shall do well to offer my book to the world for what it is worth at the stage it is now reached, even if it represents a collection of material rather than a finished work.¹⁴⁰

The intellectual evolution that Keynes references here – the sloughing off of “many skins” – is, of course, the movement away from the Marshallianism of his youth and the gradual development of ideas uniquely his own, which, as we will see, ultimately culminates in the later *General Theory*.

In the *Treatise*, Keynes begins by grouping goods into two categories: investment goods and consumption goods; the output of each is decided at the beginning of each period on the basis of the profits of the preceding period. The units of each class of good are defined so that each unit has the same production cost (i.e., the earnings of factors of production at the beginning of the respective period).¹⁴¹ Income earners spend some part of their earnings on consumption goods and save the rest.¹⁴² All output is assumed to be sold in the current period. According to the *Treatise*, the price level of investment goods is determined on the stock market, the current output of investment goods being of minor significance as compared with the stock of accumulated capital, the prices of which are determined anew each

¹⁴⁰ Keynes ([1930] 1971a, xvii-xviii)

¹⁴¹ *Ibid.*, 121-122

¹⁴² *Ibid.*, 122

day on the stock market.¹⁴³ Income earners have a choice to make, not only with respect to whether to consume or to save, but, if the latter, with regard to whether to save in terms of bank deposits (which yield interest) or in terms of equities (the values of which change with the value of the capital stock).¹⁴⁴ Keynes further assumes that the interest rate on loaned funds (the “Bank Rate”) is a variable determined entirely by monetary policy, and that changes in the Bank Rate influence the supply of money.¹⁴⁵ Moreover, the demand price of investment goods is a function of the Bank Rate: *ceteris paribus*, if the rate rises (falls), the demand price of investment goods falls (rises), and spending on investment falls (rises).¹⁴⁶

From these assumptions, the rudiments of the theory of the *Treatise* follow. The cost of the factors of production (and thus, the volume of supply) is determined at the start of each period. Spending on consumption goods is determined by the community’s earnings and tendency to save rather than consume. This spending translates into the earnings of entrepreneurs, and therefore, the price level of consumption goods and the profits to be had in their production are jointly determined. As mentioned, the prices of investment goods are determined in the stock market, and thus, too, the price level of investment goods and profits to be

¹⁴³ *Ibid.*, 127

¹⁴⁴ *Ibid.*

¹⁴⁵ *Ibid.*, 194

¹⁴⁶ *Ibid.*, 183

made in their production. Finally, if entrepreneurs make profits (losses) in one period, they respond by expanding (decreasing) output in the next.¹⁴⁷

Keynes' emphasis in the *Treatise* is on the relation between the Wicksellian "natural" rate of interest and the rate of interest at which loans are available (the "Bank Rate"). If the latter is equal to the former, then saving and investment are equal, and the economy is in equilibrium. Keynes argues that "windfall" profits (losses) appear if and only if investment is greater than (less than) saving. Thus, according to the *Treatise*, if the Bank Rate falls below the natural rate, firms respond by expanding investment, which then exceeds saving, and entrepreneurs earn windfall profits. In response to these profits, entrepreneurs compete to extend production, but, because all factors are fully employed, the result is simply wage inflation. Since the consequence of a Bank Rate below the natural rate is ultimately inflation of both profits and wages, equilibrium is ultimately restored at a higher level of general prices. In short, in equilibrium, the Bank Rate equals the natural rate, saving equals investment, windfalls are zero, and the price level remains (technically, given that the *Treatise* distinguishes the price level for investment goods from the price level for consumption goods, this should be "price *levels* remain") stable. Conversely, in the absence of equilibrium, the Bank and natural rates of interest diverge, saving does not equal investment, windfalls appear, and the price level fluctuates (rises if investment exceeds saving, falls if saving exceeds investment).

This result is very much in line with Keynes' education in the Cambridge quantity theory tradition associated with Marshall; the unique twist is the addition of

¹⁴⁷ *Ibid.*, 141

Wicksell's theory of the natural rate of interest. The *Treatise* is "Wicksellian" in that it is changes in the price level(s) that exert an influence on the course of the economy, with the price level(s) being explained in terms of differences between the natural rate of interest and the Bank Rate (the latter being a policy instrument for price stabilization) and, thus, in terms of differences between saving and investment.

With respect to Keynes' specific explanation of the credit cycle in the *Treatise*, Toshiaki Hirai offers a consolidated summary:

Suppose that something (such as a new invention or a return of business confidence) happens that increases the attractiveness of investment. The price level of investment goods then rises, and the output increases in the next period. As a result of this, the level of employment increases and expenditure on consumption increases. Therefore the price level of consumption goods rises, and the output in the next period increases. Thus the behaviour of firms of increasing output under the condition of high profit causes a rise in money wages. In this process, the volume of working capital...also increases, so that business continues to pick up at an accelerated rate (the decrease in liquid capital will, it is argued, prove surprisingly small). The turning point in the boom occurs as a result of the accumulation of several causes: (i) the evaporation of the attractions of new investment; (ii) the faltering of financial expectation (as bearishness prevails the requirements of financial circulation increase); (iii) the fall in the price level of consumption goods (this is due to the stagnation of expenditure on consumption as compared with an increase in the output of consumption

goods) and iv) the growing inability of the banking system to keep pace with the increasing requirements of the industrial circulation to begin with, and then of the financial circulation (this brings about a rise in the rate of interest). Subsequently the economy tends to decline for the following reasons: (i) a fall in the price level of consumption goods drives away the entrepreneurs whose production costs are high; (ii) financial sentiment becomes bearish and (iii) an increase in the requirements of the industrial circulation, which occurs in the upward phase of the cycle, brings about a rise in the rate of interest which finally retards investment. The fall in the price level in this period is held to prove fairly large. Soon a cut in production and wages follows, together with a decrease in working capital, so that business deteriorates rapidly (on this occasion, liquid capital increases, but the increase will, it is argued, prove surprisingly small). Having fallen to the bottom of a cycle, the economy moves into the ascending phase once again due to the following causes: (i) the price level of consumption goods stops falling, and begins to rise. This follows from the fact that the degree of decrease in expenditure on consumption is smaller than that of the decrease of output; (ii) the liquid capital increases. Together with the restored attractiveness of new investment, these factors exert a pick-up effect on the economy.¹⁴⁸

The first half of Hayek's review of the *Treatise* – published in *Economica* in August 1931 – begins with the platitudinous praisings and having-said-thats that are common to critical book reviews, thus it is that “the appearance of any work by Mr.

¹⁴⁸ Hirai (2008, 62-63)

J.M. Keynes must always be a matter of importance: and the publication of the *Treatise on Money* has long been awaited with intense interest by all economists.”

“Nonetheless,” Hayek warns, “in the event, the *Treatise* proves to be so obviously—and I think admittedly—the expression of a transitory phase in a process of rapid intellectual development that its appearance cannot be said to have that definitive significance which at one time was expected of it.”¹⁴⁹

While Hayek has minor quibbles with the classification of different kinds of money that appears in Book I of the *Treatise*, he lauds Keynes’ “excellent and much needed criticism of certain attempts to base the method of index numbers on the theory of probability”¹⁵⁰ in Book II. It is with the theoretical analysis of Books III and IV of the *Treatise* that Hayek takes serious issue, and particularly, with Keynes’ treatment of entrepreneur’s profits: “I agree perfectly when he...depicts profits as the mainspring of change in the economic system. But I cannot agree with his explanation of why profits arise, nor with his implication that only changes in ‘total profits’ in his sense can lead to an expansion or curtailment of output.”¹⁵¹ Because Keynes lacks a theoretical apparatus that would permit him to trace the effects on prices and profits of changes in the relative demand for the goods of the various stages of production, he is led to treat profits as a purely monetary phenomenon “in the narrowest sense of that expression.”

¹⁴⁹ Hayek ([1931] 1995a, 121)

¹⁵⁰ *Ibid.*, 123

¹⁵¹ *Ibid.*, 124

The cause of the emergence of those profits...is not a “real” factor, not some maladjustment in the relative demand for and supply of cost goods and the irrelative products (i.e., of the relative supply of intermediate products in the successive stages of production) and therefore, something which could arise also in a barter economy, but simply and solely spontaneous changes in the quantity and direction of the flow of money...the flow of money is treated as if it were the only independent variable which could cause a positive or negative difference between the prices of the products and their respective costs. *The structure of goods on which this flow impinges is assumed to be relatively rigid.* In fact, of course, the original cause may just as well be a change in the relative supply of these classes of goods, which then, in turn, will affect the quantities of money expended on them¹⁵² (emphasis added).

According to Hayek’s theory – a point which, though it is underemphasized in *Prices and Production*, is repeatedly stressed in his earlier *Monetary Theory and the Trade Cycle* – the initial cause of a discrepancy between the natural and loan rates of interest (and, thus, between saving and investment) need not have a monetary origin.¹⁵³

¹⁵² *Ibid.*, 124

¹⁵³ See page 25 above; also Hayek ([1933] 2008, 98):

There is no necessary reason why the initiating change, the original disturbance eliciting a cyclical fluctuation in a stationary economy, should be of a monetary origin. Nor, in practice, is this even generally the case. The initial change need have no specific character at all; it may be any one among a thousand different factors that may at any time increase the profitability of any group of enterprises.

Hayek argues that Keynes' treatment of profits as purely monetary is a consequence of the latter's adoption of only a part of the Wicksellian architecture, i.e., the interest rate theory, but not the capital theory upon which it is based, which blinds the author of the *Treatise* to the possibility that profits can arise via spontaneous changes in the relative supply of the various classes of intermediate products, and thus, need not result from monetary changes:

As I shall repeatedly have occasion to point out, [Keynes] treats the process of the current output of consumption goods as an integral whole in which only the prices paid at the beginning for the factors of production have any bearing on its profitability. He seems to think that sufficient account of any change in the relative supply (and therefore in the value) of intermediate products in the successive stages of that process is provided for by his concept of (positive or negative) investment, i.e., the net addition to (or diminution from) the capital of the community. But this is by no means sufficient if only the total or net increment (or decrement) of investment goods in all stages is considered and treated as a whole, and *the possibility of*

It is the failure to immediately return to equilibrium under such circumstances and not the initiating movement away from equilibrium which, on Hayek's account, *must* be attributed to monetary factors:

It is not the occurrence of a "change of data" that is significant, but the fact that the economic system, instead of reacting to this change with an immediate "adjustment"—i.e., the formation of a new equilibrium—begins a particular movement of "boom" that contains within itself the seeds of an inevitable reaction. This phenomenon, as we have seen, should undoubtedly be ascribed to monetary factors (*Ibid.*).

fluctuations between these stages is neglected; yet this is just what Mr. Keynes does¹⁵⁴
(emphasis added).

Hayek notes other “mischievous peculiarities of this concept of profits” that he construes as consequences of Keynes’ inadequate understanding of capital. For instance, Keynes appears to separate the functions of entrepreneurs as owners of capital and as risk-takers, which leads to an “artificial separation of entrepreneurs’ profits from the earnings of existing capital” and “has serious consequences for the further analysis of investment.”

It leads not to an explanation of the changes in the demand price offered by entrepreneurs for new capital, but only to an explanation of changes in their aggregate demand for “factors of production” in general. But, surely, an explanation of the causes which make investment more or less attractive should form the basis of any analysis of investment. *Such an explanation can, however, only be reached by a close analysis of the factors determining the relative prices of capital goods in the different stages of production*—for the difference between these prices is the only source of interest. But this is excluded from the outset if only *total* profits are made the aim of investigation. Mr. Keynes’s aggregates conceal the most fundamental mechanisms of change.¹⁵⁵

Here we have Hayek’s objection to Keynes’ method of analyzing and explaining economic activity in terms of relations between aggregate and composite variables (and which subsequently became part and parcel of modern macroeconomics):

¹⁵⁴ Hayek ([1931] 1995a, 124)

¹⁵⁵ *Ibid.*, 128

according to Hayek, the important causal phenomena are changes in *relative* prices which are only obscured by an undue focus on causally irrelevant variables representing aggregated quantities.

As might be expected, Hayek's dissatisfaction with the lack of a capital-theoretic basis of the *Treatise* comes to the fore in his discussion of Keynes' treatment of investment. Hayek argues that Keynes attempts to analyze "complex dynamic processes without laying the necessary foundations by adequate static analysis of the fundamental process"; Keynes fails "to concern himself with the conditions that must be given to secure the continuation of the existing capitalistic (i.e., roundabout) organisation of production—the creating [of] an equilibrium between the depreciation and the renewal of existing capital." Moreover, Keynes takes "the maintenance of the capital stock more or less as a matter of course (which it certainly is not—it requires quite definite relationships between the prices of consumption goods and the prices of capital goods to make it profitable to keep capital intact)." Finally, Keynes "does not even explain the conditions of equilibrium at any given rate of saving, nor the effects of any change in the rate of saving. Only when money comes in as a disturbing factor by making the rate at which additional capital goods are produced different from the rate at which saving is taking place does he begin to be interested."¹⁵⁶

All of this, according to Hayek, is due to the lack of a capital-theoretic foundation in the *Treatise*: the aforementioned misgivings "would do no harm if [Keynes'] analysis of this complicating moment were based on a clear and definite

¹⁵⁶ *Ibid.*, 128-129

theory of capital and saving developed elsewhere, either by himself or by others. But this is obviously not the case.¹⁵⁷ What's more, any attempt to develop Keynes' discussion of investment is hindered by the latter's separation of the reproduction of existing capital from additions to the capital stock:

New savings and new investment are treated as if they were something entirely different from the reinvestment of the quota of amortisation of old capital, and as if it were not the same market where the prices of capital goods needed for the current production of consumption goods and of additional capital goods are determined.¹⁵⁸

Rather than attempting the sort of "horizontal" division between goods of higher stages and goods of lower orders provided by the Austrian theory of capital, Keynes

attempts a kind of vertical division, counting for that part of the production of capital goods which is necessary for the continuation of the current production of consumption goods as a part of the process of producing consumption goods, and only that part of the production of capital goods which *adds* to the existing stock of capital as production of investment goods.¹⁵⁹

This method of vertical division of the production structure makes it difficult, Hayek argues, to determine what Keynes counts as additional capital.

¹⁵⁷ *Ibid.*, 129

¹⁵⁸ *Ibid.*

¹⁵⁹ *Ibid.*

Wicksell's theory of the natural rate of interest and of the effects of discrepancies between the latter and the market rate is a "natural outgrowth of the most elaborate theory of capital we possess, that of Böhm-Bawerk." However, Keynes accepts only the interest theory and not the conception of a horizontal structure of production that justifies it; his notion of production as a vertically-integrated process does not support the story about interest that he wants to tell.¹⁶⁰ It is Keynes' selective appropriation of Wicksell that troubles Hayek: Keynes' interest rate story is unjustified without its original capital-theoretical support.

The first part of Hayek's review of the *Treatise on Money* closes with an old complaint, first heard in *Monetary Theory and the Trade Cycle*,¹⁶¹ to the effect that monetary theorists in the Wicksellian tradition focus too much on the effects of divergences between the natural and market rates of interest upon the general level of prices rather than on the effects of such divergences on the structure of relative prices. "There can, of course, be no doubt," Hayek says "that every divergence between [investment and saving] is of enormous importance. But that importance does not lie in the direction of its influence on the fluctuations of the *price-level*."¹⁶²

All evidence suggests that Keynes was profoundly irritated with the first part of Hayek's review of the *Treatise*. According to Donald Moggridge, the editor of Keynes' *Collected Writings*, Keynes' copy of the August 1931 edition of *Economica*

¹⁶⁰ *Ibid.*, 130

¹⁶¹ See pages 10-14 above.

¹⁶² *Ibid.*, 145

is among the most heavily annotated of the surviving copies of his journals, with no less than 34 penciled marks or comments on the 26-page review. At the end of his copy of the review, Keynes summed his reaction by writing: “Hayek has not read my book with that measure of ‘good will’ which an author is entitled to expect of a reader. Until he can do so, he will not see what I mean or know whether I am right. He evidently has a passion which leads him to pick on me, but I am left wondering what this passion is.”¹⁶³

Keynes does not restrain himself in his response to Hayek’s review. He begins his November 1931 “Reply to Dr. Hayek” in *Economica* by arguing that “What [Hayek] has done...is to pick over the precise words I have used with a view to discovering some verbal contradiction or insidious ambiguity.” In his review of the *Treatise*, Hayek asks for further clarification regarding particular arguments and the intended meanings of certain terms; but, Keynes argues, regardless of whatever clarifications he might provide,

I feel sure that I shall have made little or no progress towards convincing Dr. Hayek. For it is not really my use of language or the fact that my treatment falls far short of a complete analysis (as it certainly does) which is troubling him. It is something much more fundamental. And after reading his article carefully, I have no doubt at all what it is.¹⁶⁴

¹⁶³ Keynes (1973, 243)

¹⁶⁴ *Ibid.*, 244

Keynes argues that Hayek fundamentally misconstrues the central thesis of the *Treatise*; he denies that the theory of the *Treatise* leads to the implication Hayek attributes to it that

an act of monetary expansion...or an increase in the total quantity of money ...is not merely a possible cause of investment exceeding saving, but (1) that it is a necessary cause of this and (2) that the amount of the monetary expansion exactly measures the excess of investment over saving and hence is equal to the amount of profits (in my terminology).¹⁶⁵

Indeed, referring to a passage in *Prices and Production* in which Hayek “succinctly” – perhaps too succinctly, as it turns out – “states his own theory,”¹⁶⁶ Keynes attributes the preceding proposition to the Austrian.

It is worth pausing to consider the extent to which the two principals talk past each other in the published debate: Hayek attributes a particular thesis to Keynes that the latter claims is no implication of his theory, while Keynes attributes the same thesis to Hayek, perhaps because, at the time he was writing, the former

¹⁶⁵ *Ibid.*, 246

¹⁶⁶ Keynes refers here to Hayek ([1931, 1935], 217-218):

It is perfectly clear that, in order that the supply and demand for real capital should be equalised, the banks must not lend more or less than has been deposited with them as savings. *And this means naturally that they must never change the amount of their circulation.* At the same time, it is no less clear that, in order that the price level may remain unchanged, the amount of money in circulation must change as the volume of production increases or decreases. The banks could *either* keep the demand for real capital within the limits set by the supply of savings, *or* keep the price level steady; but they cannot perform both functions at once. [Italics added by Keynes; quoted in Keynes 1973, 245]

had no access to an English translation of *Monetary Theory and the Trade Cycle* (not translated until 1933) in which the stated thesis is explicitly denied.^{167, 168} Each of the principals attributes the same view to the other, a position that neither in fact maintained.

Keynes partially absolves Hayek for misunderstanding the argument of the *Treatise*, noting that “Since Dr. Hayek has not been alone amongst competent critics of my *Treatise* in falling into this misapprehension (or into some more subtle variation of it), it must be my own fault, at least in part.”

I suspect that it may be partly due to the fact that when I first began to work on Book 3...I believed something resembling this myself. My ceasing to believe it was the critical point in my own development and was the germ from which much of my eventual theory was worked out...after I had adopted this new view, I was at great pains to bring the rest of my work into line with it. But traces of old trains of thought are not easily obliterated, and certain passages which I wrote some time ago may have been unconsciously

¹⁶⁷ Hayek ([1933] 2008, 98); see footnote 153 above.

¹⁶⁸ Keynes’ admittedly shaky grasp on the German language should also be noted: “in German I can only clearly understand what I know already!—so that *new* ideas are apt to be veiled from me by the difficulties of language” (Keynes [1930] 1971a, 178n). This fact helps explain not only Keynes’ lack of familiarity with Hayek’s earlier *Monetary Theory and the Trade Cycle*, but also his failure to build on Wicksell’s (1893, but not translated into English until 1954) *Über Wert, Kapital, and Rent* (translated as *Value, Capital, and Rent*) in which the great Swedish economist synthesizes Böhm-Bawerk’s capital theory with marginal productivity theory in a general equilibrium framework; the latter explanation of Keynes’ ignorance of the full scope of Wicksell’s work is emphasized by Caldwell (2004, 177-178).

cast into a mould less obviously inconsistent with my own former views than they would be if I were writing now.¹⁶⁹

But Keynes is not willing to accept all responsibility for the misunderstandings of his critics. The failure to recognize the transition in his monetary thought is, “[f]or anyone brought up in the old Quantity-of-Money, Velocity-of-Circulation schools of thought, whether it be Cambridge Quantity Equations or Fisher Quantity Equations...a difficult transition to make. I found it so myself.” These critics “cannot bring themselves to believe that I am asking them to step into a new pair of trousers, and will insist on regarding it as nothing but an embroidered version of the old pair which they have been wearing for years.” However, for someone who holds “what Dr. Hayek believes...the transition would be easy.” In any case, Keynes writes, “I could never have expected, if it had not been for more than one experience to the contrary, that a competent economist could read my *Treatise* carefully and leave it with the idea that it was *my view* that the difference between saving and investment could be exactly measured by changes in the quantity of money.”¹⁷⁰

Keynes then launches into an impromptu review of Hayek’s *Prices and Production*, concluding that “*My* analysis is quite different...in my view saving and investment...can get out of gear without any change on the part of the banking system from ‘neutrality’ as defined by Dr. Hayek, merely as a result of the public changing their rate of saving or the entrepreneurs changing their rate of

¹⁶⁹ Keynes (1973, 246-247)

¹⁷⁰ *Ibid.*, 247

investment.”¹⁷¹ The ersatz review continues and Keynes’ assessment is little short of withering:

The book, as it stands, seems to me to be one of the most frightful muddles I have ever read, with scarcely a sound proposition in it beginning with page 45, and yet it remains a book of some interest, which is likely to leave its mark on the mind of the reader. It is an extraordinary example of how, starting with a mistake, a remorseless logician can end up in Bedlam. Yet Dr. Hayek has seen a vision, and though when he woke up he has made nonsense of his story by giving the wrong names to the objects which occur in it, his Khubla Khan is not without inspiration and must set the reader thinking with the germs of an idea in his head.¹⁷²

This said, Keynes proceeds straight away to a concession of the central point of Hayek’s review, namely, the lack of a capital-theoretic basis for the theory of the *Treatise*. However, importantly, Keynes disputes the necessity of such a foundation to monetary theory:

Dr. Hayek complains that I do not myself propound any satisfactory theory of capital and interest and that I do not build on any existing theory... This is quite true; and I agree with Dr. Hayek that a development of this theory would be highly relevant to my treatment of monetary matters and likely to throw light on dark corners. It is very possible that, looking back after a satisfactory theory has been completed, we shall see that the ideas that

¹⁷¹ *Ibid.*, 251

¹⁷² *Ibid.*, 252

Böhm-Bawerk was driving at, lie at the heart of the problem...But there is no such theory at present, and, as Dr. Hayek would agree, a thorough treatment of it might lead one rather a long way from monetary theory. Nevertheless, substantially I concede Dr. Hayek's point. I agree with him that a clear account of the factors determining the natural rate of interest ought to have a place in a completed *Treatise on Money*, and that it is lacking in mine: And I can only plead that I had much to say for which such a theory is not required and that my own ideas about it were still too much in embryo to deserve publication. Later on, I will endeavour to make good this deficiency.¹⁷³

Despite the concluding rain check to his readers – the promise to make whole the capital-theoretic lacuna in his work – Keynes never did return to the capital problem. Whatever traces of capital theory that are present in the *Treatise* are absent from *The General Theory*.

In any case, setting aside the manifest confusion on both sides regarding the substance of the principals' respective theories, the crux of the Hayek-Keynes debate is well illustrated by the foregoing quote: Hayek and Keynes disagree about the relevance of capital theory to the sort of project in which they are both engaged. Both of the principals agree that Keynes' *Treatise on Money* is not grounded in a satisfactory capital theory; moreover, both agree that no fully satisfactory theory of capital exists; yet, because he considers capital theory to be so essential to investigations of the effects of money, and, relatedly, to explanations of the business

¹⁷³ *Ibid.*, 252-253

cycle, Hayek is prepared to move forward on the basis of the best, yet far from fully adequate, theory of capital available, i.e., the one he inherited from Wicksell and Bohm-Bawerk; while, conversely, Keynes believes that “such a theory is not required” to substantiate the things he wants to say about money and the business cycle, and so he proceeds in the *Treatise* (and also in *The General Theory*) more or less *sans* capital theory. Thus, we arrive at the still unresolved question to which Keynes and Hayek (and their respective followers) offer different responses: in the absence of an adequate and satisfactory explanation of capital, is it better to attempt to explain the cycle on the basis of no capital theory at all or on the basis of an inadequate and unsatisfactory capital theory?¹⁷⁴

Before closing with an attempt to clarify the verbal confusions raised in the first half of Hayek’s review, Keynes argues that what really separates the two is the “long-period” focus of Hayek, an emphasis for which, Keynes argues, a theory of the

¹⁷⁴ The motivation for the Austrian preference for an admittedly inadequate capital-theoretic foundation for business cycle theory rather than none at all is connected with the manifest difficulties involved in theorizing about the phenomena of capital. In the Austrian reckoning, capital is fundamentally *heterogeneous*. There are significant, economically-relevant differences between, say, a pencil and a factory in which machines that make pencils are built, although pencils, pencil-making machines, and factories that build pencil-making machines are all capital-goods. The Austrian theory of capital, with its distinctions between stages of production and the degrees of specificity of particular capital goods is a first approximation to an adequate explanation of capital phenomena. Austrians have always recognized that a completed theory of capital would be far *more complex* than the first approximation of Böhm-Bawerk and Wicksell (subsequently extended into a commensurately more complex second approximation by Hayek himself in *The Pure Theory of Capital* (Hayek [1941] 2007)). In other words, Austrians argue against Keynes’ decision to treat capital as essentially *homogeneous* on the grounds that it represents a movement *away* from what they would take to be an adequate explanation of capital phenomena.

These issues are connected with a methodological problem that arises in the third chapter of the present paper, namely, the apparent tradeoff between the need for theories that are *simple* and the need for theories that are *realistic*.

structure of production may be required; he, however, is more concerned with the conditions that determine the natural rate of interest in the “short-period”: “If I am right...our theories occupy—as I believe they do—different *terrains*.”¹⁷⁵

Hayek’s “Rejoinder to Mr. Keynes” appears in the pages immediately following Keynes’ “Reply” in the same November 1931 edition of *Economica*. Hayek appears taken aback by Keynes’ choice to reply with a review of *Prices and Production*:

Mr. Keynes’ answer does not seem to me to clear up many of the difficulties I have pointed out, or indeed to improve the basis for further discussion. Instead of devoting his answer mainly to clearing up the ambiguities which I have indicated carefully and in detail, and the existence of which he cannot deny, he replies chiefly by a sweeping accusation of confusion, not in my critical article, but in another work, and even here I am unable to reply as he does not specify my confusions in any single case. I am bound to say that while I am very ready and indeed eager seriously to consider any definite criticism which Mr. Keynes may care to make, I cannot see what possible end is served by an unproved condemnation of my views in general. I cannot believe Mr. Keynes wishes to give the impression that he is trying to distract the attention of the reader from the objections which have been raised against his analysis by abusing his opponent, and I can only hope that after

¹⁷⁵ Keynes (1973, 253)

my critical article has appeared in its entirety he will not only try to refute my objection more specifically, but also to substantiate his counter-criticism.¹⁷⁶

Whatever the merits of Keynes' critique of *Price and Production*, Hayek is none too impressed with Keynes' efforts to redress the confusions raised in the first part of the review:

It is a regrettable fact that in none of the cases in which I have shown that several interpretations of the meaning of his exposition are possible has he explained decisively which of these interpretations is to be considered as authoritative. He has failed to elucidate his concept of Investment. I am as

¹⁷⁶ Hayek ([1931c] 1995, 159). Hayek was not alone in taking a dim view of Keynes' dialectical tactics. Keynes' Cambridge colleague A.C. Pigou (himself soon to be the explicit target of Keynes' withering pen in *The General Theory*) wrote in 1935, referring to the Hayek-Keynes debate,

Are we, in our secret hearts, wholly satisfied with the manner, or manners, in which some of our controversies are carried on? A year or two ago, after the publication of an important book, there appeared an elaborate and careful critique of a number of particular passages in it. The author's answer was, not to rebut the criticisms, but to attack with violence another book, which the critic had himself written several years before! Body-line bowling! The method of the duello! That kind of thing is surely a mistake" (Pigou 1935, 23-24).

It must be said that, from a less genteel modern perspective, it is difficult to understand what all the fuss was about over Keynes using his "Reply" to criticize *Prices and Production*: Hayek had attacked the *Treatise* using the theoretical apparatus of *Prices and Production* as a weapon; surely it was open to Keynes to disarm his opponent. However, it is perhaps easier to understand the rigmarole if we note a subtle distinction between Pigou's complaint and Hayek's assessment of Keynes' tactics: unlike Pigou, Hayek's complaint is not *per se* that Keynes attacked *Prices and Production*, but that the attack is of the form of a "sweeping accusation of confusion...an unproved condemnation of [Hayek's] views in general" that does not "specify...confusions in any single case." In other words, Pigou's complaint is that Keynes, rather than sticking to a response to Hayek's review of the *Treatise*, chose to attack *Prices and Production*; Hayek's complaint is that the attack was overly broad and underspecified.

much at a loss as ever to see what it means exactly. The same thing is true of his concept of profits. Indeed, until he has elucidated the concept of investment I do not see how he can be clear in his use of the term profit.¹⁷⁷

With respect to his interpretation of the central thesis of the *Treatise*, Hayek admits that, though he has pointed out that Keynes “does not consistently adhere to the idea that a discrepancy between saving and investment can only arise as a consequence of a change in the effective quantity of money,” he has *assumed* that this must be the main thesis of the *Treatise*

because I have been unable, and indeed still am unable, to detect in his *Treatise* or his subsequent elucidations any other tenable explanation of this phenomenon, and because I refused to believe, as I am afraid I must now believe, that Mr. Keynes could possibly consider his analysis of the relation between profits and investment an independent and sufficient explanation of how this discrepancy arises.¹⁷⁸

In short, Hayek’s misapprehension of the central position is, he argues, due to the lack of any other reasonable explanation of the discrepancy between saving and investment in the *Treatise*.

Nor does Hayek consider Keynes’ clarification in the latter’s “Reply” an improvement: “As I understand him now, his position is that an excess of saving over investment will arise when part of the saving, instead of being used for new

¹⁷⁷ Hayek ([1931] 1995b, 159-160)

¹⁷⁸ *Ibid.*, 161

investments, is used to make up losses.”¹⁷⁹ Hayek considers the “simplest” such case, that of an entrepreneur who fails to earn his expected return and who responds by cutting back his personal expenditure (“to the extent to which his expenditure fell below his ‘normal’ wage, this would constitute saving in Mr. Keynes’s terminology”¹⁸⁰) so as to continue to pay out to the factors of production as much as before his losses—“these ‘savings’ would not lead to corresponding investing.”¹⁸¹ Hayek’s complaint here is familiar to readers of his earlier *Monetary Theory and the Trade Cycle*: Keynes assumes the very disequilibrium phenomena his theory purports to explain. That is, though Keynes’ explanation is

no doubt true as a consequence of the definition of the concept chosen...it explains neither how the excess of saving over investing, nor how the windfall losses arose, but only that both are *ex definitione* identical. To say that the excess of saving over investment is the cause of the losses (or the reverse) has no sense whatever. There exists only the kind of disequilibrium which has been supposed to exist at the outset when the hypothesis that the

¹⁷⁹ *Ibid.* In his “Reply,” Keynes argues that a change in the quantity of money can occur without any effect on the relationship between saving and investment: “(to indicate a general principle by means of an illustration)...if, desiring to be more liquid, I sell Consols to my bank in exchange for a bank deposit and my bank does not choose to offset this transaction but allows its deposits to be correspondingly increased, the quantity of money is changed without anything having happened either to saving or to investment” (Keynes 1973, 248). In a footnote in his “Rejoinder,” Hayek denies that such a transaction affects the effective circulation: “a sale of securities to a bank in order to improve liquidity (i.e., to hoard) does not change the effective quantity of money (in the sense of the concept on which we seem to be agreed)” (Hayek [1931] 1995b, 161n).

¹⁸⁰ *Ibid.*, 161

¹⁸¹ *Ibid.*

entrepreneurs were making windfall losses was introduced, and he does not adduce any reason for assuming that the original and unspecified cause of the losses would be eliminated by the banking system making up for the difference by lending more to investors.¹⁸²

Worse still, according to Hayek, the proffered relation between profits, money, and investment does not explain how windfalls can arise as a consequence of a discrepancy between saving and investment *in the absence of a change in the effective circulation of money*. “So long as the money goes somewhere and is not hoarded and no new money is added...it is difficult to see how there can arise that difference between the total expenditure and the total receipts of entrepreneurs which alone can create total profits in Mr. Keynes’s sense.”¹⁸³

Digging a bit deeper into the example, Hayek thinks he recognizes another point of substantive disagreement between he and Keynes: the assumption that an entrepreneur would continue to produce as before despite incurring windfall losses, even if it requires curtailing his own consumption, seems to Hayek to conceal the implicit assumption that agents do not respond to the price signals relevant to their economic activities:

What does it actually mean if part of current savings is used to make up for losses in the production of consumption goods[?]...It must mean that though the production of consumption goods has become less profitable,

¹⁸² *Ibid.*

¹⁸³ *Ibid.*, 162

and that though at the same time the rate of interest has fallen so that the production of investment goods has become relatively more attractive than the production of consumption goods, yet entrepreneurs continue to produce the two types of goods in the same proportion as before. What justification can there be for this assumption?¹⁸⁴

All of this, of course, Hayek construes as a consequence of Keynes' failure to build the *Treatise* upon Wicksell's integration of Böhm-Bawerk's capital theory with general equilibrium—there (“where there is no banking system”) the function of the interest rate is precisely to coordinate intertemporal decision making.

Finally, with respect to the central point of contention between them, Hayek indicates his position clearly: “the obvious answer” to the problem of the absence of an adequate and satisfactory theory of capital

is that even if we have no quite satisfactory theory we do at least possess a far better one than that on which [Keynes] is content to rely, namely that of Böhm-Bawerk and Wicksell. That he neglects this theory, not because he thinks it wrong, but simply because he has never bothered to make himself acquainted with it,¹⁸⁵ is amply proved by the fact that he finds unintelligible my attempt to develop certain corollaries of this theory—corollaries which are not only essential for the very problem we are discussing, but which, as

¹⁸⁴ *Ibid.*

¹⁸⁵ How could Keynes have acquainted himself with the relevant work (*Value, Capital, and Rent*) of Wicksell? It wasn't available in English at the time, and, by his own admission, Keynes' German was not up to the standards required to comprehend Wicksell's argument in the untranslated text.

experience has shown me, are immediately intelligible to every student who has ever studied Bohm-Bawerk or Wicksell seriously.¹⁸⁶

Thus, Hayek's position, *contra* Keynes, is that capital theory *is* "essential to the very problem we are discussing" and, though this may require moving forward on the basis of an incomplete theory, with respect to the problem at hand, some theory is better than none at all.¹⁸⁷

A brief written correspondence arose between the two principals in the wake of the November 1931 dustup in *Economica*. It is a sufficient indication of the fruitlessness of this correspondence to consider the first and last letters written by Keynes to Hayek. In the first letter, dated December 10, 1931, Keynes asks Hayek for further clarification of the latter's concept of saving: "I should find this clearer if you could give me a formula which shows how saving is *measured*. Also, what is the difference between 'voluntary saving' and 'forced saving' in *your* terminology"¹⁸⁸ [italics in the original]. In the final letter, dated February 11, 1932, Keynes seems as perplexed by Hayek as ever:

Going back to the point at which our correspondence started, I am left where I began, namely in doubt as to just what you mean by voluntary saving and forced saving as applied to the actual world we live in: though I think I understand now what you mean by them in certain special cases, and this of

¹⁸⁶ *Ibid.*, 162-163

¹⁸⁷ See footnote 174 above.

¹⁸⁸ Keynes (1973, 257)

course gives me some sort of general idea as to the sort of thing you have in mind.¹⁸⁹

Keynes' frustration with the brief dialogue is also illustrated by a February 1, 1932 dispatch to Cambridge Circus members Piero Sraffa and Richard Kahn; with respect to his ongoing dialogue with Hayek, Keynes writes "What is the next move? I feel that the abyss yawns—and so do I. Yet I can't help feeling that there *is* something interesting in it."¹⁹⁰

In the second part of his review of the *Treatise*, Hayek attempts to establish that, whatever the independent merits of Keynes' subsidiary arguments that saving and investment can "get out of gear" for reasons unrelated to monetary considerations, these claims are no consequence of the theoretical framework of the *Treatise*. While Keynes' explanation of deviations between the short-run market rate of interest and the equilibrium rate is intended to convince the reader that such divergences can arise independently of changes in the effective quantity of money – a point that Keynes emphasizes "so strongly that he could scarcely expect any reader to overlook the fact that he wishes to demonstrate it"¹⁹¹ – Hayek argues that the *theory* of the *Treatise* supports only the negation of this conclusion: "while he certainly *wants* to establish this proposition, I cannot find any proof of it in the *Treatise*."

¹⁸⁹ *Ibid.*, 265

¹⁹⁰ *Ibid.*, 265

¹⁹¹ Hayek ([1932] 1995a, 175)

Indeed, at all the critical points, the assumption seems to creep in that this divergence is made possible by the necessary change in the supply of money.”¹⁹²

Hayek argues that while the failure to base the explanation in the *Treatise* on a theory of capital like the Austrian one, with its distinctions between different stages of production and varying degrees of specificity of capital goods, blinds Keynes to the effects of divergences between the Bank Rate and the equilibrium rate, it does not account for Keynes unsatisfactory explanation of such divergences, “This has to be sought elsewhere, viz.,...in Mr. Keynes’s peculiar concept of saving.”

[Keynes] believes that, in order to maintain equilibrium, new investment must be equal not only to that part of the money income of all individuals which *exceeds* what they spend on consumers’ goods *plus* what must be re-invested in order to maintain existing capital equipment (which would constitute saving in the ordinary sense of the word); but also to that portion of entrepreneur’s “normal” incomes by which their actual income (and, therefore, their expenditure on consumption goods) has fallen short of that “normal” income. In other words, if entrepreneurs are experiencing losses...and make up for such losses either by cutting down their own consumption *pari passu*, or by borrowing a corresponding amount from the savers, then, argues Mr. Keynes, not only do these sums make replacement of the old capital possible, but there should also be a further amount of *new* investment corresponding to these sums. And as Mr. Keynes obviously thinks that saving (i.e., the refraining from buying consumers’ goods) may, in

¹⁹² *Ibid.*

many cases, actually cause some entrepreneurs to suffer losses which will absorb some of the savings which would otherwise have gone to new investment, this special concept of saving probably explains why he suspects almost *any* increase in saving of being conducive to the creation of a dangerous excess of saving over investment.¹⁹³

If it is assumed that, following a fall in the relative demand for consumers' goods due to an increase of saving, rather than cutting back production, producers of consumers' goods continue to produce at capacity, then there are four possible ways for entrepreneurs to recoup their losses: "they must cut down their own expenditure (or, in Mr. Keynes's terminology, they must *save* in order to cover their losses); reduce their bank balances; borrow from the people who save; or sell to these people other capital, such as securities."¹⁹⁴ The relevant question is whether under such circumstances an excess of saving over investment will cause total cost of production

¹⁹³ *Ibid.*, 179

¹⁹⁴ *Ibid.*, 180

to exceed total incomes:¹⁹⁵ “The answer to this question seems to me to be an emphatic negative.”¹⁹⁶

This said, Hayek’s real objection is to the assumption that producers of consumers’ goods might fail to respond to the relevant price signals, which he attributes to Keynes’ neglect of the relation between changes in the rate of interest and concomitant effects on the structure of production, i.e., to Keynes’ failure to base the *Treatise* on a capital-theoretic foundation:

It seems to me that a complete neglect of the part played by [the] rate of interest is involved in the assumption that, after investment in the production of consumption goods has become relatively less profitable, some other openings for investment which are now more profitable will not be found. The most curious fact is that, from the outset, all of Mr. Keynes’s reasoning which aims at proving that an increase in saving will not lead to an increase in investment is based on the assumption that, in spite of the decrease in the

¹⁹⁵ Answering this question is complicated, Hayek argues, because

as I have repeatedly pointed out, [Keynes] has not provided us with a clear and unequivocal definition of what he means by “investment.” But, for the present purpose, we can surmount this difficulty by simply taking his account of what happens when investment falls short of saving and then investigating whether these effects manifest themselves in our particular case. Now, the effect of an excess of saving over investment, according to Mr. Keynes, will be that total incomes will not be sufficient to purchase total output at prices which cover costs (*Ibid*).

¹⁹⁶ *Ibid.*, 181

demand for consumption goods, the available output is not reduced; this means, simply, that he assumes from the outset what he wants to prove.¹⁹⁷

Hayek takes the foregoing considerations to establish that there is no reason, given Keynes' arguments in the *Treatise*, why saving and new investment (in Keynes's sense) should ever be in equilibrium.¹⁹⁸

Hayek considers Keynes' specific account of the credit cycle, a discussion that is greatly complicated, Hayek argues, by the fact that "when one tries to use all these concepts as tools for the purpose for which they were forged, all the difficulties which have been pointed out not only recur but increase. To show in detail how they affect the results would require a discussion many times longer than that contained in the respective sections of the *Treatise*."¹⁹⁹ Hayek's main complaint is that, in contrast to Wicksell and his followers, Keynes makes little attempt to follow up the effects on real investment of monetary disequilibrium: "What he is really interested in is merely shifts in the money-streams and the consequent changes in price levels."

It seems never to have occurred to him that the artificial stimulus to investment, which makes it exceed current saving, may cause a disequilibrium on the real structure of production which, sooner or later, must lead to a reaction. Like so many others who hold a purely monetary theory of the trade cycle...he seems to believe that, if the existing monetary

¹⁹⁷ *Ibid.*, 183

¹⁹⁸ *Ibid.*, 184

¹⁹⁹ *Ibid.*, 192

organisation did not make it impossible, the boom could be perpetuated by indefinite inflation.²⁰⁰

The main feature of the boom phase of the cycle is not, on Keynes' reckoning as it is on Hayek's, the increase in investment, but the increase in the prices of consumers' goods and the resulting profits received by producers of consumers' goods. This means, Hayek points out, that "Direct inflation for consumption purposes [i.e., the granting of credit directly to consumers] would, therefore, create a boom quite as effectively as would an excess of investment over saving." The latter is a position which, from Hayek's perspective, makes Keynes no better than those naïve underconsumptionists, Foster and Catchings.

Since, according to [Keynes'] theory, it is the excess of demand for consumers' goods over the costs of the available supply which constitutes the boom, this boom will last only so long as demand keeps ahead of supply and will end either when the demand ceases to increase or when the supply, stimulated by abnormal profits, catches up with demand. Then the prices of consumers' goods will fall back to costs and the boom will be at an end, though it need not necessarily be followed by a depression; yet, in practice, deflationary tendencies are usually set up which will reverse the process. This seems to me to be, in broad outline, Mr. Keynes's explanation of the cycle.²⁰¹

This explanation, Hayek argues, suffers from the same problems as other underconsumptionist accounts of the cycle, and he refers readers to "The 'Paradox'

²⁰⁰ *Ibid.*, 192-193

²⁰¹ *Ibid.*, 193

of Saving” and *Prices of Production* for elaborate and extended critiques of underconsumptionist accounts.

In closing, Hayek rejects Keynes’ argument that changes in the structure of production might be ignored in the short-run; this Hayek attributes, of course, to the distance separating his own understanding of capital from that of Keynes:

I am afraid this contention merely proves that Mr. Keynes has not yet fully realized that *any* change in the amount of capital per head of working population is equivalent to a change in the average length of the roundabout process of production and that, therefore, all his demonstrations of the change in the amount of capital during the cycle prove my point.²⁰²

The “next move” in the debate, as it turns out, was not made by Keynes himself, but by Keynes’ friend, Cambridge lecturer and Circus member, Piero Sraffa²⁰³ (one of the addressees of the February 1932 transmission in which Keynes expresses exasperation with his dialogue with Hayek) whose review of *Prices and Production* appears in the March 1932 edition of the *Economic Journal*.

²⁰² *Ibid.*, 195

²⁰³ Faced with persecution at the hands of Mussolini’s regime in his native Italy, and with Keynes’ always generous benefaction, Piero Sraffa was invited to Cambridge in 1927. Already well-known to Cambridge economists due to a local controversy raised by his 1926 critique of Marshall’s theory of the firm, Sraffa eventually fell in with the group of young economists known as the Cambridge Circus, whose numbers also included Richard Kahn, James Meade, and Joan and Austin Robinson, and whose criticism and encouragement proved essential to the efflorescence of Keynes’ economic thinking in *The General Theory*.

Sraffa's review pulls few punches. Thus it is that Hayek's book only adds "to the prevailing confusion of thought on the subject"²⁰⁴; its one contribution being that it focuses on the effects of monetary changes on relative prices rather than on the effects of such changes on the general level of prices. Sraffa's first complaint concerns Hayek's starting concept of "neutral" money, which is supposed to leave relative prices as they would be in an economy without money. Such a starting point might be recommendable, Sraffa says, if Hayek were to compare the results of the moneyless economy with those realized under various monetary systems and policies,

but the reader soon realises that Dr. Hayek completely forgets to deal with the task which he has set for himself, and that he is only concerned with the wholly different problem of proving that only one particular banking policy...succeeds in giving full effect to the voluntary "decisions of individuals", especially in regard to saving, whilst under any other policy these decisions are "distorted" by the "artificial" interference of banks."²⁰⁵

According to Sraffa, Hayek fails to appreciate that the desirability of the barter case can be questioned; having believed himself to have shown that the conditions of a "neutral" money correspond to those of a barter economy, Hayek blithely proceeds to the conclusion that the related policy is the only one worth considering.

Prices and Production ignores all of those features of money not connected with its use as a common medium of exchange; if he had stuck to his original purpose of

²⁰⁴ Sraffa [(1932) 1995, 198)

²⁰⁵ *Ibid.*, 199

comparing the effects of neutral money with alternative monetary systems, Sraffa argues, Hayek would have realized that the value of money lies also in its use as a store of value and the standard of debt settlement and “other legal obligations, habits, opinions, conventions.”²⁰⁶ According to Sraffa, Hayek’s approach,

which amounts to assuming away the very object of the inquiry, appears to originate in a well-founded objection to the vagueness of the conception of “the general price-level” understood as anything different from one out of many possible index numbers of prices, and in the opinion that such a conception can have no place in a theory of money.²⁰⁷

Sraffa has no objection to such an approach *per se* provided that money’s price is one of the relative prices considered, “but Dr. Hayek goes further and rejects not only the notion of a general price level but every notion of the value of money in any sense whatever.”²⁰⁸

Sraffa argues that Hayek’s reduction of money’s value to insignificance *should* have profound effects on his theory of the effects of monetary changes. In particular, having “emasculated” money, the results of Hayek’s assessments of the effects of different monetary systems should be that money is *always* neutral. Instead, according to Sraffa, Hayek manages to find insidious effects following from particular monetary systems. One can only conclude, Sraffa argues, that either Hayek has committed some major error of reasoning or has unintentionally introduced

²⁰⁶ *Ibid.*

²⁰⁷ *Ibid*, 200

²⁰⁸ *Ibid.*

some *ad hoc* assumption “which produces the difference attributed to the properties of the system itself.”^{209, 210}

Sraffa proceeds to consider the two cases of voluntary and forced saving, arguing that no ill effects arise in the latter case; Hayek “accepts the above conclusions as far as they go, and must now try to find in a different set of considerations the reasons why inflation has not the same effects as saving.”

The true difference between the two cases is, according to [Hayek], that the change in the structure of production brought about by saving is permanent, being due to the “voluntary decisions of individuals”; whereas the same change, if due to inflation, is “forced”, and therefore the consumers, as soon as inflation ceases and their freedom of action is restored, will proceed to consume all the capital accumulated against their will, and re-establish the initial position. That the position reached as the result of “voluntary saving” will be one of equilibrium...is clear enough...But equally stable would be that position if brought about by inflation; and Dr. Hayek fails to prove the contrary.²¹¹

²⁰⁹ *Ibid.*, 201

²¹⁰ Sraffa doesn't offer a specific critique of Hayek's theory of capital, noting merely that “Dr. Hayek as it were builds up a terrific steamhammer in order to crack a nut—and then fails to crack it,” and that the role played by the theory of capital in relation to the rest of the exposition is “little more than that of obscuring the main issue” (*Ibid.*, 201)

²¹¹ *Ibid.*, 203

Hayek argues that once consumers have regained their previous purchasing power following an expansion of credit in the form of producers' credits – that is, once all incomes have risen as a consequence of the injection of money at the far end of the structure of production – they will attempt to expand the volume of their consumption to its prior proportion, but, according to Sraffa, “nothing of the sort will happen. One class has, for a time robbed another class of a part of their incomes; and has saved the plunder. When the robbery comes to an end, it is clear that the victims cannot possibly consume the capital which is now well out of their reach.”²¹²

If Hayek were consistent, Sraffa argues, then, believing that he has shown producers' credits to cause serious harm, Hayek should conclude that consumers' credits cause little harm; instead Hayek argues that consumer inflation also leads to capital destruction: “Thus Dr. Hayek will have it both ways.”²¹³ Moreover, Hayek ignores two cases that appear immediately pertinent to his analysis; in particular, the case where inflation is granted in such a way as to leave undisturbed the proportion between the relative demand for producers' goods and that for consumers' goods, and the case in which banks increase loans to producers to the extent that they simultaneously decrease credit to consumers, leaving the effective circulation of money unchanged; the latter being a case in which, by Hayek's lights, money is “neutral” but the structure of production is altered.

²¹² *Ibid.*, 203-204

²¹³ *Ibid.*, 204

What has happened is simply that, since money has been thoroughly “neutralised” from the start, whether its quantity rises, falls, or is kept steady, makes not the slightest difference; at the same time, an extraneous element, in the shape of the supposed power of the banks to settle the way in which money is spent, has crept into the argument and has done all the work. As Voltaire says, you can kill a flock of sheep by incantations, plus a little poison.²¹⁴

Sraffa considers Hayek’s theory of the relation between the money and natural rates of interest and argues that, *contra* Hayek, the divergence between the two rates of interest is by no means a phenomenon unique to monetary economies:

If money did not exist, and loans were made in terms of all sorts of commodities, there would be a single rate that satisfies the conditions of equilibrium, but there might be at any one moment as many “natural” rates of interest as there are commodities, though they would not be “equilibrium” rates. The “arbitrary” action of the banks is by no means a necessary condition for the divergence; if loans were made in wheat and farmers (or for that matter the weather) “arbitrarily changed” the quantity of wheat produced, the actual rate of interest on loans in terms of wheat would diverge from the rate on other commodities and there would be no single equilibrium rate.²¹⁵

²¹⁴ *Ibid.*, 204-205

²¹⁵ *Ibid.*, 205

This same fact can be seen by considering futures markets where loans are made in terms of non-monetary commodities, like cotton:

In equilibrium the spot and forward price coincide, for cotton as for any other commodity; and all the “natural” or commodity rates are equal to one another. But if, for any reason, the supply and the demand for a commodity are not in equilibrium (i.e., its market price exceeds or falls short of its cost of production), its spot and forward prices diverge, and the “natural” rate of interest on that commodity diverges from the “natural” rates on other commodities.²¹⁶

None of this, Sraffa says, is intended as a criticism of Wicksell, for “there is a ‘natural’ rate of interest which, if adopted as a bank-rate, will stabilize a price-level (i.e., the price of a composite commodity): It is an average of the ‘natural’ rates of the commodities entering into the price-level, weighted in the same way as they are in the price-level itself.”²¹⁷ However, this route of escape is blocked for Hayek, who eschews the use of composite variables like average price-levels and average “natural” rates of interest.

The foregoing considerations establish, Sraffa argues, that non-monetary economies “retain the essential feature of money, the singleness of the standard; and

²¹⁶ *Ibid.*, 205-206

²¹⁷ *Ibid.*, 207; Sraffa continues, “What can be objected to Wicksell is that such a price-level is not unique, and for *any* composite commodity arbitrarily selected there is a corresponding rate that will equalise the purchasing power, in terms of that composite commodity, of the money saved and of the additional money borrowed for investment.”

we are not much the wiser when we have been shown that a monetary policy is ‘neutral’ in the sense of being equivalent to a non-monetary economy which differs from it almost only by name.” In any case, no policy can precisely replicate the results of “the other conceivable and more truly non-monetary” economies. But this is no cause for concern, Sraffa contends, because disequilibria would arise under a system of barter just as in monetary economies: “the essential characteristic of a divergence between the demand and supply of consumption goods is common to monetary and non-monetary economies.”²¹⁸

In the main, Sraffa’s complaints with *Prices and Production* are three in number. The first issue is that, in a book ostensibly dealing with monetary issues, Hayek doesn’t really address money; his focus is instead on changes in the income stream available for the purchase of producers’ rather than consumers’ goods and concomitant effects on the structure of production. However, the latter issue really just obscures another, more fundamental difference between the two: Hayek, but not Sraffa, takes, the assumptions of the prevailing theory of equilibrium for granted; Hayek, but not Sraffa, assumes that equilibrium obtains in a moneyless world, and this justifies the former’s choice of starting from the point where equilibrium theory leaves off. Of course, Hayek defends this premise at length in *Monetary Theory and the Trade Cycle*,²¹⁹ which was not available in English at the time of Sraffa’s review, but it is unlikely that the latter would have accepted Hayek’s argument there, for the root disagreement between the two concerns the validity of equilibrium theory itself.

²¹⁸ *Ibid.*, 207

²¹⁹ See pages 4-8 above.

Relatedly, Sraffa complains that Hayek's use of the equilibrium construct surreptitiously elides into a defense of a policy judged to secure what Hayek takes to be the conditions of a barter economy.

Secondly, and perhaps more importantly, Sraffa claims that Hayek's argument with respect to the divergent effects of voluntary as opposed to forced saving depends on a crucial and unproven assumption, namely, that consumers react differently in the two cases, leaving the capital created in virtue of saving untouched in the former case, but destroying the capital taken from them in the form of inflation in the latter case. According to Sraffa, there is no reason to believe this will happen as Hayek describes; consumers might opt to leave capital untouched in both cases. The significance of this objection is that the shrinkage of the structure of production that is alleged to follow the end of an inflation in the form of producers' credits (or the beginning of an inflation in the form of consumers' credits) is *no necessary consequence of Hayek's theory*. In the words of Hayek's biographer, Bruce Caldwell, "while the scenario painted by Hayek is a possible one, he neither demonstrated the necessity nor gave adequate attention to the lags implicit in the process of adjustment he portrayed. Hayek's theory fits some, but not all, trade cycles: It is not, as Hayek portrayed it to be, a general theory of the cycle."²²⁰

Sraffa's third objection contends that, in a barter economy, there is no unique "natural" rate of interest; that there might be as many natural rates as there are commodities and none of them "equilibrium" rates. Related to this objection is

²²⁰ Caldwell (1995, 39)

Sraffa's point that Hayek cannot resort to an average "natural" rate given his eschewal of the method of aggregates and composite indices.

As mentioned above, Sraffa's article appears in the March 1932 edition of the *Economic Journal*, edited at the time by Keynes. In the final dispatch in the 1931-1932 correspondence between Hayek and Keynes, dated March 29, 1932, Keynes obliges Hayek's request for space for a reply to Sraffa in the next edition of the *Journal*, and indicates both his unwillingness to return to their former controversy and that he has moved on to better things:

Having been much occupied in other directions, I have not yet studied your *Economica* article [i.e., the second part of Hayek's review of the *Treatise*] as closely as I shall. But, unless it be on one or two points which can perhaps be dealt with in isolation from the main issue, I doubt if I shall return to the charge in *Economica*. I am trying to re-shape and improve my central position, and that is probably a better way to spend one's time than in controversy.²²¹

Hayek's reply to Sraffa does indeed appear in the June 1932 edition of the *Economic Journal*. Hayek considers Sraffa's objections in turn, merely referring his critic to the then-forthcoming English translation of *Monetary Theory and the Trade Cycle* for a discussion of his perspective on the monetary issues raised by Sraffa and for a defense of the methodological choice to start from the conclusions of equilibrium theory. Of course, given that a root issue between the two concerns the validity of equilibrium theory, it should not be expected that Sraffa would have

²²¹ Keynes (1973, 266)

concurred with Hayek's argument in *Monetary Theory*. Recognizing this, Hayek argues that Sraffa's methodological position seems to be "a curious mixture of, on the one hand, an extreme theoretical nihilism which denies that existing theories of equilibrium provide any useful description of the non-monetary forces at work; and, on the other hand, of an ultraconservatism which resents any attempt to show that the differences between a monetary and a non-monetary economy are not only, and not even mainly," dependent on those characteristics of the value of money highlighted by Sraffa.²²²

With respect to Sraffa's allegation that Hayek illegitimately elides into a defense of the policy of neutral money, the latter responds that he merely assumes that "it is generally thought desirable to avert any developments which lead the system away from an equilibrium position, and which, therefore, make a revulsion inevitable sooner or later." However, beyond this, Hayek contends, "there is no justification for the suggestion that...my exposition takes certain aims of economic policy for granted—which I assume [in Sraffa's words] 'will be found desirable by every right-thinking person'."²²³

With respect to the charge of *ad hocness* leveled by Sraffa against the analysis in *Prices and Production*, Hayek concedes that his theory "stands or falls" upon the truth of the proposition that the capital accumulated in virtue of forced saving must be, "at least partly, dissipated as soon as the cause...disappears."²²⁴ Against the

²²² Hayek ([1932] 1995b, 211)

²²³ *Ibid.*, 211-212

²²⁴ *Ibid.*, 212

charge, Hayek contends that Sraffa has misunderstood the reasons why the consequences of forced saving are different from those of voluntary saving. He proceeds to an analysis of forced saving under the assumption that no new voluntary saving occurs so that the proportion between the demand for producers' goods and the demand for consumers' goods is "entirely determined by what is necessary to maintain the existing capital."²²⁵ Under such conditions, when the proportion that can be spent on capital falls in virtue of the end of a credit expansion in the form of producers' credits, and the incomes of the original factors of production have all risen in virtue of the previous inflation,

the proportion which [entrepreneurs] are able to spend on capital goods must fall. This means, however, not only that they must stop adding to the existing capital, but also that they will be unable to maintain and replace all the capital which is the product of the forced saving. Except in so far as they are able, and find it profitable, to make up for this at the expense of their own increased income, they will be able to replace their capital only at the same rate as before the forced saving took place, and their capital will, therefore, be gradually worn down to something approaching its former state.²²⁶

In short, Hayek argues, at least part of the capital created by forced saving will be destroyed once the inflation slows. It is important to note that this, in essence, is a concession of Sraffa's central point that Hayek has not proven that the capital

²²⁵ *Ibid.*, 214

²²⁶ *Ibid.*, 216

created by forced saving must soon or later be destroyed. “I have, of course,” Hayek indicates in a footnote, “never said, as Mr. Sraffa suggests...that the banks cannot cause *any* accumulation of capital.”²²⁷ Be that as it may, the implication is certainly present in *Prices and Production* that capital destruction due to the end of forced saving will be both severe and deleterious; it doesn’t sound nearly as scary to be told that such capital will be destroyed “at least partly.”²²⁸

In any case, with respect to Sraffa’s claim that Hayek’s analyses of the different effects of producers’ and consumers’ credits are inconsistent, Hayek argues that it is not a

contradiction to say that an inflation for productive purposes will cause little *permanent* increase of capital, while an inflation for consumptive purposes will actually cause a consumption of capital. The fact is simply this, that any increase of incomes paid for consumptive purposes relatively to the sums available for productive purposes, will tend to decrease the “purchasing power” of these sums (i.e., the purchasing power of money-capital); and that, whereas in the former case, where the relative rise of incomes follows only a preceding rise in the demand for capital goods, only part of the capital created by the inflation is destroyed again, in the latter case, the destruction of capital is not offset by any preceding gain.²²⁹

²²⁷ *Ibid.*, 217

²²⁸ *Ibid.*, 212

²²⁹ *Ibid.*, 217

With respect to Sraffa's criticism of his use of the concept of a money rate of interest which is different from the "equilibrium" rate, Hayek notes that it is "a concept which [his theory] has in common with the theories of a number of other contemporary writers."²³⁰ Otherwise, Hayek largely concedes the point that there might be many "natural" rates in a barter economy, perhaps one for every commodity; however, "I think it would be truer to say that, in this situation, there would be *no single rate* which, applied to all commodities, would satisfy the conditions of equilibrium rates, but there might, at any moment, be as many 'natural' rates of interest as there are commodities, *all of which would be equilibrium rates.*"²³¹ Otherwise, Hayek does not specifically address the point that he could only carry on with the "natural" rate construct if he were to accept the introduction of composite variables into his explanatory scheme.

Sraffa's brief rejoinder, which appears alongside Hayek's reply in the June 1932 edition of the *Economic Journal*, adds little to his previous complaints beyond the point, offered in response to Hayek's attempt to again establish the deleterious effects of forced saving, that, the new money injected into the economic system, far from eventually raising incomes across the structure of production and thereby contributing to a destruction of "at least part" of the capital created by the inflation, will – on Hayek's own assumptions – be completely absorbed in the cash holdings of the new stages of the extended structure of production. "Let me remind him," Sraffa writes,

²³⁰ *Ibid.*, 212

²³¹ *Ibid.*, 218

that he has assumed...that capital will be accumulated in proportion to the quantity of money issued in the form of loans to producers; that the number of stages of production will increase in proportion to the quantity of capital; that the quantity of payments to be made will increase in proportion to the number of stages: As a result, the quantity of payments to be made increases in proportion to the quantity of money, and the whole of the additional money is absorbed in cash holdings for performing such payments.²³²

Over the course of the next several years, the theory of *Prices and Production* became the focus of considerable criticism from other prominent economists—John Hicks, Frank Knight, and Gunnar Myrdal (with whom Hayek would eventually share the 1974 Nobel Prize) among them. Much of this criticism centered around Hayek’s theory of capital and the use to which it was put in his analysis of the trade cycle. Hayek’s reaction was to retrench and reconstruct the theory of capital upon more solid foundations than those he inherited from Bohm-Bawerk, Wicksell, and Mises. It would be an arduous task, and one with which the results of which Hayek was never satisfied. Moreover, the ultimate goal of integrating this new theory with the other required elements in a more comprehensive explanation of the trade cycle was never completed by Hayek. By the time that Hayek’s *Pure Theory of Capital*²³³ – in which he attempts to describe the processes of capital creation and destruction in more detail than had theretofore been attempted – appeared in 1941, the world was at war, and few paid attention. Moreover, by 1941, the economics profession had

²³² Sraffa ([1932] 1995b, 224)

²³³ Hayek ([1941] 2007)

started to come under the sway of Keynes' *General Theory*, and the long process of transition from earlier modes of thought known as the "Keynesian revolution" was under way.

5 Keynes' *General Theory of Employment, Interest, and Money* (1936)

The central thesis of Keynes' *General Theory* is that, in the absence of countervailing influences, the market economy tends to equilibrate at a level of less-than-full employment. Keynes defends this view partially by way of criticism of competing approaches, namely, the "classical," and, in a few places, the "neoclassical" schools of economics. Keynes identifies Hayek and economists influenced by Wicksell²³⁴ with the latter school and the economists of the British tradition, i.e., Mill, Marshall, and (especially) Keynes' Cambridge colleague Pigou with the former.

According to the classical school, Keynes argues, the forces of the free market tend to ensure equilibrium at a point of full employment; persistent *involuntary* unemployment is impossible.²³⁵ If (real) wages are too high and unemployment

²³⁴ Keynes offers little sustained criticism of Wicksellian economics in *The General Theory*; his objections more often take the form of critical digressions from the main argument. In Chapter 7, Keynes objects to the notion that saving and investment can diverge, thereby rejecting the possibility of forced saving (Keynes [1936] 1973, 81-85). Keynes denies the usefulness of the concept of the "natural rate of interest" (*Ibid.*, 243). He further rejects the Austrian theory of capital, with its conception of roundabout production processes (*Ibid.*, 213-217), as well as the related treatment of interest as the factor that coordinates intertemporal production and consumption decisions (*Ibid.*, 192-193).

²³⁵ It may be more accurate to say, "According to the arguments *Keynes attributes* to the classical school..." It is a long-standing objection to the *General Theory* that Keynes' arguments don't address the classical theory properly understood, but only a straw man. See, e.g., Schumpeter (1954, 1179):

appears, then production shifts to less labor-intensive methods, which raises the costs of the non-labor factors of production relative to the costs of labor, making labor-intensive methods of production comparatively more attractive, production shifts in the direction of these methods, and unemployment disappears. *Voilà!*

Keynes argues that the latter theory of employment is based on two fundamental postulates. The first postulate, which he accepts, determines the demand for labor and states that wages equal the marginal product of labor, i.e., that “the wage of an employed person is equal to the value which would be lost if employment were to be reduced by one unit.”²³⁶ The second postulate, which is supposed to determine the supply of labor, states that the real wage of a worker is equal to the marginal disutility of labor. This Keynes rejects for two reasons: he argues that labor is not typically withdrawn in the event of a general rise in prices (equivalent to a fall in real wages) so long as money wages remain unchanged²³⁷; further, Keynes argues, it is not true, as the classical school implicitly assumes, that the “wage bargain” between labor and entrepreneurs determines the level of real wages, i.e., it is false that workers are able to dictate their real wages by adjusting their demands for money wages.²³⁸

The arguments Keynes set forth against what he conceived to be the classical theory (in his sense) are entirely irrelevant against any correct statement of the full-employment equilibrium theory and his indictment that the classical theory knows no unemployment except a frictional one is true only if frictional is defined so widely as to rob the indictment of all significance.

²³⁶ *Ibid.*, 5

²³⁷ *Ibid.*, 8-10

²³⁸ *Ibid.*, 10-12

True enough, the theory of unemployment of the classical school, as Keynes describes it, leaves room for the appearance of both *frictional* unemployment, as workers move between jobs, and *voluntary* unemployment, as a consequence of workers who opt to remain jobless rather than accept the prevailing wage, but the classical image of the economy leaves no room for *involuntary* unemployment, i.e., in Keynes peculiar phrasing, unemployment that arises when, “in the event of a small rise in the price of wage-goods relatively to the money wage, both the aggregate supply of labour willing to work for the current money-wage and the aggregate demand for it at that wage would be greater than the existing volume of employment.”²³⁹ On Keynes, picture, in short, there is involuntary unemployment if a small price-induced reduction in real wages encourages both employers to demand more labor and workers to supply it; consequently, there is full employment at that level of output where this condition is absent. In a monetary economy, Keynes argues, there is no fundamental mechanism that ensures the market’s tendency toward equilibrium at a level of full employment; the market can and does equilibrate in the presence of involuntary unemployment.

Keynes’ explanation is directed toward the determination of the volume of employment. The theory itself – not to say the reasoning behind it – is simply stated in a number of places in the book.²⁴⁰ According to Keynes in *The General Theory*, given a particular state of resources, equipment, technology, and costs, the community’s income (Y) is a function of the volume of employment (N). The

²³⁹ *Ibid.*, 15

²⁴⁰ *Ibid.*, 27-29, Chapter 18

proportion of the community's income that it spends on consumption depends on a psychological factor – *the propensity to consume* (D_1 rewritten as $\chi(N)$); assuming the latter remains constant, consumption depends on the level of income, and so, on the volume of employment. The latter is determined by the expectations of entrepreneurs both with respect to the community's intended consumption (i.e., D_1) and their own plans for investment (D_2). The sum of the community's consumption and the entrepreneur's investment is the *effective demand* (D). In equilibrium, effective (or aggregate) demand equals aggregate supply (Φ): "Hence, the volume of employment in equilibrium depends on (i) the aggregate supply function, Φ , (ii) the propensity to consume, χ , and (iii) the volume of investment, D_2 . This is the essence of the General Theory of Employment."²⁴¹

For every level of employment, Keynes argues, there is a particular marginal productivity of labor. Given that the latter determines the real wage, it follows that the level of employment cannot be greater than the figure that reduces the real wage to the marginal disutility of labor, and thus, that "the supply of labor available at a given real wage sets a *maximum* level of employment."²⁴²

Perhaps the central assumption in Keynes' argument is that when employment increases, both consumption and effective demand increase, but the former less than the latter; that is, that the result of a rise in an individual's income tends to be that she consumes a smaller proportion of it: "The key to our practical problem is to be found in this psychological law."

²⁴¹ *Ibid.*, 28-29

²⁴² *Ibid.*, 30

For it follows from this that the greater the volume of employment the greater will be the gap between the aggregate supply price (Z) of the corresponding output and the sum (D_1) which the entrepreneurs can expect to get back out of the expenditure of consumers. Hence, if there is no change in the propensity to consume, employment cannot increase, unless at the same time D_2 is increasing so as to fill the increasing gap between Z and D_1 . Thus—except on the special assumptions of the classical theory according to which there is some force in operation which, when employment increases, always causes D_2 to increase sufficiently to fill the widening gap between Z and D_1 —the economic system may find itself in stable equilibrium with N at a level below full employment...If the propensity to consume and the rate of new investment result in a deficient effective demand, the actual level of employment will fall short of the supply of labour potentially available at the existing real wage, and the equilibrium real wage will be *greater* than the marginal disutility of the equilibrium level of employment.²⁴³

In a bit plainer language, the upshot of Keynes' theory is that involuntary unemployment arises when aggregate demand is insufficient to absorb the output available, i.e., when there is a "gap" between output and what is consumed, which leads to unsold goods and, thus, moving forward, less-than-potential output, and unnecessarily high unemployment.²⁴⁴

²⁴³ *Ibid.*, 30

²⁴⁴ Conversely, if aggregate demand overruns the output available – if there is demand for more goods than there are goods – price inflation will result.

On Keynes' picture, full employment requires that the level of investment be sufficient to absorb all of the unconsumed income paid out to wage-earners, i.e., D_2 must fill the gap between Z and D_1 . The level of investment, according to Keynes, is determined by the intersection of the schedule of the marginal efficiency of capital with the interest rate. The marginal efficiency of capital is a psychological factor determined by expectations of the yield of a particular capital asset relative to its replacement cost.²⁴⁵ The rate of interest,²⁴⁶ Keynes argues, is determined jointly by the quantity of money (which is taken to be an exogenous policy variable determined by the central bank) and the function of liquidity preference, which is another psychological factor determined by the public's preference to hold a particular proportion of the part of its income that it does not consume in liquid (i.e., cash and its equivalents) rather than interest-bearing assets. The extent to which investment is able to fill the gap between output and demand is thus determined by two endogenous factors – the marginal efficiency of capital and the community's liquidity preference – and one exogenous variable, the quantity of money. It follows that the route to full employment involves the effective manipulation of these factors.

Keynes' discussion of the trade cycle in *The General Theory*, such as it is, appears as a series of "Notes" in chapter 22, and, given the loose form of presentation, should likely not be taken to constitute a full-fledged explanation of the cycle so much as a series of hints suggested by Keynes' theory of employment. Here

²⁴⁵ *Ibid.*, 136

²⁴⁶ Keynes' second major criticism of the classical school concerns its theory of interest. See, esp. Chapter 14 of *The General Theory*, in which Keynes argues that the classical theory ignores both the dependence of investment on income and the simultaneous determination of the rate of interest and income (*Ibid.*, Chapter 14)

Keynes attributes the cycle primarily to fluctuations in the marginal efficiency of capital, “though complicated and often aggravated by associated changes in the other significant short-period variables of the economic system.”²⁴⁷ The basic idea is that as the public’s expectations with respect to the future – their “animal spirits”²⁴⁸ – wax and wane, so too do expectations with respect to the future yields of particular capital assets relative to their replacement costs, i.e., the marginal efficiency of capital, and, as this factor fluctuates, so too does the capacity for investment to close the breach between output and consumption. *Ceteris paribus*, pessimistic (optimistic) public spirit means a fall (rise) in the marginal efficiency of capital, a concomitant fall (rise) in investment, and the widening (closing) of the output gap.

When consumption falls short of output, thereby indicating less-than-full investment, there are a number of steps that the government can take to stimulate the respective factors influencing investment. If less money is spent than is necessary to sell all of the goods available, then either the central bank can increase the quantity of money in circulation, thereby lowering interest rates and encouraging income-earners to save less and consume more (and, conversely, encouraging entrepreneurs to invest more), or the government can spend directly on fiscal projects, i.e., “public works.”

The effects of these stimulus measures, as postulated by Keynes, are twofold. In the first place, by stimulating demand, they tend to raise prices, especially those of consumption goods. (Recall that Keynes definition is such that involuntary

²⁴⁷ *Ibid.*, 313

²⁴⁸ *Ibid.*, 161-163

unemployment disappears if, assuming no fall in money wages, real wages are brought down by an increase in the prices of consumption goods or “wage-goods,” in Keynes’ verbiage.) In the second place, they tend to directly bring about more employment, and in subsequent periods, as the newly employed spend their incomes, yet more employment. According to Keynes, such government “investment” translates into increased income for labor, which workers spend (in part) on consumption, thereby stimulating further production, more income, further spending, and so on. In short, the initial round of government stimulus sets in motion a series of events, the net effect of which is a total increase in economic activity that is posited to be a multiple, greater than unity, of the original stimulus amount. This is the fundamental idea behind Keynes’ *investment multiplier*, a concept borrowed from Circus member Richard Kahn and subsequently modified to fit Keynes’ unique approach, and defined as a function of the community’s marginal propensity to consume.

With respect to the objectives and methods of government administration of the economy relative to the trade cycle, Keynes argues that

the remedy for the boom is not a higher rate of interest but a lower rate of interest! For that may enable the so-called boom to last. The right remedy for the trade cycle is not to be found in abolishing booms and thus keeping us permanently in a semi-slump; but in abolishing slumps and thus keeping us permanently in a quasi-boom.”²⁴⁹

²⁴⁹ *Ibid.*, 322

And later, he adds, “The State will have to exercise a guiding influence on the propensity to consume partly through its scheme of taxation, partly by fixing the rate of interest, and partly, perhaps, in other ways.”²⁵⁰

For whatever reason – and he offered many different and not entirely consistent reasons over the subsequent decades – Hayek’s pen fell silent in the wake of the publication of *The General Theory*.²⁵¹ There would be no scathing disassembling by Hayek of Keynes’ latest book in the pages of *Economica*; and the heavyweight bout between the two principals would feature no rematch.

6 Concluding Remarks: The Terms of the Debate

The goal of this chapter has been to uncover the differences between Hayek and Keynes that inform their famous debate over business cycle theory. We first considered Hayek’s early writings on the business cycle, which provide the basis of his attack on Keynes’ *Treatise*. In turn, we considered the theory of the latter book, Hayek’s criticism of it, and Keynes’ response to this criticism. We also considered the objections of Cambridge Circus member Piero Sraffa to Hayek’s *Prices and Production*, as well as Keynes’ masterpiece *The General Theory of Employment, Interest, and Money*. Now is the time to take inventory.

Once one gets past the verbal quibbles and manifest confusion, Hayek’s objections to the theory of the *Treatise* — add up to three main points, all of which are echoes of previous criticisms that Hayek offered against earlier

²⁵⁰ *Ibid.*, 378

²⁵¹ Caldwell (1995, 40-45) considers the various explanations Hayek offered over the years for what he himself described as “shirking” of “what should have been a plain duty” (Hayek [1963] 1995, 60) to review *The General Theory*.

underconsumptionist theories of industrial fluctuations. In the first place, Hayek is opposed to any attempt to explain the activities of monetary economies without resort to a capital-theoretic basis. Capital theory, he admits, is not a finished product and is not, in its given state, entirely adequate to the task, but that is no justification for ignoring capital's role entirely; its role in monetary economics is too essential. Moreover, the shortcomings of the existing capital theory concern its failure to adequately capture the fundamental heterogeneity of capital phenomena. Thus, the next step in capital theory is likely to involve a shift away from the relatively simple account of the Austrian theory and toward a more complex explanation. Hayek ultimately objects to both Keynes' reticence to confront these problems directly and, relatedly, his choice to treat capital-goods as homogenous.²⁵²

Secondly, Hayek rejects any explanation of the presence of unused resources that assumes the phenomenon it intends to explain. The only legitimate way to explain the appearance of unemployment is to start from a point where it is absent; this essentially reduces to a requirement that such an explanation start from a position of economic equilibrium with full employment.

Finally, Hayek rejects any explanation that runs in terms of composite and aggregate variables, especially, the notion of the "general level of prices"; this method obscures the phenomenon – the relations between particular prices – which Hayek argues are central to an understanding of industrial fluctuations; economic aggregates and composites possess no causal efficacy, thus, whatever correlations might be established between them using the methods of statistics must be spurious.

²⁵² See footnote 174 above

All of this said, it is important to recognize that the latter two complaints are images of the first. Hayek's insistence on an equilibrium framework is a consequence of his belief that appending a theory of capital, including a proper explanation of the rate of interest, to equilibrium theory, is essential for an explanation of saving and investment in a money-using economy. Moreover, Hayek's notion of equilibrium must be understood in an intertemporal sense, i.e., equilibrium obtains when a sustainable proportion exists between the demand for producers' goods relative to the demand for consumers' goods, and this picture of equilibrium is nonsensical without Austrian capital theory according to which production is a time-consuming process guided largely by changes in the interest rate. Similarly, Hayek's rejection of the price-level notion is a consequence of his belief that the effects of monetary changes upon relative prices are the interesting phenomena, where the relevant price relations are those of the intermediate products of successive stages of the structure of production. Thus, Hayek's subsidiary complaints are really just reflections of the primordial difference separating his approach from Keynes', namely, the emphasis on capital's role – more precisely, changes in the structure of production – in explaining industrial fluctuations.

Of course, as we have seen, Hayek's insistence on the importance of capital theory failed to impress Keynes. Ideally, yes, Keynes argues, monetary theory should be built upon a capital-theoretic foundation, but extant capital theory is unfinished and inadequate; in any case, Keynes argues, capital's role is not all that essential to the really interesting problems anyway. What's more, as Sraffa's review of *Prices and*

Production shows, Hayek's capital theory is flawed and may not do all of the work that Hayek claims.

It is little surprise then, that, rather than convincing Keynes, Hayek's criticisms seem to have had almost precisely the opposite effect. Indeed, with respect to all of Hayek's main criticisms, Keynes moves, in *The General Theory*, further away from Hayek's position: there is less discussion of capital theory in the latter book than in the *Treatise*; moreover, far from starting from a position of equilibrium with full employment, *The General Theory* assumes that unemployment is the normal state of the economy and seeks the factors that account for changes in its level; related to this, as much as his previous book, Keynes' *General Theory* runs in terms of aggregates (the *levels* of "employment," "output," "consumption," "investment," "government expenditure," etc) and composites (Hayek's dreaded "price-level").

Thus, it seems that the Hayek-Keynes debate amounts to a disagreement about the need to explain fluctuations in industrial activity on the basis of capital theory. Hicks' question – "Which was right?" – reduces to the question whether it is better to proceed on the basis of an inadequate capital theory or none at all. However, while this may do justice to the details of the debate between Hayek and Keynes, it says little about the broader context of the dispute and the respective purposes served by the theories of the principals. For it is absolutely essential to keep in mind just what Hayek's approach – with its emphasis on the structure of production and the coordinating role of the rate of interest – allows him to show, namely, that saving and not consumption is the ultimate driver of economic growth, and that it is only when consumption decisions are allowed to get out of gear with

saving decisions that negative consequences follow; a point, it must be said, which is hard to make without some framework like that provided by the theory of capital Hayek inherited from his Austrian predecessors. Conversely, by largely ignoring the role of capital and focusing on the relations between aggregates and composites, Keynes is able to show that consumption drives economic activity and that increased saving is deleterious; a point which – again, it must be said – would be hard to prove on the basis of Austrian capital theory. So it is that understanding the terms of the Hayek-Keynes debate – the subject of subsequent business cycle theorists’ “footnotes” – requires not merely appreciating their respective arguments concerning particular assumptions, but also recognizing where these assumptions lead, namely, to fundamentally different conceptions of the operation of market economies.

The next issue to consider concerns whether consensus is possible with respect to Hicks’ question, and thus, more broadly, with respect to these competing visions of the economy, and, in particular, the extent to which the choice between the theories of Hayek and Keynes is – indeed, *can be* – guided by empirical evidence. It is to this problem that we now turn.

CHAPTER 2

UNDERDETERMINATION AND THE HAYEK-KEYNES DEBATE

The goal of the present chapter is to articulate the various respects in which the choice between the theories of Hayek and Keynes²⁵³ is underdetermined by the empirical evidence. This requires specifying the observable implications of each theory, i.e., what the evidence would look like were one or the other theory right (or wrong). It also requires uncovering any strategies that are available to protect each theory from what appears to be falsifying evidence. Only if there is some bit of possible evidence that favors one theory rather than the other is there any reason to think that the Hayek-Keynes debate is empirically tractable.

The present chapter considers these matters from the perspective of the current (Summer 2012) global, but primarily American, economic context. It may seem queer in a paper that addresses philosophical issues in economic science to deal directly with the passing events of the real world; traditional philosophers don't typically get their hands mucked in *terra firma*. But such is the course of recent economic history that it provides a convenient and familiar perspective from which to explicate the relevant theoretical issues.

Moreover, it has been suggested that the so-called "Great Recession" amounts to a natural-experimental test of the theses of the Hayek-Keynes debate.²⁵⁴ However, if the arguments of the present chapter are sound, it is absurd to believe that there could ever be a test the empirical results of which would favor one and not

²⁵³ We are specifically concerned with Keynes' *General Theory* in the present chapter rather than the theory of the *Treatise*.

²⁵⁴ See, e.g., Murphy (2008), Krugman (2011), and Murphy (2011)

the other; and this not so much, as the reader might expect, because of the manifest difficulties of interpreting the results of natural experiments, but because there are lacunae in both theoretical frameworks that make it convenient to pardon any threatening evidence. The choice between the two theories is underdetermined not (only) because of the idiosyncrasies of natural experiments, but because the theories of Hayek and Keynes leave the predictive possibilities wide open.

1 Keynes' Theory and Holist Underdetermination

What does Keynes' theory imply about the events of the Great Recession? A simple explanation according to *The General Theory* might go something like this: the ongoing recession²⁵⁵ is an indication of a shortfall in aggregate demand: sometime in or around 2005, a gap arose between output and the total sum spent by consumers, entrepreneurs, and government. The ultimate source of the gap could lie in any combination of changes in the expenditures of the latter three groups; however, Keynes', in his discussion of the trade cycle in *The General Theory*, suggests that the ultimate cause of recessions typically resides in a fall in the schedule of the marginal efficiency of capital, which discourages private investment, and thereby creates a gap between output and aggregate demand. Moreover, the economy has not returned to full employment in the years since 2005 because the gap has never been properly closed. The total sum spent by consumers, investors, and government continues to fall short of output.

²⁵⁵ Were he alive today, it is entirely likely that Keynes would have considered an economy with an 8.2% unemployment rate (June 2012), given the historical trend of American unemployment, to be in recession; arbitrary statistical definitions of recessions be damned.

What's more, since, according to *The General Theory*, there is no natural mechanism that ensures that output and aggregate demand reach equilibrium at a level of full employment, the output gap is likely to remain until and unless the respective psychological considerations that determine the marginal propensity to consume and the marginal efficiency of capital spontaneously turn cheerful; more likely though, the government will have to endeavor to close the gap either by the adoption of monetary measures that encourage a sanguine turn in the psychological factors relevant to consumption and investment or by its own expenditure on fiscal projects. Simply put, an appropriate combination of fiscal and monetary policies, prudently designed, implemented, and administered, can serve to fill the gap between output and aggregate demand.

We can summarize the observable implications of Keynes' theory in terms of what the evidence would look like if the theory is right (and what it would look like if it is wrong). If *The General Theory* is correct, then full employment will be obtained – i.e., the gap between output and aggregate demand will be closed – if and only if the combined application of fiscal and monetary stimulus is both *sufficiently large* and *adequately designed, implemented, and administered*. More generally, we might say that Keynes' theory predicts that the level of involuntary unemployment that results from any political effort to reduce it is determined by the extent of the *size* (in monetary terms) and *adequacy* of the political means adopted. Of course, conversely, if this prediction is false, then Keynes' theory is incorrect.

But the question that immediately arises, given our purposes in the present essay, is: can this implication be tested against the empirical evidence? It is certainly

not the case that the implementation of just any old stimulus program constitutes a test of the implications of Keynes' theory. Even if we otherwise accept Keynes' account, we should expect only those political measures to lower the level of involuntary unemployment that are both large enough to do so and not spoiled by inadequacies of either design, implementation, or administration²⁵⁶—but, which are *these?*

Consider the illustration provided by certain recent economic events. One might be tempted to think that the observable implications of Keynes' theory have recently been tested and failed. The American government has passed two fiscal stimulus packages since the beginning of the financial crisis in 2008: one during the waning days of the Bush administration and another in the first year of the Obama administration; moreover, the Federal Reserve has maintained an easy money policy intended to encourage consumption and investment since the end of the housing bubble in 2005—aren't these just the sorts of policies that Keynes advocates in *The General Theory* as a means of reducing involuntary unemployment and doesn't the fact that these policies do not seem to have substantially positively affected the level of unemployment *prove* that Keynes was wrong? What, if anything, is problematic about the inference from the failure of these policies to contribute to full(er) employment to the failure of Keynes' theory?

²⁵⁶ Testing Keynes' theory in practice might require knowledge of circumstances other than just knowledge of the adequacy of both the magnitude and bureaucratic aspects of a stimulus program. For example, it is not immediately obvious that Keynes' definition of involuntary unemployment (see page 118 above) distinguishes it from voluntary unemployment sufficiently for the purposes of practical testing.

One problem with this inference is a particular consequence of a general and infamous methodological predicament: the implications of *The General Theory* do not confront experience in isolation. What meets experience are the implications of Keynes' theory *plus* particular stimulus policies (and numerous implicit subsidiary assumptions). If a stimulus measure fails to produce the results postulated by Keynes, the failure need not be ascribed to the theory, but can be attributed to inadequacies in these other elements – typically, to flaws in the particular program enacted – instead. In other words, if a particular stimulus program fails to lower the level of involuntary unemployment, it can be claimed without inconsistency that either *a larger or better designed, implemented, and administered stimulus program would have worked* where the actual one failed.

It may seem reasonable to believe that we could approach a consensus if only we could put a finer point on the twin notions of both what makes a stimulus measure *large enough* and what counts as the *proper design, implementation, and administration* of such a program. In other words, if *The General Theory* implied something substantive about what constitutes both a sufficiently large quantity of economic stimulus and a properly designed, implemented, and administered stimulus program, then we might be in a better position to test Keynes' theory. If Keynes' theory implied, e.g., that a government stimulus package of $\$X$ implemented using methods *A*, *B*, and *C* will lower the level of involuntary unemployment to *Y*, then the failure of such a prediction would seem, at least at first glance, to provide evidence of the falsity of Keynes' theory. We could then implement an economic stimulus of $\$X$ with methods *A*, *B*, and *C* and see whether the level of involuntary

unemployment falls to Y . However, it is important to recognize that, even if we possessed such a precisified implication, we would still not be compelled by threatening evidence to ascribe the failure of such an implication to Keynes' theory: what counts as both a sufficiently large economic stimulus program and a properly, designed, implemented, and administered one is unspecified by Keynes; nothing specific with respect to the required size and / or appropriate political instruments of economic stimulus is implied by *The General Theory*. Thus, there is no stimulus program the apparent failure of which compels the rejection of Keynes' theory.

In order to see this, it helps to consider certain aspects of *The General Theory* in more detail. Recall that the concept of the investment multiplier is supposed to do the work with respect to the required size of a stimulus package. That is, according to Keynes, government stimulus sets in motion a series of events, the net effect of which is a total increase in economic activity that is a greater-than-unity multiple of the original stimulus amount. In order to enact a stimulus program that is large enough to narrow a particular output gap, we need to know both the extent of the shortfall in aggregate demand and the value of the multiplier. Keynes argues in *The General Theory* that the value of the multiplier is a function of the relevant community's marginal propensity to consume (MPC). Numerous statistical investigations have been made to determine the value of the MPC at various places and times. However, whatever the scientific value of these statistical inquiries, they are ultimately independent of Keynes' theory of the investment multiplier. The failure of a stimulus program predicated on a statistical analysis of some community's MPC does not necessarily reflect poorly on Keynes: such failure might be attributed

instead to shortcomings in either the relevant data used to estimate MPC or the particular statistical methods applied to them.²⁵⁷

In other words, if we statistically estimate the value of the multiplier in a particular community and otherwise properly design, implement, and administer a stimulus program on the basis of this estimate, and the posited results fail to materialize, it can always be argued that this is due not to shortcomings in Keynes' theory of the multiplier, but to the particular estimate of its value in the given

²⁵⁷ According to Keynes' most prominent mid-century American defender, Alvin Hansen, the apparent failure of the multiplier to produce the results posited by Keynes might also be attributed to "leakages":

Among the most important of these leakages are the following: (1) a part of the increment of income is used to pay off debts; (2) a part is saved in the form of idle bank deposits; (3) a part is invested in securities purchased from others, who in turn fail to spend the proceeds; (4) a part is spent on imports, which does not help home employment; (5) a part of the purchases is supplied from excess stocks of consumers' goods, which may not be replaced. By reason of leakages of this sort, the employment process peters out after awhile. (Hansen 1953, 89-90)

Hansen also lists other "offsetting factors which may nullify (or intensify) the original impetus" from an increment of additional investment:

The method of financing the public works may raise the rate of interest and so retard private investment...An increase in public works might raise the cost of capital goods and so affect private investment unfavorably. In addition, the government program might affect 'confidence' unfavourably and so curtail investment. Also, public capital expenditures in an open economy might create a demand for foreign materials and equipment and so help employment abroad rather than at home. (*Ibid.*, 104)

Hansen concludes that these offsetting factors "must all be taken into account in appraising the net effect of a given increment of public or private investment" (*Ibid.*). However, taking such factors into account requires some guidance on how to identify their offsetting influence, which neither Keynes nor Hansen provides.

context.²⁵⁸ Indeed, it is worth noting that Keynes himself appears to do precisely this in *The General Theory*:

It should not be difficult to compile a chart of the marginal propensity to consume at each stage of a trade cycle from the statistics (if they were available) of aggregate income and aggregate investment at successive dates. At present, however, our statistics are not accurate enough (or compiled sufficiently with this specific object in view) to allow us to infer more than highly approximate estimates. The best for the purpose, of which I am aware, are Mr. [Simon] Kuznets figures for the United States...though they are, nevertheless, very precarious. Taken in conjunction with estimates of national income these suggest, for what they are worth, both a lower figure and a more stable figure for the investment multiplier *than I should have*

²⁵⁸ I have yet to mention Keynes' concept of the *liquidity trap*, a notion that further confounds the evaluation of the implications of *The General Theory*. Keynes argues that a situation can arise in which monetary policy is powerless to stimulate the economy. Such a circumstance arises in Keynes' framework when the demand for money becomes infinitely elastic (i.e., the money demand curve becomes horizontal), an indication that expansion of the money supply will not lower the interest rate and, therefore, will not serve to stimulate spending and investment. However, the shape and position of the money demand curve is not directly observable; without a statement of some observable conditions that constitute a liquidity trap, we have no way of identifying one. It doesn't suffice to assert that a liquidity trap exists if and only if a particular monetary injection fails to lower the interest rate: the same consequence would follow if an expanded money supply were to be compensated by a spontaneous commensurate rightward shift in a downward-sloping money demand function. And for the same reasons mentioned in the text immediately above, statistical estimates of a particular money demand function cannot compel a decision with respect to the presence or absence of a liquidity trap: given an estimate of a downward-sloping money demand function, the subsequent failure of monetary stimulus to lower the interest rate might be ascribed to shortcomings in the particular estimate; and the same can be said for the conjunction of an estimate of a horizontal demand function and a successful monetary stimulus. There is no evidence that compels the rejection of the liquidity trap concept.

expected. If single years are taken in isolation, the results look rather wild. But if they are grouped in pairs, the multiplier seems to have been less than 3 and probably fairly stable in the neighbourhood of 2.5. This suggests a marginal propensity to consume not exceeding 60 to 70 percent—a figure quite plausible for the boom, but surprisingly, and, in my judgment, *implausibly low for the slump*. It is possible, however, that the extreme conservatism of corporate finance in the United States, even during the slump, may account for it. In other words, if, when investment is falling heavily through a failure to undertake repairs and replacements, financial provision is made, nevertheless, in respect of such wastage, the effect is to prevent the rise in the marginal propensity to consume which would have occurred otherwise. I suspect that this factor may have played a significant part in aggravating the degree of the recent slump in the United States. On the other hand, it is possible that *the statistics somewhat overstate the decline in investment...* a moderate change in these estimates being capable of making a substantial difference to the multiplier.²⁵⁹ [Italics added]

Parsing this passage is a challenge: Keynes begins by arguing that the available statistics aren't accurate enough to infer more than approximate estimates of the relevant values (MPC, and consequently, the multiplier), but then he proceeds to infer rather precise estimates from "precarious" data. The estimates that accord with his theoretical hunches are deemed "quite plausible," while others are "improbably low." With respect to the latter, Keynes offers two possible explanations for the

²⁵⁹ Keynes ([1936] 1973, 128)

apparent failure of his theory to accord with the data: either American corporate practices are to blame or the statistics are wrong. Indeed, every possibility is considered but that the theory is wrong.

At the same time, what Keynes says about the various methods of stimulus *design, implementation, and administration* does not suffice to license the more precisified implication stated above. In *The General Theory*, Keynes argues that monetary methods alone are unlikely to suffice to close an output gap, and thus, that fiscal methods are likely required as well; however, he offers no specific guidance with respect to adequate modes of design, implementation, and administration of stimulus programs.²⁶⁰ There is an infinite variety of such modes and there is no reason to think that they are all equally effective at lowering the level of involuntary unemployment. Though Keynes sometimes speaks in *The General Theory* as though *any* method will have some positive effect on unemployment, his rather off-the-cuff comments in this regard should probably not be taken too seriously.²⁶¹ One can easily imagine a stimulus program the effectiveness of which we'd have every reason to doubt in advance. Secretly scuttling a treasure ship carrying billions of dollars in booty in the Marianas Trench in the hope that consumers and investors will discover the treasure and put it to work is probably a bad way to stimulate the economy; one could not legitimately infer the failure of Keynes' theory on the basis of the failure of such a stimulus program.

²⁶⁰ "It would need a volume of a different character from this one to indicate even in outline the practical measures in which [Keynes' policy recommendations] might be gradually clothed" (*Ibid.*, 383).

²⁶¹See, e.g., *Ibid.*, 129

Thus, if we take Keynes at his word when he appears to assert that *any* stimulus measure whatever will have some positive effect on the level of unemployment, then he – and the theory that supports such a claim – is simply wrong. However, best philosophical practices require that we interpret Keynes more charitably than this. Unfortunately for Keynes, because he provides little further comment from which one might infer criteria of the adequate design, implementation, and administration of a stimulus program, there is no way to know whether a particular policy measure constitutes a natural-experimental test of Keynes' theory.

In essence, because of these twin lacunae in Keynes' theory, one cannot infer the failure of Keynes' theory on the basis of the apparent failure of *any stimulus program*. It can always be claimed without inconsistency that another stimulus policy would have worked where some actual one appeared to fail. One cannot infer from Keynes' comments in *The General Theory* either what it means for a stimulus package to be sufficiently large or specific details of legitimate modes of stimulus distribution. Thus, though we are free to qualify the implications of Keynes' theory to make them more precise in the way suggested above, any such qualifications must come from *outside Keynes' theory*. This means that the apparent failure of such an implication need not threaten Keynes. The defense can always be offered in the face of an apparent predictive failure that the auxiliary elements of the prediction went awry and that, since these elements form no part of Keynes' theory, the latter is unscathed by the seemingly threatening evidence.

In short, it is not Keynes' theory that gets falsified by the evidence, but only constellations that include both Keynes' theory and beliefs about the effects of particular stimulus policies, and should any such "web of belief" be threatened by the available evidence, one can get to an unthreatened constellation by making the necessary adjustments, not to Keynes' theory, but to beliefs about the effects of particular stimulus policies. Thus, the rational response to an apparent disconfirmation of Keynes' theory is underdetermined in the holist sense.

2 Hayek's Theory and Holist Underdetermination

According to Hayek's picture of the business cycle, the Great Recession is a consequence of a divergence between the rate at which loans can be had and the equilibrium rate that would balance the volume of voluntary savings with that of investment. This divergence, which likely first arose during the initial years of the current century, especially after September 11, 2001, when the American Federal Reserve loosened monetary terms,²⁶² encouraged excessive investments in industries – particularly in residential and commercial building construction – at the long end of the structure of production, which led to a boom in these industries. However, because the rate of interest at which the relevant loans were made was not indicative of the demand that would be available for the products of these industries once they reached market – that is, because, once their incomes increased in virtue of the credit granted to producers, consumers attempted to reassert their prior demands for consumers' goods – these investments were ultimately revealed to be unprofitable,

²⁶² It could also be argued, with some reason, that the Federal Reserve's easy credit stance has its source in actions taken as far back as 1994 to stem the effects of the Mexican peso crisis.

many were forced to either shut down or cutback production as a consequence, and the bust arrived.

Moreover, full employment (i.e., equilibrium in Hayek's sense) has not returned because, rather than allowing the structure of production to realign to the voluntary decisions of savers and investors, the central bank has continued to hold interest rates low in the hopes of stimulating the economy in a Keynesian fashion. Of course, according to Hayek's theory, such credit expansion during a recession only prevents the realignment of the structure of production necessary for equilibrium; at worst, such measures contribute to further misalignments.

Again, we can summarize the observable implications of Hayek's theory in terms of what the evidence would look like if the theory is right (and what the evidence would look like if it is wrong). If Hayek's theory is correct, then, if the other assumptions of equilibrium theory obtain, keeping the interest rate on loans equal to the natural rate of interest is both necessary and sufficient to ensure that no boom-bust cycle is ever set in motion; conversely, if either the assumptions of equilibrium theory do not hold or loans are made at a rate of interest below the natural rate, then *within some unspecified timeframe* an artificial and unsustainable lengthening of the structure of production occurs, which *within some unspecified timeframe* is reversed in the form of a bust, "crisis," what have you; in other words, an artificial lengthening of the structure of production leads in the short-run to what appear to be positive effects, which are reversed in the long-run: boom is followed by bust. On the other hand, if Hayek's theory is wrong, then either the boom-bust cycle is a feature even of those economies in which both the conditions of equilibrium theory hold and the

loan rate is set equal to the natural rate, or the trade cycle does not manifest where both of the latter conditions do not obtain.

Of course, the difficulties in evaluating Hayek's theory begin at the beginning, so to speak. As Hayek himself notes,²⁶³ the natural rate of interest is not observable in economic systems that feature fractional-reserve banking. Banks (central or otherwise) can never know whether their activities constitute credit expansion; they never know in fact whether the current loan rate is below, above, or at par with the rate that would obtain in a possible world, distinct from our own, in which everything is as it is in this world except that banks hold full reserves and the conditions of equilibrium theory obtain. It cannot be known whether the current rate of interest on loans is encouraging an artificial lengthening or shortening of, or is neutral with respect to, the structure of production. In short, it's not possible to decide on the basis of the observable evidence the truth of the biconditional "the trade cycle appears if and only if the structure of production is artificially lengthened."

Now, this latter difficulty can be set aside in principle. On the assumption that the natural rate remains relatively stable over time,²⁶⁴ it is a consequence of Hayek's theory that, other things equal, the extent of encouragement given to more roundabout production processes varies inversely with the level of the loan rate. So, if it can be shown that changes in the loan rate correlate with the severity of the

²⁶³ See page 60 above

²⁶⁴ Such an assumption would not be appropriate where those factors are operative that influence the natural rate of interest in one direction or another, e.g., changes in risks, technology, new discoveries of natural resources, etc.

effects of the business cycle,²⁶⁵ one might acquire some evidence that Hayek's theory is right in the absence of direct evidence of the value of the natural rate of interest. Or, to put the point another way, one might acquire evidence that Hayek was wrong if it can be shown that no such correlation exists. Thus, in principle, the non-observability of the natural rate does not prevent the evaluation of Hayek's theory.

But this is not to say that the implications of Hayek's theory can be compared with the empirical evidence in practice. The problem with Hayek's theory vis-à-vis appraisal stems from the fact that it provides zero guidance with respect to *when*, following a lowering of the loan rate, one can expect to observe the effects of the boom-bust cycle. Evaluating the truth of the posited inverse correlation mentioned above requires knowledge of the relevant relata; these should be economic conditions at different points in time, but which points in time? The posited inverse correlation states, in essence, that *the extent to which the loan rate is lowered at time t inversely correlates with the severity of the business cycle at time t + x*; however, Hayek never indicates the value (or range of values) of *x*. A boom fostered by lower interest rates could turn to bust tomorrow, a year from now, or a decade from now. Until the bust arrives, Hayek's theory will keep predicting "the bust is coming," and when the bust does materialize (which, of course, with a long enough time horizon, it eventually must), it can be said that "Hayek's theory predicted it!" But, logically, this is little different from the meteorological sage who predicts rain every day: eventually he's bound to be right, but that doesn't redound predictive excellence upon the theory from which he infers his daily prediction. For our purposes, what

²⁶⁵ That is, on the assumption that there is some meaningful way of determining the *severity* of the effects of forced savings, which is probably a false assumption.

this means is that there is no observable evidence which compels the rejection of Hayek's theory. Any evidence that appears to threaten the implications of the theory can always be excused on the grounds that insufficient time has passed for evaluation.²⁶⁶

We could, again, attempt to avoid this problem by making the implications of Hayek's theory more precise. However, it is a common theme of Hayek's methodological writings that specific economic events, like the turning point from boom to bust, are impossible to predict in advance with any satisfactory degree of precision. Given the highly complex phenomena that they investigate, economists can make only "pattern predictions" to which temporal parameters cannot be adduced. So, whereas Keynes' theory fails to provide the relevant advice on how to make predictions more precise, Hayek's methodological views augur against the possibility of making precise predictions of complex phenomena. If we add temporal parameters to the implications of Hayek's theory, not only are we then *not* in any better position to appraise the theory (because, in a fashion similar to our attempts to precisify the implications of Keynes' theory, such parameters are no consequence of Hayek's theory, so predictive failure could always be attributed to shortcomings with

²⁶⁶ It should be mentioned that a parallel problem arises for Keynes. We're told by Keynes that certain consequences follow the implementation of appropriate policies in the short-run and others in the long-run (see, e.g., Keynes [1936] 1973, 306-309), but we're never told *when* the short-run ends and the long-run begins, which makes it convenient for Keynes' defenders to excuse any seeming predictive failure with the claim that insufficient time has passed for the evaluation of the respective prediction: given more time, the evidence will show that Keynes was right. Of course, it goes without saying that Keynes' famous quip about us all being dead in the long run provides no sufficient criterion for distinguishing the short- and long-runs. Hansen apparently considers the "relatively short-run" capable of lasting "one, two, or three decades" (Hansen 1953, 33).

our temporal estimates rather than inadequacies in Hayek's account of the business cycle), but we're actually doing something that Hayek argues is impossible to do well.

In summary, it is not Hayek's theory that gets falsified by the evidence, but only constellations of beliefs that include both Hayek's theory *plus* beliefs about how much time must pass for the effects of particular policies to materialize, and should any such constellation be threatened by the observed evidence, one can move to an unthreatened constellation by making the necessary adjustments, not to Hayek's theory, but to beliefs about how much time must pass for the effects of particular policies to materialize. Thus, the rational response to an apparent disconfirmation of Hayek's theory is underdetermined in the holist sense.

3 A Brief Aside on Pattern Predictions and Underdetermination

In his (later) methodological work, Hayek argues that economic phenomena are fundamentally *complex* in a way that the phenomena of many natural sciences are not, and that, as a consequence, our capacity for theorizing about such phenomena is circumscribed. In particular, with respect to complex phenomena like those of the trade cycle, we are never able to achieve the epistemic position required to derive predictions of particular events: "A theory will always define only a kind (or class) of patterns, and the particular manifestation of the pattern to be expected will depend on the particular circumstances (the 'initial and marginal conditions' to which...we shall refer to as 'data'). How much in fact we shall be able to predict will depend on how many of those data we can ascertain."²⁶⁷ Hayek argues that

²⁶⁷ Hayek ([1964] 1967, 3)

The multiplicity of even the minimum number of distinct elements required to produce (and therefore also of the minimum number of data required to explain) a complex phenomena of a certain kind creates problems which dominate the disciplines concerned with such phenomena and gives them an appearance very different from that of those concerned with simpler phenomena. The chief difficulty in the former becomes one in fact of ascertaining all the data determining a particular manifestation of the phenomena in question, a difficulty which is often insurmountable in practice and sometimes even an absolute one.²⁶⁸

Hayek's point here is that in order to derive a prediction of a particular event, we need to marry statements of laws to statements of the pertinent data – the relevant 'initial and marginal conditions'. However, as the phenomena under investigation grow increasingly complex, the number of relevant data increase in number, and consequently, our ability to discover and base predictions on them diminishes.

Nonetheless, predictions of a different sort are possible with respect to complex phenomena like those of economics. We cannot to any particular degree of satisfaction predict individual economic events, but we can make what Hayek calls *pattern predictions*; moreover, such predictions are, according to Hayek, falsifiable:

We are...interested not only in individual events, and it is also not only predictions of individual events which can be empirically tested. We are equally interested in the occurrence of abstract patterns as such, and the prediction that a pattern of a certain kind will appear in defined

²⁶⁸ *Ibid.*, 7-8

circumstances is a falsifiable (and therefore empirical) statement. Knowledge of the conditions in which a pattern of a certain kind will appear, and of what depends on its preservation, may be of great practical importance. The circumstances or conditions in which the pattern described by the theory will appear are defined by the range of values which may be inserted for the variables of the formula.²⁶⁹

Given the infamous problems associated with the names Duhem and Quine, Hayek's claim that pattern predictions are falsifiable is rather doubtful. Hayek's arguments with respect to the distinction between predictions of individual events and pattern predictions gives no hint as to why only the former and not the latter might be subject to the problems of underdetermination. Indeed, given that he was active in philosophy of science at the time that Quine's 1951 paper "Two Dogmas of Empiricism" reopened the profession's eyes to the problems of underdetermination, there is a shocking absence of any awareness of the Duhem-Quine problems in Hayek's philosophical *oeuvre*. True enough, Hayek does recognize that pattern predictions possess less empirical content (in Popper's sense) than predictions of particular events,²⁷⁰ but he never acknowledges that the problems of

²⁶⁹ *Ibid.*, 9; According to Hayek, the difference between predictions of particular events and pattern predictions is one of degree, not of kind. We might imagine a spectrum of predictability determined by the extent of the data that we possess: on one end of the spectrum are those cases where we possess sufficient data to generate specific predictions; as we move away from this end, our knowledge of the relevant data progressively decreases, and thus, our ability to predict with precision decreases as well. Naturally, at the opposite end of this spectrum are those cases in which we are completely ignorant of the relevant data, and all our knowledge is of the pattern predicted by the given theory.

²⁷⁰ *Ibid.*, 9

underdetermination effectively annihilate the possibility of testing isolated predictions of particular events, and thus, *a fortiori* make pattern predictions unfalsifiable as well. Evaluating a pattern prediction, i.e., determining whether an observed pattern of events fits within the range of events to be expected on the basis of some theory – is just as subject to interpretation (probably more so) than the question of whether the observation of an individual event conforms to a precise prediction.

Indeed, it seems that we perform something like²⁷¹ tests of pattern predictions in the real world and, when we do, the results are as open to interpretation as predictions of particular events. Consider that, when someone, even an economist, asserts that, e.g., Keynes' theory "explains" the Great Depression, they typically don't mean that one can literally deduce statements concerning specific events of the Depression from the statements of Keynes' theory plus statements of relevant subsidiary assumptions and initial conditions. What they mean is that the general pattern of economic activity of the Depression conforms to a broad pattern predicted by Keynes' theory. But, of course, non-Keynesians can and do offer alternative interpretations of the same pattern that support their view.²⁷² Conversely, Hayek's supporters usually count the pattern of economic activity observed during the stagflation of the 1970s as a vindication of Hayek's position. But, Keynes' theory has never been rejected once and for all as a consequence of stagflation; Keynes'

²⁷¹ I say "something like" because of considerations discussed below.

²⁷² Amity Shlaes (2007) bestseller *The Forgotten Man* gives a quasi-Austrian gloss to the Depression. The canonical Austrian accounts of the Great Depression are Robbins (1934) and Rothbard (2008). See also Garrett (2007) and Phillips, McManus, and Nelson (2007)

followers manage to interpret the pattern of economic events of the 1970s in a way that does not refute Keynes.

It is no less the case with respect to pattern predictions as it is with respect to predictions of individual events that they never confront experience in isolation. Hayek may be right that, with respect to complex phenomena, we typically do not possess all of the statements of conditions necessary to derive a prediction of an individual event, but two points are important to keep in mind here: 1) even if we don't know all of the conditions, we presumably do know some of them, and the failure of a pattern prediction so derived can always be ascribed to inadequacies with these subsidiary statements, and 2) *the mere fact* that we don't possess all of the statements of conditions necessary to derive a prediction of an individual event itself *constitutes a means of excusing predictive failure* that is not available with respect to predictions of individual events, i.e., we can adduce the *absence* of the relevant data as an explanation of some apparent failure of a pattern prediction. It would seem, then, that pattern predictions are no more falsifiable, and probably less so, than their more precisely specified brethren.²⁷³

In any case, it is important to emphasize that the foregoing discussion of pattern predictions and their (lack of) falsifiability is relevant in the present context not because we can only hope to get pattern predictions from the theories of Hayek and Keynes, but because the implications of the latter two theories *don't even rise to the*

²⁷³ Hayek comes close to recognizing this fact, but never draws what seems to be the appropriate conclusion about the falsifiability of pattern predictions: "Such a theory will, of course, in Popper's terms, be one of small empirical content, because it enables us to predict or explain only certain general features of a situation which may be compatible with a great many particular circumstances" (Hayek [1964] 1967, 9).

level of pattern predictions. According to Hayek, a pattern prediction is derived from a theory from which we could derive a prediction of a specific event if only we possessed all of the relevant data: that is, a pattern prediction is separated from a prediction of a particular event merely by an ocean of unknown data. However, as the discussion of the present chapter has hopefully made clear, these data are not all that we lack with respect to the business cycle theories of Hayek and Keynes; there is some theoretical lacuna in each theory that further separates us from the possibility of predicting specific economic events. With respect to Keynes's theory, we lack criteria of bureaucratic best practice, i.e., of principles of adequate policy design, implementation, and administration. If we cannot determine whether a particular stimulus measure falls within the boundaries of what counts as best practice – and we do not get any such criteria from Keynes – then we cannot generate a prediction (pattern or otherwise) the falseness of which necessarily reflects poorly on *The General Theory*. Similarly, with respect to Hayek's theory, we are separated from predictions of particular economic events by more than just data, we also need a theory of the timing of the effects posited by Hayek; predictions of particular events require temporal parameters that we are methodologically debarred from appending to the implications of Hayek's theory.

Of course, Hayek's argument that economics is limited to pattern predictions implies that there is no way to non-arbitrarily assign temporal parameters. Thus, the failure of his theory to make temporally parameterized predictions is very much in keeping with his methodology. Moreover, it is no criticism of Keynes to point out that he failed to provide a theory of how to design, implement, and administer the

policies he recommended: it is by no means obvious that the phenomena of bureaucratic best practice fall under the economist's purview; indeed, it's not obvious that these latter phenomena are the business of *any* established academic discipline. So, in neither case should the failure of the theories to generate even pattern predictions be blamed on their respective authors, who each seem to have done the best they could with the tools they thought relevant to their *economic* analyses—it's just that generating even pattern predictions from either of the theories requires going beyond economics proper.

In any case, the failure of the implications of the theories to rise to the level of pattern predictions is absolutely essential to recognize for anyone interested in achieving consensus with respect to the answer to Hicks' question "Which was right? Keynes or Hayek?" Recall that, directly after claiming that pattern predictions are falsifiable, Hayek states that "The circumstances or conditions in which the pattern described by the theory will appear are defined by the range of values which may be inserted for the variables of the formula."²⁷⁴ What I'm arguing here is that there are variables of each theory that are not so defined; there are variables for which we are not given even a *range* of acceptable values. Thus, the implications of both theories are separated from predictions of particular events by more than a mere ocean of unknown data. As serious a problem as the collection of the relevant data may be in economics – and I don't deny that it is a serious problem – overcoming this obstacle will not suffice to generate testable predictions of either theory. Whether or not we are willing to go along with Hayek with regard to the falsifiability of pattern

²⁷⁴ Hayek ([1964] 1967, 9); see pages 146-147 above.

predictions, the epistemological problems of the two theories are more pronounced than those of more robust theories of other complex phenomena. A theory that generates pattern predictions is distinguished from a theory that generates predictions of specific events by the cognitive availability of the data required to fill in its parameters. A theory that fails to generate even pattern predictions is distinguished from a theory that does by the absence in the former of all of the relevant parameters – such theories are “gappy” in respects essential to empirical evaluation – and it’s this that I’m trying to establish with respect to the theories of Hayek and Keynes.

All of this said, it would appear that Hayek missed an opportunity to use his later methodology to attack Keynes’ theory. If the foregoing arguments are sound, then according to the criteria of *science* that prevailed in Hayek’s day, namely, falsifiability, the macroeconomics of *The General Theory* fails the test. Of course, such an argument would have required that Hayek cannibalize his own theory of the business cycle, which he may have been loath to do. But, in the political world of Hayek’s day, where policy was most highly esteemed where it was administered according to “scientific” principles, surely the argument that Keynes’ theory did not qualify in this respect would have been a powerful one. That he *should* have taken up the suggested argument against Keynes and rejected his own account of the cycle in the process is supported by the fact that the argument appears to justify, better than any technical-economic argument probably could, one of Hayek’s ultimate concerns, namely, *the need for caution in the political administration of monetary affairs*. If the argument is sound, then it means that we have no empirically-grounded reason to accept

Keynes' posited correlation between stimulus policies and involuntary unemployment; indeed, we have no empirical reason to believe that such measures do not cause more harm than good. We might then imagine Hayek unfurling this argument in defense of a sort of Hippocratic Oath for monetary policymakers: *caution is required where we are ignorant of the effects of our political machinations, as we always are with respect monetary policy.*

4 Contrastive Underdetermination and Short-run Evidence

I take it to be established at this point that the choice between the theories of Hayek and Keynes is underdetermined in the holist sense, i.e., that there are convenient responses available that protect each theory from what appears to be threatening evidence, or, what is the same thing, that there is no possible empirical evidence that compels a univocal choice between the two theories. But, what of the two theories with respect to the form of underdetermination distinguished above as “contrastive”?

Kyle Stanford describes the distinction between holist and contrastive underdetermination in the following way: “It is perhaps most useful to think of holist underdetermination as starting from a particular theory or body of beliefs and claiming that our revision of those beliefs in response to new evidence may be underdetermined, while contrastive underdetermination instead starts from a given body of evidence and claims that more than one theory may be well-supported by that very evidence.”²⁷⁵ I’m inclined to think that the distinction between contrastive and holist underdetermination can be too hastily overdrawn. As Stanford writes, “the

²⁷⁵ Stanford (2009)

two problems are not *entirely* disconnected, because it is open to us to consider alternative possible modifications of the web of beliefs as alternative theories or theoretical ‘systems’ between which the empirical evidence alone is powerless to decide.”²⁷⁶ In other words, there’s little substantive difference between saying that, given some theories, the rational response to new evidence is underdetermined and saying that, given some pieces of evidence, the choice between theories is underdetermined. Nonetheless, the distinction seems to be useful for our purposes in the present essay.

The sense in which the choice between the two theories is contrastively underdetermined is well-represented by an alternative explanation that Keynes’ defenders have offered for what might otherwise be interpreted as the failure of recent stimulus measures to significantly positively affect unemployment figures. Where Keynesians have not argued that the seeming failure is due to inadequacies in the particular stimulus policies adopted, they have argued instead that the programs enacted – whatever their imperfections – have done much to prevent the further deterioration of economic conditions in the United States, i.e., they claim, in essence that involuntary unemployment is lower than it would have been in the absence of the stimulus policies in question. Whether this assertion is true is not our concern; what does need emphasis is that it does nothing to facilitate the evaluation of Keynes’ and Hayek’s respective theories. That is, even if we all agree that the evidence supports the claim that the policies have had some stimulatory effects, we cannot infer from this evidence that Keynes was right and Hayek wrong. This is

²⁷⁶ *Ibid.*, see footnote 5 above

because it is a consequence of *both* theories that fiscal and monetary policies can manifest stimulatory effect in the short-run. Evidence of such short-run stimulatory effects (as well as evidence of the *absence* of such effects) underdetermines the choice between the theories of Keynes and Hayek. Whether it is true or not that the stimulus programs enacted since the start of the Great Recession have had some positive effects on economic growth, both theories make essentially the same claims with respect to these near-term effects, and so, both theories rise or fall together on this point. More carefully, given some body of evidence with regard to the short-run effects of particular policies, both theories are equally supported (threatened) by the evidence. Thus, any given body of short-run evidence contrastively underdetermines the choice between Keynes and Hayek.²⁷⁷

²⁷⁷ Consider further that Keynes' political program is not one of complete socialization: there remains a vital role for private enterprise to play in Keynes' ideal political economy: "I do not suppose that the classical medicine will work by itself or that we can depend on it. We need quicker and less painful aids...But in the long run these expedients will work better and we shall need them less, if the classical medicine is also at work. And if we reject the medicine from our systems altogether, we may just drift from expedient to expedient and never get really fit again" (Keynes 1946). However, given this, and moreover, given the enormous difficulties involved in pulling apart causal connections in macroeconomics, it may be that the claim that the success (or failure) of some stimulus policy is due to political factors is more or less equally supported by *any* evidence as the claim that the success (or failure) of the same program is due to elements of private enterprise. In a modern, complex economy in which private and public factors comeingle, a rational decision whether some evidence better supports claims about the causal influence of public rather than private factors is well nigh impossible to make, and could be made only on the dubious assumption that it is possible to non-arbitrarily distinguish public activity from private: for example, how should we classify the economic influence of an ostensibly private but heavily regulated industry which, for better or worse, would be run very differently in the absence of such regulation? It doesn't seem likely that we can either quantitatively or qualitatively determine the causal impact of the private and public spheres. Thus, those cries that are so often heard during difficult economic times to the effect that "capitalism (government) is the source of all of our

Of course, where Keynes and Hayek differ is in the long-run implications of their respective theories with regard to such policies. Keynes claims that stimulus policies – properly tended and maintained, of course – can lead to full employment, the end of boom and bust, while a consequence of Hayek’s theory is that the stimulatory effects of such policies are always transitory: they will be (at least partially) reversed when the artificial nature of the lengthening of the structure of production is discovered (whenever this might occur). And, of course, for reasons already mentioned under the heading of holist underdetermination, this distinction in the long-run consequences of the two theories *plus* evidence of the long-run consequences of particular stimulus policies doesn’t compel a unique choice between them.

economic woes” can probably never be any more justified than one’s brute political biases.

CHAPTER 3

CHOOSING BETWEEN UNDERDETERMINED THEORIES

The present chapter considers the Hayek-Keynes debate from the perspective of various alternate strategies in the literature for dealing with the consequences of underdetermination. In the first section, we address whether Dan Hausman's criteria for belief in vaguely-qualified generalizations might be fruitfully applied to the debate, and, in the second section, we consider the relevance for the two theories of Larry Laudan's argument that choice in the face of underdetermination proceeds on the basis of inductive methodological criteria.

1 Belief in Vaguely-Qualified Generalizations

The theories of Hayek and Keynes are each underdetermined in the holist sense in part because their implications are only vaguely specified. That is, the implications of both theories are meant to hold only under certain conditions, which are not all explicated. This makes it convenient to excuse apparent predictive failure with the claim that the underspecified conditions that must be present in order for the respective theory to work do not in fact obtain. In essence, the implications of both theories can be understood to include implicit clauses that specify the conditions in which they can be expected to work (or not work); however, in both cases, the components of the clauses are left understated.

In *The Inexact and Separate Science of Economics*, his well-known work on the methodology of microeconomics, Dan Hausman identifies two ways in which the generalizations of economics are qualified. There are those qualifications that could, at least in principle, be populated with elements that economic theory itself identifies

as causal factors capable of confounding the generalization: “[T]he meaning and justification of ‘laws’ with only such qualifications is relatively unproblematic. Provided that one takes for granted fundamental economic theory, the term ‘*ceteris paribus*’ can be replaced with a list of specific causal factors, the effects of which are considered separately.”²⁷⁸ On the other hand, there are qualifications that could only be populated with causes of economic phenomena that are not identified by fundamental economic theory.²⁷⁹ The qualifications with which we are concerned with respect to the theories of Hayek and Keynes are of this latter kind. In the case of each theory, we need to know something that “fundamental” economic theory cannot provide: it is not the business of economics to grade specific modes of policy design, implementation, and administration, nor can economic theory (which, at its most basic, is static) tell us with any satisfactory degree of precision how much time must pass for specific political modes to reveal their effects.

Hausman argues that belief in vaguely-qualified generalizations is justified only if four conditions are met. The relevant question for our purposes is whether these criteria can be fruitfully applied to the debate between Hayek and Keynes—does application of Hausman’s criteria support belief in one but not the other theory?²⁸⁰ According to Hausman,

²⁷⁸ Hausman (1992, 134)

²⁷⁹ “Fundamental economic theory considers only some of the causes of economic phenomena. The basic claims of economics are true only under various not fully specified conditions.” (*Ibid.*)

²⁸⁰ Hausman’s book is specifically concerned with *microeconomics*, not business cycle theory. However, his discussion in the relevant passages of the book is directed at

One is justified in regarding a counterfactual claim with a vague antecedent or a statement with a vague *ceteris paribus* clause as a law only when four necessary conditions (lawlikeness, reliability, refinability, and excusability) are met: First, the statement must be *lawlike*. It must be the sort of statement which would be a law if it were true...Second, the *ceteris paribus* law must be *reliable*. In some class of cases, after ignoring the *ceteris paribus* clause or allowing for *specific* interferences, the scientist should rarely need to explain away apparent disconfirmations. Reliability is a statistical requirement. A generalization such as “*ceteris paribus* all *F*'s are *G*'s” is reliable only if (perhaps after making allowances for specific interferences) almost all *F*'s are *G*'s. The evidence for reliability will typically be sample frequencies...Third, one does not have good reason to regard a qualified claim as a law unless it is *refinable*. If scientists add specific qualifications, the generalization (stripped of its *ceteris paribus* clause) should become more reliable or reliable in a larger domain...Finally, no one is justified in regarding a statement with a *ceteris paribus* clause as a law unless it is *excusable*. One should not invoke the *ceteris paribus* clause blindly. One should know which interferences are important and should usually be able to justify relying on the *ceteris paribus* clause as an excuse. The excusability condition differs from both the reliability and refinability conditions, because it does not demand good statistical results. Unlike the refinability condition it is also unconcerned with modifying generalizations. Instead, the excusability condition demands that, after

vague qualifications in general and not those of microeconomics alone, so exporting Hausman's criteria to business cycle theory is not inappropriate.

scientists have done their tests and identified those cases in which the generalization is not reliable, they be able to cite the interfering factors except possibly in a few anomalous cases. It should not seem a miracle that the generalization “works” sometimes and fails others.²⁸¹

Hausman’s requirements are fine as far as they go; unfortunately for our interests here, they don’t go very far with respect to resolving the underdetermination problems present in the Hayek-Keynes debate. Consider the reliability requirement first.²⁸² Whether either theory satisfies this condition cannot be determined on the basis of the evidence: one or both theories might in fact be reliable, but for the same reasons that the evidence doesn’t compel a decision one way or another with respect to the question “Which was right, Keynes or Hayek?” it similarly fails to compel a decision with respect to the question “Which is more reliable, Keynes or Hayek?” In particular, the interpretative problems that arise in evaluating the truth of the implications of the respective theories are in no way alleviated by the requirement that they be *almost* always true. Given that there is no evidence that a defender of either theory is logically compelled to accept as a falsification, one can simply dig in and claim that her favored theory is more reliable. In other words, in order for statistical reliability to serve as a criterion for choosing

²⁸¹ *Ibid.*, 140-141

²⁸² Hausman’s first condition – *lawlikeness* – is both the most difficult of the conditions to define and the easiest to set aside. Hausman offers no specific account of lawlikeness, and there are numerous, competing accounts in the literature. However, no one on either side of the Hayek-Keynes debate has asserted of the implications of the rival theory that they are not sufficiently lawlike to count as laws if true. One thing that both sides accept is the lawlikeness of the implications of the rival theory. Thus, there’s no need to consider the lawlikeness condition.

between the two theories, there must be agreement about what the relevant statistics reveal, but given the holist underdetermination of the two theories, there is no such agreement. We might say then that whether either theory is in fact reliable, *in practice* the question is epistemically intractable in much the same way and for the same reasons that the question “Which was right, Keynes or Hayek?” is epistemically intractable.

This same epistemic problem also confounds the fourth condition, excusability. The latter requires that economists be able to identify cases in which the vague generalization is not reliable. An economist’s use of a vaguely-qualified generalization is excusable only if (“except possibly in a few anomalous cases”) she can cite the relevant interfering factors. However, discovering these interfering factors requires tests for reliability, the results of which are epistemically intractable. In other words, the evidence that one acquires by testing the vague implications of the two theories is not going to compel agreement with respect to the cases in which the implications are not reliable: different economists will generate different lists of interfering factors depending on their judgments of whether the relevant theory is reliable in a particular case; there will be no consensus with respect to the excusability condition.

With respect to refinability, we’re told by Hausman that it is related to reliability: a vaguely-qualified generalization is refinable only if adding specific qualifications makes the resulting precisified generalization more reliable or reliable in a larger domain. Setting aside the just mentioned epistemic difficulties involved in determining the reliability of the implications of the respective theories, the question

arises whether it is possible to make the required refinements in a way that might settle the debate. It is true that we can add specific qualifications to the vague implications of both theories, but, as has been emphasized above, such precisifications are always auxiliary to the respective theories, which leaves available the convenient excuse that the failure of either theory to become more reliable in virtue of such refinements is due not to the shortcomings of the respective theory, but to inadequacies in the auxiliary refinements. We might say then, in a way that accords with economic practice, that each theory is only *exogenously* refinable, but that a minimum requirement for consensus is *endogenous* refinability, a condition which, in principle, neither theory can satisfy.

2 The Failure of the Appeal to Ampliative Criteria

So far it has been argued that the choice between the theories of Keynes and Hayek is underdetermined in both the holist and contrastive senses and that there are convenient strategies that can be adopted to protect either theory from what appears to be threatening evidence. However, nothing has been said about the rationality (or lack thereof) of adopting such protective measures. In part, this is due to the fact that the expressed concern of the present paper is with the absence of consensus with respect to the two theories, and for this purpose, the mere convenience of these excuses is probably sufficient to explain their adoption. Regardless of the *rationality* of adopting such strategies, their convenience makes their adoption highly alluring *psychologically*.

In his well-known paper *Demystifying Underdetermination*, Larry Laudan argues that certain philosophers and sociologists of science move too hastily from the

acknowledged truth of deductive (or “Humean”) underdetermination to a defense of epistemic relativism.²⁸³ Laudan argues that deductive underdetermination – i.e., the acknowledged fact that a correct explanation of some body of evidence cannot be deduced from the evidence alone – does not support the stronger claims concerning underdetermination that W.V.O. Quine (and, especially, certain of his immediate followers) inferred from Quine’s arguments in *Two Dogmas of Empiricism*.²⁸⁴ Much of Laudan’s paper is concerned with the proper interpretation of the arguments of *Two Dogmas*. Laudan argues that the only acceptable interpretation of the Quinean claim that any theory may be retained “come what may” is a psychological one, i.e., that it is always psychologically possible to hold on to a theory in the face of threatening evidence. However, Laudan argues that deductive underdetermination does not support the logically stronger (relativistic) claim that it is always *rational* to retain a theory in the face of recalcitrant evidence and, moreover, that one cannot draw the latter conclusion from the psychological premise without a further and, he argues, doubtful premise. According to Laudan, the transition from the psychological possibility of retaining a theory in the face of threatening evidence to the relativistic conclusion requires the truth of the claim that there are no non-deductive rules that might determine theory choice. However, Laudan argues that even in the face of the truth of both Humean underdetermination and the psychological possibility of retaining a seemingly falsified theory, there remains the possibility of rationally choosing between underdetermined theories on the basis of “ampliative” rules of

²⁸³ Laudan (1990)

²⁸⁴ Quine (1951)

theory choice. In short, according to Laudan, though it might be reasonable to hold that the resources of deductive logic alone do not suffice to pick out a uniquely rational response to threatening evidence, it is not reasonable to believe that a deductive logic augmented with ampliative rules also fails to determine theory choice; the addition of inductive principles of scientific reasoning like *simplicity*, *conservatism*, *fecundity*, etc. suffices to determine the rational choice between multiple deductively-underdetermined theories.

It is important to note that such an illegitimate elision from the psychological premise to the relativistic conclusion is not what is going on in the present paper. We have assumed that the convenience of the relevant strategies makes them appealing, and so, probably explains the lack of a consensus with regard to Hicks question; however we have pointedly refrained from attaching the adjective “rational” to the choices of economists between the theories of Hayek and Keynes. This said, the purpose of the present section is to establish that Laudan’s appeal to ampliative criteria provides no route to consensus with respect to Hicks’ question. Laudan’s argument is powerless to convince those attached to either theory to acquiesce in favor of the other for it is not the case that the application of inductive methodological rules leaves one theory as the uniquely rational choice between the pair.

The difficulty with Laudan’s suggestion in the context of the Hayek-Keynes debate arises mainly from the fact that those who prefer Hayek’s theory emphasize the importance of certain ampliative rules that are mostly discounted by those who prefer Keynes’ theory and *vice versa*. For example, consider the apparent tension

between the rule that relatively *simpler* theories (on some definition of theoretical simplicity) are to be preferred and the rule that, within certain boundaries in part determined by the need for simplicity, theories should be based on plausibly *realistic* assumptions. The application of these rules, by even the most unbiased of observers, to the theories of Hayek and Keynes is likely to lead to divergent assessments.

As we have seen, Hayek criticized Keynes' for failing to deal with the complexities of capital, and Keynes responded that his concerns were too pressing to wait for an adequate theoretical analysis of these complexities. Regardless of the level of realism of Hayek's own treatment of capital, there is little doubt that he was right about the difficulties of capital theory and that confronting the role of capital in a money-using economy would have involved Keynes in many unpleasant entanglements—even Keynes seemed to accept as much. Indeed, Keynes seemed also to accept that the addition of a capital-theoretic foundation could only improve the realism of his account: recall his comment that “a development of [capital] theory would be highly relevant to my treatment of monetary matters and likely to throw light on dark corners.”²⁸⁵ It seems that whatever one thinks of Hayek's capital theory, his choice to confront the difficulties involved in theorizing about the phenomena of capital represents a choice in favor of (what he took to be) realism; while Keynes' decision to set capital aside must count as a choice in favor of simplicity. Thus, those who de-emphasize simplicity in favor of realism (as Austrians typically do) are likely

²⁸⁵ Keynes (1973, 252); Keynes said this of the *Treatise*, but given that the role of capital is no more considered in *The General Theory* than in the previous book, unless there were a reason to think Keynes changed his mind about the realism that capital theory would bring to his account, it is relevant to a discussion of *The General Theory* as well.

to prefer Hayek's theory, while those who think that the realism rule is less important than the simplicity rule (as Keynesians tend to do) are apt to choose Keynes' theory. How do we settle this dispute between realists and simplicitists? It seems that the appeal to these ampliative rules leaves us exactly where we started: in the absence of empirical evidence that unambiguously supports the more realistic theory in favor of the simpler one, or the other way around, how are we to convince those who prefer relatively simpler theories to agree with those who prefer relatively more realistic ones?

Examples of such tensions in the Hayek-Keynes debate can be iterated. Consider the fact that Hayek's theory is so complex that its failure to convince more economists is often attributed to its complexity, and especially, to the fact that it is seemingly impossible to express mathematically. Keynes' theory, on the other hand, was formulated in mathematical terms almost immediately upon its initial public airing. In fact, it was our very own Sir John Hicks who made his name in part by mathematizing Keynes' theory in the form of the famous IS-LM model.²⁸⁶ We might be inclined to think this shortcoming of Hayek's theory to be a fatal one; however, Austrian economists generally, and few more than Hayek, are well known for denying that mathematics has anything but a heuristic role to play in economic theorizing. From the Austrian perspective, the inability to formulate Hayek's theory mathematically is no real demerit, and – in part because Austrians hold that an economic theory can typically be mathematized only if much of its realism is sacrificed – the ease with which Keynes' theory is expressed mathematically does not

²⁸⁶ Hicks (1937)

particularly count in its favor. On the other hand, from the Keynesian perspective, that a sacrifice of a certain degree of realism might be required to make Keynes' theory mathematically tractable is not much of an argument against it and certainly no argument in favor of Hayek's theory. Again, in the absence of evidence that either the Keynesian sacrifice of realism or the Hayekian sacrifice of mathematical tractability has improved the empirical *bona fides* of one or the other theory relative to its rival, the appeal to these ampliative rules does not contribute to consensus building.

Moreover, as we have seen, Hayek disparaged the level of aggregation that Keynes' theory trucks in; the effects of individual price changes on other prices and not the effects of one aggregate or composite variable on some other are, according to Hayek, the phenomena really relevant to the trade cycle. This said, it must be admitted that a degree of aggregation higher than that with which Hayek was comfortable tends to facilitate the application of the methods of statistics to economic phenomena. However, Hayek always denied that either statistics or econometrics has a fruitful role to play in testing economic theories; Keynes' followers (though not Keynes himself²⁸⁷) typically deny this latter denial. Thus, in the absence of evidence that a highly aggregative theory is empirically superior to a less aggregative theory, the appeal to the rule that, other things equal, more aggregation is preferable to less (or its opposite) leaves us *sans* consensus.

In another place, his well-known *Progress and Its Problems*, Laudan argues that scientific rationality is ultimately parasitic upon scientific progress rather than the

²⁸⁷ See esp. Keynes (1939)

other way around.²⁸⁸ According to Laudan's argument in *Progress*, the aim of science is the solution of problems, and different "maxi-theories" called *research traditions*,²⁸⁹ each constituted by a methodology, an ontology, and various ("mini-") theories constructed according to the attendant methodology and ontology, are evaluated on the basis of the extent to which they a) solve important empirical problems in the relevant domain of inquiry, b) avoid anomalous problems that they cannot solve despite being solved by a competing research tradition, and c) avoid raising "conceptual problems," i.e., problems of either internal consistency or tension with some external belief. In Laudan's words, "the *acceptability* of a research tradition is determined by the problem-solving effectiveness of its latest theories,"²⁹⁰ where "*the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates.*"²⁹¹ [Italics in the original]

In *Progress*, Laudan argues that, even in cases in which both the evidence fails to favor one theory over another and we lack a methodological Archimedean point from which to evaluate competing theories, a rational choice between rival research traditions can be made on the basis of comparisons of *internal* assessments of the

²⁸⁸ Laudan (1977, 5-6)

²⁸⁹ That is, sets of "*general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain*" (*Ibid.*; italics in the original).

²⁹⁰ *Ibid.*, 119

²⁹¹ *Ibid.*, 69

problem-solving effectiveness of the respective traditions. That is, Laudan's view in *Progress* is that it's not necessary for competing research traditions to share similar appraisals of the importance of different methodological rules in order to rationally decide between them; all we need to do, besides deciding how well each tradition solves the empirical problems it sets out to solve, is determine how well each accords with its own methodological and ontological precepts. We don't need a neutral Archimedean point from which to simultaneously judge both research traditions; we can first judge how well each research tradition performs according to its own particular precepts and then compare these latter evaluations with each other:

We simply ask whether a research tradition has solved the problems which it set for itself; we ask whether, in the process, it generated any empirical anomalies or conceptual problems. We ask whether, in the course of time, it has managed to expand its domain of explained problems and to minimize the number and importance of its remaining conceptual problems and anomalies. In this way, we can come up with a characterization of the progressiveness (or regressiveness) of the research tradition.

If we did this for all the major research traditions in science, then we should be able to construct something like a progressive ranking of all research traditions at a given time. It is thus possible, at least in principle and perhaps eventually in practice, to be able to compare the progressiveness of different research traditions.²⁹²

²⁹² *Ibid.*, 146

What I'm claiming in arguing against the fruitfulness for the Hayek-Keynes debate of Laudan's appeal to ampliative criteria in *Demystifying Underdetermination* is that the respective research traditions are, in any such "progressive ranking of research traditions," essentially tied. Each research tradition is about as effective (or not) as the other in solving the empirical problems it sets out to solve and avoiding anomalies and conceptual problems. More carefully, as we have seen, both research traditions can claim to solve the business cycle problem²⁹³ about as effectively as the other, and at the same time, each recognizes anomalies only in the rival account. Moreover, no conceptual problems arise either, in the first instance, between Keynes' theory and the methodology according to which simplicity and both mathematical and statistical tractability are, other things equal, to be preferred to their opposites, or, in the second instance, between Hayek's theory and the methodological rules according to which, even at the expense of simplicity and quantitative tractability, a more realistic theory is preferable. There's little reason to suspect that a comparison of internal appraisals of the problem-solving effectiveness of the respective research traditions yields anything other than a sister-kissing tie.

Finally, we should mention another of Laudan's arguments in *Progress and Its Problems* concerning rational choice between competing theories, for its failure to settle the Hayek-Keynes debate encapsulates the ultimate incommensurability of the two rival theories of the business cycle.

²⁹³ There are, of course, empirical problems unrelated to the business cycle that each research tradition aims to solve, but I'm ignoring such complications here. The application of Laudan's argument to all of the relevant problems confronted by the two research traditions would require another, much longer, essay, and, given the problems raised here, there's little reason to suspect that such an exercise would lead to a different conclusion.

Prior generations of philosophers of science had held that the rational evaluation of competing theories required a basis of shared evaluative standards:

Given the dominance at the time of the linguistic metaphor, this was usually conceived as a process of translating the predictions of competing theories (via so-called correspondence rules) into some purely observational language. Because the observational language was held to be free of any speculative, theoretical biases, it was thought to provide objective grounds for the empirical appraisal of vying theories.²⁹⁴

However, with time, philosophers grew rather despondent about the possibilities for correspondence rules and observation languages not laden with theoretical biases and some went so far as to reject the possibility of objective inter-theoretical appraisals. Against this, Laudan argues that neither correspondence rules nor theory-free observation languages are required, for “we can still talk meaningfully about different theories being *about the same problem*, even when the specific characterization of that problem is crucially dependent upon many theoretical assumptions.”²⁹⁵

The terms in which a problem is characterized generally depend upon the acceptance of a range of theoretical assumptions, T_1, T_2, \dots, T_3 . These assumptions may, or may not, constitute the theories which solve the problem. If a problem can be characterized only within the language and the framework of a theory which purports to solve it, then clearly no competing theory could be said to solve the same problem. However, *so long as the theoretical assumptions*

²⁹⁴ *Ibid.*, 143

²⁹⁵ *Ibid.*

*necessary to characterize the problem are different from the theories which attempt to solve it, then it is possible to show that the competing explanatory theories are addressing themselves to the same problem.*²⁹⁶

According to this argument, establishing the incommensurability of two theories with respect to some problem is a matter of showing that the problem is characterized by the two theories, each of which purports to solve it, in distinct ways.

This is precisely the case in the debate between Hayek and Keynes over the best solution to the business cycle problem. The problem that each theory attempts to solve is characterized within the unique language of that theory. Recall that the problem that Hayek's business cycle theory tries to solve is "the emergence of a *disproportionality* among the various productive groups, and in particular the excessive production of capital goods."²⁹⁷ This problem, as Hayek attempts to solve it, is characterized in terms of the very capital theory that is part of its solution. Hayek's problem is how it is that a disproportionality can arise between the supply of goods in different stages of the *structure of production* (a term that appears nowhere in Keynes' system) and the demand for the products of those stages. Hayek's business cycle problem is Hayek's problem alone. Similarly, the problem that Keynes set out to solve is the presence of *involuntary unemployment* as defined according to the very theory that purports to solve it. There is no such concept and, therefore, no such problem to be solved, according to the ontology associated with the Austrian tradition. Keynes' problem too is uniquely his own.

²⁹⁶ *Ibid.*

²⁹⁷ *Ibid.*, 25

Laudan's suggestion would be applicable to the debate between Hayek and Keynes (and their respective followers) only if the business cycle problem could be characterized in a way that is independent of the particular theoretical conceptions that each research tradition has respectively devised to both formulate and solve the problem. In other words, according to Laudan's argument, the theories of Hayek and Keynes would be commensurable only if a characterization of the business cycle could be found that depends upon neither the Austrian theory of capital nor Keynes' theory of unemployment.²⁹⁸ Unfortunately, in the present state of things, no such theory-neutral characterization of the business cycle problem is on the horizon.

²⁹⁸ Alternatively, commensurability (which is not to say consensus) would follow if the members of one research tradition could be convinced to give up their characterization of the problem in favor of their rivals'. Of course, for all the reasons discussed in the present paper and more, there are few grounds for optimism here.

CHAPTER 4

CONCLUSION - METHODOLOGICAL PLURALISM AND POLITICAL CAUTION

Though, at first glance, the various measures adopted by the American government in the wake of the mortgage crisis might appear to be justified by Keynes' theory, and though it may seem reasonable to think that the Great Recession represents practically ideal conditions for testing the Hayek-Keynes debate in the real world, the considerations discussed herein should give pause to anyone inclined to such a belief. In fact, we get no principles of political best practice from Keynes. If the particular policies implemented fail to result in economic recovery, one can always argue that it's not because of any flaw in Keynes' theory, but because of the inadequacies of the respective policies. On the other hand, if the negative results that his theory alleges to follow from the policies implemented do not materialize, it can always be argued that it's not because of any flaw in Hayek's theory, but because more time needs to pass—"sooner or later" Hayek's predictions will be realized and he will be vindicated.

So, regardless of the results of the natural experiment that the American government would seem to be conducting on behalf of macroeconomists the world over, those inclined to Keynes' view of things can and likely will resort to an interpretation that saves Keynes, and Austrians can and will do the same for Hayek. The Hayek-Keynes debate is intractable in large part because economists on both sides of the divide can easily adopt alternative interpretations of whatever evidence may come that protects their favored theory.

The main consequence of this paper is that there is no decision algorithm that can be applied to yield a uniquely rational choice between the theories of Hayek and Keynes. Though there are reasons to choose one way or the other, these reasons descend from distinct methodological perspectives, i.e., different “research traditions.” The methodological rules of one tradition are in direct opposition to the rules of the rival tradition, with no uniquely and obviously rational “winner” in sight among them, and so, the application of such rules to the theories of Hayek and Keynes do not permit a univocally rational choice among the theories to emerge. More carefully, each tradition is approximately equally progressive relative to its own precepts, and so, a comparison of assessments of internal progressiveness does not favor one theory / tradition over the other.

If there is anything to this argument, then the series of myopic convergences on one and then another explanation of cyclical phenomena that is part of the history of the last century of economics must be troubling to anyone concerned about the state of the world economy. Economists first shifted their early 20th-century theoretical infatuations from Austrian-style explanations of the business cycle to Keynesian accounts and, from there, to the various explanations associated with the Chicago school. Each of these shifts manifested in differing degrees the professional isolation of adherents of the ancestor traditions. The last fifty years or so has seen the increasing division of academic macroeconomics in the United States into programs committed to either the “freshwater” (i.e., “conservative” / Chicago-influenced) approach or the “saltwater” (“liberal” / Keynesian-influenced)

approach;²⁹⁹ while economics departments with a strong Austrian presence exist at George Mason University and New York University. If the argument advanced here is sound, then *none* of the shifts and divisions that have cleaved macroeconomics over the last century has been dictated by the empirical evidence. It is simply not the case that Keynesians displaced Austrians, only to be subsequently displaced by the Chicago school, in virtue of the empirical superiority of the usurper; over time, one explanation gained at the expense of its predecessors on the basis of considerations entirely unconnected to the pressing empirical question of how to get out of an actual recession.

So, given this, and assuming it is true that the subsequent history of business cycle theory represents nothing more than footnotes on Hayek and Keynes, what is the proper path forward in business cycle research? The arguments of the present paper leave two distinct routes open: either nihilism or pluralism. That is, either we can give up the business cycle project entirely or we can reopen it to different methods both old and new thereby liberating it from the empirically-unjustified segregations that have repeatedly divided the discipline.

A methodological nihilist might claim that the epistemological problems of business cycle theorizing are both devastating and permanent. An argument for methodological nihilism with respect to the business cycle might go something like this: A business cycle prediction (either a precise prediction or a pattern prediction) is the conclusion of a very long deductive argument. A business cycle theory (*any*

²⁹⁹ As the reader may imagine, the freshwater / saltwater distinction has its roots in the geographical locations of the programs first associated with the respective approaches to macroeconomics, i.e., Chicago and the American Midwest in the case of the freshwater approach, and the American coasts in the case of the saltwater.

such theory) represents an infinitesimal proportion of the premises necessary to deduce such a conclusion. The other premises involve statements about, at minimum, multifarious political and financial conditions, institutional arrangements, and even, perhaps, the attitudes of individual market participants, all of which fall outside the purview of economic science proper. There are many such premises. So many, in fact, that discovering all of them is quite likely to be impossible for cognitively-limited human beings. Moreover, there will always be many such premises: no matter how specific and precise we make our theory, there will always be many more auxiliary premises required to deduce a prediction. The blame for an apparent predictive failure can always be laid at the feet of one or more of these extra premises: the impossibility of testing business cycle predictions is a permanent condition.

In offering such an argument, the nihilist is essentially extending to the variables themselves Hayek's arguments about the impossibility of gathering all of the data necessary to deduce a precise prediction, and is thereby denying the possibility of even pattern predictions with respect to business cycle phenomena. In sum, the nihilist's argument is that, far from possessing knowledge of the relevant data, we lack knowledge even of the relevant variables – we don't even have enough components to make out a "pattern" – and forever it shall be. Of course, the nihilist makes the further inference that, as a consequence, business cycle theorizing should be scrapped altogether.

I must confess that I find the nihilist's argument appealing all but for this last inference. For the reasons given – i.e., the impossibility of discovering all of the

relevant variables much less their values in a particular context – I’m skeptical of the possibility of even pattern predictions with respect to the business cycle (and I’ve already expressed above my skepticism with respect to the possibility of falsifying such pattern predictions); however, the last inference to the annihilation of business cycle theory would seem to require for its justification the claim that accurate prediction is the sole criterion by which a business cycle theory is to be judged. In other words, if you believe on the basis of the arguments offered in the present paper that business cycle theorizing is a lost cause that ought to be scrapped altogether, then – *given* the arguments of the present paper – you must also believe that both accurate prediction and a univocal method for discovering the quality of predictions are required for a business cycle theory to count as “successful,” “acceptable, “valuable,” what have you.

I want to suggest that this latter inference is made too hastily. More carefully, I want to suggest that beyond the point established in the present paper – namely, that there is no decision algorithm that can be applied to yield a uniquely rational choice between the theories of Hayek and Keynes (and thus, on the relevant assumption, business cycle theories more generally) – we simply do not know at present what the value of business cycle theory is or will be in the long run. To put an end to theorizing about the business cycle is to make a final assessment before many other methods have been tried. Macroeconomics grew up in the age of logical empiricism. Outside of the Austrian school, which has always been deeply skeptical of a narrow identification of the scientific value of an economic theory with predictive success, most business cycle theorizing has been directed at successful

prediction. If the arguments of the present paper are sound, then it is doubtful that we can recognize a successful business cycle prediction when we see it, but it does not follow that investigating the business cycle is entirely without value and that it ought to be scrapped altogether. A call for the annihilation of business cycle research is made far too fast. Thus, in the present state of knowledge, nihilism appears to be an epistemological bridge too far.

This leaves methodological pluralism with respect to the business cycle. However, given the arguments of the present paper, it must be a pluralism that is aware of the predictive limitations inherent in its object. To repeat the (I believe) justified portion of the nihilist's argument: these limitations are permanent and not something to be overcome in the due course of time. It would require a radical reconception of the business cycle project to avoid the complications raised here; a reconception so radical that it would be hard to deem the result a part of economics proper for it would require, at minimum, the addition of the principles of political best practice that Keynes (given that he was an economist rather than a scientist of bureaucratic practice, rightfully³⁰⁰) did not explicate. It would also require economists to add fairly detailed temporal parameters to their predictions, but if Hayek's arguments are sound with respect to precise prediction of complex phenomena like those of the business cycle, then this is simply too much to expect from economics. In short, the reconception of the business cycle project so as to make the implications of resulting theories more easily testable might well result in its

³⁰⁰ The unnamed editor of W.H. Hutt's *Politically Impossible...?* (1971, vii) makes the trenchant observation in the preface to that work that "the economist is not equipped, and he has no authority, to judge which of his conclusions are 'politically practicable' (or 'administratively feasible')."

amputation from the realm of what has heretofore been considered economic science proper (and, given the quasi-mystical undertones associated with predicting the future of human beings and of their societies, perhaps even *science* proper. Such a reconception seems rather ill-advised).

In *Beyond Positivism*,³⁰¹ his treatise on the history of 20th-century economic methodology, Bruce Caldwell provides a picture of what such a self-aware pluralism, as applied to economics generally, might look like in practice. In particular, he emphasizes that such a pluralism would take as a virtual axiom 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.)”³⁰² Nonetheless, Caldwell argues that interesting and important problems remain for the methodologist: “A partial listing of these would include: to foster an understanding of the scientific process among members of his profession; to systematize jargon; to rationally reconstruct the methodological content of various research programs; to promote an environment in which both novelty and criticism can operate freely.”³⁰³

A further suggestion is that economic theories be judged on the basis of the principles that govern the research tradition from which they descend rather than from some particular perspective (erroneously) taken by the respective judge to be

³⁰¹ Caldwell (1982, 244-252)

³⁰² *Ibid.*, 245; Obviously, if there is anything to the argument of the present paper, then this point is well established with respect to business cycle theory.

³⁰³ *Ibid.*, 244-245

“more rational,” “better,” “right,” or what have you. In Caldwell’s terms, only criticism of an “internal” sort is valid:

It is in this area, the methodological evaluation of alternative research programs, that methodological pluralism has the most to offer. Such programs should be criticized either on their own terms, or for failing to show how they can be compared to other programs. This approach ensures that novelty is promoted, that criticism is not dogmatic, and that a dialogue takes place among members of alternative research programs.³⁰⁴

Given the arguments of the present paper concerning the fruitlessness for the Hayek-Keynes debate of Laudan’s view that comparisons of assessments of internal progressiveness provide a means of objective theoretical evaluation, there is room for skepticism about Caldwell’s rosy assessment of the value of internal criticism. Nonetheless, in the end, Caldwell’s suggestion may amount to little more than the eminently reasonable advice that economists be more open than they have heretofore to both older and newer perspectives on the business cycle, and there’s little that can be said against such sage counsel.

Finally, as argued above, the conclusion that there is no decision algorithm that yields a uniquely rational choice between Hayek and Keynes seems to support a mild skepticism with regard to Keynesian-style political actions taken to ameliorate the consequences of the business cycle. To the extent that Keynes’ name is invoked (which, of course, it almost always is), such policies lack an empirically-grounded theoretical justification. Economists often offer advice to governments with respect

³⁰⁴ *Ibid.*, 248

to the business cycle. Indeed, it is sometimes even the case, as it is with the decision-makers at the head of both the U.S. Federal Reserve and the U.S. Treasury department, that the professional economist who offers advice and the policymaker who acts upon it are one and the same person. In a perfect world, economists would base their advice on their knowledge of *the right economic theory* of the business cycle; but, in the world that we are in, economists cannot possess such infallible access to the truths of their subject. The most that we might hope is that political advice be based on unbiased assessments of the empirical evidence. However, it is the argument of the present paper that the theoretical commitments of economists with respect to the business cycle cannot be based on empirical considerations. If it's true that the relevant theories of the business cycle are epistemologically suspect, then so too, it seems, is any policy based upon such theories; any such policy lacks an empirically-grounded theoretical underpinning.

While this provides no positive warrant for Hayek's recommendation of political *inaction* with respect to the business cycle, it does provide some negative warrant against Keynes' recommendation of political *action*, or, more carefully, against political action of the variety associated with Keynes' theory. As has been suggested above, if there is anything to the argument of the present essay, then action on the basis of the political advice suggested by any given business cycle theory should be taken only with some measure of caution.

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