**Slaves of the defunct: the epistemic intractability of the Hayek-Keynes debate**[[1]](#footnote-2)

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The present essay addresses the epistemic difficulties involved in achieving consensus with respect to the Hayek-Keynes debate. 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 relevant theories are such that, regardless of what is observed, both theories can be interpreted as true, or at least, as not falsified. Regardless of the evidence, both Hayek and Keynes can be interpreted as right. The essay explicates the respects in which the empirical evidence underdetermines the choice between the relevant theories. 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 and that, for particular kinds of evidence, the two theories are empirically equivalent.

**Keywords:** Hayek; Keynes; underdetermination; business cycle theory; macroeconomics; pattern predictions

So long as man remains free he strives for nothing so incessantly and so painfully as to find some one to worship. But man seeks to worship what is established beyond dispute, so that all men would agree at once to worship it. For these pitiful creatures are concerned not only to find what one or the other can worship, but to find something that all would believe in and worship; what is essential is that all may be *together* in it. This craving for *community* of worship is the chief misery of every man individually and of all humanity from the beginning of time. For the sake of common worship they’ve slain each other with the sword. They have set up gods and challenged one another, “Put away your gods and come and worship ours, or we will kill you and your gods!” And so it will be to the end of the world, even when gods disappear from the earth; they will fall down before idols all the same.

–Dostoevsky, *The Brothers Karamazov*

Which was right? Keynes or Hayek?

–Sir John Hicks

The present essay is concerned with one of the more controversial topics in the history of economic thought, namely, the (in)famous debate between F.A. Hayek and John Maynard Keynes (and their respective followers). The paper considers the epistemological problems involved in arriving at a consensus with respect to John Hicks’ oft-quoted question concerning ‘Which was right? Keynes or Hayek?’[[3]](#footnote-4) In particular, the paper asks ‘How would we know which was right? What would the evidence look like were one of this eminent duo right and the other wrong?’ The thesis is that the absence of consensus is explained by the fact that *we can’t know which was right*: 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 least, as not falsified.[[4]](#footnote-5)

The first part of the essay explicates the respects in which the choice between the theories of Hayek and Keynes is underdetermined in the *holist* sense. It is argued that there are lacunae in each theory that leave the rational response to falsifying evidence undecided. These lacunae make it convenient to absolve the theories from fault in the face of threatening evidence.

The second part considers this underdetermination problem from the perspective of Hayek’s later methodological writings. It is argued that Hayek was wrong to claim that (what he called) ‘pattern predictions’ are empirically testable, even in the limited sense in which he held them to be. That being said, the main argument of this part is that the implications of the theories of Hayek and Keynes are too underspecified to qualify even as Hayekian pattern predictions.

The third part argues that, with respect to short-run evidence, the two theories are empirically equivalent, i.e., that the choice between the two theories is underdetermined in the *contrastive* sense.[[5]](#footnote-6)

The fourth part of the essay considers the debate from the perspective of a well-known strategy for choosing between underdetermined theories. It is argued that Larry Laudan’s (1990) appeal to inductive (or ‘ampliative’) criteria of theory choice does not suffice to settle the Hayek-Keynes debate.

The concluding section considers the consequences of the argument both for the future of research into and for policies aimed at ameliorating economic fluctuations.[[6]](#footnote-7)

**1 The worst rock ‘n’ roll supergroup ever: Hayek, Keynes, Duhem, and Quine**

It has been suggested, either explicitly or less so, that the economic conditions of the present century, especially those of the so-called ‘Great Recession’, provide an opportunity for something like a natural-experimental test of the Hayek-Keynes debate.[[7]](#footnote-8) The argument of the present part of the paper is that this is a false intimation and, indeed, that no such test can exist. The debate is decidable on the basis of empirical evidence only if it is possible for the world to appear some way that uniquely favors one theory rather than the other. This condition is not satisfied – and the choice between the theories is underdetermined in the holist sense – if, for any possible evidence, there is an interpretation that saves the theory that might otherwise be endangered.

In order to derive testable predictions from the theories, subsidiary assumptions must be introduced that are not implications of the theories themselves; when predictions derived in this manner fail to meet experience, such failure can always be attributed to these extraneous assumptions rather than to the given theory. Interestingly, the underdetermination problem arises in the present context not so much, as one might expect, as a consequence of the acknowledged difficulties of interpreting the results of natural experiments, but because there are lacunae in the relevant 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.[[8]](#footnote-9), [[9]](#footnote-10)

* 1. ***Keynes’ theory and holist underdetermination***

It is important to recognize that the implications of Keynes’ theory are inescapably political: they purport to describe the consequences of particular policy actions. If the test of Keynes’ theory is the practical success of his policy recommendations,[[10]](#footnote-11) then testability requires some means of determining the extent to which these recommendations are successful in practice; and this requires that we know which policies qualify as legitimately Keynesian. The argument of the present section is that this latter condition cannot be satisfied: we have no means of deciding which policies qualify as Keynesian; thus, we can’t determine the success of the policy implications of Keynes’ theory; therefore, we can’t test Keynes’ theory.

Keynes’ discussions of policy[[11]](#footnote-12) generally take one of two forms, neither of which is conducive to testing the theoretical conception that underlies the relevant policy recommendations. Keynes’ prescriptions are either too specific – in the sense of being intended to redress problems particular to a unique time and place and, thus, not testable on the basis of applications outside of their original contexts[[12]](#footnote-13) – or too vague, i.e., they suggest, without specifying details required for testability, that there is *some* policy or other that will work toward the proposed end. In the first case, which we won’t consider in detail in the present paper, implementing one of Keynes’ context-specific policy suggestions does not represent a natural-experimental test of the theory since it can always be claimed that failure reveals the contextual inappropriateness of the particular policy, given the conditions then prevailing,rather than flaws in Keynes’ theory.[[13]](#footnote-14) In the second case, implementing one of Keynes’ vague policy hints does not count as an experimental test because failure can always be attributed to the details of the particular policy, which are no part of Keynes’ theory. In short, either excessive specificity or its opposite can always bear the burden of predictive failure and Keynes’ theory can be retained.

It is essential to recognize the extent to which multiple, not entirely consistent, policy prescriptions can be – and, to be sure, have been – inferred from Keynes’ writings. Keynes’ followers, both immediate and less proximate, do not all agree on the policy implications of his theoretical arguments.[[14]](#footnote-15) These disagreements typically revolve around a number of issues left underspecified by Keynes. In particular, there is the well-known question of whether and to what extent Keynes came to doubt the efficacy of a purely monetary remedy to an economic slump in favor of a fiscal prescription; and there is the related question of whether and in what cases Keynes preferred discretion to a rules-based approach to policy.

It is not the business of the present paper to mediate these disputes between Keynes’ interpreters. Indeed, for our purposes, it almost suffices to point out that the mere fact of these disagreements undermines the testability of Keynes’ theory. This means that when some purportedly Keynesian policy appears to fail it can be claimed that fault lies with the particular interpreter’s mistaken inference from Keynes’ theory to the kind of policy implemented, and that a different sort of policy can be validly inferred from Keynes’ arguments that would have manifested the postulated results. Predictive failure can always be attributed to an invalid interpretation of Keynes’ policy suggestions and the theory can be retained come what may. However, the problem actually runs deeper than this, for, distinct from the question of their status *qua* implications of Keynes’ theory, the various attempts to specify Keynes’ policy recommendations do not lead to testable predictions.

Both of these issues are exemplified by the epistemological problems raised by ‘Functional Finance’ (FF). Even today, FF is the political program most intimately associated with Keynes’ name in the public imagination (and in the minds of some economists as well). This fact notwithstanding, Leijonhufvud (1968, 403; also Bateman 2006, 277-280) argues that FF is an invention of certain of Keynes’ immediate followers, Abba Lerner, in particular. It is not necessary to resolve the question of the relationship of Functional Finance to Keynes’ theory.[[15]](#footnote-16) What is important to note is that the ambiguous status of FF’s relation to Keynes’ theory allows the latter to come up aces regardless of what the evidence indicates about the practical success of the former. If FF is successful, then Keynes’ theory can take the credit (or, at least, can’t be counted as falsified); however, if FF fails, then Lerner (and Hansen) can take the blame, and Keynes’ theory can be exonerated.

That said, the epistemological problems of Functional Finance run deeper than its uncertain status vis à vis Keynes’ theory. The claims of FF enthusiasts (whether or not Keynes should be counted among them) are not testable. There is no evidence that need undermine belief in the success of Functional Finance as a principle of policymaking. Consider Lerner’s ([1943] 1983, 298-299) ‘first law of Functional Finance’:

The first financial responsibility of the government (since no one else can undertake that responsibility) is to keep the total rate of spending in the country on goods and services neither greater nor less than the rate which at the current prices would buy all the goods that it is possible to produce. If total spending is allowed to go above this there will be inflation, and if it is allowed to go below this there will be unemployment. The government can increase total spending by spending more itself or by reducing taxes so the taxpayers have more left to spend. It can reduce total spending by spending less itself or by raising taxes so that taxpayers have less money to spend. By these means total spending can be kept at the required level, where it will be enough to buy all the goods that can be produced by all who want to work, and yet not enough to bring inflation by demanding (at current prices) *more* than can be produced.

What this ‘law’ predicts is that for any given set of economic conditions, there is *some* ‘government fiscal policy’[[16]](#footnote-17) that will, depending on existing conditions, either raise or lower total spending to, or keep it at, the rate (or ‘level’) that just suffices to buy at existing prices no more and no less than all the goods that can be produced by all the people who want to work at existing wages.

If the state were to implement Functional Finance and successfully avoid both inflation and unemployment, then, naturally, accolades of predictive excellence would redound upon this law. However, the evidence would not compel the rejection of the law were either unemployment or inflation (or both) to follow upon its implementation. If unemployment but not inflation (or *vice versa*) were to manifest in the wake of the application of the law to some set of economic circumstances, it could be readily claimed that either a) the latter economic conditions were misestimated, b) the rate of total spending required to buy at existing prices everything that could be produced by everyone eager to work was miscalculated, c) the value of the *multiplier*, i.e., of the rate at which ‘every dollar spent by the government…create[s] several dollars of income’ (*Ibid*., 302-303) – which is supposed to do the work of closing the fissure between current spending and the level of spending that ensures both zero inflation and zero unemployment – was misestimated,[[17]](#footnote-18) or d) other errors in either the *design*, *implementation*, or *administration*[[18]](#footnote-19) of the government’s fiscal policy prevented the realization of the postulated effects. Not even evidence of the simultaneous manifestation of both inflation and unemployment (‘stagflation’) need compel the rejection of Functional Finance, which, exclusively concerned as it is with the realization of a particular level of demand, does not rule out the possibility of supply conditions being such as to undermine the effectiveness of an otherwise flawless fiscal policy; stagflation can always be attributed to supply shocks[[19]](#footnote-20) and the first law of Functional Finance can be retained.[[20]](#footnote-21)

Considerations of brevity must limit our explicit discussion of alternative interpretations of Keynes’ policy advice.[[21]](#footnote-22) Suffice it to say that the literature reflects a number of common conceptions whatever the unique features of particular interpretations. Moreover, although they differ as to the specific means to be applied to the end of mitigating involuntary unemployment to the greatest extent possible without (greatly) exacerbating inflation, these interpretations agree that we should expect only those political measures to be effective that are both the right size and not spoiled by inadequacies of either design, implementation, or administration.[[22]](#footnote-23) But, which are *these*? Without some method of distinguishing stimulus programs the failure of which can be legitimately attributed to errors in theoretical reasoning from those programs the failure of which is more appropriately attributed to errors in practical application we do not know in the case of any particular stimulus policy whether or not we are in fact testing Keynes’ theory.

It is not difficult to see how these testability problems are a consequence of *The General Theory* (or, more carefully, of the theoretical lacunae the book fails to address).[[23]](#footnote-24) The difficulties concerning the appropriate size of policy are a consequence of both the goal that the book sets out for policy action, namely, that of mitigating involuntary unemployment to the greatest extent practicable, and the means postulated for the achievement of this goal, i.e., the investment multiplier. In order to enact a stimulus program that is large enough to narrow a particular breach between existing demand conditions and full employment (*sans* excessive inflation), we need to know both the extent of the shortage (or excess) in aggregate demand (i.e., we need to know both the state of existing demand conditions and the quantitative parameters of the end to which policy is to be applied) and the value of the multiplier. However, there is nothing in Keynes’ book by way of advice for ensuring the accuracy of our beliefs concerning these values.

Keynes argues in *The General Theory* that the value of the multiplier is a function of the community’s marginal propensity to consume (MPC). Whatever the scientific import of some statistical inquiry into the value of the multiplier at a particular time and place, it is ultimately independent of the postulate that the value of the investment multiplier is greater than one. The failure of a stimulus program predicated on a statistical analysis of some community’s MPC need not reflect poorly on Keynes: such failure can be attributed instead to shortcomings in either the relevant data used to estimate MPC or the particular statistical methods applied to them.[[24]](#footnote-25)

At the same time, Keynes offered no specific guidance with respect to the features of adequate modes of policy design, implementation, and administration.[[25]](#footnote-26) There is an infinite variety of suchmodes and there is no reason to think that they are all able to influence involuntary unemployment.[[26]](#footnote-27) Regardless of whether Keynes actually believed in the productive merits of building pyramids or mindlessly digging and refilling holes,[[27]](#footnote-28) we can easily imagine a stimulus program the effectiveness of which we’d have reason to doubt in advance.[[28]](#footnote-29) 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 his theory.[[29]](#footnote-30)

To sum up the argument of this section, if we are to test Keynes’ theory on the basis of the practical success of its policy implications, we need to know which policies do and do not qualify, but we have not been provided criteria upon which to base this distinction. We don’t know what kind of policies – monetary rather than fiscal, discretionary rather than rules-based – properly count as Keynesian. But, even if *this* question were decided, we would have no means of distinguishing adequately-Keynesian policies from inadequately-Keynesian ones, i.e., we would not know which policies were both the right size and adequately designed, implemented, and administered. We would not be able to distinguish those cases in which it is legitimate to reject one or more of the assumptions that either a) our estimates of existing economic conditions are accurate, b) our calculations of the specific goal to which policy is to be applied are appropriate, c) our estimates of the prevailing multiplier are accurate, or d) our policies have been adequately designed, implemented, and administered, from those cases in which it is more appropriate to reject Keynes’ theory. One cannot infer the failure of Keynes’ theory from the apparent failure of *any stimulus program*. It can always be claimed that another policy would have worked where some actual one seemed to fail.[[30]](#footnote-31) Thus, the rational response to an apparent disconfirmation of Keynes’ theory is underdetermined in the holist sense.

* 1. ***Hayek’s theory and holist underdetermination***

According to Hayek’s theory, economic fluctuations are ultimately a consequence of the institution of fractional reserve banking (FRB) and, in particular, of the epistemic limitations this institution imposes upon those in control of the money supply. Economic stability in a money-using economy, according to Hayek, requires monetary neutrality, i.e., the maintenance of equality between the rate of interest at which loans can be had and the ‘natural’ rate of interest that equilibrates the supply of voluntary savings with the demand for loans. In a FRB-world, the latter magnitude is a mere hypothetical: where it is possible to expand lending beyond the extant base of voluntary savings – i.e., where fractional-reserve banking is practiced – the natural rate is an unknowable quantity.[[31]](#footnote-32)

Of course, this ignorance, combined with the fact that, under normal operating conditions, bankers *are* encouraged by the promise of profits to expand their lending activities, typically leads to a circumstance in which the loan rate falls below the natural rate of interest. It is this which, on Hayek’s theory, sets the business cycle in motion. In particular, investors are incentivized by the availability of loans at unnaturally low rates of interest to engage in projects the expected profitability of which is ultimately revealed to be an illusion fostered by these low rates: certain investments are made to seem more profitable than they would appear at a loan rate commensurate with the hypothetical natural rate. However, either incipient inflation or the exhaustion of banks’ reserves eventually forces loan rates back up – more in line with the hypothetical natural rate – and, when this happens, these latter (mal)investments are revealed to be unprofitable. Many are forced to either shut down or limit production as a consequence—the bust has arrived.

The implications of Hayek’s theory are no less political than those of Keynes’ theory. Indeed, the main prediction of the former account is that the business cycle will be moderated if and only if both the assumptions of general equilibrium theory (upon which Hayek’s theory is based) obtain and the loan rate is kept equal to the natural rate. If, on the other hand, the loan rate is kept unnaturally low, as Keynes suggests it should be, then, *within some unspecified timeframe*, an unsustainable investment boom will occur, which, *within some unspecified timeframe*,will be reversed in the form of a bust, ‘crisis’, recession, what have you. According to Hayek’s theory, unnaturally low interest rates lead in the short run to what appear to be positive effects that are inevitably reversed in the longer run: boom is eventually and unavoidably followed by bust.

Hayek was under no illusions regarding the possibility of either ensuring that the assumptions of equilibrium theory obtain or keeping loan rates in line with the unknowable natural rate.[[32]](#footnote-33) The upshot of Hayek’s explanation of the cycle is that there is no perfect, practicable, policy in the presence of fractional-reserve banking. The only solution to the problem of the cycle is a radical one involving drastic changes in banking policy, namely, the cessation of FRB in favor of full-reserve banking; but, even this would not suffice to eradicate the cycle in the absence of the practically impossible circumstance that the assumptions of equilibrium theory also obtain. In short, Hayek’s theory implies the practical impossibility of preventing the trade cycle.[[33]](#footnote-34)

Hayek’s followers disagree as to the practicable policies – typically, either a full gold standard (or some other commodity standard) or a system of ‘free’ banking – that might best *moderate* the cycle,[[34]](#footnote-35) but none of these suggestions *rules out* episodes of economic stagnation. Indeed, there are only two circumstances that Hayek’s theory excludes, namely, the appearance of the cycle in a world in which both monetary neutrality and the assumptions of general equilibrium theory obtain and the non-occurrence of the cycle following conditions in which the natural rate of interest exceeds the loan rate. Enough has been said to establish that the former possibility is no criterion for testing Hayek’s theory: we live in a world where both fractional-reserve banking is practiced and the conditions of equilibrium theory are impossible to realize. Unfortunately, the latter possibility that the cycle might fail to follow from an unnaturally low loan rate is also not a standard against which Hayek’s theory might be tested.

As noted, the natural rate of interest is not observable in economic systems that feature fractional-reserve banking. Bankers (central or otherwise) can 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 in which everything is the same as it is in this world except that banks hold full reserves and the assumptions of equilibrium theory obtain. This latter difficulty can be set aside in principle. On the assumption that the natural rate remains relatively stable over time,[[35]](#footnote-36) it is a consequence of Hayek’s theory that, other things equal, the extent of encouragement given to malinvestment varies inversely with the level of the loan rate. So, if it can be shown that changes in the loan rate are inversely correlated with the severity of the effects of the business cycle,[[36]](#footnote-37) one might acquire some evidence that Hayek’s theory is right in the absence of direct evidence of the value of the natural rate. Thus, the non-observability of the natural rate does not prevent the evaluation of Hayek’s theory in principle.

However, there are other considerations that decisively undermine the testability of Hayek’s theory. The crucial problem is the yawning lacuna in Hayek’s theory with respect to *when*, following a lowering of the loan rate, one can expect to observe the effects of the boom-bust cycle. The inverse correlation posited above states, in effect, 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 unnaturally low loan rates of interest could turn to bust tomorrow, or a year from now, or a decade from now. Until the bust arrives, Hayek’s theory will continue predicting that it is imminent, and when the bust does eventually materialize – which, of course, with a long enough time horizon, it must – it can be said that ‘Hayek’s theory predicted it!’ But, logically and epistemologically, this case is no different from that of the meteorological sage who predicts rain every day: eventually, he’s bound to be right, but that doesn’t indicate the soundness of the theory from which he infers his daily prediction.

This means that there is no observable evidence which compels the rejection of Hayek’s theory.[[37]](#footnote-38) 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.[[38]](#footnote-39) We are at all times, following a bout of unnaturally low market rates of interest, living either in a world where Hayek’s postulated effects have occurred (or are occurring) or in a world in which these effects will eventually, with a sufficiently long time horizon, occur. Come what may, it can be asserted that Hayek was ‘right’. Thus, the rational response to an apparent disconfirmation of Hayek’s theory is underdetermined in the holist sense.

**2 A brief aside on (what Hayek calls) pattern predictions**[[39]](#footnote-40)

It is a common theme of Hayek’s later methodological writings that specific economic events, like the turning point from boom to bust, are impossible to predict in advance with any reasonable precision. Given the complex phenomena that they investigate, economists can only offer ‘pattern predictions’ to which temporal parameters cannot be non-arbitrarily adduced. Thus, if we added temporal parameters to the implications of Hayek’s theory, not only would we then *not* be in a better position to appraise the theory (because predictive failure could always be attributed to these parameters, which are not consequences of the theory), but we would actually be doing something that Hayek argued is impossible to do well.

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 in which we possess sufficient data to generate detailed predictions; as we move away from this side of the continuum, our knowledge of the relevant data decreases and, thus, our ability to predict with precision decreases as well. Naturally, at the opposite end of the spectrum are those cases in which we are utterly ignorant of the relevant data and all of our knowledge consists of the pattern predicted by the given theory. Nonetheless, according to Hayek, such pattern predictions are testable:

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 circumstancesis 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. (*Ibid*., 9)

However, given the problems of underdetermination, this claim that pattern predictions are testable is doubtful on its face.[[40]](#footnote-41) True enough, Hayek (*Ibid*.) did recognize that pattern predictions possess less empirical content (in Popper’s sense) than predictions of particular events, but he did not acknowledge that the problems of underdetermination effectively annihilate the possibility of testing pattern predictions.

It is no less the case with respect to pattern predictions than it is with respect to predictions of individual events that they never confront experience in isolation. Hayek may have been right that, with respect to complex phenomena like those of the trade cycle, we typically do not know all of the empirical statements necessary to derive a prediction of a particular event, but 1) even if we don’t know all of these statements, presumably, we do know some of them, and – just as Duhem-Quine establishes with respect to more highly-specified predictions – 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, i.e., we can adduce the *absence* of the relevant data as an explanation of the failure of a pattern prediction. If, as Duhem-Quine establishes, a prediction of a particular event cannot be tested in isolation, then – *a fortiori* – pattern predictions are also untestable.[[41]](#footnote-42)

This point is relevant because the implications of the two theories *don’t even rise to the level of pattern predictions*. As Hayek defines the concept, a pattern prediction is separated from a prediction of a particular event by an ocean of unknown empirical facts. However, as the discussion of the first part of the paper has (hopefully) made clear, empirical data are not all that we lack with respect to the theories of Hayek and Keynes. There are purely *theoretical* lacunae that keep us even from the possibility of pattern predictions much less from the possibility of predictions of specific economic events. Recall that directly after claiming that pattern predictions are falsifiable Hayek states that ‘[t]he 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’ (*Ibid*). However, 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. A theory that generates Hayekian 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 lead to such predictions by the absence in the former of all of the relevant parameters and it is this problem that we confront with respect to the appraisal of the theories of Hayek and Keynes.

**3 In the short run we’re all right (or all wrong)**

The sense in which the choice between the two theories is contrastively underdetermined is exemplified by an alternate explanation that Keynes’ defenders sometimes offer for what might otherwise be interpreted as the failure of stimulus measures to positively affect unemployment. Where Keynesians have not argued that such seeming failures are 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, i.e., they claim that involuntary unemployment is lower than it would have been in the absence of the stimulus policies in question.[[42]](#footnote-43)

Whether such assertions are true is not our concern; what does require emphasis is that these claims do not facilitate the evaluation of the relevant theories. Even if it could be established that particular policies have had some stimulatory effects, we could not infer from this evidence that Keynes was right and Hayek wrong. It is a consequence of *both* theories that fiscal and monetary policies can manifest stimulatory effects in the short run. Evidence of such short-run effects (or evidence of the *absence* of such effects) contrastively underdetermines the choice between the theories of Hayek and Keynes. Both theories make essentially the same claims with respect to near-term effects, and so, both theories rise or fall together on this point. Thus, any given body of short-run evidence contrastively underdetermines the choice between Hayek and Keynes.[[43]](#footnote-44)

**4 The failure of the appeal to ampliative criteria**

In ‘Demystifying Underdetermination’, the historian and philosopher of science Larry Laudan (1990) argues that certain authors move too hastily from the acknowledged truth of deductive (or ‘Humean’) underdetermination to a conclusion of epistemic relativism. Laudan argues that deductive underdetermination – i.e., the uncontroversial fact that a correct explanation of some body of evidence cannot be deduced from the evidence alone – does not support the stronger (relativistic) claims concerning underdetermination that Quine (and, especially, certain of the latter’s immediate followers) inferred from Quine’s (1951) arguments in ‘Two Dogmas of Empiricism’.

Laudan argues that the only acceptable interpretation of the Quinean claim that any theory may be retained ‘come what may’ is 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 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 claim without an implicit and, he argues, doubtful premise to the effect that there are no non-deductive rules that might determine theory choice. Laudan argues for the possibility of rationally choosing between underdetermined theories on the basis of ‘ampliative’ rules of theory choice. That is, though it might be reasonable to hold that the resources of deductive logic do not suffice to pick out a uniquely rational response to threatening evidence, it is Laudan’s view that the addition of inductive principles of scientific reasoning like *simplicity*, *conservatism*, *fecundity*, etc.typically suffice to determine the rational choice between deductively-underdetermined theories.

It is important to note that such a surreptitious elision from the psychological premise to the relativistic conclusion is not part of the present paper. We have assumed that the convenience of the relevant strategies makes them appealing, and so, explains the absence of consensus with regard to Hicks question; however, we have refrained from attaching the adjective ‘rational’ to the choices of economists between the theories of Hayek and Keynes. That being said, it can be shown that Laudan’s appeal to ampliative criteria does not provide a route to a rational consensus.

The difficulty 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.

In their published debate, Hayek ([1931] 1995a and [1931] 1995b) criticized Keynes for failing to address the complexities of capital theory and Keynes (1973, 252-253) 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 monetary economy would have involved Keynes in many unpleasant entanglements—even Keynes seemed to accept as much. Indeed, Keynes (*Ibid*., 252)seemed to accept that the addition of a capital-theoretic foundation could only improve the realism of his account: consider 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’.[[44]](#footnote-45) It seems that whatever one thinks of Hayek’s capital theory, his choice to confront the difficulties of capital represents a choice to sacrifice some degree of simplicity in favor of (what he took to be) realism; while Keynes’ decision to set capital theory aside must count as a choice in favor of simplicity.[[45]](#footnote-46) Thus, those who de-emphasize simplicity in favor of realism (as Austrians typically do) are likely to prefer Hayek’s theory, while those who think that the realism rule is less important than the simplicity rule (as Keynesians tend to) are apt to choose Keynes’ theory. How do we settle this dispute between realists and simplicitists?[[46]](#footnote-47) In the absence of empirical evidence that unambiguously supports the more realistic theory in favor of the simpler one, or the other way around, we are impotent to convince those who prefer relatively more realistic theories to agree with those who prefer relatively simpler 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 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. Given the centrality of mathematical expression to modern economics, we might be inclined to think this shortcoming fatal to Hayek’s theory. However, Austrian economists are well known for denying that mathematics has anything but a heuristic role to play in economic theorizing. 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 building a rational consensus.

Furthermore, Hayek ([1931] 1995a, 128) famously disparaged the level of aggregation in which Keynes’ theory trucks; the effects of individual price changes on other prices and not the effects of one aggregate or composite variable on some other(s) are, according to Hayek, the phenomena really relevant to the trade cycle. However, the degree of aggregation of Keynes’ theory – a degree higher than that with which Austrians are comfortable – facilitates the application of the statistical method to economic material.[[47]](#footnote-48) In the absence of evidence that a highly aggregative theory is empirically superior to a less aggregative theory the appeal to the relevant ampliative rules fails to contribute toconsensus.

In his *Progress and Its Problems*,Laudan (1977, 5-6) argues that the aim of science is the solution of problems and that different ‘maxi-theories’ called *research traditions*,[[48]](#footnote-49) each constituted by a methodology, an ontology, and various (‘mini-’) theories, 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’ they fail to solve despite being solved by some rival tradition, and c) avoid raising ‘conceptual problems’, i.e., problems of either internal consistency or tension with some external belief. In Laudan’s (*Ibid*., 119)words, ‘the *acceptability* of a research tradition is determined by the problem-solving effectiveness of its latest theories’, 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*. [Italics in the original]

According to Laudan (*Ibid*., 146), a rational choice between research traditions can be made on the basis of comparisons of *internal* assessments of the *problem-solving effectiveness* of the respective traditions. It is 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.

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.

Laudan’s appeal to ampliative criteria is fruitless for the Hayek-Keynes debate because, in any such ‘progressive ranking’, the research traditions are essentially in a dead heat. Each research tradition is about as effective as the other both in solving the empirical problems it sets out to solve and in avoiding anomalies and conceptual problems. More carefully, as we have seen, both research traditions can claim to solve the business cycle problem[[49]](#footnote-50) about as effectively as the other, and at the same time, each recognizes anomalies only in the rival account. 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.

According to Laudan (*Ibid*., 143), previous generations of philosophers of science held that the rational evaluation of competing theories requires a basis of shared evaluative standards. 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 notion 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’.

The terms in which a problem is characterized generally depend upon the acceptance *of a range of theoretical assumptions, T*1, *T*2, …, *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 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*. [*Ibid*.; italics in the original]

In other words, it is sufficient to establish the incommensurability of two theories with respect to some problem to show 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. The problem that each theory attempts to solve is characterized within the unique language of that theory. The problem that Hayek’s ([1933] 2008, 25) business cycle theory seeks to solve is ‘the emergence of a *disproportionality* among the various productive groups, and in particular the excessive production of capital goods’. This problem is characterized in terms of the very capital theory that is the basis of its solution. More carefully, Hayek’s problem is how it is that a state of disequilibrium can arise between the supply of and demand for the products of different stages of the *structure of production* (a concept that appears nowhere in Keynes’ system). The description of this problem runs in terms of the same Austrian theory of capital that provides its solution: such disequilibria are distortions of the structure of production, caused by the banking system’s injection of credit at particular stages of the structure, which draws more resources to these stages than is justified by the demand for their products. The business cycle problem described by Hayek and the Austrians is their problem alone. Similarly, the problem that Keynes set out to solve is the presence of *involuntary unemployment* as defined (Keynes [1936] 1973, 15) according to the very theory that purports to solve it. The problem of involuntary unemployment is that of a shortfall in aggregate demand; the solution of the problem is the maintenance of sufficient aggregate demand. There are no such concepts as involuntary unemployment and aggregate demand and, therefore, no such problem to be solved, according to the ontology associated with the Austrian tradition. Keynes’ problem, too, is uniquely his own.

The theories of Hayek and Keynes would be commensurable only if the problem of economic fluctuations could be characterized in a way that is independent of the particular theoretical conceptions that each research tradition has devised to both formulate and solve the problem. More carefully, the theories of Hayek and Keynes would be commensurable only if a characterization of the problem amenable to members of both traditions could be found that depends upon neither the Austrian theory of capital nor Keynes’ theory of unemployment.[[50]](#footnote-51)

**5 Concluding remarks**

The Hayek-Keynes debate remains unresolved largely because there is no decision algorithm that yields a uniquely rational choice between the theories. The nature of the theories is such that each can be preserved come what may by way of empirical evidence. What’s more, the methodological rules of one tradition are mostly in opposition to the rules of the rival tradition and, thus, the application of such rules does not permit a single rational choice to emerge.

The arguments of the present paper would seem to leave two possibilities open for business cycle research, namely, either nihilism or pluralism: either we can give up the business cycle project entirely or we can open it up to different approaches both old and new. A methodological nihilist might argue that a business cycle theory always represents an infinitesimal proportion of the premises necessary to deduce (even) a pattern prediction. The other premises reflect considerations of, e.g., multifarious political and financial conditions, institutional arrangements, and perhaps even the psychologies of individual market participants, all of which fall outside the traditional purview of economic science. Discovering all of these premises is beyond the abilities of cognitively-limited human beings. The blame for an apparent predictive failure can always be laid at the feet of one or more of these extra premises (or attributed to our failure to adduce all of the relevant premises): the impossibility of testing predictions about the business cycle, goes the nihilist’s argument, is a permanent condition. In offering such an argument, the nihilist is essentially extending to the variables themselves Hayek’s arguments concerning the impossibility of gathering the data necessary to deduce a precise prediction. The nihilist’s argument is that, far from possessing knowledge of all of the relevant data, we lack knowledge of all of the relevant variables – we don’t even have enough components to make out a ‘pattern’ – and forever shall it be.

Of course, the nihilist makes the further inference that, as a consequence, business cycle theorizing should be scrapped altogether. I want to suggest that this latter inference is drawn too hastily. Beyond the thesis that there is no decision algorithm that yields a uniquely rational choice between the theories of Hayek and Keynes (and thus, on the assumption that other explanations are mere ‘footnotes’ on the latter accounts,[[51]](#footnote-52) between business cycle theories in general), we simply do not know at present what the value of business cycle theory is or will be in the long run. Macroeconomics grew up in the age of a philosophy of science that has subsequently been revealed to have focused too exclusively on prediction and the unity of scientific method. To put an end to theorizing about the business cycle is to make a final assessment before other methods less focused on prediction have been tried.

This leaves methodological pluralism as the best path moving forward in business cycle research. However, given the arguments of the present paper, it must be a pluralism that is alive to the predictive limitations inherent in its object and which, ideally, encourages a more liberal attitude among macroeconomists with respect to rival traditions. Methodological pluralism in the absence of both humility with respect to the accomplishments of one’s research tradition and tolerance of competing approaches is likely to lead to the same sort of cleavages that have repeatedly divided the discipline over the last century, and which, if the argument of the present essay is sound, cannot have been motivated by empirical considerations. Indeed, given the perennial survival of fringe heterodox traditions, one could reasonably argue that pluralism has always been the actual (meta-) methodology of economics. It could not be argued, however, that this pluralism has been practiced with an abundance of epistemological tolerance and humility. It seems not undue to speculate that avoiding such cleavages in future requires a methodological liberalism to which economists have in the past worshipped with, at best, insincere piety.

It remains to briefly consider the political implications of the present argument. If the argument is sound, then belief in the effectiveness of the policy guidance associated with either theory is empirically unfounded. Indeed, the final upshot of the argument may just be that – at least with respect to policies related to the amelioration of economic fluctuations – political belief is and, indeed, must be, more like religious faith than scientific belief.

Nonetheless, one might be inclined to think that the argument is more damaging to the Keynesian perspective on the grounds that it seems to justify one of Hayek’s lifelong concerns, namely, the need for caution in the political administration of economic – especially monetary – affairs.[[52]](#footnote-53) That is, one might think that, because we are ignorant of the effects of our monetary machinations, the best counsel is to proceed carefully. However, this latter argument depends on an implicit and disputable premise, namely, that political caution is the proper response to political ignorance. There is nothing in the argument of the present paper that substantiates this latter premise. Indeed, on the narrow basis of what has been said here, one might just as easily reason that the argument is more damaging to the Hayekian perspective on the grounds that it is consistent with Keynes’ belief that governments should *do something* (anything!)to alleviate the worst effects of economic stagnation. One might infer that, because we are largely ignorant of the relevant effects of stimulus policies, we lack any legitimate excuse for not trying anything that might make the situation better. Of course, this latter inference depends upon the no-less-debatable premise that the proper response to political ignorance is something like political haste.

All of this is just a way of saying that the present argument establishes the manifest fact of political ignorance with respect to the cycle, but by itself, bears no particular implications for policy action. It is also a way of suggesting that, in the final reckoning, the debate between Hayek and Keynes and their respective followers might just reduce to another question that the empirical evidence is powerless to decide, namely, the question of whether, under conditions of pervasive ignorance of the appropriate economic theory, it is or is not true that only fools rush in where angels fear to tread.

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1. The phrase ‘the Hayek-Keynes debate’ (and its cognates) is used in two different, but closely related, senses in the present paper. In a narrow sense, the phrase is used to indicate the short, acrimonious debate between the two principals that was published in *Economica* during late 1931 and early 1932. In a more broad sense, the phrase indicates the longer – indeed, ongoing – debate between the respective followers of the two principals. [↑](#footnote-ref-2)
2. \* Email: scott.scheall@asu.edu [↑](#footnote-ref-3)
3. The question was originally posed at the beginning of Hicks’ (1967, 203) discussion of ‘The Hayek Story’ in *Critical Essays in Monetary Theory*:

   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? [↑](#footnote-ref-4)
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? [Italics in the original]

   Rothbard (*Ibid*.)answers this question to the effect that, ‘[c]learly, the only possible way of resolving the issue is in the realm of pure theory—by examining the conflicting premises and chains of reasoning’. This latter thesis is taken up in a fashion in the latter part of the essay. Suffice it to say here that ‘examining the conflicting premises and chains of reasoning’ raises epistemic difficulties no less considerable than does an empirical appraisal of the implications of the theories. Rothbard’s solution solves little, but merely pushes the lump to another part of the rug. [↑](#footnote-ref-5)
5. For more on the distinction between holist and contrastive forms of underdetermination, see Stanford (2009):

   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.

   I’m inclined to think that the distinction between contrastive and holist underdetermination marks a distinction without much of a difference. The present paper does not aim to reify these varieties of underdetermination. As Stanford writes, ‘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’. 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 is useful in the present essay for heuristic reasons. [↑](#footnote-ref-6)
6. The current essay is as much an exercise in autobiography as it is in methodology. It is important then to explicate the presuppositions upon which the argument is based. In particular, the reader is advised that the author was once a true-believing Hayekian about industrial fluctuations. A combination of casual empiricism plus (it cannot be denied) a pre-theoretical bias against the wisdom of government officials led the author to the conclusion that Hayek was right. However, the economic events of the current century and, especially, the running commentary among professional economists concerning these events, made it impossible to ignore the fact that, *regardless of what the evidence seemed to indicate*, discussants virtually neverconceded ground to adversaries. This circumstance forced me to consider the possibility that my own position on the matter was less a result of the persuasive powers of empirical evidence than a reflection of my confessed pre-theoretical bias. Extensive contemplation of this quandary led me to the rather dismal conclusions of the present essay. [↑](#footnote-ref-7)
7. See, e.g., Mankiw (2008), Murphy (2008), Krugman (2008), Woods (2009), Krugman (2009), Horwitz (2010), Kates (2010), Rizzo (2010), Bateman, Hirai, and Marcuzzo (2010), Bateman (2010), Krugman (2011), Murphy (2011), Chapters 17 and 18 of Wapshott (2011), Krugman (2012). [↑](#footnote-ref-8)
8. It should be emphasized that the paper is not intended as a criticism of either Hayek or Keynes. The import of the argument is more that the development of a testable explanation concerning economic fluctuations, i.e., of an explanation that covers the important assumptions that testability requires, is probably beyond the cognitive abilities of mere mortals and not that Hayek or Keynes erred in failing to provide such explanations. [↑](#footnote-ref-9)
9. This is perhaps the appropriate place to explain why the various successor traditions in macroeconomics – e.g., monetarism, the rational expectations school, and Real Business Cycle theory – are not considered here alongside the theories of Hayek and Keynes. It is assumed in the present paper, although no attempt is made to defend the assumption, that there is some substance to Mario Rizzo’s (2010) comment that ‘The great debate is still Keynes versus Hayek. All else is footnote’, and, thus, that for the purposes of the present paper, it is safe to treat these successor traditions as mere ‘footnote’ in Rizzo’s sense. [↑](#footnote-ref-10)
10. See Hoover (2006, 95) [↑](#footnote-ref-11)
11. Perhaps the single best treatment of the relationship between Keynes’ theoretical work and both his popular writings and political activities is Clarke (1988). Also see Moggridge and Howson (1974), Hirai (2008), Clarke (2009), and the relevant volumes of Keynes’ *Collected Writings*. [↑](#footnote-ref-12)
12. For example, consider Leijonhufvud’s (1968, 404-405) argument that Keynes’ followers misinterpreted as a universal dictum Keynes’ context-sensitive pessimism concerning the efficacy of monetary policy: ‘These time- and place-bound considerations naturally cannot without further ado be invoked in judgingthe prospective success of monetary measures, say, in the United States of the sixties’. Also consider Bateman ‘s (2006, 289n) argument that Keynes’ suggestion that interest rates be held low for an indefinite period can be excused on the grounds of its context sensitivity. [↑](#footnote-ref-13)
13. Moreover, for the reasons considered at length in the text of the paper, Keynes’ more context-sensitive policy suggestions cannot be retrospectively tested in the context in which they were originally proposed: their apparent failure in context would not compel their rejection for such failure could always be attributed to the same practical considerations that undermine the testability of Keynes’ less specific policy *dicta*. [↑](#footnote-ref-14)
14. Indeed, the variety of different interpretations of the policy significance of Keynes’ theory is rather staggering. Meltzer (1981) discusses several of these readings before adding his own ‘different perspective’. See also Chapter 14 of Hirai (2008) for an overview of various interpretations of Keynes. [↑](#footnote-ref-15)
15. That Functional Finance is not explicit in Keynes is undoubtedly true; that it is not implicit in Keynes is more doubtful. Lerner ([1943] 1983, 296) attributes the first explicit formulation of the concept to Alvin Hansen, but notes that ‘the new fiscal theory…was first put forward in substantially complete form by J.M. Keynes in England’. The question of the relation of Keynes’ theory to Functional Finance seems to concern the extent to which the former licenses deficit spending as a means to achieve the end of full employment. According to Bateman (2006, 275), ‘as regards the policy most associated with Keynes’s name, deficit spending by the central government, his views were relatively consistent over time: the fact is that he rarely explicitly supported such a policy’. But, see Clarke (1988, 287), who quotes Keynes in a 1933 radio broadcast arguing – in concert with the principles of Functional Finance – that the Exchequer’s priorities should be to ‘[l]ook after the unemployment, and the budget will look after itself’. That said, according to Clarke (2009, 147), this is ‘virtually the only endorsement [Keynes] ever gave to running a fiscal deficit on current account’. For more on Keynes’ opinion of Lerner’s policy suggestions, see Colander (1984). [↑](#footnote-ref-16)
16. I.e., some combination of policies with respect to the government’s ‘spending and taxing, its borrowing and repayment of loans, its issue of new money and its withdrawal of money’. (Lerner [1943] 1983, 298) [↑](#footnote-ref-17)
17. Hansen (1953, 104) lists other ‘offsetting factors which may nullify (or intensify) the original impetus’ from an increment of additional investment and 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’. However, taking such factors into account requires some guidance on how to identify their offsetting effects, which Hansen does not provide. Apparent failures of the theory that the value of the multiplier is greater than unity can be attributed to some combination of either miscalculation of its value or effects of the just-mentioned ‘offsetting factors’. That is, there is no evidence that compels the rejection of the notion of a greater-than-unity multiplier. Moreover, this non-testability of the proposition of a greater-than-unity multiplier is clearly *a problem for Keynes’ theory*, which (regardless of the status of Functional Finance) depends crucially on the multiplier concept; see note 23 below. [↑](#footnote-ref-18)
18. In order to qualify as legitimately Keynesian, a policy must be designed in a way that accords with the requirements of Keynes’ theory. Moreover, even the best-designed policy is likely to fail if it is notcompetently implemented and administered. Any policy placed under the control of inept, corrupt, or recalcitrant managers, is unlikely to work as designed. [↑](#footnote-ref-19)
19. See Krugman (2012, 154) for the argument that stagflation does not falsify Keynes’ theory. [↑](#footnote-ref-20)
20. This is perhaps the appropriate place to discuss the epistemic problems of the liquidity trap concept. The secondary literature reflects considerable disagreement concerning both the real-world significance of the liquidity trap notion and its proper place in Keynes’ economics. On the latter question, see Keynes ([1936] 1973, 207), Leijonhufvud (1968, 410-411), Jackman (1974, 270), Bateman (2006, 289n), and Hirai (2008, 194-198). In any case, according to believers in the liquidity trap, a situation can arise in which the demand for money becomes infinitely elastic (i.e., the money demand curve becomes horizontal), an indication that expansionary monetary policy will not lower interest rates and, thus, will not serve to stimulate investment. However, the evidence of an apparent failure of monetary policy is neutral between the possibility of a liquidity trap and other explanations. One more inclined to Hayek’s view might attribute the ineffectiveness of monetary stimulus to either the ignorance, administrative incompetence, or moral corruption of the monetary authorities. Otherwise, a failure of monetary stimulus might be attributed to a spontaneous rightward shift in a downward-sloping money demand function. Generally, without a statement of some observable conditions that distinguish a liquidity trap from other possible explanations of the failure of monetary stimulus, we have no way of identifying a liquidity trap. [↑](#footnote-ref-21)
21. For other interpretations of Keynes’ *General Theory*-era policy advice, see Harrod (1951, 461), Leijonhufvud (1968, 345-349), Moggridge and Howson (1974, 231-232, 245-246), Meltzer (1981), Bateman (1996, 150-164), …. [↑](#footnote-ref-22)
22. Bateman’s (1996, 150-164) interpretation of Keynes’ policy position does not fit neatly into this schema, but it is no more testable for this. According to Bateman, Keynes held that the road to full employment is paved with good conventions. If only the (primarily, monetary) authorities could convince the public of their commitment to both maintain low long rates of interest and stabilize (private and public) investment, then new social conventions would come to be established regarding these matters, and full employment would follow as a consequence. In other words, Bateman has is that Keynes believed that good economic policy consisted of the adept management of public expectations regarding the rules that policymakers follow in making policy: ‘If a conventional outlook could be established that the economy would run at full tilt, then [Keynes’] new policy conventions would be successful’ (*Ibid*., 161). Notice that, while this interpretation alleviates much of the epistemic burden on policymakers to possess adequate quantitative data concerning existing economic conditions, the end to which policy is applied, and the value of the multiplier, it expands the epistemic burden involved in the qualitative aspects of the design, implementation, and administration of economic policy. On Bateman’s interpretation, successful policy depends on the effective manipulation of expectations, which requires some understanding of a) existing expectations, b) the expectations necessary for full employment, and c) how to get from a) to b). Of course, the apparent failure of some attempt to establish the new conventions necessary for full employment can always be attributed to the failure of policymakers’ knowledge with respect to some combination of a) – c) and Bateman’s interpretation of Keynes’ policy advice can be retained come what may. [↑](#footnote-ref-23)
23. See Keynes ([1936] 1973, 377-378) for the extent of his elaboration of the policy implications of *The General Theory*. [↑](#footnote-ref-24)
24. Indeed, Keynes ([1936] 1973, 128) himself opens the door to this escape route from apparent predictive failure. In considering an estimate of the multiplier based on Simon Kuznets’ groundbreaking statistical work, Keynes begins by arguing that the available statistics aren’t accurate enough to infer more than approximate estimates of the relevant values, but he then proceeds to infer rather precise estimates from these ‘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 apparent failure of his theory to accord with the data: either American corporate practices of the day are to blame or the statistics are wrong. Indeed, every possibility is considered but that Keynes’ theory of a greater-than-unity multiplier is wrong. [↑](#footnote-ref-25)
25. ‘It would need a volume of a different character from this one to indicate even in outline the practical measures in which [specific policy recommendations] might be gradually clothed’. (Keynes [1936] 1973, 383). [↑](#footnote-ref-26)
26. See, e.g., *Ibid*., 129 [↑](#footnote-ref-27)
27. *Ibid*., 129; Clarke (2009, 170) describes these as ‘satirical’ suggestions. [↑](#footnote-ref-28)
28. To offer a trivial example, secretly scuttling a treasure ship carrying billions of dollars in booty in the Marianas Trench in the hope that consumers and investors will somehow 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. But, of course, without some criteria for distinguishing cases like these from those the failure of which is due to theoretical shortcomings, we’re *always* in the position of being unable to infer the failure of Keynes’ theory on the basis of the failure of a stimulus program. [↑](#footnote-ref-29)
29. It should perhaps be emphasized again that this latter point is not intended as a criticism of Keynes. It is doubtful that it is an economist’s responsibility to engage in detailed theorizing about policy design. Nonetheless, the point remains: if we are to test a theory according to which the right policy can lead to the right results, we need to know quite a lot about what constitutes the right policy for any given set of conditions we might confront; without such information, we’re in no better position with respect to testing than the scholastic philosophers who asserted that the right number of angels could sit comfortably on the head of a pin. [↑](#footnote-ref-30)
30. For an example of an application of this *ad hoc* strategy to a real-world case, see chapter seven (‘Anatomy of an Inadequate Response’) of Krugman (2012). See especially the quote from Joseph Stiglitz (2009; quoted in *Ibid*., 118)offered therein: ‘I think there is a broad consensus but not universal among economists that the stimulus package [i.e., the 2009 American Recovery and Reinvestment Act] was *badly designed and not enough*’ [Italics added]. It must be emphasized – contra Krugman’s implicit argument – that the fact that he, Stiglitz, and certain other Keynesian economists apparently predicted in advance the inadequacy of the American Recovery and Reinvestment Act does nothing to undercut the *ad hoc*ness of the strategy. The explanation that some stimulus measure failed because of political rather than theoretical shortcomings allows Keynes’ theory to be retained come what may; this fact is not changed because the explanation might be presented *ex ante* rather than *ex post* of said failure. To put the point another way, the fact that it is apparently so easy for well-intentioned policymakers to err in applying Keynes’ theory bodes ill for its testability. [↑](#footnote-ref-31)
31. Hayek ([1933] 2008, 87-88): ‘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. [↑](#footnote-ref-32)
32. Hayek ([1931, 1935] 2008, 303). [↑](#footnote-ref-33)
33. Moreover, Hayek ([1933] 2008, 103) was clear that the eradication of the cycle, even if it could be made practicable, would have profound drawbacks of its own and should, perhaps, not be the main focus of monetary policy. [↑](#footnote-ref-34)
34. Hayek (1976) came to advocate the ‘denationalization’ of money. [↑](#footnote-ref-35)
35. Such an assumption would not be appropriate where those real (i.e., non-monetary) 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. [↑](#footnote-ref-36)
36. That is, on the (probably false) assumption that there is some meaningful way of determining the severityof a credit-induced boom-bust cycle. [↑](#footnote-ref-37)
37. Even David Hume was susceptible to this fallacy of the purport of non-temporal predictions. With respect to a context not too far removed from one that concerned Hayek, Hume ([1752] 2007, 105-106n) – in what Eugene Rotwein ([1955] 2007, lxxxvi) accurately described as an ‘escape-clause’ from eventual predictive failure – wrote:

    One would incline to assign to this event a very near period, such as half a century, had not our father’s prophecies of this kind been already found fallacious, by the duration of our public credit, so much beyond all reasonable expectation. When the astrologers of France were every year foretelling the death of Henry IV, *These fellows*, says he, *must be right at last*. We shall, therefore, be more cautious than to assign any precise date; and shall content ourselves with pointing out the event in general. [Italics in the original]

    [↑](#footnote-ref-38)
38. A parallel problem arises for Keynes’s theory. We are told by Keynes (see, e.g., [1936] 1973, 306-309) that certain consequences will follow the implementation of appropriate stimulus policies in the short-run and others in the long-run, 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 is no criterion for distinguishing the short period from the long period. Hansen (1953, 33) considered the ‘relatively short-run’ capable of lasting ‘one, two, or three decades’. [↑](#footnote-ref-39)
39. This section is exclusively concerned with Hayek’s treatment of pattern predictions and not with any other conception that might go by the same name in the philosophical literature. [↑](#footnote-ref-40)
40. A potential topic for future inquiry concerns Hayek’s apparent ignorance of the Duhem-Quine problem. Given that Hayek was active in philosophy of science at the time that Quine’s (1951) ‘Two Dogmas of Empiricism’ reawakened the profession to the problems of underdetermination, there is a surprising absence of any recognition of the Duhem-Quine problem in Hayek’s philosophical *oeuvre*. It is little exaggeration to say that the latter problem was the central issue in philosophy of science at the time that Hayek was most active in the field (and writing about subjects relevant to underdetermination like Popper’s philosophy of science, falsifiability, and pattern predictions), yet one searches Hayek’s philosophical writings in vain for any explicit acknowledgement of the problems of underdetermination. Indeed, it is the argument of the present part of the text that the non-testability of pattern predictions as Hayek defines them is a trivial consequence of Duhem-Quine. The fact that Hayek, widely acknowledged as a social-scientific methodologist of considerable skill, failed to address the significance of underdetermination for economics is a mystery. [↑](#footnote-ref-41)
41. Hayek ([1964] 1967, 9) came close to recognizing this fact, but failed to draw what seems to be the appropriate conclusion regarding the non-testability of pattern predictions.

    . [↑](#footnote-ref-42)
42. Krugman (2012, 117) unfurls both *ad hoc* strategies in the course of two adjacent sentences. Regarding the 2009 American Recovery and Reinvestment Act, he writes ‘[u]nfortunately, the bill…was *too small for the job*. It surely *mitigated the recession*, but it *fell far short of what would have been needed to restore full employment*, or even to create a sense of progress’ [Italics added]. It should be emphasized that it is no part of the present argument that such claims can never be true. The problem is that, because of the lacunae detailed here, we can never know in the context of the Hayek-Keynes debate whether such claims are true or not, and it is this latter fact that makes their assertion universally dubious. Such claims are not far removed from, for example, the coach who asserts that his team would have won the championship game if only his star player had not been injured in the first half. Perhaps there’s some sense in which such counterfactual assertions can be true, but without some practicable means of distinguishing truth from falsity, they are little more than meaningless. [↑](#footnote-ref-43)
43. 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* evidenceas the claim that success is due to elements of private enterprise. In a modern, complex economy in which private and public factors comingle, a rational decision whether some evidence better supports claims about the causal influence of public rather than private factors can be made only on the assumption that it is possible to non-arbitrarily distinguish public activity from private. But, such an assumption is dubious: for example, how should we classify the influence of an ostensibly private but heavily regulated industry which, for better or worse, would have a different impact in the absence of such regulation—is it more legitimate to treat its effects as part of private enterprise because it is privately owned or as part of the public sphere because it is highly regulated? Such a question is of supreme relevance in the context of the (American) Great Recession, where so much blame has been placed – by parties of all political persuasions – at the feet of privately-owned-but-highly-regulated financial institutions, which operate in a world where the money supply is controlled by a quasi-governmental central bank. It doesn’t seem that we can non-arbitrarily distinguish the causal impact of the private sphere of economic activity from that of the public sphere. [↑](#footnote-ref-44)
44. Keynes said this of the *Treatise on Money*, but it is relevant to *The General Theory* as well, given that no more prominent treatment of capital is offered in the latter book. [↑](#footnote-ref-45)
45. G.R. Steele (2001, 22) offers the standard Austrian view that ‘Keynes’s own treatment of capital in *The General Theory*…is inadequate to the task of dealing with the problems of a money economy’. [↑](#footnote-ref-46)
46. It is not necessary to accept my particular taxonomy of the ampliative rules accepted by Austrians and Keynesians in order to see the relevant point. For example, one might reject the classification of Austrians as ‘realists’ and Keynesians as ‘simplicitists’, but still acknowledge that the differences in the methodological preferences of the two schools are, without some empirical means of deciding between them, intractable. [↑](#footnote-ref-47)
47. See, e.g., Hayek ([1964] 1967); Steele (2001, 188-189) offers a clear statement of the contemporary Austrian argument against statistical and econometric methods. [↑](#footnote-ref-48)
48. 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] [↑](#footnote-ref-49)
49. 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. [↑](#footnote-ref-50)
50. 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’. Needless to say, for all the reasons discussed in the present paper and more, there are few grounds for optimism here. [↑](#footnote-ref-51)
51. See footnote 8 above [↑](#footnote-ref-52)
52. In general, Hayek’s ‘vision suggests an explicitly evolutionary approach to social cohesion that is necessarily retrospective’.

    There are no means to identify in advance the rules or safe actions that are likely to enhance the prospects for survival. The justification of traditional practices is that they are plausible in having passed the test of time; but there is no suggestion of their immutability. Proposals for reform are not inconsistent with the value placed upon custom and tradition, but political action must proceed with caution to enable some account to be taken of the limitless unintended consequences. (Steele 2001, 36) [↑](#footnote-ref-53)