Symposium on Scott Scheall's
F. A. Hayek and the
Epistemology of Politics:
The Curious Task of Economics
SYMPOSIUM ON SCOTT SCHEALL’S
F. A. HAYEK AND THE EPISTEMOLOGY
OF POLITICS: THE CURIOUS TASK OF
ECONOMICS

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Editorial Introduction

WILLIAM N. BUTOS
Deputy Editor, COSMOS + TAXIS

Scott Scheall’s recent book, *F.A. Hayek and the Epistemology of Politics: The Curious Task of Economics*, merits serious consideration for social scientists and, indeed, philosophers. The book is an exploration of an important problem that has largely been ignored: the problem of policymaker ignorance and the consequences of limited political knowledge.

Professor Scheall explores the possibilities for effective political action as constrained by policymakers’ epistemic limitations. The book explains why policymaking often fails and why constituents, whatever their political affiliations, are so often disappointed with political leaders. In this philosophical examination of his work, Hayek’s ideas are not merely discussed, analyzed, and contextualized, but extended; the book both draws and defends previously unrecognized implications from the Hayekian canon.

This Symposium has drawn the interest of an invited group of scholars from the U.S. and Europe with the charge of critically assessing the book and its significance. Their papers are original, substantive, and critical. The Symposium starts with a “Prologue” by Scheall that provides the reader with a concise summary of the book, followed by papers by luminaries Peter Boettke, Irwin Dekker, Roger Koppl, and Eric Schliesser, and thorough “Responses” by Scheall to his contributors.
I am delighted for the chance to reply to the challenging and constructive comments of my friends and colleagues, Peter Boettke, Roger Koppl, Erwin Dekker, and Eric Schliesser, concerning my book, *F. A. Hayek and the Epistemology of Politics: The Curious Task of Economics*. I would like to thank Bill Butos for his time and efforts in organizing this symposium.

It may help readers of this symposium who have yet to read the book to have a summary of its central arguments, before they engage with the comments of my critics.

In the first instance, the book is meant as a contribution to the philosophy of Austrian economics. However, the book has another ambition beyond this ostensible purpose, namely, to suggest the need to re-orient political inquiry around a unique conception of politicians as, in some ways and to some extent, *ignorant*. The standard conceptions of policymakers employed throughout the history of political analysis treat them as either *altruistic* or *knavish*, but always as *knowledgeable*. Whether they are conceived as aiming to realize their constituents’ ends or their own ends, politicians are assumed to always, somehow, *know enough to succeed*. The one significant exception to these standard conceptions can be found in the criticisms of the Austrian economists, especially Ludwig von Mises ([1920] 1935; [1922] 2009) and F. A. Hayek ([1935a] 1997; [1935b] 1997; [1940] 1997; [1945] 2014; [1975] 2014), of centrally-planned socialism and of other forms of interventionist economic policymaking. Mises and Hayek made no assumptions concerning the selfishness, self-interestedness, or moral probity of policymakers, but started instead from the assumption that it was an open question whether economic policymakers possessed the knowledge required to realize relevant policy goals. Mises and Hayek then provided reasons to think that, in the particular contexts of economic policymaking they considered, this open question could only be be answered in the negative. In effect, *F. A. Hayek and the Epistemology of Politics* argues that the Austrian notion that politicians are not epistemically privileged and may well be epistemically deficient—that it is, at least, always an open question whether policymaker knowledge is adequate to some policy objective—makes a better starting assumption for political analysis than does the standard assumption that policymakers are somehow epistemically special, and should be adopted throughout political inquiry.

I argue that the reason it is advisable to amend the conceptions of policymakers assumed in political analysis concerns a fully general but little recognized fact about human decision-making: the nature and extent of our ignorance concerning various courses of action serve to determine which
courses of action appear to us as options worth consciously considering, and the degree to which we are initially motivated, or incented, to pursue them (Scheall and Crutchfield 2020). Human decision-making always moves from reflection (if perhaps only sub- or un-conscious reflection) on what we know to what we ought to do according to some normative (moral, prudential, pecuniary, etc.) criteria. This is the logical priority of the epistemic. Ignorance constrains decision-making.

I offer two arguments in the book (pp. 21-24; also see Scheall and Crutchfield 2020) for the logical priority of the epistemic.³

The first argument is introspective: reflection on our own thought processes reveals that the options that appear to us in consciousness rarely, if ever, encompass all of the courses of action that could be pursued in the relevant decision context. The options from which we choose in any given context have been filtered and ranked prior to reaching consciousness, seemingly, according to the relative weight of their comparative epistemic burdens, i.e., according to the nature and extent of the ignorance that must be overcome, of the knowledge that must still be learned, in order to deliberately pursue a course of action to its end. Courses of action that we take ourselves to be relatively more ignorant about either do not consciously appear to us as options or are ranked lower in our initial incentive structures below courses of action about which we take ourselves to be comparatively more knowledgeable.

The second argument for the logical priority of the epistemic concerns the meaning of the word “can” in principles like ought implies can and logically weaker variants.⁴ I do not take a position on the truth of ought implies can as opposed to one of these weaker alternatives. Rather, I argue that whatever the correct logical relationship between ought and can, if the correct principle is to be practically useful, the word “can” must mean deliberately can. But, “deliberately can” just means knows enough to. Thus, ought implies (or whatever) knows enough to.

If either of these arguments is sound, then decision-making proceeds from what is known (and is, therefore, constrained by what is not known, i.e., by ignorance) to what ought to be done in some moral, prudential, pecuniary or other normative sense, and not from normative considerations in isolation without prior epistemic, if perhaps only unconscious, evaluation.

That decision-making proceeds from what the subject knows to what the subject ought (in some sense) to do is an important element of any explanation why we survive, inasmuch as we do, in a complex world. We rarely, if ever, fail to pre-consciously reflect on relevant epistemic burdens, such that we afflict ourselves with obligations that we cannot fulfill without the intervention of luck, fortune, or other spontaneous forces. That we filter and pre-rank courses of action according to our relevant ignorance helps us avoid disasters personally and, by extension, evolutionarily.

However, in cases of surrogate decision-making, where someone decides on behalf and supposedly in the interests of someone else, we regularly saddle ourselves (and others) with purported obligations, without first considering whether related actions are within the supposed obligee’s ken and control (Crutchfield and Scheall 2019). The so-called obligations of surrogate decision-makers presuppose a condition that may not obtain, indeed, that probably rarely obtains, in surrogate settings, namely, that the surrogate knows what is in the interests of the surrogated and knows how to effect actions that promote these interests, or, otherwise, that the surrogate knows what the surrogated would decide, if they could. The alleged obligations of surrogates assume no gap—a gap that typically exists, in fact—between what the surrogate decision-maker knows and what the person surrogated knows about the latter’s interests, or would-be decisions.⁵ There is no mechanism, as there is in cases of personal decision-making (inasmuch as a person knows their own mental states), for avoiding disaster.

Of course, policymakers are surrogate decision-makers: they are expected to decide on behalf and in the interests of their constituents. This is true, in varying degrees, in all forms of government. Whether or not they actually do in practice, even autocrats are expected to look out for the interests of the people (and are frequently criticized when they do not). This is not to deny that history is chock-full of politicians who simply did not care about citizens. The point is that, even if they had cared, there is no obvious mechanism that could have provided them with the knowledge required to do so effectively. Even where the people are...
supposed to be sovereign, voting mechanisms and other institutions of modern democracy provide but a pail reflection of the knowledge policymakers require to decide as good surrogates are supposed to decide (DeCanio 2014). There is no mechanism for avoiding disaster-via ignorance in political decision-making.

The upshot of all of this for political inquiry is that the standard conceptions of policymakers as either altruistic or knavish put “the normative cart before the epistemic horse” (Scheall 2019, p. 43) that drives decision-making. Whether policymakers (and persons, more generally) are altruistic or knavish is not some brute fact. The extent to which people are altruistic or knavish is a function of their ignorance, inter alia. Where policymakers know everything that they need to know to realize goals associated with their constituents’ interests or demands, there is less scope for non-constituent-minded policy pursuits. Where policymakers are ignorant, in whole or in part, of knowledge necessary to realize their constituents’ interests or demands, it is easier—policymakers face a greater incentive—to not be constituent-minded.

What’s more, if policymakers recognize that they are ignorant of knowledge required to realize goals in their constituents’ interests or to satisfy constituents’ demands, if they recognize that such pursuits are likely to end with frustrated constituents, they are unlikely to even try to satisfy their constituents. Other things equal, such policymakers are likely to pursue other, less epistemically burdensome, less constituent-minded, goals. If policymakers understand that sincere pursuit of constituent-minded goals is likely to end in failure and recognize that they can accrue similar benefits by simply pretending to pursue constituent-minded goals, while incurring less weighty epistemic burdens, then they are more likely to engage in political playacting than to sincerely pursue constituent-minded goals.

Methodologically speaking, we can enrich political analysis by discarding the standard conceptions of policymakers drawn in unfalsifiable moralistic terms and by starting instead from a conception of policymakers as epistemically limited that, I argue, lends itself to empirical analysis. That is, we can ask what theoretical knowledge and empirical data policymakers would have to possess in order to deliberately realize various policy objectives. We can also investigate whether such theories and data are in fact available to policymakers. And we can ask about the consequences of a shortfall between the knowledge required and the knowledge available. In particular, we can ask whether there are any relevant spontaneous forces that might either provide the missing knowledge or otherwise compensate for the goal-defeating consequences of policymaker ignorance. Indeed, given that constituent-mindedness varies directly with policymaker knowledge (and inversely with policymaker ignorance), we can use what we learn through such analysis as a proxy for the question of policymakers’ altruistic versus knavish motivations. It is at this point that traditional approaches to political analysis, based on the standard conceptions of policymakers, properly qualified for policymaker ignorance, return to the fore.

Practically speaking, if we want to either avoid political disaster or just mitigate political failure, we should reason in politics more like we do in our personal lives. Before we start indiscriminately assigning obligations to policymakers to pursue various policy objectives, we should try to get some empirical purchase on the limits that policymaker ignorance places around what can be deliberately realized and, concomitantly, we should try to achieve greater understanding of the spontaneous forces required to realize goals when policymakers are to some degree ignorant of relevant knowledge. Any obligations attributed to policymakers ought to be things that are to some degree within their ken and control.

However, notice that this conclusion implies nothing about the particular policies that might be to some degree within (or without) the ken and control of relevant policymakers. It implies nothing about the political philosophies or systems that may or may not be deliberately realizable. In a world of expansive policymaker knowledge or in a world where relevant spontaneous forces operate effectively, more policy goals and political systems will be realizable than in a world of limited policymaker knowledge and anemic spontaneous forces. What kind of political-epistemological world we occupy at any given time and place is an empirical question, not one that can be answered a priori of the required analysis. Thus, in the absence of analysis based on the conception of policymakers as ignorant—analysis that the book recommends, but does not carry out—the book is officially agnostic about policy advice.
However, even if the results of such an analysis were in hand, nothing would follow directly about what ought to be done politically. I argue that ignorance is logically basic, not that it is the only consideration that matters, in human decision-making. Pre-conscious reflection on epistemic considerations comes before the application of moral, prudential, pecuniary, etc., considerations to a range of options, but these latter considerations are nevertheless critical to determining choice. As in a personal decision, in order to make a policy decision, it is still necessary to apply relevant normative criteria to the options that have survived the pre-conscious culling of courses of action. Thus, nothing like a defense of a particular political philosophy or policy recommendations can be inferred from the central argument of the book. Anyone who feigns to find advice in the book other than Humean skepticism in the absence of the suggested analysis is reading more normativity into the book than is implied by its central argument.

The first chapter ends with a taxonomy of ignorant policymakers and of the effects of various kinds of policymaker ignorance (pp. 27-29). Most of the first chapter considers policymakers who know they are ignorant, to some degree, with respect to some policy objective. Such policymakers confront an incentive, other things equal, to pursue other objectives. If policymakers take themselves to be ignorant with respect to constituent-minded policy goals, so much the worse for constituents, who will get less constituent-minded policymaking than if policymakers possessed more relevant knowledge. Policymakers might instead be ignorant of their knowledge with respect to some policy objective: they might not know that, in fact, they know enough to deliberately realize the objective. Such policymakers also face an ignorance-distorted incentive, ceteris paribus, to pursue other objectives, because they (mistakenly) believe that pursuing the relevant goal will end in failure, unless spontaneity intervenes. Hayek described policymakers who are ignorant of their ignorance with respect to some objective as suffering from a "pretence of knowledge": they do not know enough to deliberately realize the goal, but mistakenly believe that they do know enough and, thus, they face an incentive to pursue the goal that they would not face, if they were better attuned to their actual deficient epistemic circumstances. Such policymakers are potentially dangerous: they are incented to pursue goals that they are too ignorant to achieve, the probable failure of which (without spontaneity) they are too ignorant to recognize. Only policymakers who know that they know enough to deliberately realize some objective do not confront ignorance-distorted incentives. The motivations, incentives, reasons, etc., of such "wise captains" of the ship of state cannot be distorted by ignorance, because they are neither first-order nor second-order ignorant. They know that they know.

That policymakers have been exclusively conceived as wise captains of the ship of state throughout the history of political analysis is implied by the fact that the distorting effects of ignorance on the motivations of policymakers (and humans more generally) have not been previously noted. Perhaps needless to say, there are no wise captains of the ship of state and the universal assumption that there are only wise captains has done little for the explanatory value of political analysis. The conception of policymakers as knowledgeable enough to realize their ends, whether altruistic or knavish, is the deus ex machina of existing political thought.

As compared to the complex argument of the first chapter, the arguments of the following five chapters of the book are relatively simply stated.

In the second chapter, I argue that the logical priority of the epistemic is implicit in the Austrians’ arguments against various forms of economic policymaking. The history of the development of Mises’ socialist-calculation argument and, especially, of Hayek’s arguments against market socialism and Keynesian demand management displays a gradual generalizing over time of the contexts to which they applied their political-epistemological analysis. I argue that the Austrians’ political-epistemological approach can be further extended to other policymaking contexts. Once we move away from wise-captain conceptions of policymakers, it is always an open question whether they possess the knowledge to deliberately realize some objective and what the consequences might be, if they do not. Indeed, it is always an open question about human action, in general, whether human actors possess the knowledge required to realize some end and what the consequences may be of their ignorance. The problem of policymaker ignorance is just an instance of a fully general problem of ignorance.
In the third chapter, I turn their political-epistemological approach against the Austrians. Given the
generality of the problem of policymaker ignorance, it is an open question—which, I argue, Austrians have
yet to answer—whether their own preference for liberalizing policies and political liberalism can be deliber-
ately realized, and, if not, whether spontaneous forces exist adequate to overcome the consequences for
liberalization of policymaker ignorance. I argue that policymakers are likely to confront heavy epistemic
burdens in effectively liberalizing relatively illiberal and in sustaining existing liberal societies. Austrians
(and liberals, more generally) have not shown either that these burdens are surmountable by epistemically-
limited human policymakers (i.e., by unwise captains) or that, where policymakers are too ignorant to real-
ize effective liberal states deliberately, spontaneous forces are likely to manifest such states.

Having established the problem of policymaker ignorance as the fundamental problem of political in-
quiry and its full generality across the political spectrum, I turn in the second part of the book to consider
possible methods for both the analysis of the problem, in general and in particular policymaking contexts,
and its possible eventual mitigation in the real world.

In Chapter Four, I consider the general epistemological underpinnings of a tenable political epistemol-
ygy. I argue that, in order to promote intersubjective discussion about policymaker knowledge and igno-
rance, political epistemology must proceed as an empirical discipline and, therefore, that it must be found-
ed on an empiricist general epistemology. I argue that Hayek’s own evolutionary empiricism—which makes
knowledge actionable belief, i.e., the (explicit and tacit) ingredients of plans of action that can be deliberately
realized—fits the required bill. I also argue that Mises’ epistemology, which I interpret as built on an un-
sound rationalistic apriorism, is not tenable as a general foundation for the future development of the Aus-
trians’ political-epistemological approach.

In Chapter Five, I describe an “epistemic-mechanistic” method of analyzing (and, at least in principle,
of mitigating) the effects of policymaker ignorance. We need mechanisms in politics that serve the epis-
temic function that prices serve in market economies. That is, we need mechanisms that provide signals to
relevant actors, policymakers and constituents, that tell them what to do to adapt their plans to changes in
politically-relevant circumstances. I also argue that allegedly democratic governments in which policymak-
ers are ignorant of their constituents’ interests / demands, or are ignorant how to realize associated goals,
fail to respect the principle of popular sovereignty and are, thus, at best, superficially democratic. Constitu-
ents are not sovereign where policymakers lack the knowledge that such sovereignty requires.

In the sixth and final chapter, I describe a “constitutional” approach to the problem that would empiri-
cally investigate policymakers’ knowledge and ignorance to determine what kinds of policy objectives are,
and to what degree, within their ken and control. In principle at least, policymakers might then be constitu-
tionally prohibited from pursuing objectives that, because of their relevant ignorance, they could not posi-
tively contribute to realizing. By limiting policymakers to the pursuit of goals with regard to which their
knowledge was adequate, we would, in effect, make them functionally omnipotent and omniscient.

My expectations for political-epistemological analysis are quite humble. The second part of the book is
intentionally speculative about the future development of political-epistemological inquiry. The suggestions
that I offer are mere suggestions. The main purpose of the book is to expose and analyze the problem of pol-
cymaker ignorance and its many detrimental, if heretofore unacknowledged, consequences. The book will
have served this purpose if the problem of policymaker ignorance is finally recognized for what it is, the
fundamental problem of political analysis and, indeed, of political life.
NOTES

1. The late Professor Gerald Gaus was scheduled to contribute an essay to the symposium. Alas, his untimely passing in the Summer of 2020 was a great loss for the worlds of political philosophy and Hayek studies, not to mention, for the present symposium.

2. The conception of policymakers as altruistic is rarely explicated, yet it has a long history in political thought. That (some) policymakers are altruistic or, at least, that some policymakers place their constituents’ interests above their own personal interests is implicit in (many, if perhaps not all) arguments to the effect that the quality of political life hinges on identifying the right kind of people to serve as policymakers. Of course, the most famous such argument is found in Plato’s (1991) Republic, the fount from which so much subsequent political thought flows. The conception of policymakers as knavish was explicated by David Hume in his “Of the Independency of Parliament.” According to Hume ([1741, 1777, 1889] 1987, 44), “[p]olitical writers have established it as a maxim, that, in contriving any system of government, and fixing the several checks and controuls of the constitution, every man ought to be supposed a knave, and to have no other end, in all his actions, than private interest.” The knavish conception of policymakers is maintained, at least implicitly, by those schools of thought (public choice and constitutional political economy, most famously) that look to Hume for inspiration.


4. Stuart Hampshire (1951) and R. M. Hare (1951, 1963) argue that ought presupposes can. Walter Sinnott-Armstrong (1984) defends ought conversationally implicates can. For further references to other related literature, see Scheall and Crutchfield (2020, p. 5, fn. 8).

5. Inasmuch as individuals are fallible about their own mental states and, thus, about their own knowledge, interests, emotions, etc., it is possible for such a gap to emerge even in personal decision-making cases.

6. On my way of thinking, there is no need to “get inside the heads” of individual policymakers. Once we move away from a conception of policymakers as epistemically privileged, the relevant question is less what individual policymakers know than what theories and data are publicly available. Policymakers are not epistemically privileged. They do not have access to theories and data that are not, at least in principle, available to the rest of us.

7. I erroneously indicated in the book (p. 29) that Plato’s wise captain or “true pilot” of the ship of state appears in Book IV, when, in fact, the wise captain is discussed in Book VI, of The Republic.

8. More carefully, no one is always a wise captain, as this implies true omnipotence and omniscience, but one might occasionally be a wise captain with regard to some particular (probably, only very simple) goals.

9. Hayek was a naturalist about epistemology. However, he accepted the use of mentalistic terms, such as “belief,” in the absence of a full reduction of the mental to the physical (which he thought unlikely, if not impossible). Thus, for Hayek, “belief” is ultimately shorthand for some patterns of physical phenomena in the brain that we cannot, in the current state of scientific knowledge, detail more precisely. To re-state the point that knowledge is actionable belief in the terms of Hayek’s ([1952] 2017) The Sensory Order, one knows inasmuch as the “models” derived from one’s “map” of the world permit successful purposeful action.

10. The epistemic-mechanistic and constitutional methods might be combined. We might inquire into various mechanisms that could provide the signals to both policymakers and constituents that members of each class need to adapt their politically relevant plans, while also investigating the objectives that are, and to what degree, within the ken and control of policymakers.
REFERENCES


Let me start with the obvious, for otherwise we wouldn’t be engaged in this symposium, Scott Scheall’s *F. A. Hayek and the Epistemology of Politics* is an outstanding work in the history and philosophy of economics, and the result of years of sustained study of the works of F. A. Hayek. For students of Hayek, and for students of social philosophy, Scheall’s book is required reading and will be for some years to come.

As anyone who reads the book will learn soon enough, Scheall and I have fundamental disagreements in our understanding of Hayek, and in the implications we draw for building a *progressive research program in the social sciences* (Scheall 2020, pp. 92, 108-11). That last statement might be a bit of an overstatement because Scheall is a philosopher, and as he states throughout his book he is not a social scientist. He has strong opinions about how a Hayek inspired social science should proceed, but he believes in the division of labor so he is inviting social scientists to work along the lines he is suggesting. My work is that of a practicing political economist and social scientist who is trying to give other aspiring political economists a framework for analysis of the social world—past, present and prospective. That alone might explain our differences.¹ But there are also argumentative preferences involved. Scheall finds Menger and Hayek persuasive, and Mises and Rothbard retrogrades. He doesn’t find Lavoie’s efforts in reconstructing and refining the socialist calculation debate particularly helpful nor does he find Lavoie’s efforts to build on the growth of knowledge literature in the philosophy of science persuasive. And he finds my own efforts to steer a course in economics, political economy and social philosophy that draws on these writers and merging them with Buchanan and the Ostroms (both Vincent and Elinor) to be an incoherent rendering of the Hayekian contribution.

I will return to some of these points in what follows, but my intent in this essay is *not* to rehash our disagreements, but to try to stress where I think Scheall has made significant contributions in advancing our understanding of the problem situation that Hayek identified in social science and ultimately why I believe to answer the very questions his epistemic turn demands one must take a radical institutional turn. My appeal in the end is for further development of Scheall’s argument and gains from intellectual exchange that could result putting our differences aside.

Scheall views epistemology—the theory of knowledge—differently from epistemics—the use of knowledge in society. Hayek wrote a very powerful paper, I would argue, entitled “Within and About Systems: A Statement of Some Problems of a Theory of Communication.”² The reason I am highlighting this paper is the differences between
the knowledge of the theorist and the knowledge of the actors within the system under investigation. One of Hayek’s critical contributions was to turn the attention of social scientists to the question of learning by the actors within a system, by studying how they discover and put to use the knowledge they acquire, and revising their actions based on this knowledge and adapting and adjusting based on feedback they received in pursuing their various plans as critical to the operation of the system. Studying that process of the discovery, utilization, and communication of knowledge in society is what theorists do to learn about the operating principles of the system they are examining. The knowledge of the theorist is wholly different from the knowledge of the actor.

Though he doesn’t discuss this during his discussion of the debate between Keynes and Hayek, it would be valuable for the reader to contrast the knowledge assumptions that are made by Hayek, Keynes and the other contesting theorist Robert Lucas (see Scheall 2020, pp. 54-63; 165-170). To Keynes, the actors in the system are “ensnared by the dark forces of time and ignorance”, and as a result they cannot see their way through to coordinate their economic plans with one another. The theorist of the economy does not suffer this problem, and as a result provides the necessary correctives from outside the system. Scheall raises a good challenge to this on a practical level, but I think the discussion misses the fundamental problem. Lucas and the rise of New Classical Macroeconomics challenged the Keynesian hegemony at a fundamental level on the issue of the knowledge assumptions deployed by the theorist. Why would monetary and fiscal illusion persist in the Keynesian model? Rational actors would be able to pierce through those dark forces of time and ignorance precisely because their inability to do so had significant costs that they would have to bear. What economists learned in their microeconomics class had to be applied to what they were learning in their macroeconomics class, and when they did so, the persistence of illusions would be called into question. The upshot was Lucas basically argued that the actors in the economy know what the theorist knows about the operation of the economy, and the consequences of public policy. The corrective effect of the policies would be neutralized. Policy could either be effective but random, or it could be rational but ineffective. The Lucas critique from the core modeling problem to the applied policy level of analysis was devastating to the Keynesian model and resulted in a revolution in macroeconomic thinking that is still evident in the scientific literature. A curiosity of this, is that when Lucas first embarked on his scientific revolution he often would refer to his alternative as Neo-Austrian, and cite favorably Hayek’s work on business cycles. Kevin Hoover (1984), among others, pointed out the problems with this version of intellectual history, and so its history was very brief indeed. However, even in those correctives a critical subtle difference emerged between Hayek and Lucas on the knowledge assumptions respectively employed in their theorizing about within systems and about systems. To Lucas, no actors within the system doesn’t know what the theorist knows about the system. But to Hayek, no theorist can know what the actors in the economy know and act upon. The context of the two exercises of studying about and acting within is radically divorced for Hayek, and cannot be bridged. The curious task of economics, is to demonstrate to men (substitute theorists) how little they really know about what they imagine they can design.

The knowledge discovered, utilized, communicated and revised within the system is knowledge of particular time and place not available to any single mind nor reducible to data that can be collected and subjected to statistical analysis. It is rather knowledge embedded in the interpretations of signals, the judgment of the situation, and perhaps judgments of others judgments that are made by the actors on the spot. It is contextual knowledge of thoughts put into action, not abstract knowledge of thoughts put into theories. And unless you are in that context, you cannot access it. Theorist can never know what the actors in the system know. The task of finding that knowledge is not difficult; it is not complex, it is not computational in nature at all. This is the realm of epistemic institutionalism as I have dubbed it (see Boettke 2018). Economists and political economists from the Classics to the Moderns have all sought to deploy the technical principles of economics to explore how alternative institutional arrangements either promote or hinger the ability of individuals to pursue productive specialization and realize social cooperation through exchange. The vast majority of these efforts focused on a simple and profound point: incentives matter. And indeed they do. But in the first half of the 20th century a few things conspired to confuse that simple and profound
point—most pernicious of which were excessive aggregation and excessive formalism. A consequence of both of these intellectual developments was that the individual decision maker and their institutional context of choice were pushed to the side, and abstract principles of optimality and maximizing a stable and coherent social welfare function came to dominate theoretical economics. While many economic thinkers in the 1930-1960 period were poking various holes in this abstract theoretical edifice, Hayek honed in on his critique based on the knowledge problem associated with these exercises. Hayek focused on the social epistemics of alternative institutional environments, and the contextual nature of that knowledge.

None of this should actually be taken as a criticism of Scheall’s efforts to clarify and critically expand Hayek’s epistemology and the “theory problem” that Hayek confronts. My view is that it is instead a complement to his work. The knowledge problem of the theorist of the social system of exchange and production is one thing, the knowledge problem of the actors within that social system is another. The science of complexity and of computability are problems theorists face in understanding social order, reading price signals, pursuing profit opportunities, adjusting in the wake of losses, and ultimately the decision to buy and sell, or abstain from buying or selling, are problems faced by actors in that system. We cannot divorce them completely from one another, and make progress, just as we cannot collapse them onto one another and make progress. Scientific progress in economics, political economy and social philosophy is made when we understand the difference between knowledge about systems and knowledge within systems, but also how that knowledge within the system imposes upon the theorist to use Scheall’s terminology an epistemic burden that must be met and which demands that the technical principles of the logic of choice must be blended with the situational logic of institutional analysis to provide a theoretical framework to be deployed in empirical analysis of the social world.

Scheall makes a strong argument throughout that Hayek is identifying a radically empirical problem that must be solved if his theory of society is to hold. And this is a completely correct interpretation. The 1937 paper of Hayek is the urtext for this radically empirical turn in economic research. But it is critical to my mind to remember that not only did Hayek in 1937 not abandon the pure logic of choice, in the 1960s when he returned to theoretical economics his working title was The Economic Calculus (see Caldwell 2016). The point Hayek (1937) was making was that the pure logic of choice was a necessary, but not sufficient component in the theory of the price system and the competitive market process. The pure logic of choice had to be cojoined with the situational logic of human interaction within alternative institutional arrangements. And the criteria Hayek focused on was how human actors learned in these alternative environments to coordinate their plans with one another, to discover new knowledge, to deploy it in valuable ways, and to adapt and adjust to changing circumstances through time. In short, social epistemics of the market is the radically empirical program, and that requires that the epistemic turn Hayek took in the wake of both the socialist calculation debate and the dispute with Keynes had to evolve along with the institutional turn he took as he came to stress more and more that framework within which actors interacted with others in the marketplace, in the polity, and in society in general.

The other strong argument Scheall makes is flipping the Hayekian critique back on itself with respect to the liberal project in general and the change from the status quo mid-20th century to a true radical liberalism that Hayek envisions and begins to articulate in the second half of the 20th century (see Scheall 2020, pp. 75-104). Scheall is right, there is an epistemic burden to be met.4 There are also questions of incentive compatibility that must be met. By who though? The theorist, no doubt must meet an epistemic burden. The actors themselves do as well, but how can you tell if they are or aren’t meeting the epistemic burden that is relevant to them? Well, presumably if there were wide-spread coordination failures in the system, we would say that the actions within the system were not meeting some sort of incentive compatibility and epistemic burden. But the theorist theorizing about the system if they could identify those dysfunctions and present them in a way that met the Quinean argumentative virtue test would certainly pass Scheall’s epistemic burden test. Critical to passing that test, however, is the social scientific investigations of how alternative institutional arrangements either hinder or promote the social learning among actors that is required to pursue productive specialization and realize peaceful social cooperation. The division of labor leads us to highlight
how the division of knowledge in society is coordinated. None of us knows how to make a common woolen coat or a #2 pencil by ourselves, but the social system does, and the theorist comes to appreciate that outcome in a way that the actors inside the system never do; the actors inside the system know details about how, what, and when with regard to that process that the theorist can never know. Again, planning that system on the basis of the theoretical knowledge about the system is a category error, and it is not just a difficult complicated task but an impossible task to pursue. And it can only become more obfuscated and confused when assumptions of omniscience and omnipotence are slipped in to the analysis by the theorist for the sake of formal tractability in the modeling exercise.

F. A. Hayek and the Epistemology of Politics is an outstanding contribution to the literature in Hayek studies, and Scott Scheall should be congratulated for the “window” he opens for scholars to view Hayek through. Scheall actually touches on most of the issues I raise, so my suggestions are really ones of emphasis and development of the argument. However, I believe he is leaving a number of proverbial $20 bills lying on the sidewalk in the scholarly marketplace of ideas. To give but one example, Scheall (2020, pp. 146ff) finally turns to the question of the social context of science itself. “Scientists,” he tells us, “are no more gods than are policymakers.” This is a profoundly true statement, but it also could result in an engagement with Michael Polanyi and the Growth of Knowledge Literature (that Lavoie draws on), or it could lead to Gordon Tullock’s discussion of the organization of inquiry, and his reflections on the relevance of “Flatland” for the saga of social sciences, or to David Levy and Sandra Peart’s adjudications and reflections on Polanyi and Tullock and the quest for a social science of natural equals once we insist that the theorist be placed in the model (see Levy and Peart 2017a; 2017b; 2019). In a very specific setting of science and public policy, of course, Roger Koppl’s Expert Failure (2018) could be leveraged more prominently in Scheall’s analysis to the benefit of his own narrative concerning policymaker ignorance and the epistemology of politics.

Scheall to my mind misses out on these opportunities in the current book, but would no doubt benefit greatly by seeking mutual gains from intellectual trade with these and other sources in future work. I think as Scheall continues to develop and advances his epistemology of politics agenda with further work the very social science he is inviting will lead him to the sort of social epistemics which one finds in writers like Levy and Peart and Koppl about the nature of science and the task of theorizing about social order, and Buchanan and the Ostroms about the nature of the problems actors within the social order in their various attempts to solve dilemmas, resolve tensions, and in engaged in collective action. All of which must be explained if a complex adaptive system of social order is to be understood as functioning. Buchanan’s critique of Arrow’s rendering of the problem with democratic decision making and the Ostroms work on polycentric orders can both greatly advance the agenda that Scheall is seeking to pursue in making Hayek’s work the focal point of a renewed appreciation of the central puzzles in economics, political economy and social philosophy. I find the absence of any serious engagement on the relationship between Hayek and Buchanan to be particularly problematic for a work on the epistemology of politics. And, of course, I would be remiss if I didn’t mention also that Scheall could also benefit from leveraging the work of Israel Kirzner on alertness to entrepreneurial opportunities in commercial ventures, or Ronald Coase’s focus on the ability of neighbors to bargain away of conflicts, and Douglass North’s examination of exchange, production and distribution activities through time as history marches ever forward. In short, Scheall will be compelled as he goes forward with this line of research if he hopes to operationalize his program in a productive way to focus scientific attention on how alternative institutional arrangement hinder or promote the ability of actors within the system to meet the epistemic burden that the complex coordination of plans places upon them.

The social science is already there to be built upon tracing from Adam Smith to Vernon Smith, Scheall just hasn’t adjudicated it, pursued it, incorporated it, and leveraged it in his own unique way yet. It is there in conceptual form, in experimental evidence, in computer simulations, and in historical narratives. That he doesn’t draw on this, however, is perfectly understandable as he is a philosopher and he had other tasks to accomplish in this book and other demons (real or imagined) to slay. Readers will learn much from a careful study of what he has done. But in future work, following his own call for a radically empirical pro-
gram in an epistemology of politics, I think he will be led away from philosophy and away even from ab-
stract conversations in psychology and theory of mind and appeals to complexity science and back instead
to economics and political economy and forced to find a place for Hayek in the context of modern politi-
cal economy. That quest will lead him to see Hayek’s effort in the second half of the 20th century as one de-
veloping a genuine institutional economics that will serve as the basis for his restatement of the principles
of justice and political economy. And critical to that task is always remembering the distinction between
knowledge about the social system of exchange and production, and knowledge within the social system
that brings about the coordination of plans concerning exchange and production.

Scheall’s basic insight in F. A. Hayek and the Epistemology of Politics is unassailable, however, and for
the political economy and social philosophy project to succeed as a living and vibrant research program,
theoretical attention at all times must be on the epistemic burden that must be met at each level of analysis.

NOTES

1 Scheall draws his line of continuity in Hayek’s long career through his early student work in theoretical psychol-
yogy and his theory of mind, later fully developed in The Sensory Order (1952a). I have, of course, noted Hayek’s
contributions to this field and the influence on his subsequent work, but I have tended to focus on his early work
on imputation and the coordination of economic activities through time as the thread that connects his various
interests throughout his career. Thus, the point of emphasis if we look at Hayek through my “window” is on Hayek
as a social scientist and social theorist, and so works like The Counter-Revolution of Science (1952b) become focal
text. Those two points of departure alone lead to different interpretative frames in Hayek studies.

2 See the discussion in Lewis (2016).

3 One avenue that Scheall could have pursued that would potentially link Hayek’s theory of mind with Hayek’s the-
ory of economy and society would be the theory of expectations. Expectations play a critical role in Hayek’s own
tory of equilibrium and the coordination of plans through time. The theme of expectations in Hayek has been
explored in the work of Butos and Koppl (e.g., 1997).

4 The difficulty of institutional transformation was a major theme of my own work in transitional political eco-
omy and development economics, and can be seen in various works by Chris Coyne and Peter Leeson. See Boettke
(2001), Coyne (2008), and Boettke, Coyne and Leeson (2008). Scheall’s (2020, pp. 75-104) critical reflections on the
hubris sometimes exhibited by libertarians and classical liberals is an important challenge for those working in
the Hayekian tradition to address.

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of Economics & Sociology, 67(2): 331-358.
There is a kind of nihilism lurking in the Austrian tradition, a sense that the world cannot be improved, that all our efforts will be in vain. In its moderate version it exhibits itself as a conservative sensibility which favors the status quo and is skeptical about proposals for major overhauls in morality, culture, the economy, and society. It is undeniable that Hayek had such a sensibility, it is for good reason that we associate his work with that of Michael Polanyi and Michael Oakeshott. But Hayek did not want to be a conservative, and he certainly did not want to be a nihilist. In fact, his work between *Freedom and the Economic System* (1939) and *Law, Legislation and Liberty* (1982) is written from the firm belief that liberalism must be a constructive program, a legislative program, and yes, a policy program.

Scheall in his book *F. A. Hayek and the Epistemology of Politics* appears to want to revive the more nihilist version of the program. At the very least he seems determined to use the knowledge arguments from the more skeptical and critical parts of Hayek’s work against the more constructive parts of Hayek’s work. He does so by generalizing the epistemological critique that Hayek employed against the socialists and later the macro-economic planners, most notably in his Nobel address *The Pretence of Knowledge* (1975). Not merely institutional changes in a centralizing direction are epistemologically too demanding, but the same is true for institutional changes in a decentralizing direction. Scheall draws attention to the problem of policymaker ignorance and argues that policymakers will inevitably lack the necessary knowledge to achieve policy goals. Even if we would grant policymakers sufficient knowledge of the functioning of the economy to reach a particular policy goal, they would still be ignorant about the goals of their constituents. The ignorance and darkness appear to be all encompassing in many parts of the book.

Scheall seems to reach a kind of impossibility theorem in the book. He suggests as much: “For these reasons, the value of empirical political epistemology is likely to be more negative than positive, that is, to consist more often in the knowledge that policymakers probably cannot deliberately achieve some goal” (p. 163). I do not wish to deny in this reflection that Hayek would have disagreed. After all, in his most optimistic book *The Constitution of Liberty* Hayek early on suggested: “If we are to understand how society works, we must attempt to define the general nature and range of our ignorance concerning it. Though we cannot see in the dark, we must be able to trace the limits of the dark areas” (Hayek 1960, p. 23). For Hayek knowing what we cannot do,
however, is a step in the direction of knowing what we can do. I am not sure whether this is also true for Scheall, as he at least does not offer any practical examples of positive knowledge about policy. Even if we agreed that a Hayekian minimal or constitutional state was possible, we would not know how to get there according to Scheall. It is therefore at least a little bit ironic, that Scheall’s own proposal is a similar kind of constitutional proposal.

In these reflections I wish to explore what the constructive Hayek would have to say to Scheall’s, at times radical, skepticism. After all, there are passages in Scheall’s book that suggest we have a duty to look for what is possible, not merely point out what is impossible: “If we are serious about upholding democratic ideals, if we mean to pay more than mere lip service to these ideals, we need to think creatively about mechanisms that might ensure that goals associated with these ideals are actually pursued and realized.” So, let me proceed with seriousness.

POLICYMAKERS ARE IGNORANT, BUT WHAT ABOUT US?

At various points of Scheall’s book I was reminded of the public choice literature. Not in the sense that Scheall is interested in analyzing the incentives of policymakers, he is very explicit that for him epistemological concerns are primary, and incentive problems second. But in the sense that Scheall is intent upon demonstrating that policymakers are mere mortals, they live with the same epistemological defects, constraints and biases as citizens do. It is a fair and accurate critique, policymakers are no angels, and neither are they anywhere close to the omniscience of Laplace’s demon.

Hayek’s theory of human behavior is rooted in the idea that human beings act based on incomplete and subjective knowledge. Humans will rely on heuristics to obtain information, most famously market prices. And they will often follow cultural rules, which tend to contain a lot of built-up knowledge (of which individuals are incompletely aware). Since the future is uncertain, and the success of individual plans depends on the plans of other individuals, they will make mistakes. Hayek is very explicit about the lack of knowledge humans face in formulating their plans. He is less explicit about the extent to which humans face similar knowledge problems in determining their goals, the ends they pursue, but it would not be inconsistent with his overall philosophy. But despite all this lack of knowledge, the ignorance even, humans act.

They do so despite the uncertainty, despite their epistemological limits, despite their ignorance. And this is not merely true for individuals, it is also true for business leaders of major corporations, who certainly can be modelled as (a kind of) policymakers. The same is true for leaders of social movements, and managers of non-profits, who face presumably even more epistemological limitations since they cannot rely on the price system for important subsets of their actions. Of course, Scheall never in his book suggests that humans do not act, nor does he suggest that policymakers do not act. They do, despite their ignorance, and that is one of the motivations for his arguments about policymaker ignorance. But if all actors are ignorant, and epistemologically constrained that can never be an argument why some actors’ decision power should be severely restricted. Politics without epistemological romance, is still politics.

All actors in economy, in society, face the kind of epistemological limits that Scheall identifies. Scheall nowhere offers reasons why policymakers are in a fundamentally different position, although he does highlight that policymakers face the additional problem of limited knowledge about their constituent’s goals. But that problem is certainly not unique to political actors and holds for managers as much as it does for union leaders, and for civic leaders, as much as it does for women or men managing a household. Acting on behalf of others, is not the unique defining feature of policymakers. Nor was I ever quite sure that Scheall wanted to claim that this was the most important type of ignorance, it seemed that he gave precedence to the ignorance about how to achieve policy goals.

The crucial epistemological feature of markets, in the Hayekian approach to markets, is that the market is a dynamic process in which there is not merely learning but also discovery. Curiously Scheall’s discussion of the socialist calculation debate is relatively static, and his discussion of the epistemological properties of
markets, although based on Lavoie’s work, has very little eye for either learning processes or the role of entrepreneurship. To make matters worse the entire book does not mention the concept of uncertainty once (!).

This is odd since Scheall devotes an entire chapter to Hayek’s theory of knowledge and its connections to Viennese psychology. In that chapter there is ample attention to the sources of knowledge, as well as the nature of knowledge, but the discussion remains purely qualitative. Either one knows, or one doesn’t know. Either one is ignorant, or one has knowledge. Consequently, policymakers are either ignorant, or they possess the relevant knowledge. In my view that perspective is thoroughly un-Hayekian for several reasons.

First, Scheall, perhaps unconsciously, remains far too close to the neoclassical paradigm in which agents have preferences and policymakers are expected to act upon those clear preferences, as if the relevant paradigm for Hayekian political epistemology would be Kenneth Arrow’s world of preference aggregation. Secondly, ignorance, despite the occasional nod to the fact that it is a matter of degree, remains an on-off property which obscures processes of adaptation, learning, and entrepreneurial action. Thirdly, there is very little attention in the book to the institutional structure of democracies, which makes it virtually impossible to seriously discuss the epistemological problem of politics based on the framework that Scheall lays out. In the next section I will jointly discuss point one and two, and in the final section the third point.

DISCOVERING WHAT WE WANT, POLITICALLY

Neoclassical economics and the associated welfare economics are based on the idea that individuals have a clear set of preferences. I firmly believe that Hayek’s thought, although his early work started from that assumption, is incompatible with this perspective. While it is beyond the scope of this commentary to develop this argument fully, Sugden (2018) has laid out a convincing argument along Hayek-Buchanan lines for the importance of behavioral insights, and the recognition of cognitive limitations about individual choice (and its implications for welfare economics). Sugden is justly skeptical of the idea that individuals have a clear conception of their own goals, and consequently of the idea of preference-aggregation.

In the Austrian approach fallible knowledge and uncertainty play a key role in all human decision-making. Strictly speaking this might merely pertain to the set of available goods, and how these can be used to pursue specific ends. But I see no good reason to argue why this not also pertains to the ends pursued by individuals. If we accept Hayek’s argument that markets are a discovery procedure, then certainly this should also apply to the ends individuals pursue (Hayek 2002). More generally I regard it as consistent with, or even necessary for, a liberal political economy to assume that the goals that individuals pursue evolve over time.

This means that a key aspect of the markets process is not merely the discovery of the most efficient means of producing goods for firms, and for consumers the most efficient means to reach their ends. Markets processes are also a discovery mechanism for what we should desire, what is worthy of our attention, and what we should aspire to. In Frank Knight’s words: “life is at bottom an exploration of values, an attempt to discover values, rather than on the basis of knowledge of them to produce and enjoy them to the greatest extent” (Knight 1935, p. 105). This process appears to me just as important in politics which complicates the first type of policymaker ignorance that Scheall identifies: the ignorance about the preferences of their constituents. If citizens are engaged in a discovery process about their (political) ends, we cannot demand that policymakers have knowledge of these goals. So, we must arrive at a process view of the political opinion formation to arrive at a Hayekian political epistemology, much like Hayek argued for a process view of competition.

I am not the first one to make this observation, in several articles Michael Wohlgemuth has explored what a Hayekian perspective on the political process would look like. He argues that in a perspective in line with Hayek, Buchanan (and Popper): “the virtue of democracy is predominantly assessed according to its ability to serve as a rule-guided procedure for the formation, discovery and utilization of opinions and conjectural problem-solutions” (Wohlgemuth 2002, p. 228). And he quotes Hayek to that effect: “Democracy is,
above all, a process of forming opinion (...) It is in its dynamic rather than its static, aspects that the value of democracy proves itself” (Hayek 1960 cited in Wohlgemuth 2002).

Wohlgemuth makes several important points. He provides good reasons why citizens in modern society might seek to develop informed opinions about political issues even if the ballot box itself is a weak incentive to do so (mostly for reputational reasons). He highlights the fact that politicians, in their role as political entrepreneurs, depend for their success on the creation of public issues, the formulation of alternatives, and what is particularly useful, the rebuttal of their opponent’s proposals.

Wohlgemuth’s most interesting argument is about the importance of minority opinions in a democracy. It is well-known from public choice theory that small groups might lobby for specific policies, something that is likely to succeed if the benefits of such a policy are concentrated, while the costs are dispersed. But the reverse is also the case, if the costs of a particular policy fall heavily on a particular group (are concentrated) then that group is likely to speak out against the policy, and will seek to change public opinion against it.

Wohlgemuth develops this idea into a more general point in which he emphasizes that in a Hayekian political epistemology heterogenous political opinions should be just as important as a starting point as in Hayekian economic analysis (another argument against preference-aggregation of the Arrow-type). The benefit of heterogenous political opinions is that it creates an arena of contestation, in which different goals and policy measures will have to compete with one another. Proponents will have to draw upon reason and evidence to argue for the superiority of their specific proposal and seek to transform public opinion in their favor. Wohlgemuth again cites Hayek to make this point:

The conception that the efforts of all should be directed by the opinion of a majority or that society is better according as it conforms more to the standards of the majority is in fact a reversal of the principle by which civilization has grown . . . it is always from a minority acting in ways different from what the majority would prescribe that the majority in the end learns to do better (Hayek 1960, cited in Wohlgemuth 2002).

To the best of my knowledge a very similar view of democracy, based on Hayek, has been developed by the late Gerald Gaus. He similarly defended the importance of plurality of opinions and worldviews to improve ‘public reason’ (Gaus 2010).

Perhaps a truly Hayekian political epistemology should be skeptical about the virtues of such an, ultimately, deliberate process. But I don’t think that invalidates the general insight. The ‘experiments in living’ in Nozick’s Utopia (1974) as well as the liberal archipelago of Chandran Kukathas (2003) are based on the same positive evaluation of difference and diversity for a liberal society, and not, or far, less reliant on the qualities of public debate. They seek to promote the discovery process through a process of selection, in line with Hayek’s later work.

Wohlgemuth’s idea that the democratic policy process functions as a kind of falsification mechanism of policy proposals is perhaps overly optimistic. Policies are rarely, if ever, tested in isolation. Another point that he does not fully develop is why political entrepreneurship might be beneficial. But if we extend Hayek’s analysis in the direction that G. L. S. Shackle has done it becomes quite evident why politicians might fulfill an important role in the political learning process. In Shackle’s perspective the imaginative act in the formulation of plans, in the face of deep uncertainty, is key for economic actors (Shackle 1972). We could extend that idea to the role of politicians and policymakers, who offer, based on some degree of imagination, possible avenues for an uncertain future. In his recent work Jens Beckert has highlighted the essentially forward looking nature of plan-formation, and extended that to the political sphere (Beckert 2016; 2020).

From this perspective the ignorance of individuals and policymakers is symmetrical. Nobody knows what the future holds, and nobody knows in advance precisely what they are pursuing. And yet, individuals, politicians, business leaders must act. They do so, based on plans. The role of policymakers (or poli-
ticians) in the political process is the development of such plans, the development of what Beckert calls ‘imagined futures’.

One might argue that this imaging of futures is a dangerous undertaking, especially in the political realm. And Hayek indeed never tires of making that argument. But pointing out the dangers is different from denying the importance of the process itself. Popper famously argued for piecemeal engineering, Hayek had much to say about general rules. There might be all kinds of other considerations, rules of thumb, conventions and heuristics that are used in policymaking and the formulation of political plans for the future that the liberal economist might wish to stress, or that the Hayekian political epistemologist might wish to highlight.

But to convincingly promote such heuristics or policy rules we need a sense, not merely of policymaker ignorance, but also of policymaker knowledge (and capacity). What is the set of policies about which we more or less know the effects, about which is our knowledge too limited, and which policies are mere wishful thinking? It is telling that the two practical problems that Scheall tackles are two of the most difficult policy problems imaginable: managing the business cycle and major political transformations. I grant immediately that these are areas where there is much ignorance and very little knowledge. But is this equally true for the introduction of an import tariff, for industrial subsidies, for an increase in the income tax? Are these really the type of policies for which policymaker ignorance is the most important problem? And is it true that: “constituents do not know what to do to avoid circumstances in which their policy demands are likely to be disappointed” (p. 154)? Is it true that constituents do not understand the relative difference in difficulty between a general speed limit, and the micro-management of traffic movements? Granted, there might be unintended consequences, and these are hard to foresee in many cases. But these are secondary effects, frequently small, and sometimes well recognized. The primary effect of a large set of everyday policies appears to be well known by policymakers and understood by citizens.

Scheall argues repeatedly: “On Hayek’s theory of knowledge, policymakers know enough to realize some policy objective to the extent that they can make a plan and realize the objective on the basis of this plan, without any need for the intervention of spontaneous forces” (p. 7). I am not quite sure what to make of that statement. Hayek’s favored policies are typically general rules which depend for their effect, precisely on the adjustment of the spontaneous forces in society. Policymakers can hardly ever directly realize a policy objective, if only for the simple fact that enforcement of policies is typically in the hands of regulatory agencies or other parts of the government, and not in the hands of policymakers themselves. More importantly, the practical (desired) effects of, say a tariff, depend very much on the spontaneous forces of adaptation after the introduction of the tariff. It might make more sense to take Hayek’s point about pattern predictions to heart: policymakers can at best make pattern predictions about the (expected) effects of their policies. For some policies we know more about the expected patterns, for others we are mostly groping in the dark.

INSTITUTIONS FOR DEMOCRATIC LEARNING

The most surprising sections in Scheall’s book, to this reader, were those where he sought to criticize recent work on epistemic institutionalism in the Hayekian tradition. Much of the relevant work in that direction has been done by Pete Boettke in his recent book on Hayek, and in a more practical sense in his book with Aligica and Tarko on public governance (Boettke 2018; Aligica, Boettke, and Tarko 2019). Scheall appears dissatisfied with this work since it never considers how the relevant epistemically desirable institutions could be realized. And hence he desires to move the problem one level up, not epistemic institutionalism, but political epistemology. The knowledge about how to realize the desirable epistemic institutions.

I am not at all convinced that such a move, one level up, would be desirable from a Hayekian perspective. What is distinct about Austrian political economy, next to its emphasis on processes, is its unwillingness to engage in ideal theorizing. In its best Hayekian versions—I might be guilty of idealizing here political economy seeks to compare real-world institutions with each other. The knowledge problem means that
ideal theories are unlikely to be of much use. Who would have the knowledge to analyze all the implications of such a situation? For real-world institutions on the other hand we have more practical knowledge, knowledge obtained from experience: experimentation, failure, and occasional success. According to Hayek’s later work most real-world institutions are typically a wealth of stored up knowledge gathered through a long process of experimentation.

In that sense we can only learn about the epistemic properties of different political institutions through experimentation. Much like the relevant set of goods, the best methods of production, and the most efficient allocation of resources can only be discovered through the market process, the process of experimentation. Fortunately, many earlier ‘experiments’ have taken place across time and place, and so there is some material to analyze for the relative epistemic properties of different (democratic) political institutions. That knowledge is imperfect, flawed, perhaps misleading, and most importantly incomplete. And yet we must act, and if we are to proceed with seriousness, keep thinking about how we can improve the epistemic properties of our democratic political institutions. This similarly holds for the problem that Scheall gives primacy, the epistemic requirements for institutional change.

Various institutions have been singled out for their epistemic properties by political economists in the Austrian tradition. Hayek himself suggested that organically grown institutions are generally to be preferred over designed one’s. Decentralization is praised for its epistemetic benefits in market settings by Hayek, and federalism is generally believed to be its political equivalent. Recently various Austrian economists have been attracted to self-governing arrangements as exemplified by the commons in the work of Elinor Ostrom. Julian F. Müller has explored the epistemic virtues of a polycentric democracy (Müller 2019). His book contains a section on implementation. It is also worth pointing out that in political philosophy there is now a literature on political feasibility, explicit motivated by a desire for a non-ideal theory of politics (Gilbert and Lawford-Smith 2012).

I do not deny that Scheall is right in pointing out the importance of the epistemological aspect of politics, even the primacy of epistemology in politics. Most of the work I have mentioned so far is exploratory in nature and very recent. The most important work remains to be done, and I think rather than less institutional and more philosophical as Scheall suggests, it should be less philosophical and abstract, and more institutional. A good example is the role of the (political) press and more broadly the (social) media in modern democracies. Scheall briefly mentions the role of the press, but only to paint it as a further corrupting element next to the politicians, who he labels ‘bullshit artists’ (p. 154).

What is instead required is a serious analysis of the further epistemic characteristics of the political process. Market coordination does not happen only through prices. It occurs through a wide variety of additional coordination mechanisms: product categories, industry standards, reputation mechanisms, review systems, expert scrutiny and so on. These institutions, which vary from market to market, are important elements of the epistemological properties of markets. In similar fashion we should come to appreciate the political process not merely as an interaction between idealized policymakers and isolated citizens. Instead we should come to understand the complex process of coordination in the political process.

The institutions in the political process which facilitate learning and discovery are varied. Ideologies, much like product categories, are informational heuristics for the general direction in which solutions are sought. Political parties can function as screening mechanisms on the one hand, and function like brands (reputations) in a marketplace on the other hand. The press can, through the principles of division of labor (and knowledge), facilitate communication between policymakers and their constituents, as well as uncover scandals. Think-tanks and other research institutes could engage in policy evaluation as well as the exploration of new policy ideas. Opinion polls, focus groups and other types of mechanisms inform policymakers about the views and desires of constituents. Social movements have the power to combine many of the functions above, including more activist strategies such as protests and strikes.

Such institutions can work well, but they can also function poorly. Liberal theorists have typically regarded it of great importance that the state allows freedom of the press and association to ensure effective checks on government. More socially oriented liberals have suggested that states might have important fa-
cilitating functions in enabling such institutions. There are many epistemological aspects to differences in party-systems, the press functions very differently across the world. Germany has a system of think-tanks associated with the major political parties, whereas these are mostly privately funded and independent from state and politics in the United States. Techniques for surveying the public have evolved enormously over time. Social and political movements have existed in a wide variety of guises from lobby groups to terrorist groups. From an epistemological process they all seek to add knowledge to the process of political coordination, to signal what they want and how bad they want it.

CONCLUSION

A Hayekian political epistemology means the study of the process of political coordination. Just like the Hayekian economic epistemology finds its realization in the study of the process of economic coordination. That investigation should recognize the lack of knowledge among all participants of the political process, the deep uncertainty that they are facing. Consequently, the starting point can never be the aggregation of individual preferences, but must be the process of exploration and discovery. This open-ended coordination process is the starting point. From there we should proceed to the analysis of the epistemological properties of the existing (and historical) variety of democratic institutions, and empirically study these properties. We do not have the luxury of imagining idealized institutions, or ideal epistemological conditions. It is only in the political process itself that we can discover the relevant institutions, and in which they will prove their worth.

To pursue this Hayekian political project, we better engage with the enormous empirical literature on democratic, and other political institutions, already in existence. Like in other fields, a Hayekian or Austrian perspective will provide a fresh perspective. In my commentary I have sought to make clear that this should not be restricted to formal democratic institutions, but include the wide variety of supporting and related institutions such as parties, ideologies, the press as well as social movements. After all a Hayekian political scientist will be interested in all the ways in which knowledge in society is used. Both in terms of perspective, and in terms of scope a Hayekian political epistemology has much to offer. Scheall is, most certainly, correct in pointing out that the epistemological perspective is not the default perspective for economists and political scientists.

All of this makes me wonder, where the extreme skepticism about the abilities of policymakers come from in his book. Why is it that Scheall seeks to return to the almost nihilistic agenda of the Austrian program which comes close to the denial of any type of policymaking. Perhaps it is a sign of the times, a reflection as much of the current American political situation as of the general epistemological challenges that policymakers face. If that is true, then we should proceed toward the improvement of our democratic institutions not only with seriousness, but also with hope. Or as Hayek put it: “I have drawn encouragement from the fact that it [liberalism] has often emerged from adversity with renewed strength” (Hayek 1960, p. 7).
NOTES

1 An implication of this symmetry of ignorance or lack of knowledge is that traditional political theorists were perhaps somewhat justified in focusing on the question of the legitimacy of political authority. After all, if there are no epistemological reason for granting policymakers authority, then these justifications must be found elsewhere.

2 For example: “The actionability of plans is not dichotomous but a matter of degree. Where individuals are less than omniscient and omnipotent, the disappointment of plans is an unavoidable aspect of social life” (p. 140).

3 Although it is interesting that in the discussion of political transformations Scheall highlights a particular type of knowledge, contextual knowledge of time and place, which was neglected. Implicitly he highlights here the crucial challenge for a Hayekian political epistemology I promote here: making the best use of all knowledge in society.

4 The public choice literature is in fact full of instances where policies are pursued primarily for their secondary effects, with good evidence that the actors were aware of these secondary effects. A good example is the literature on the minimum wage, which was often used to protect incumbent workers.

5 I was surprised to miss any reference to indeterminacy results on second-best policies in the book (Lipsey and Lancaster 1956). These results appear relevant to the feasibility of liberalization policies if one remains, as Scheall does, within the mainstream welfare economics perspective. I think in non-ideal theories these results are far less relevant, and the notion of pattern predictions is far more relevant.

REFERENCES


Scheall on the Epistemic Limits of Policy

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O, these deliberate fools, when they do choose,
They have the wisdom by their wit to lose.
~Portia in The Merchant of Venice

Ignorance slays deliberation. This bold claim is the mustard seed from which grows the rest of Scott Scheall’s argument in F. A. Hayek and the Epistemology of Politics. Scott has privately told me that his view might be expressed more “delicately” as, “Ignorance constrains deliberation.” Be that as it may, Scheall’s point is that “deliberation” requires, somehow and in some degree, adequate knowledge of what you deliberate on. I don’t know who is in the senior class of Omaha’s largest public high school, nor which public high school is the city’s largest. I cannot, therefore, deliberate on which senior is most likely to succeed, which is the best athlete, and which is the most charming. If I were to open a medical practice, I could, perhaps, deliberate on therapies for my patients. But my ignorance of medical science would greatly constrain such deliberations.

This connection between ignorance and deliberation is the most important contribution Scheall’s book makes to political epistemology. In the main, Scheall believes, this epistemological point has gone unrecognized in political philosophy, or at least been given too little weight. Scheall’s central epistemological claim (that ignorance slays deliberation) fits nicely with his view that our knowledge is ultimately wholly empirical. Scheall even seems to suggest that his point requires empiricism. “If one insists,” he says, “that there is a source of knowledge . . . other than the environment, then one cannot consistently argue that policymakers lack knowledge requisite to their peculiar tasks, because it can always be asserted that decision-makers can discover the missing knowledge a priori by inner reflection on the significance of their humanity” (pp. 111-112).

I said that for Scheall, knowledge is “ultimately” wholly empirical. The word “ultimately” is doing work by opening up the possibility that the experience shaping one’s knowledge may be that of one’s cultural and biological ancestors. One might think our knowledge of color is “a priori” because each of us individually is born with it. But that color knowledge was hammered out in evolutionary time by natural selection. It is, therefore, “ultimately” empirical. Scheall gives the similar example of “olfactory sense experience” (p. 122). In Scheall’s interpretation, Hayek has a theory in which knowledge is “empirical” in just this way. When it comes to epistemology, therefore, he stands solidly beside Hayek and fully repudiates the apriorism of Hayek’s great mentor, Ludwig von Mises.
Scheall applies his “political epistemology” to political philosophy in Chapters 5 and 6 of his book. His “epistemic-mechanistic approach to the problem of policymaker ignorance” is “meta-theoretic.” Thus, he is doing philosophy and not social science. He does not propose any particular epistemic mechanism, survey some list of epistemic mechanisms used in the past, or rank them on some normative scale such as justice or efficiency. He just points out the role of epistemic mechanisms. In his view, if we are to have large-scale cooperation, we must also have reasonably functional epistemic mechanisms. The question is, “How do people know what to do in large-scale societies?” They need, Scheall tells us, reliable signals of what to do. Good epistemic mechanisms create and distribute such signals. Emergent social rules, market prices and scientific citations are all paradigmatic mechanisms of this sort. If our actions are to be coordinated without commands, they must be coordinated by signals, and an epistemic mechanism generates and delivers such signals. Presumably, Scheall would say that a good epistemic mechanism does not generally deliver such signals randomly. Nor does it typically deliver all signals to everyone. For then there would be information overload. Thus, I think, Scheall would say that a good epistemic mechanism effects a serviceable distribution of signals across agents in the system.

Importantly, Scheall argues that democracy is often, perhaps generally, a poor epistemic mechanism. We should therefore have a written constitution that limits politicians to doing what they can, epistemically, do. Attempting to stay within the constraints of the critique of constitutional design by Devins et al. (2015), Scheall does not suggest a once-and-for-all re-writing of the Constitution to reflect policymakers’ existing epistemic capacities. Given that these capacities will continue to evolve, Scheall believes, so must any constitution that is drawn up on the basis of his constitutional approach.

A constitution drawn up on the lines Scheall recommends would give us a broadly liberal political economy. It would give us something along the lines of Adam Smith’s “obvious and simple system of natural liberty,” in which “the sovereign has only three duties.” The first duty, Smith tells us, is “the duty of protecting the society from the violence and invasion of other independent societies.” The second is “protecting, as far as possible, every member of the society from the injustice or oppression of every other member of it, or the duty of establishing an exact administration of justice.” And the third is, “erecting and maintaining certain public works and certain public institutions, which it can never be for the interest of any individual, or small number of individuals, to erect and maintain.” In this system, Smith tells us, “The sovereign is completely discharged from a duty, in the attempting to perform which he must always be exposed to innumerable delusions, and for the proper performance of which no human wisdom or knowledge could ever be sufficient; the duty of superintending the industry of private people, and of directing it towards the employments most suitable to the interest of the society.” (Smith 1776, pp. 184-185) Thus, Scheall is developing Smith’s point that governments trying to go beyond “the obvious and simple system of natural liberty” will attempt things for which “no human wisdom or knowledge could ever be sufficient.”

Scheall’s book is a work in philosophy by a philosopher. And he is careful to stay in his lane. In particular, Scheall does not try to decide just how much politicians can do without exceeding their epistemic bounds. He does not attempt to hide his broadly liberal point of view. The centrality of F. A. Hayek to Scheall’s argument does not strictly imply that he takes a liberal posture, of course, but it is suggestive. True to his empiricism, however, Scheall seems to leave it as an empirical matter how far deliberation can take us in political decision making. And it is for others, especially social scientists, to study where those epistemic limits might be.

I surely share Scheall’s enthusiasm for Hayek’s epistemology. I think he is right to say that Hayek has an idea of knowledge quite different from “justified true belief,” which might still predominate among Anglo-American philosophy professors specializing in epistemology. It is generally recognized, Scheall avers, that Gettier (1963) problems have revealed the need for “supplementation” of the standard definition. No “consensus,” however, has emerged on how to do that (p. 132, n. 15). Thus, it seems, we should consider Hayek’s epistemology non-standard. In particular, Scheall notes several passages in Hayek in which, seemingly, “knowledge” need not be “true.” And Scheall quotes Hayek calmly speaking of “contradictory knowledge”
(Hayek [1945] 2014, p. 93 as quoted in Scheall 2020, p. 116). It is my understanding as well that Hayekian “knowledge” need not be “true” and that one bit of Hayekian “knowledge” may contradict another.

I also appreciate the general thrust of Scheall’s argument, although he seems less skeptical of written constitutions than I am. I think Adam Smith was right to warn against the “innumerable delusions” governments may fall into when they attempt to do things requiring more “wisdom or knowledge” than human beings are capable of. In other words, I think Scheall is right to say that limits to knowledge imply limits to our political ambitions. Such limits of knowledge were essential to the “Austrian” position in the socialist calculation debate, as Scheall notes. Essentially, without a functioning stock market, socialist planners won’t be able to compute opportunity costs and the system will be flying blind (Lachmann 1969, p. 161). I added the qualifier “functioning” because in a controlled system there may be a stock market that does not perform the its essential function of price formation. "Under the Nazis," Temin (1991, p. 580) has explained, “the stock market largely lost the ability to direct resources into alternative uses” because prices were controlled by the state. For example, the “interest rate was stabilized at 4.5 per cent and dividends were limited to 6 per cent.” It looked like a stock market, but it didn’t walk like a stock market or quack like a stock market.

Overall, then, I support the general proposition that limits of knowledge imply limits to what we should ask governments to do. And yet I must confess to some perplexities about Scheall’s central point.

Part of my difficulty concerns the words “deliberate” and “deliberation.” Recall Scott’s gracious treatment of my summary statement, “Ignorance slays deliberation.” Scott’s very mild reply was to substitute “constrains” for “slays.” But the word “deliberation” appears only once in his book (on p. 133) in an endnote briefly characterizing some of Helmholtz’s views. In substance, it is not the word he used in his book. And rightly so, I think. Aristotle says that “deliberating” is “investigating and calculating” (Nichomachean Ethics 6.9.2 & 6.9.3, Bekker page 1142b). He says, "deliberation takes a long time" (Nichomachean Ethics 6.9.2, Bekker page 1142b). If “deliberation” means “thinking it through carefully,” then we may deliberate more when our ignorance is greater. Some people who buy lottery tickets, for example, agonize over what numbers to choose. They take a long time and weigh many considerations precisely because they are conscious of their ignorance. They often hope to seize upon some consideration that will reveal itself to be relevant and definitive. “Why, it’s my cousin’s birthday today! But should I pick month then day or day then month?” And thus, endlessly pirouettes the peroration.

I don’t think Scheall meant to say that ignorance constrains “deliberation” in that sense. I think he meant to say that ignorance prevents us from doing something “deliberately,” that is intentionally or on purpose. He opens the introductory chapter with the words, “The present work is a plea for inquiry into an important, but heretofore largely neglected, problem. The problem of policymaker ignorance is the simple fact that the success of purposeful political action is necessarily limited by the nature and extent of policymakers’ ignorance and their capacities to learn. We cannot deliberately realize policy objectives beyond the ken and control of our political representatives” (p. 1). It might seem a straightforward principle that we cannot do deliberately what we know not of. And yet I feel perplexed.

Scheall says, “[B]y definition, deliberately can just means knows enough to” (p. 3). If that sentence defines “deliberately,” then “deliberately” would seem to mean “knows how.” But what would “knows how” mean? Since Scheall accepts and, indeed, emphasizes that knowledge may be tacit (pp. 15, 107, 123, 125, 128) and since he favorably cites Ryle’s (1946) “knowledge how” (p. 123), it would seem that Scheall’s “knows how” means “is able to.” Scheall says of the socialist planner seeking agreement, “In particular, they must know how, that is, they must possess the ability, to mollify those who resist the plan without simply murdering or otherwise coercing them into quiescence” (p. 53). Thus, it seems fair to say, “knows enough to” means ”is able to.” Thus, Scheall’s position seems to be that, by definition, “deliberately can” just means “is able to.” But then Scheall’s political epistemology is reduced to the proposition that politicians are not able to do what they are not able to do.

I don’t know whether my perplexities about Scheall’s central claim show he is to be criticized for obscurity and confusion or, what is more likely, that I am to be criticized for getting his plainly stated views
wrong. Perhaps neither of those things is true, but something else. In any event, I cannot pretend to have a definitive definition of “knowledge” that will somehow disperse all fogs of obscurity, ambiguity, and multiple meaning. I don’t think I know what knowing is.

I don’t know what knowing is, but it seems connected in some essential way to acting adaptively or appropriately. If you have been frozen into inactivity by unexpected events, you may later exclaim, “I didn’t know what to do!” The unexpected contingency did not call forth an action suited to your desires, purposes, and interests. A party guest has made an unexpectedly crude remark, grossly inappropriate to the situation. You might wish generously to relieve them of their embarrassment or, cruelly to draw attention to their error, or selfishly to suggest that you would never and could never utter such a crudity. Whether your impulses were generous, cruel, or selfish, you were, we imagined, frozen into inactivity. You were, therefore, unable to promote your (generous, cruel, or selfish) ends, unable to act appropriately to the situation.

In some cases, acting appropriately is saying or writing something. You know your multiplication tables if you can recite them. You know how to add if you can write down the right answer. You know the capital of Vermont if you chirp “Montpelier” at the right moment. You may also “know” what you cannot say. You may know when to hold ‘em and when to fold ‘em. You may know how to ride a bicycle or how to swim. You may know how put others at ease. In such cases we imagine your adaptive actions to have been enabled by some disposition, habit, skill, or internal state or condition. We imagine that you possess some enduring property that lets you to act appropriately in a variety of cases that may be more or less well defined or only vaguely apprehended. If we think that you do not “know” how to swim, we will describe your escape from drowning this morning as “lucky” or “a fluke.” If, however, you “know” how to swim, there was nothing surprising or unusual in the fact that you crossed the pond safely. Thus, you “know” only if you reliably act appropriately in some class of cases.

In appealing to the notion of reliability I have probably been influenced by Alvin Goldman (1986), who offered a “reliabilist” account of knowledge. And Goldman’s project of “veritistic social epistemology” has influenced my own efforts in Expert Failure and elsewhere. As far as I can tell, however, I am more epistemologically radical than Goldman in part because (like both Hayek and Scheall) I do not require “knowledge” to be a belief or to be in any way justified or even true.

I have come around to a tentative definition of “knowledge” as reliably acting appropriately in some class of cases. Humans are not the only entities that may reliably act appropriately in a class of cases. Your dog may know how to sit, roll over, or heel. Kaufmann says, “complex living systems must ‘know’ their worlds. Whether we consider E. coli swimming upstream in a glucose gradient, a tree manufacturing a toxin against a herbivore insect, or a hawk diving to catch a chick, organisms sense, classify, and act upon their worlds. In a phrase, organisms have internal models of their worlds which compress information and allow action” (Kauffman 1993, p. 232). Notice that even a tree knows something in this view of knowledge.

If knowledge is reliably acting appropriately to circumstance in a class of cases, then the knowing entity must respond to its environment. It must, therefore, sense its environment. Because its actions must respond to what it senses, it must classify what it senses. “In this environment, go up.” Or “In this environment, stop moving.” The knowing entity’s action must depend on which elements in its classification are activated by what it senses. The number of particulars in the environment that a knowing entity might respond to is very large. Its classification, therefore, will simplify, picking out only a relatively small number of environmental particulars. E. coli, swimming in its host’s intestine, responds to nutrient concentrations and little or nothing else. I am eliding some complexities. Beisel and Afroz (2016), for example note that E. coli have strong preferences among sugars. Micali et al. (2017) show that E. coli sensors can get overwhelmed when they swim toward food, leading to “tumbles” and other wiggly irregular movements. Nevertheless, in this and all other cases, the knowing entity will discriminate among only relatively broad classes of environmental states. E. coli don’t consider, for example, whether their host is standing or sitting, sleeping or awake. They do not discriminate among these environmental states.

The knowing entity’s classificatory system, its model of the environment, simplifies. In this sense, it compresses information. If it did not, the knowing entity’s model of the world would be as detailed and
complicated as the world. But then the knowing entity would not be in the world; it would be the world. One may think immediately of Borges’ (1946) “On exactitude in science.” But the point had been made more mirthfully by Lewis Carroll in *Sylvie and Bruno Concluded* (1898, p. 169):

Mein Herr looked so thoroughly bewildered that I thought it best to change the subject. “What a useful thing a pocket-map is!” I remarked.

“That’s another thing we’ve learned from your Nation,” said Mein Herr, “map-making. But we’ve carried it much further than you. What do you consider the largest map that would be really useful?”

“About six inches to the mile.”

“Only six inches!” exclaimed Mein Herr. “We very soon got to six yards to the mile. And then came the grandest idea of all! We actually made a map of the country, on the scale of a mile to the mile!”

“Have you used it much?” I enquired.

“It has never been spread out, yet,” said Mein Herr: “the farmers objected: they said it would cover the whole country, and shut out the sunlight! So we now use the country itself, as its own map, and I assure you it does nearly as well.”

If a knowing entity were the world, it could not act appropriately to its circumstances because anything, everything, and nothing would be appropriate no matter the state of the world. The world cannot respond “appropriately” to itself. This point seems related to Wittgenstein’s private-language argument (1958, pp. 89e-96e [§§244–271]). The phenomenon in both cases, knowledge or language, cannot exist without multiple interacting agents. If we divide agents from one another and narrow our focus down to one atomic unit, language disappears. If we merge agents into one another and expand our focus up to one all-encompassing unit, knowledge disappears. Recall that a language is “private” for Wittgenstein if it cannot be understood by others because it refers to supposedly private sensations. Such a “private language” is impossible, Wittgenstein maintained, in part because “I have no criterion of correctness” for the use of the language. He says, “One would like to say: whatever is going to seem right to me is right. And that only means that here we can’t talk about ‘right’” (1958, p. 92e [§258]). Similarly, there would seem to be no criterion of appropriateness or adaptivity for the “actions” of the world as a whole.

Presumably, “my” definition of knowledge as “reliably acting appropriately to circumstance in a class of cases,” is wholly unoriginal. It seems to be no more than what Kauffman had in mind in the passage quoted above. It seems close to Hayek’s (1978, p. 41) view that knowledge “consists in the action patterns” that “stimuli tend to evoke.” Hayek, however, seems to have thought that only a “mind” can “know,” whereas plants and, I think, machines can “know” under my definition.

I look forward to learning whether my definition of knowledge is agreeable to Scheall. This definition is meant to capture how we use the word knowledge in a broad class of cases. One may, I suppose, stipulate that knowledge is justified true belief. It may then be an open question, however, whether anyone ever “knew” anything in that stipulated sense. And, of course, such a stipulative definition is incomplete without a reasonable account of justification. Nor am I personally confident that I know what the word “belief” means in this context.

Similar questions could be raised of “my” definition as well. In particular, I have been pretty vague about what I mean by “a class of cases.” If I am right to think that a definition of “knowledge” should not be imposed upon the reader, but emerge from practice, then it may be acceptable to be vague about the mean-
ing of “class of cases.” I might meditate, like Descartes in his closet, upon the true essence of “knowledge” and then capture that imagined essence in some deft verbal formulation. And perhaps that procedure would be appropriate and successful if knowledge were individual. But if knowledge is social, then it is probably impossible to meditate oneself to anything like its true essence. Such a Cartesian meditation is unlikely to achieve its end because the “true essence” of knowledge is a matter of social science and biology. But then our knowledge of what knowledge is must be in part a product of empirical studies in the social and biological sciences. And in that case, we cannot separate the question of what knowledge “is” from the question of how some relevant scholarly community might best construe the term. In this case as in so many others, it seems, “the meaning of a word is its use in the language” (Wittgenstein 1958, p. 206 [§43]). Debates over the meaning of “knowledge” become debates over how a potentially ill-defined community of scholars should use the word. Whether the “class of cases” is adequately identified within a scholarly community will depend on that community and may change as the community’s conversation unfolds. As Imre Lakatos’s Proof and Refutations (1976) illustrates, scholarly standards may evolve over time. If so, then it would be inappropriate for me to pretend to specify what, precisely, “a class of cases” means. It would be inappropriate for me to specify for an indefinite host of other scholars, present and future, what criteria they should apply in deciding whether someone’s articulation of a “class of cases” is definite enough to be useful for their scholarly purposes.

We imagine, I said above, that a knowing entity’s adaptive actions are “enabled by some disposition, habit, skill, or internal state or condition.” If that statement is correct and coherent, then it makes perfect sense to say, with Scheall, “the success of purposeful political action is necessarily limited by the nature and extent of policymakers’ ignorance and their capacities to learn” (p. 1). The “ignorant” policymaker has no disposition, habit, skill, or internal state or condition enabling them to reliably act appropriately to circumstance. If my perplexities about Scheall’s position were not mistaken in the first place, then perhaps they can be successfully answered by defining “knowledge” as “reliably acting appropriately to circumstance in a class of cases.”

I have elsewhere (Koppl 2018) argued that knowledge is in the main “synecological, evolutionary, exosomatic, constitutive, and tacit.” Briefly, knowledge is “synecological” if the knowing unit is not an individual, but a collection of interacting individuals. It is “evolutionary” if it emerges from an undirected or largely undirected process of variation, selection, and retention. It is exosomatic if it is somehow embodied in an object or set of objects such as a book or egg timer. It is constitutive if it constitutes a part of the phenomenon. The “knowledge” of Roman augurs studying bird flights was constitutive because it influenced events such as when or whether an enemy was attacked. And, finally, knowledge is tacit if it is not “discursively ef-fable.” I have suggested the acronym SELECT as a memory aid. The “L” in SELECT is meant to represent the L in “evolutionary.” Thus, knowledge is Synecological, EvoLutionary, Exosomatic, Constitutive, and Tacit.

Garzarelli and Infantino (2019) seem to say that my “SELECT knowledge” is but a repackaging of Vernon Smith’s (2009) “ecological rationality.” Perhaps. Whether my view is or is not identical to Smith’s, we both draw heavily on Hayek, as does Scheall. As far as I can tell, my discussion of “SELECT knowledge” is consistent with the meaning of “knowledge” that I have given above. And it may be, perhaps, helpful in working out the epistemic limits of political actors in social systems.

Peter Boettke’s (2018) articulation of Hayek’s “epistemic institutionalism” is helpful as well, I think, both in clarifying the general nature of the “Austrian” project in political economy and in driving that project forward. I value especially Boettke’s analysis of the “epistemic limits of democracy” (2018, pp. 246–251). I value highly the radical epistemic egalitarianism of Boettke’s analysis. Scheall cites Boettke’s book, but expresses disappointment over its failure to engage epistemology proper more deeply. In an apparent reference to Hayek’s famous work in theoretical psychology, The Sensory Order, Scheall says that it is “problematic” that “Boettke neglects Hayek’s theoretical psychology and epistemology” (n. 4, p. 131). I confess that Scheall’s criticism seems misplaced to me. Boettke was doing political economy and not philosophy. It is probably true that, like Scheall, I place greater weight on Hayek’s The Sensory Order (1952) than Boettke.
Rather than cursing the supposed omission, however, it might be better to supply the missing complementary arguments.

I was also disappointed in Scheall’s attack on Mises’ methodology, which I have defended in the past (2002). More recently, Zanotti and Cachanosky (2015) have interpreted Mises’ methodology in more or less the way I do. As I will explain presently, I think Scheall’s misunderstanding (as I view it) may be rooted in a failure to distinguish different time scales in biology.

Scheall says,

As an evolutionary epistemologist, Hayek was open to the possibility that some knowledge might be either naturally selected or passed along to future generations as a kind of genetic inheritance, but he argued that, in the last analysis, all knowledge is due to the organism’s or its ancestors’ encounters with the environment. On the other hand, Mises ([1933] 2003, [1949] 1998, 1962) insisted on the possibility of rationalistic a priori knowledge, that is, knowledge that the organism somehow possesses in advance of its first encounters with the environment, prior to its first experiences, and can discover via internal self-reflection, merely in virtue of being the kind of organism that it is (p. 110).

Scheall is moving the goal posts when he turns from Hayek to Mises in this passage. With Hayek, we are told, an organism’s knowledge “is due to” its individual experience or the experience of its ancestors. Scheall then complains that for Mises, “the organism” has knowledge “in advance of its first encounters with the environment.” Scheall considers evolutionary shaping to be relevant experience when discussing Hayek, while excluding it from consideration when discussing Mises. But, as I develop below, Mises also recognized the role of evolutionary shaping in forming the knowledge of the human organism.

For both Mises and Hayek, all organisms have what Scheall describes as, “knowledge that the organism somehow possesses in advance of its first encounters with the environment.” In his introduction to The Sensory Order, Heinrich Klüver makes the point. “[T]here is, on every level, a part of our knowledge which, although it is the result of experience, cannot be controlled by experience because it constitutes the ordering principle” (p. xxi). In developing his “central contention” that memory precedes sensation (1952, p. 53), Hayek explicitly rejects a central tenet of empiricism as traditionally understood. “John Locke’s famous fundamental maxim of empiricism that nihil est in intellectu quod non antea fuerit in sensu is therefore not correct if meant to refer to conscious sense experience. And it does not justify the conclusion that all we know (quod est in intellectu) must be subject to confirmation or contradiction by sense experience” (p. 167). Instead, he says, “there will exist certain general principles to which all sensory experiences must conform (such as that two distinct colours cannot be in the same place) – relations between the parts of such experiences which must be true” (p. 167). Hayek emphasized that his rejection of some of the views “traditionally associated with empiricism” did not come from an “opposite point of view, but on the contrary, by a more consistent and radical application of its basic idea. Precisely because all our knowledge . . . is due to experience, it must contain elements which cannot be contradicted by experience” (p. 172).

Hayek infers the principle of methodological dualism from his heavily qualified or, rather, more radical form of empiricism. The insight that not all of an organism’s knowledge is subject to empirical control leads Hayek, by a kind of Cantorian diagonal logic (1952, pp. 184-190) to conclude that we cannot hope for a “complete ‘unification’ of all sciences” that reduces all scientific propositions to physical language (p. 191). (See Koppl 2010. Note also Hayek’s explicit reference to Cantor in 1967, p. 61, n.49.) Hayek explains, “In the study of human action, in particular, our starting point will always have to be our direct knowledge of the different kinds of mental events, which to us must remain irreducible entities” (p. 191). Hayek speaks favorably of “introspective” knowledge, which lets us “understand” others. This is “introspective psychology” takes “our direct knowledge of the human mind for its starting point” (p. 192). He even uses the term “verstehende psychology” (ibid) as if to leave us in no doubt that he is invoking the “understanding” tradition of Wilhelm Dilthey, Max Weber and others, upon which Mises built.
Mises also attributes the a priori to biological evolution. “The problem whether there are or are not a priori elements of thought,” Mises insists, “must not be confused with the genetic problem of how man acquired his characteristically human mental ability” (1949, p. 33). He goes on to attribute this acquisition to biological evolution, noting that humans are “descended from nonhuman ancestors.” The human mind, therefore, is “an adaptation of man to the conditions of his environment.” As if to squash all doubt on these points, he says, “Reason, intellect, and logic are historical phenomena” (p. 33). And, “Reason and mind . . . are embedded in the continuous flow of zoological events. They are neither eternal nor unchangeable. They are transitory” (p. 34).

Scheall notes Mises’ evolutionary account of the categories, but rather mysteriously dismisses it. He says it is, “sketchy and manifestly inconsistent with other passages that surround it in the relevant texts, which seem to deny experience a role in the construction of a priori knowledge and appear to double-down on rationalist apriorism, that it is difficult to take seriously” (p. 135, n. 38). Scheall provides no argument or evidence for the supposed inconsistency between Mises’ comments on the evolution of human cognition and his methodological apriorism. Scheall shares only his dismissive judgment that it is so. Does the “experience” of our biological ancestors count? When discussing Hayek, Scheall says that it does. When discussing Mises, Scheall says that it does not. In my view, we can and should construe Mises methodology to be self-consistent. I think we should seamlessly integrate his “methodological apriorism” with his evolutionary account of the emergence of human rationality. This reading seems to be correct and straightforward. Scheall, instead, simply dismisses Mises’ evolutionary account, which frees him to then criticize Mises for neglecting the role of evolution in shaping human knowledge.

Scheall includes prehuman experience with Hayek, but excludes it with Mises. I think a greater attention of the role of time in evolutionary theories might have helped to avoid this unwitting shifting of standards when moving from Hayek to Mises. In his classic article, “Time in Biology,” J. B. S. Haldane (1956, p. 398) said, “It is clear that the different time scales used in biology require different types of thought. Further, our knowledge about the events on these scales is based on different sets of facts.” He illustrated this principle by providing very distinct answers to the question “Why does the male chaffinch sing in spring as he does?” (Haldane 1956, p. 388). He gives five very different answers, each appropriate for a different timescale. In the shortest timescale, the answer is that certain “muscles contract as the result of transformations of adenosine triphosphate and other substances.” At one of the intermediate timescales the answer is that “longer spring days” have “produced hormones which act on his brain.” And on the evolutionary time scale, it is because it yields and evolutionary advantage “that small birds should sing . . . to repel other males.” When we consider the role of experience in regulating and shaping an organism’s knowledge, we must not neglect the different time scales Haldane points us to. The knowledge a human may acquire through personal experience may change quickly. The human knowledge hammered out in evolutionary time may be impossible to change in the span of one person’s life or, indeed, in several generations. The experience of the individual organism cannot regulate such knowledge even though it is subject to revision in evolutionary time.

Hayek and Mises both say that a part of the evolved structure of the human mind is immutable and beyond control or regulation by experience because, in Klüver’s words, “it constitutes the ordering principle” (p. xxi). This idea we have seen in Hayek’s The Sensory Order. It is in Mises’ Human Action as well. “Man acquired . . . the logical structure of his mind,” Mises says, “in the course of his evolution from an amoeba to his present state. But these tools are logically prior to any experience” (1949, p. 35). He then explains in what sense he is an apriorist. “The fact that man does not have the creative power to imagine categories at variance with the fundamental logical relations and with the principles of causality and teleology enjoins upon us what may be called methodological apriorism” (1949, p. 35, emphasis in original).

The pragmatic character to Mises’ supposed “apriorism” clarifies, I think, an otherwise ambiguous statement I have purposefully skipped over until now. While commenting on the evolution of the human mind, Mises says, “Hence the empiricist concludes that the fundamental principles of reasoning are an outcome of experience and represent an adaptation of man to the conditions of his environment” (p. 33). The
sentence seems to take distance from empiricism. But the rest of Mises' commentary in this part of Human Action is unambiguously favorable to a broadly Darwinian understanding of the biological evolution of humans. Was the quoted passage, then favorable or unfavorable to the view that “the fundamental principles of reasoning” are adaptations shaped by experience in evolutionary time? I think we can now see that Mises is in agreement with the idea that “reason” is a biological adaptation but wishes to distance himself from the mistaken inference that nothing is beyond regulation by the individual organism’s experience. Indeed, Hayek and Mises both seem to be saying that it is precisely the evolutionary shaping of the mind that puts some core principles of its operation beyond the control of experience for the organism, as opposed to the species, genus, family, order, class, phylum, kingdom, or domain.

Mises also seems to have objected to the way in which the word “experience” is being used by “the empiricist.” In The Ultimate Foundations of Economic Science he again puts forward the “hypothesis” that the categories evolve. “However,” Mises warns us sternly, “reference to this interpretation of the origin of the a priori categories does not entitle us to call them a precipitate of experience, of a prehuman and prelogical experience as it were” (1962, p. 15). Mises restricted the word “experience” to mean “a mental act on the part of thinking and acting men” (Ibid). While I would prefer to give the word a wider meaning, it must be admitted that dictionary definitions often seem to link experience to conscious human knowledge. In any event, a stipulation that only biologically modern humans have “experience” would prevent one from saying, with “the empiricist,” that “the fundamental principles of reasoning are an outcome of experience.” One would be prohibited from saying so even if one agreed with the substance of the claim that the “experience” of our biological ancestors has shaped our reason.

Mises’ stakes out the same basic methodological position in Ultimate Foundations and Human Action. But there seem to be differences between them. It seems to me that Mises position in Ultimate Foundations is less subtle, although I am not prepared to properly defend that impression. His position on “the reality of the external world,” for example, seems less nuanced in Ultimate Foundations. As far as I can tell, however, these differences don’t matter for this essay. If I am mistaken on that point and they do matter, then I will stipulate that it is Mises’ earlier position that I am defending against Scheall’s criticism. Such a stipulation may be reasonable given that Scheall does not distinguish between Mises’ earlier and later methodological positions.

I think it will surprise many readers to be told that Mises’ much derided apriorism was a pragmatic methodological contrivance thrust upon us only by our want of “creative power to imagine” a mind functioning much differently than the human minds we are familiar with, our own and others’. It will surprise readers to be told that Mises’ “methodological dualism” does not necessarily preclude philosophical empiricism. And yet no less a figure than John Stuart Mill was both a philosophical empiricist and methodological apriorist, as Mises notes. Mises points out that Senior, Mill, Cairnes, and Weiser all had methodological views “not, in fact, very far” from his views (Mises 1933, p. 18). Mill rejected any “supposed mode of philosophizing, which does not profess to be founded upon experience at all” (1877, p. 143). But this philosophical position did not prevent Mill from recognizing both “induction” and “ratiocination” in science. Mill’s “method à priori” is “reasoning from an assumed hypothesis,” which he took to be “a mixed method of induction and ratiocination.” The “method à posteriori . . . requires, as the basis of its conclusions, not experience merely, but specific experience” (p. 143). With Mill’s “method à priori,” the empirical element in “the science of Political Economy” comes in the application of its conclusions. Says Mill, “To verify the hypothesis itself à posteriori, that is, to examine whether the facts of any actual case are in accordance with it, is no part of the business of science at all, but of the application of science” (p. 143, emphasis in original). Mises makes a similar, if not identical, claim when he says, “All theorems of economics are necessarily valid in every instance in which all the assumptions presupposed are given” (1949, p. 66). For example, “The theorems referring to indirect exchange are not applicable to conditions where there is no indirect exchange. But this does not impair their validity” (pp. 66–67).

Hayek rejected the view that consciousness is self-transparent. By the “diagonal” logic we noted above, Hayek was led to the conclusion that “mental activity must always be guided by some rules which we are
in principle not able to specify” (1967, p. 60). Mises, too, averred that the mind’s workings are not always, in Scheall’s words, “discursively effable.” Mises distinguished between “conception” and "understanding.” Mises says, “Conception seeks to grasp the meaning of action through discursive reasoning. Understanding seeks the meaning of action in empathic intuition of a whole” (1933, p. 133). Thus, “understanding” is not (in Scheall’s words) “discursively effable.”

To be sure, Mises understanding of “understanding” differs from Hayek’s to at least some extent. Whereas Hayek comes to it through a rigorous scientific argument about how mind emerges from matter, Mises appeals to the philosophy of Henri Bergson. To explain his phrase “empathetic intuition,” Mises favorably quotes Bergson’s definition of “intuition,” as “the sympathy by which one is transported into the interior of an object in order to coincide with what there is unique and consequently inexpressible” (Bergson 1946, p. 190). While I’ve given the quote in English, Mises quoted the original French. It reads, “la sympathie par laquelle on se transporte à l’intérieur d’un objet pour coïncider avec ce qu’il a d’unique et par conséquent d’inexprimable” (Bergson 1934, p. 205 as quoted in Mises 1949, p. 49). It seems unlikely that Hayek would have endorsed this characterization of intuition. This difference does not change the fact, however, Hayek and Mises alike recognized that not all of our knowledge is “discursively effable.”

And yet I would not wish to exaggerate the difference between Hayek’s and Mises’ understandings of understanding. It seems worth noting in this connection what Hayek said in the 1920 manuscript that The Sensory Order was based on. “It is worth noting that H. Bergson reached very similar results by a very different approach and also rejected this atomistic conception most vigorously” (Hayek 1920). Elsewhere, Hayek links the self-opacity of mental function to “Verstehen,” citing precisely Mises’ distinction between “conception” and “understanding” (1967, pp. 58-60). In connection with “understanding” or “Verstehen,” Hayek and Mises both quote Empedocles saying, “Knowledge is of like by like.” (Many years ago, both Sudha Shenoy and Richard Ebeling kindly translated for me the Greek original – γνωσις του οµοιου τω οµοιω – used by both Mises and Hayek.) And Hayek reports that he "owes the quotation from Empedocles" to Mises’ (1933) use of it in “Conception and Understanding.” (In Koppl 2010 I discussed these issues in the context of then-recent developments in cognitive science.)

Mises’ “apriorism” is methodological. And, like Hayek’s “pragmatic dualism,” which Scheall extolls, his “methodological apriorism” is rooted in a model of experience shaping, in evolutionary time, the governing principles and categories of the human mind. And for both Hayek and Mises, this evolutionary shaping of consciousness implies that the human mind is not fully self-transparent. I must, therefore, disagree with Scheall when he says Mises, unlike Hayek, did not recognize that some parts or aspects of our knowledge is “1) due to the species’ confrontations with the environment and therefore modifiable in virtue of new confrontations with the environment and 2) often known only unconsciously or tacitly; that is, they are often only implicit in the organism’s actions and not discursively effable” (p. 123).

I also disagree with Scheall’s harsh judgment that “Mises’ rationalist epistemology must be counted among the worst and more incoherent ideas in the history of economic thought” (p. 7). This judgment seems to be based on a misunderstanding of Mises’ views. It also seems inappropriate to rank Mises’ methodological ideas, which are close to those of J. S. Mill and F. A. Hayek alike, so low when other more wretched candidates can be found. Within the methodology of economics, Othmar Spann’s “universalism” provides an example that may be relatively non-controversial today. In Mises’ day, Spann was an important professor at the University of Vienna. He defended “universalism” against individualism. He taught that society is “based on an objective spiritual reality, upon something which must be conceived as an aggregate of a peculiar kind on a higher plane than the individual and therefore super-individual” (1930, p. 59). This is the view that “the mental or spiritual associative tie between individuals exists as an independent entity; that it is super-individual and primary, whereas the individual is derivative and secondary” (Spann 1930, p. 60). Whatever defects Mises “methodological apriorism” may have, it seems in a different category from Spann’s “universalism” and any number of other miserable ideas that have slithered their way into the economics literature over time.
I have been roused to leap once again to the defense of Mises’ apriorism. But we read the works of other scholars to learn and not to snipe or, certainly, condemn. And, as I hope to have conveyed earlier in this paper, Scheall has something to teach us. His insistence on the centrality of political epistemology to political philosophy is right, I think. And it should rouse many scholars to develop the point in multiple directions. Following Cazzola Gatti et al. (2020) I would value studies of the evolutionary and biological dimensions of political epistemology. Work in Boettke’s “epistemic institutionalism” could be enriched by Scheall’s penetrating analysis. And so on. There is much work to do, many questions to answer, and many questions to ask. Scheall has made an important contribution to the collective work of learning how the production and distribution of knowledge in society shapes the politically possible.¹

NOTES

¹ I thank Scott Scheall for a helpful email exchange on his book. I wish I could blame him for any errors I may have made. Regrettably, however, they are all on me.

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In *F. A. Hayek and the Epistemology of Politics*, Scott Scheall offers an excellent, philosophical and immanent critique of what is now known as ‘classical’ liberal political philosophy, or, Austrian, political economy, and, thereby, renews it. And while some bits involve a creative interpretation of Hayek’s works, it would be a shame if only Hayek-friendly folk read this book. The arguments are fruitful to a wider audience interested in both what we may call ‘low level public policy’ as well as more significant political transitions. This is so, even though at times the fact that it is an immanent critique means it fails to engage with already existing discussions in other parts of political philosophy and public policy/public administration.¹

But rather than griping about the book that Scheall did not write, in what follows I focus primarily on the book that he did write. And before I criticize I call attention to features that make the book significant beyond a very clever self-criticism of the Austrian school. In particular, I am thrilled that Scheall puts what one may call, ‘transition problems’ at the heart of theorizing about political life. Prior to reading Scheall’s book, I understood *the transition problem*, as the challenge of [I] how to move from an unjust (or otherwise bad) status quo to an ideal or vastly improved polity and, in particular, with a population raised under bad institutions, that is, the status quo.

In fact, as I realized by studying Serene Khader’s (2018) work, there are (at least) three species of the transition problem: the first version [A] turns on the challenge of finding or developing the right sort of people (with the right education or dispositions, etc.) to get us from here to there and then to have the skills and temperament to make the new circumstances work out well. The second version [B] is to create mechanisms such that the incentives of policy-makers line up with the goals to be pursued and the true interests of people/constituents and how to get from here (under bad institutions) to there (the institutions with the right mechanisms). The third version is a collective action problem [C] that a population raised under bad institutions may rationally prefer a bad status quo if getting to the better state involves high costs to *them*. Of course, in practice these three versions can be blended in various ways.

To simplify a complex history of political philosophy, perfectionists, including many illiberal types (Plato, Al-Farabi, etc.) tend to focus on [A]. Inspired by Machiavelli and Spinoza, the liberal tradition, including recent work in public choice and mechanism design, is focused primarily on [B].² And progressives/feminists (including liberal feminists) tend to focus on [C].
Scheall is responding to scholars focused on [B] and ignores other versions of the transition problem. He calls this the problem of ‘policy maker incentives’ (p. 15). And given his interest in classical liberalism, Scheall rightly focuses on “Hume and his modern descendants in the public choice tradition of political economy, which make the assumed knavery of policymakers the sine qua non of their analyses,” (p. 17).

But in his argument, and I take this to be an important contribution, he is also very sensitive to the fact that [II] at each step of the way from an imperfect status quo to a better destination involves a transition problem. So, for example, Scheall notes that the mere fact that “policymaking is less epistemically burdensome within more liberal contexts”—let’s stipulate this—“is of no use to the would-be liberalizing policymaker in a comparatively illiberal and thus more epistemically burdensome policymaking context,” (p. 99).

As one of the founder of the Chicago school of economics put it, with perhaps no exaggeration, “one must plan for free-market controls just as carefully as (indeed, more so than) for socialization” (Simons 1945: 213). To put my point historically: Scheall helps recover one of the distinctive insights of the so-called neo-liberal moment in classical liberalism: that getting to spontaneous orders, and getting them to work, may be challenging work. If Scheall’s argument is right, this may be even more challenging than previously realized.

And, once one thinks about it, [III] even maintaining a status quo over time is or can be, in some respects, similar to a transition problem (in the sense of [II]). As illustration, I quote my favorite passage in the book:

Austrian economists and others sympathetic to markets must do more than merely bemoan “crony capitalism.” They have to confront head-on the criticism that cronyism is inherent in capitalism, that making profits necessarily implies the ability to garner and use political power to an individual’s or a firm’s further benefit. It is not enough to simply institute the rule of law. Once instituted, it must be perpetually maintained if the resulting liberal order is to survive and thrive. Otherwise, those citizens not in a position to take advantage of it, unable to nullify the rule in their favor, will eventually come to think the game of society rigged—and they will not be wrong. It is the responsibility of policymakers, if it belongs to anyone, to keep the system from being gamed by the wealthy and powerful. However, this bears an epistemic burden. The effectiveness of the rule of law and, therefore, the effectiveness (qua liberal) of a liberal society depends on the know-how of the policymaker to enforce the rule properly, that is, to prevent its abrogation by the powerful (p. 97).

As Scheall notes, this point generalizes to all versions of transition problems for all political traditions. Scheall’s insight here is central to what makes the best of Ordo-liberalism a distinctive tradition within classical liberalism (e.g., contrast their antitrust policy focused on effective competition to a Chicago inspired antitrust policy focused on consumer welfare). It is notable that the epistemic burden Scheall alerts us to here is not just one of figuring out when and how policy works, but also to prevent moral and political decay and corruption. Or to put it more subtly, the political process is a constitutive feature of the epistemic burdens that policymakers (and other citizens) face. And a liberalism worth having—one that avoids enlightened despotism—better face up to this fact.

Now, in commenting on the differences and similarities among ordo-liberalism, the Chicago school economists, and their imitators in different countries, Foucault notes in the Birth of Biopolitics that, unlike in the origin of the Austrian school, there is a tendency, to take the existence and survival of liberalism as a kind of permanent background tradition for granted in American classical liberalism (including Austrians today) (Foucault 2008: (14 March, 1979), Lecture 9, pp. 217-219). So we can see that Scheall hearkens back to the spirit of the earlier Austrian tradition, which was founded in great political defeat, and thereby reinvigorated Anglophone classical liberalism.

Below I return to what I take to be some of the limits of Scheall’s approach to thinking about transition problems. But first I pause at Scheall’s core conceptual claim that the problem of policy-maker ignorance...
has logical priority to aligning policy-maker incentives with her constituents or society’s needs. The core argument is summarized as follows:

[O]n the assumption that some principle like *ought implies can* is true and practically useful as a guide to action, then the word “can” in such principles must mean *deliberately can*. Other candidate meanings for the word “can” render such principles practically useless. Thus, by reductio, “can” in *ought implies can* (and related principles) means *deliberately can*. But, by definition, deliberately can just means *knows enough to*. Therefore, *ought implies knows enough to*: nothing that we cannot know enough to deliberately realize can be an obligation. The nature and extent of our ignorance place brackets around our potential obligations. Epistemic burdens are logically prior to other normative considerations. Since this is a general fact about human decision-making in all contexts, it follows that ignorance is logically prior to incentives in specifically political contexts (p. 3; emphases in original).

Leaving aside concerns about the possibly plural nature of this *ought*, I offer two criticisms. First, I doubt Scheall is right to assume and argue that ‘ought implies can’ applies to all policy contexts. That’s required for his conclusion to follow. For; some policy decisions and ends are neither normative nor obligations, and not understood as such by decision-makers. (Pick your favorite pork-barrel spending!) So, the way feasibility enters into these decisions may be more as a constraint like other forms of instrumental reasoning (with cost-benefit analysis, opportunity costs, etc.).

In Scheall’s argument what’s happened is an unjustified slip from the fascinating claim that “epistemic burdens are logically prior to other normative considerations” to the conclusion which presupposes something like the thought that decision-making in all political contexts is normative in character. But this has not been shown to be true. Now, there are political theorists/philosophers for whom political philosophy just is ethics or applied ethics, who might endorse this claim, but it is not obviously true.⁸

In addition, second, Scheall ignores that it may at times make sense to pursue infeasible (but by no means utopian) ends *in virtue* of the fact that in some environments aiming too high is better than aiming at a target within reach. Nobody really believes, perhaps, that European states will manage to secure the closest possible union, or that agreeing on transnational functional legal collaboration secures prosperity. But the fact that, as the Treaty of Rome memorably put it, they have tried “to lay the foundations of an ever-closer union among the peoples of Europe,” probably has secured some non-trivial by-products (including a lasting peace between Germany and France after centuries of regular warfare).

But notice that in my two criticisms the epistemic burdens on policymakers do not disappear (although how to think about these epistemic burdens in the second kind of criticism is not trivial). So, even if one wishes to deny Scheall’s general claim, one can preserve the spirit of his project.

I now offer some criticisms of Scheall’s execution of his project in terms that I hope he can agree. It is natural to assume that throughout the book Scheall seems to assume that if policymakers lack the knowledge to pursue an end, they ought not pursue it. For, when, the “do-nothing policy is aimed at no particular end, it bears no epistemic burden,” (p. 98).⁹ But this raises the problem that we might never discover what works.

Here’s a version of what I have in mind. Let’s say one agrees with Popper—who is no Austrian economist, but a fellow traveler—that policy should be informed by the spirit of or guided by trial-and-error.¹⁰ That is to say, and one need not be Popperian to recognize this, only if errors are permitted can policymakers and policy-scientist discover empirically what works, what are unexpected side-effects, and how to make mid-way adjustments.

There are three more subtle points lurking in this Popperian point. First, Scheall tends to treat each policy-decision as a one-off: if you have the knowledge (*ceteris paribus*) you may act for an otherwise moral end, if you lack the knowledge (*ceteris paribus*) you may not. But a Popperian treats public policy as many decisions extended over time. And by doing so a public policy is conceived as learning both what the means
are and what the ends are or might be. That is to say, and this is in the spirit of Scheall, Popperian public policy treats much policy-making as instances of [II], which, recall, involves a kind of transition problem from each imperfect status quo to a better destination.

Second, Scheall’s approach has relatively little space for policy-makers that learn over time. And that’s in part because he tends to think of them as elected (p. 82) politicians (or officials and assemblies) with short time-spans and lots of epistemic burdens. But the modern state has tried to address this: it is full of bureaucracies and dedicated research agencies that are, if institutional memory is allowed to be built up, capable of learning over time.11 On my reading of Lippmann, part of the point of government is to generate conditions—including access to reliable data—that reduce epistemic burdens of policy and social life more generally (Schliesser 2019). This is why some kinds of government budget cuts can be self-undermining.

And it’s not just bureaucrats. As the citations in The Open Society reveal, Popper was a careful reader of Walter Lippmann. And as Lippmann notes one benefit of the ‘revolving door’ is that it would create a kind of permanent circulation of college graduates from universities into government; and technical, government bureaucrats returning regularly to train and teach at universities, etc.12 This is a means of reducing epistemic burden of policymakers.

Now, it’s true that this, in turn, raises other familiar problems (rent-seeking, how to decide which experts to trust, some of which mentioned by Scheall), but this is why Scheall’s work on [II-III] is so important. But the more important point is that given that Scheall understands “epistemic burdens” in terms of “the nature and extent of the ignorance that an actor in a particular context must overcome in order to use some means to deliberately realize some end” (p. 3; emphasis added), he should be receptive to ways that social learning, including by policymakers and their administrative systems, can reduce epistemic burdens of policy making over time.13

I would like to develop the point by dwelling on a fascinating passage in Hayek that Scheall mentions (p. 102, note 5) as an “instance for the conditions in which it might be necessary to deliberately revise the existing system of rules of conduct”:

Where a real change in the law is required, the new law can properly fulfill the proper function of all law, namely that of guiding expectations, only if it becomes known before it is applied.

The necessity of such radical changes of particular rules may be due to various causes. It may be due simply to the recognition that some past development was based on error or that it produced consequences later recognized as unjust. But the most frequent cause is probably that the development of the law has lain in the hands of members of a particular class whose traditional views made them regard as just what could not meet the more general requirements of justice. There can be no doubt that in such fields as the law on the relations between master and servant, landlord and tenant, creditor and debtor, and in modern times between organized business and its customers, the rules have been shaped largely by the views of one of the parties and their particular interests—especially where, as used to be true in the first two of the instances given, it was one of the groups concerned which almost exclusively supplied the judges. This, as we shall see, does not mean that, as has been asserted, ‘justice is an irrational ideal’ and that ‘from the point of rational cognition there are only interests of human beings and hence conflicts of interests’, 37? at least when by interests we do not mean only particular aims but long-term chances which different rules offer to the different members of society. It is even less true that, as would follow from those assertions, a recognized bias of some rule in favour of a particular group can be corrected only by biasing it instead in favour of another. But such occasions when it is recognized that some hereto accepted rules are unjust in the light of more general principles of justice may well require the revision not only of single rules but of whole sections of the established system of case law. This is more than can be accomplished by decisions of particular cases in the light of existing precedents (Hayek 1973, Law, Legislation and Liberty, pp. 88-89, emphasis added).14
I have a reductive tendency to read Hayek’s political theory, especially the parts that treat the rule of law as a spontaneous order, as flirting with a variety of potentially troubling forms of status quo bias. So, it is good to be reminded of this material. Hayek explicitly notes three sources in which an evolved common law can have reached a kind of cul-de-sac such that it requires “radical change” (a term Hayek does not use frequently): (i) an erroneous application of legal principles or a judgment based on errors of fact; (ii) or that some legal principles generate unjust consequences over time; (iii) perversions of judgment—a failure to be properly impartial—due to class bias. On the previous page, Hayek also recognizes (iv) that circumstances may change faster than the law does—here he echoes Walter Lippmann’s focus on how technological innovation generates a need for a a spirit of legal adaptation (Schliesser 2019).

And, in fact, (i-ii-iii) are often connected; these can endure a very long time. These facts are clear from Hayek’s own footnote 36, where Hayek cites, without qualification, the nineteenth century economist, Jevons (one of the co-inventors of marginalism), as follows, “The great lesson we learn [from 650 years of legislation of English Parliaments] is that legislation with regard to labour has almost always been class-legislation. It is the effort of some dominant body to keep down a lower class, which had begun to show inconvenient aspirations’ (Jevons 1882, p. 33). Not to put too fine point on it, but Hayek explicitly recognizes that in some areas the vaunted rule of law, which has generated reliable expectations, has entrenched class warfare for much of its existence. Austrians would be rhetorically more persuasive about shaping the future if they were willing to echo Hayek’s point about the historical trajectory of our institutions more regularly. This requires us to distinguish between the idea of the common law as a spontaneous order, which provides Hayek with a kind of existence proof, and really existing common law.

Because of Hayek’s focus on possible solutions that will not work, and the intriguing thought that a spontaneous order can require ‘radical change,’ it is easy to miss, and so especially worth noting that Hayek also explicitly recognizes that it is difficult for judges to become truly impartial when they are surrounded by like-minded with sympathy for their own. They become effectively what used to be known as a ‘faction’. So, I read Hayek as suggesting that class bias is not noticed as class bias in homogenous population of judges and legislators. That is to say, Hayek recognizes that, at least momentarily, (even class) diversity may be constitutive, epistemically, for the discovery of more general, and more impartial principles of justice.

My view (Schliesser 2017, pp. 202-208) is that Adam Smith was inclined to support as remedy one of the measures rejected by Hayek: “a recognized bias of some rule in favour of a particular group can be correct-ed only by biasing it instead in favour of another.” But Hayek prefers that in practice impartiality is aimed at. So, how can this be generated on his account?

It seems to me that when it comes to the selection of judges (and legislators) some kind of carefully announced and cautiously implemented temporary affirmative action or extra effort to recruit for especially salient forms of diversity can be derived from Hayek. Obviously, this is not required on Hayek’s account if society has other means of discovering ‘more general principles of justice’ and making this discovery felt on the legislative process and case law decisions.

Undoubtedly there are Hayekian arguments against more general use of affirmative action. But if the class of legislators/judges has the character of a faction, that is, it systematically serves the group interests from which judges are appointed, then this can be a useful remedy that does not seem ruled out by Hayek.

I recognize that the previous paragraphs will be met with disbelief by some (but hopefully not Scheall). But notice that Hayek recognizes that the existing rule of law is not sacrosanct and can require quite serious remedy, including the abandonment and overturning of well-established common law. That is itself very costly. I am unfamiliar with Hayek’s views on judicial appointments. But, from the perspective of preventing such radical, destabilizing change, preventing a homogeneous judiciary may well be welcome.

You may well wonder why I have strayed so far from Scheall’s argument. But if we think of judges as experts of a certain sort, we can see that Hayek’s concern generalizes. And this is a concern explicitly explored in Chapter 5 of Scheall’s book. Epistemic systems can present themselves as authoritative, but can be subordinated to special interests (pp. 146-151). And, as Hayek’s account of judicial authority reveals, these interests may include class or faction and be sustained for long periods. So, somewhat surprisingly if one
thinks through the implications of Scheall’s argument one subtly moves from Vienna to Frankfurt. How to protect science from the political process (broadly conceived, including other biases, including the effects of a potentially malfunctioning credit economy) is, thus, a constitutive, even reflexive feature of the epistemology of politics.

I now turn to my third Popperian inspired criticism. A further fact these two features have in common is Scheall’s apparent risk aversion to policy error. There is an interesting question—familiar from debates over Rawls’ original position—how much risk aversion is justifiable in foundational questions of political philosophy. But my interest here is to note something slightly different: the effect of such risk aversion is not just status quo bias in policy, but also status quo bias in policy science. The uptake of Scheall’s approach in political epistemology by policymakers will create a foreseeable demand for the kind of scientific work that compares relatively focused policy interventions in restricted contexts. In fact, it is unlikely that such research will draw on Austrian insights or promote liberal goals (let alone Hayekian liberal utopia), but rather it will use data-mining of large administrative data-sets to make policy slightly more effective and efficient. Perhaps this is a welcome effect for Scheall?

Now I have hedged my way of formulating these three criticisms because one can read Scheall as being agnostic on what ought to be done in the presence of policymaker ignorance. And it is easy to see why; in such circumstances, doing nothing also counts as something. And in personal correspondence he acknowledges “There may be (non-epistemic) reasons to pursue a policy, even in the presence of policymaker ignorance and weak spontaneous forces.” As Scheall puts it in the book, “skepticism—in the Humean sense of unbelief/agnosticism rather than positive disbelief/atheism—is the ideological implication of the problem of policymaker ignorance” (p. 77; emphasis in original). And, as is well known, Humean skepticism is directed at theory/reason not directed at action. The worry, of course, is that such skepticism may be a cover for unprincipled political decisionism.

Scheall’s acknowledgment that there may be non-epistemic reasons to pursue policy recalls my slightly different critical point above that not all given policy decisions and ends are normative in character even if taking a policy decision may have normatively valuable consequences. As Hume already noted, government’s or political leaders’ roles in coordinating social life may be done for the worst reasons, but that they have such a coordinating function may well generate important and valuable social consequences (Sabl 2015; Schliesser 2018).

Let me close with a lament that is, perhaps, more orthogonal to Scheall’s own understanding. Scheall does not explore how we should think about existing examples of effective public policy for uncontroversial normative ends. I am thinking of, say, the eradication of highly contagious and costly diseases with high mortality and/or awful symptoms (e.g. polio, measles, etc.). Even if one allows opportunity costs, waste and true mistakes, these policies tend to survive reasonably robust cost-benefit analyses and are worth having. Some of these eradications have been coordinated by WHO.

Now I do not mean to suggest WHO is perfect or that its recent track-record this pandemic year does not merit criticism. But as Scheall notes, the “evaluation of epistemic mechanisms is always relative and never absolute. What matters is how well some mechanism coordinates knowledge and facilitates the making of actionable plans in a particular domain as compared to other possible mechanisms or to absence of epistemic mechanism altogether” (p. 140). Scheall here misses his own best insight. For, the comparison we must make is not to another possible mechanism (or the absence of an epistemic mechanism), but also the costs and challenges of a route or path to such a possible alternative. The WHO may be imperfect, but it exists and has done important work.

The question is now how Scheall’s political epistemology understands how policy-makers should relate to such existing institutions or to possible institutions that may arise from feasible extensions from the status quo. That is to say, I hope Scheall writes a follow up book that will engage with the more messy details of our political life. What we need now, I guess, is applied political epistemology.
NOTES

1. So, for example, concern with ‘epistemic feasibility’ is central to the book (pp. x; 8; 85-102). But the huge literature on feasibility in political philosophy is ignored (e.g., Lawford-Smith (2013); Gilabert and Lawford-Smith (2013); Southwood (2016); and Gheaus (2013)).

2. For a narrative that unpacks that claim, see Schliesser (2021).

3. This point is not entirely original in Scheall. It has also been grasped by sometimes activist-scholars who embrace so-called prefigurative politics. See Raekstad and Grdin (2020); Rossi (2019).

4. Scheall does not engage with the voluminous work prompted by the theory of second best (and nth best, etc.) While that literature is not Austrian in character, it gives a sense of how to begin to think about transitions under constraints. See, for example, Lipsey and Lancaster (1956); Rodrik (2008).

5. This resonates, indirectly with deeper roots in the Austrian tradition. See Dekker (2016).

6. For more on this see, Schliesser (2019).

7. That’s really one of the theses of Dekker (2016).

8. There is a whole school of political realism that objects to the ethics first approach of political philosophy. Austrian political economists, who may embrace a normative/positive distinction, may also naturally disagree with Scheall. For discussion of the latter see Gordon (2019).

9. I worry this fails to adhere to his own insight [III] unless we sharply distinguish between do-nothing policy aimed at status quo and do-nothing aimed at no particular end.

10. This is not to deny that Popper’s stance is more receptive to a generally more activist policy stance than Hayek’s. For useful work in the vicinity of the idea presented in the body of the text, see Kerstenetzky 2007. For a more critical perspective, see Friedman (2005).

11. Here the problem of incentives may well be more important than Scheall allows.

12. For more details that draw on Lippmann’s (1922) Public Opinion, see Cowen and Schliesser 2020.

13. Scheall alerted me to this in correspondence. See also his claim on p. 1: “The problem of policymaker ignorance is the simple fact that the success of purposeful political action is necessarily limited by the nature and extent of policymakers’ ignorance and their capacities to learn.” Developing the administrative state, with associated research institutes and labs, is also a development of the policymaker’s capacity to learn over time.

14. The passage also figures importantly in Levy (2019), especially the discussion on p. 57.

15. For historical context, see Jackson (2012).

16. See also Foucault 1979, Lecture 5.

17. I have in mind the kind of work done by Raj Chetty and his collaborators, see, e.g., Card et al.

18. This is how Scheall understands his own position (personal correspondence, 10/21/2020).


20. Cf. My praise for Smith’s claim that we need the right sort of systems to guide social life (Schliesser 2017).

21. If you think those ends are controversial, we may not be able to talk to each other!

22. For a useful framework, see Barrett (2004).


24. I thank Erwin Dekker for his feedback, and William Butos for his editorial suggestions.
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RESPONSE TO BOETTKE

Professor Boettke and I have been engaged in a running debate over the last few years that has mostly escaped wider notice, as it involves extremely mundane matters of interest only to Austrian methodology nerds.¹ There is some irony in the fact that I have chosen to confront Boettke on his home territory. Not only is he the reigning undisputed world expert about all things Austrian, but he has always been an extremely kind and generous supporter of my career, a generosity I have repaid by repeatedly attacking his arguments in print.² So, I welcome the opportunity to clarify what, from my perspective, separates us on the crucial, if painfully nerdy, issues at stake.

I agree with Professor Boettke that my project in “Hayekian political epistemology” is consistent with and complementary to his project in “epistemic institutionalism.” A few contributors to this symposium suggest that I reject Boettke’s epistemic institutionalism, but this is not true. In fact, I accept epistemic institutionalism, albeit with the qualification that there is more still to be said about the epistemic side of epistemic institutionalism. Boettke should take the epistemic aspect of epistemic institutionalism as seriously as he takes the institutionalist aspect. My book can be read as an effort to fill in what I take to be the lacunae in this part of Boettke’s epistemic institutionalism.

From my perspective, the problem is that Boettke wants to talk about epistemics—i.e., he wants to talk about the study of “the use of knowledge in society”—but not about epistemology. He wants to talk about how knowledge is used in society, without saying what knowledge is and how it is acquired. I deny that epistemics can proceed without a grounding in some general theory of knowledge. The general epistemology we adopt will determine much about our epistemics. If we assume, say, a Cartesian epistemology in which individuals have automatic and infallible access to their own mental states, a theory of knowledge in which certainty about the world of experience is assumed to be epistemically possible, then the study of the use of such knowledge in society—epistemics—will lead to different results than if we assume, say, a Humean epistemology in which certainty of empirical matters is impossible and individuals do not have infallible access to, well, anything. Founded on a Cartesian epistemology, epistemics will frequently invoke the use of knowledge of one’s own mental states, conceived as certain and infallible. Based on a Humean epistemology, epistemics will never invoke infallible knowledge of one’s own mental states; all knowledge other than that of the demonstrable will be conceived as uncer-
tain and merely probable. Epistemics will look different depending on the theory of knowledge, the epistemology, that grounds it.

So, my first complaint against Boettke is that epistemic institutionalism needs an underlying theory of knowledge and its acquisition. Epistemic institutionalism without an underlying epistemology is empty or, at best, shallow. If the epistemic institutionalist does not define what he means by knowledge, if he remains silent on the nature of learning, then he is ultimately relying on an unrigorous, folksy, conception of human psychology and epistemology that is likely of dubious value for scientific research.

My second complaint is closely related to the first. Inasmuch as Boettke countenances epistemology, he continues to push the, to my mind, untenable notion that there is positive value to be found in Mises’ epistemological writings.

I will address this issue in more detail below when I consider Professor Koppl’s invocation of an evolutionary interpretation of Mises’ epistemology, but the problem with Mises’ epistemology is not that it cannot be rationally reconstructed. The problem is that it can be rationally reconstructed in too many ways, in ways that are mutually inconsistent with each other, and that we lack criteria for choosing between these mutually inconsistent interpretations. So, even though it is true, as Koppl notes, that Mises offered a sketch (but, I think, no more than a sketch) of an evolutionary epistemology. It is also true that Mises wrote other things that cannot be squared with an evolutionary epistemology. Neither is this a simple matter of Mises’ epistemological views changing over time. He was occasionally inconsistent about epistemological matters within the same work, sometimes from one page to the next, as I will show below.

So, which was the true Mises, the evolutionary epistemologist or the epistemologist who denied experience any part in our knowledge of human action? Or some other Mises?

The various parties to this debate—and there are many, corresponding to diverse, mutually-inconsistent, interpretations of Mises’ epistemology—simply pick their favorite Mises on whatever grounds they happen to find appealing and ignore all of the textual evidence that conflicts with their preferred interpretation. There is a Kantian Mises, an Aristotelian Mises, a Millian Mises, a Lakatosian Mises, a conventionalist Mises, an anti-conventionalist Mises, and still others. These cannot all be the real Mises.

My own view is that we should not pick and choose between various readings of Mises’ epistemology, but that we should take it on its own terms, acknowledge its manifest inconsistencies, and look for a consistent alternative that serves the purposes of Austrian economics. Austrians have available an internally consistent, scientifically and philosophically respectable, naturalistic epistemology—Hayek’s—that can replace Misesian apriorism without loss (indeed, I suspect, with some considerable gain) to the Austrian canon. All that is required is to utter those three little words that, for whatever reason, stick in the throats of so many Austrians: “Mises was wrong.”

So, from my perspective, the difference between Boettke and I concerns the epistemological underpinnings of epistemic institutionalism: in the first instance, whether such underpinnings are necessary (I think they are) and, in the second, whether Misesian underpinnings are sufficient (I think they are not) for the purposes of epistemic institutionalism.

Before proceeding, I’d like to clarify a few further points about the book in light of Boettke’s review.

First, I am a great admirer of Don Lavoie’s work on the history and significance of the socialist-calculation debate. I refer to it repeatedly, and mostly positively, in the book. My only real criticism of Lavoie’s work in this area is that he basically had, all the way back in the early 1980s, all of the ingredients required to make the argument concerning the priority of the problem of policymaker ignorance that I make in the first chapter of the book, but 1) he did not pull all of these ingredients together, and 2) he did not recognize the full generality of the problem and, thus, its relevance to liberalization policies.

Related to this, I do in fact engage with the work of James Buchanan many times in the book (although some of this discussion is relegated to footnotes). The first chapter of the book includes an explicit attack on the conception of policymakers (knaveish, but knowledgeable) employed in public choice analysis and constitutional political economy. Indeed, there is a long footnote in the first chapter (p. 31, fn. 7) in which I
explicitly criticize Buchanan for getting the logical relationship between ignorance and incentives exactly wrong, while praising Lavoie for getting it exactly right.

Boettke also suggests that I should engage with Koppl’s *Expert Failure*, but this is a completed task. In my contribution to the *Cosmos + Taxis* symposium on *Expert Failure*, I explicate what I take to be the implications of my project for Koppl’s and of Koppl’s project for mine (Scheall 2019, pp. 45-46).

One might infer from Boettke’s comments that I ignore expectations in the book. It is true that I do not use the word very often, but expectations are central to the Hayekian epistemology developed in Chapter Four, according to which knowledge is actionable belief. For Hayek, knowing is all about putting one’s beliefs to work to act effectively in the future. To know is to be able to plan (if only tacitly) for the future and to realize relevant goals without the need for the intervention of spontaneity. One knows inasmuch as one’s expectations of (i.e., beliefs about) the future are actionable.

**RESPONSE TO KOPPL**

Professor Koppl takes me to task, justifiably so, for failing to proffer a straightforward definition of knowledge in my discussion of Hayek’s epistemology. The definition of knowledge as actionable belief is in fact there in Chapter Four, but it is admittedly rather obscure and certainly not highlighted as much as it should be, given its central importance. Knowledge is “belief that can be put to work in the service of deliberately realizing the believer’s goals” (p. 128). With regard to a particular policy goal, policymakers “know to the extent that they can design, implement, and deliberately (i.e., without need for intervention of spontaneous forces) realize the goal—that is, inasmuch as they can form, even if only in principle, an actionable plan for the realization of the goal—and their (policy-relevant) knowledge consists of everything that is assumed, implicitly or explicitly, in their actionable political plans” (p. 128; italics added).

There are two points to note about this definition vis à vis Koppl’s criticisms.

First, given Hayek’s epistemological naturalism and practical dualism about mentalistic language, “belief” is ultimately mentalistic shorthand for some natural phenomena occurring in the physical brain about which our knowledge is, and is likely to remain, too limited to explain and predict in detailed physical terms. I hope this addresses Koppl’s comment that, in his view, knowledge need not be a kind of belief. If what he means by this is that knowledge is ultimately a physical phenomenon in the brain, then we are not far apart. Indeed, if what he means—and this seems to be what he suggests—that knowledge is a physical phenomena that affords successful action or, what may be the same thing, effective adaptation to relevant stimuli, then our positions are very close, if not identical.

Second, I believe that the italicized qualifications—“if only in principle” and “implicitly or explicitly”—take care of Koppl’s concern about the role I attribute (or do not) to deliberation in decision-making. Perhaps because the book is primarily concerned with the knowledge and ignorance of human policymakers and, concomitantly, with the knowledge required to make political plans effective, it might seem that I attribute to conscious deliberation a necessary role in successful action and, thus, to actionable belief. However, the qualification that actors—be they policymakers or, to consider Professor Koppl’s example, *E. coli* bacteria (inasmuch as there is a difference between policymakers and bacteria)—need only be able to plan in principle, and the related qualification that these in-principle plans may be built entirely on implicit (i.e., tacit) assumptions should suffice to counter Koppl’s concern that, on my rendering of Hayek’s epistemology, organisms incapable of conscious deliberation, like *E. coli* bacteria, cannot possess knowledge. Given that it hinges on the deliberate realization of an actor’s goals, my definition of knowledge requires a certain purposefulness or, perhaps better, a directedness, of action, toward various states of affairs, but it does not require that the actor (or organism) be conscious of these states of affairs or of the knowledge that might figure in attempts to realize them.

Professor Koppl’s claim that I am “moving the goal posts” in crediting Hayek with an evolutionary account of *a priori* knowledge that I deny to Mises is, however, less justified. If the relevant question concerned whether Mises ever wrote anything that can be construed as favorable to an evolutionary epistemol-
ogy, then I would, indeed, be moving the goal posts in crediting this to Hayek and denying it to Mises. But this is not what is at issue in the relevant passages of the book. The question is whether Mises or Hayek offered a coherent and well-developed evolutionary epistemology that is consistent with their other epistemological pronouncements, and with how they used epistemological concepts in their social-scientific work. The question is whether we can look at the scope of their respective epistemological arguments and their use of these arguments for substantive purposes, and discover that a consistent evolutionary-epistemological story is being told. Looking at their respective careers, I think it is clear that this question can be answered in the affirmative with respect to Hayek, but not with regard to Mises.

Perhaps I should have better explained in the book my resistance to a reconstruction of Mises’ apriorism along evolutionary-epistemological lines, but, as I have addressed this issue before, most extensively in my “What is Extreme About Mises’ Extreme Apriorism” (Scheall 2017a; also see Scheall 2017b), I chose to merely cite this literature, rather than repeat the relevant arguments. In this essay, I argue that the extremeness of Mises’ apriorism lies not in the extent of his apriorism—after all, Mises claimed only that the so-called “action axiom,” “Man acts,” was a priori—but in his epistemological justification of the claim that the action axiom was known a priori. I then compare Mises’ arguments with the epistemological justification of every other committed apriorist in the history of economic thought—including Nassau Senior, John Stuart Mill, J. E. Cairnes, and Friedrich von Wieser—to show that no other apriorist offered a justification of their apriorism as epistemologically extreme as Mises’ justification of the action axiom.

In particular, only Mises defended both the “Reason without Experience” and “Greater Certainty” theses. According to the first, pure reason and not engagement with the environment is the source of the economist’s knowledge of fundamental principles, axioms, etc. Mises defended this thesis in a number of places across several decades (Mises [1933] 2003, pp. 13-14; [1949] 1998, p. 64; 1962, pp. 71-72). According to the second thesis, pure reason delivers to the economist more secure knowledge of his explanans than the natural scientist can acquire via observation and experimentation. Again, Mises defended this thesis across the decades (Mises [1949] 1998, pp. 39–40; 1962, p. 71). It is this combination of theses that constitute, what I call in the book, Mises’ “rationalistic apriorism.” Whatever else might be said about other elements of Mises’ epistemology, he was clearly an extreme rationalist about knowledge of the action axiom.

As noted above, it is true that Mises sketched the contours of an evolutionary account of a priori knowledge. However, I deny that his gestures toward the role of evolution in shaping the human mind amount to anything more than a mere sketch, that they constitute anything comparable to the systematic and scientifically-astute evolutionary epistemology that Hayek developed over the course of his career, starting with his first academic writing in 1920. More importantly, I deny that Mises’ sketch of an evolutionary epistemology is consistent with his extreme stance on both the Reason without Experience and Greater Certainty theses.

Rather than repeat myself, I will merely quote some relevant passages of my review of Alexander Linsbichler’s excellent book Was Ludwig von Mises a Conventionalist? (Scheall 2017b) that are relevant to Koppl’s defense of Mises.

Mises ([1949] 1998, 34) [...] insisted upon the “essential and necessary” “character of the logical structure of the human mind” immediately before claiming that “[m]an acquired...the logical structure of his mind in the course of his evolution from an amoeba to his present state” (Mises [1949] 1998, 35). The evolutionary epistemology implied by this latter statement would, if adequate, put paid to the former assertion of the necessity of the mind’s logical structure. If it is a contingent fact that we evolved as a species in the way that we did—which is, of course, an implication of biological evolution—then the logical structure of the human mind could have been different than it is and, thus, cannot be essential and necessary. This conjunction of propositions suggests confusion, either about the multiple meanings of necessity by then common in the philosophical, logical, and scientific literatures, or about the highly contingent nature of evolutionary processes [see following footnote]. The problem of rationally reconstructing Mises’s epistemology is that of reconciling these (and other) instances of seeming epistemological incoherence.
The relevant footnote reads:

There are passages in Mises’s methodological writings that suggest he was either not privy to or did not appreciate the significance of many developments in contemporary philosophy and natural science. For example, as late as his last methodological work, 1962’s *Ultimate Foundations of Economic Science*, Mises (1962, pp. 12-14) argued for Euclidean geometry as an example of the Kantian synthetic a priori, a position undermined by the discovery of non-Euclidean geometries in the nineteenth century (Caldwell 1984, p. 368 makes the same point) and further confounded by Einsteinian relativity. Similarly, Mises ([1949] 1998, pp. 72-91) continued to argue against the possibility of polylogism long after the existence of multiple logics had been established empirically.

I conclude this review:

Any proposition can be inferred from a contradiction. The fact that many mutually impossible epistemological propositions have been inferred from Mises’s writings is an abductive warning that there may be nothing of substance—no “there”—there. That is, the best explanation of the chaotic state of the literature is Mises’s own epistemological confusion. Ultimately, we simply do not know what the historical Ludwig von Mises believed about epistemology.

Suffice it to say that I am unmoved by Professor Koppl’s defense of Mises’s apriorism as built on evolutionary epistemological grounds and his view that Mises and Hayek are epistemologically reconcilable, a view that, as I noted above, Hayek himself always denied.

RESPONSE TO DEKKER

*F. A. Hayek and the Epistemology of Politics* recommends a kind of analysis and suggests ways that this analysis might proceed in the future, but it does not engage in such analysis. It is not my view that “policy-makers will inevitably lack the necessary knowledge to achieve policy goals.” Neither do I argue that, “[e]ven if we would grant policymakers sufficient knowledge […] they would still be ignorant about the goals of their constituents.” Rather, I argue that the nature and extent of policymaker ignorance is always and everywhere an empirical matter open (to some degree) to scientific analysis. In the absence of the suggested analysis, it is not appropriate to advance positive or negative policy, or political-philosophical, recommendations. What Dekker reads as nihilism is a studied agnosticism consistent with—indeed, necessitated by—the book’s philosophical nature.

The analysis that the book recommends is precisely inquiry into “the general nature and range of our ignorance concerning” society that Hayek (1960, p. 23) advocated as a necessary precursor to understanding “how society works” and to the positive policy advice that Dekker seeks. Part of my argument, however, is that Hayek’s positive policy and political-philosophical proposals failed to meet the standards he set for others. Hayek’s constructive suggestions were not—at least, not obviously—immune to policymaker ignorance and he made no attempt to show that they were. Though he recognized that policymaker ignorance could undermine the case for illiberal economic policies, he apparently did not see that ignorance also complicated the case for liberalism and liberalization policies. I have no objection to Hayek’s view that liberalism should be a positive program, but any positive program, however liberal (or not), that has not been shown to be either immune to the problem of policymaker ignorance or spontaneously realizable, given the nature and extent of relevant policymaker ignorance, is in need of justification of exactly the sort that Hayek demanded from defenders of more interventionist economic policy programs. So, Dekker’s critique of the book through a constructive Hayekian lens leaves the central problem of Hayek’s positive programming unresolved.
Professor Dekker’s claim that there is nihilism lurking in the Austrian tradition strikes me as problematic, at least as an assertion about Austrian economists of all but the first few generations. It seems to me that, more or less universally, modern Austrians do believe that the world can be improved, namely, by further liberalizing existing political conditions. If what Dekker means by this claim of latent nihilism is that Austrians ultimately lack a justification for their preference for liberalism that meets the standards they demand from socialists and other economic interventionists, then I agree. This is the central argument of Chapter Three. In my view, Austrians (and other liberals of varying degrees of liberality) need to develop a justification for their preferences that stands up to the problem of policymaker ignorance—which is, in effect, the justification they demand from socialists and other economic interventionists—or embrace, not nihilism, but skepticism about particular policies and political philosophies.

It is important to emphasize, however, that the skepticism I advise is not of a “radical” variety, as Dekker suggests it is. I do not argue that liberals should, like Pyrrho, do nothing about the man drowning in the canal. In the absence of evidence that a particular political program is either immune to the problem of policymaker ignorance or spontaneously realizable, it does not follow that inaction is the appropriate policy response. (Inaction is itself a political program that, inasmuch as it is conceived as a means to the end of realizing particular states of affairs, can involve heavy epistemic burdens, as I argue in Chapter Three.) Rather, the skepticism suggested is of a Humean variety (p. 77), as Professor Schlessler perceptively notes in his commentary on the book. In the absence of evidence that a particular political program is either immune to the problem of policymaker ignorance or spontaneously realizable, the appropriate epistemic response is to withhold positive belief in the proposition that the program can realize its ostensible goals. This implies nothing about the appropriate policy response under such circumstances.

I have no objection to policy experimentation. I agree with Dekker that we will probably learn much of value to political epistemology by observing the results of various policy experiments. However, such experimentation should proceed in full awareness of the effects of ignorance on options, preferences, goals, motives, incentives, etc. More to the point, in the absence of evidence that relevant policies are either immune to the problem of policymaker ignorance or spontaneously realizable, given the nature and extent of relevant ignorance, such experimentation should proceed skeptically, not optimistically (as one might think it too often does).

Knowledge is never binary, but always a matter of degree. I speak throughout the book of the “nature and extent” of a person’s ignorance. That knowledge is a matter of degree is also implied by the significant role I assign to spontaneity, if some goal is to be realized despite a degree of relevant ignorance. The pertinent question to ask of the pursuit of a goal, in politics as in everyday life, is always “How much (and what kind of) ignorance is there and how much (and what kind of) spontaneity is required?”

The central concept of an epistemic burden is defined in the book in terms of the “the nature and extent of the ignorance that an actor in a particular context must overcome [i.e., the knowledge that must yet be learned] in order to use some means to deliberately realize some end” (p. 3). Similarly, the problem of policymaker ignorance is defined as “the simple fact that the success of purposeful political action is necessarily limited by the nature and extent of policymakers’ ignorance and their capacities to learn” (p. 1; italics added). Thus, the role of learning processes in possibly mitigating policymaker ignorance is well accounted for in the book.

Uncertainty is not some category distinct from knowledge and ignorance on the Hayekian epistemology described in Chapter Four and, consequently, it gets cut by Occam’s Razor. If you are uncertain about the occurrence of some event, then you lack quantifiable knowledge about its occurrence. You are limited in your ability to put related beliefs to work to form an actionable plan—you are ignorant, to some degree. Hayekian ignorance encompasses Knightian uncertainty.

Professor Dekker claims that I think individuals have “clear” sets of preferences. If this is supposed to mean that I believe persons’ preferences are given to them, then the arguments of the first chapter for the logical priority of the epistemic undermine this assertion. The courses of action over which a person might
have preferences in some decision context are never simply given. What constitutes an option is always filtered through and sorted according to the actor’s relevant relative epistemic burdens.\(^8\)

I do not claim that one cannot be ignorant of ends. Ignorance can pertain to ends as well as to means. Only if I assumed some sort of Cartesian infallibilism about mental states could I be fairly accused of denying the possibility that a person might fail to know their goals, but I explicitly reject Cartesianism (p. 126). Dekker draws a hard-and-fast distinction between means and ends that appears nowhere in the book and which most philosophers, myself included, would reject as untenable. Some ends can be means and some means ends. The adoption of some means can be a subsidiary end to some ultimate end. The ultimate end of, say, arriving in New Orleans, might require the subsidiary end of driving to New Orleans as a means to the ultimate end and the sub-subsidiary end of acquiring an automobile as a means to the subsidiary end. Nothing in the book implies the mutual disjointedness of means and ends.

**RESPONSE TO SCHLIESSER**

I am delighted that Professor Schliesser emphasizes what I take to be one of the book’s most significant (if not necessarily original) arguments. From the perspective of the problem of policymaker ignorance, every political action is what Schliesser calls a “transition problem.” Every political action (indeed, every human action) aims at some state of affairs, either at creating some new or at sustaining existing circumstances. Whether we wish to discover or to develop through Platonic education persons with the knowledge and inclinations necessary to serve as philosopher-kings, whether we want to write a constitution that ensures that existing policymakers (such as they are) are adequately motivated to pursue goals in their constituent’s interests, whether we aim to mitigate to some degree a perceived social problem, whether we wish to institute the rule of law in a previously lawless society or, indeed, whether those of us in lawful societies want to secure the continued enforcement of the rule of law in perpetuity, in our political lives, we are always trying to “get there from here” (p. 81). The problem of policymaker ignorance is just that those policymakers charged with getting us to there from here may lack either an adequate roadmap or a functioning vehicle, or both.

Professor Schliesser’s “hedged” criticism of my argument that *ought implies* (or whatever) *deliberately can* offers an opportunity to emphasize some of its more subtle aspects that are easily missed.

First, the argument is deployed in defense of the thesis of the logical priority of the epistemic. It is *not* offered as a criterion of rational political choice: I do not argue that policymakers ought not to pursue goals they cannot deliberately realize. Recall the restatement of the book’s central arguments in the prologue to this symposium: when the knowledge required to deliberately realize a goal is absent, any purported obligation to pursue it evaporates; this *does not* mean that a new obligation emerges in its place to not pursue the goal. Logically speaking, the contrapositive of “If ought to X, then deliberately can X” is “If not deliberately can X, then not ought to X,” not “If not deliberately can X, then ought not to X.” “Not ought to X” is not logically equivalent to “Ought not to X.” The first means there is no obligation to X, the second means there is an obligation to not X. To draw the inference that policymakers ought not to pursue goals they cannot realize is to mistake the logic of the *ought implies* (or whatever) *can* principle, whether or not “can” means *deliberately can.*

Second, it is important to note that I use the concept of *normativity* throughout the book in multiple senses other than just the moral sense. Epistemic considerations are logically prior to— and serve to determine—the moral, prudential, pecuniary, and other normative criteria that also figure in decision-making. In other words, to say that political decision-making is normative is just to say that it proceeds according to norms, standards, or criteria (which may be either explicit or tacit). Political—indeed, all human—decision-making is aimed at satisfying some combination of perhaps moral, perhaps prudential, perhaps pecuniary, perhaps some other kind of criteria concerning how the decision-maker wants the world to be. To say that the epistemic is logically prior to the normative is just to say that, in order to apply these moral, prudential, pecuniary, etc., criteria to a range of options, epistemic criteria—the nature and extent of the
decision-maker’s relevant ignorance—must first do their work in pre-sorting courses of action. Ignorance constrains the various ways we might aim to make the world and the various means we might employ to make the world a particular way.

As I use the term, policymakers encompass everyone involved in the processes of deciding, designing, implementing, and administering a policy, which can be anything from a constitution for global government to the ordinances of a local homeowners’ association (p. 15). Thus, many more than elected officials are encompassed in my usage of the term, in particular, the concept includes all those bureaucratic agencies in which institutional memory might agglomerate and all those bureaucrats that Schliesser suggests might be capable of mitigating their epistemic burdens over time. This is an opportunity to reflect a bit more on the knowledge that effective policymaking typically requires.

The kind of knowledge required to deliberately realize some goal is a contextual matter that depends on the goal in question. However, in the final analysis, policymakers will not be able to deliberately realize a goal unless they possess knowledge of the causes and of the relationships that obtain between causes of the phenomena that they need to control in order to bring about the intended state of affairs. In effect, they need to know whether there are any and, if so, what are the pulleys, gears, and levers to society that they might use to deliberately realize the relevant goal, and they need to know how to manipulate these mechanisms effectively. More carefully, they need an adequate theory of the relevant phenomena. Given an adequate theory, they need data concerning the phenomena that are compatible with the theory such that predictions adequate to the degree of control required to realize the goal follow from the marriage of theory and data. Given a conjunction of theory and data adequate to prediction and control, they need the knowhow—the abilities, capacities, talents, and powers—to exert the kind and degree of control required. I am open to the possibility that, through the process of attempting to realize various goals, policymakers might learn relevant knowledge, thereby reducing their epistemic burdens either with respect to these goals or in general, but is it at all plausible to think in line with Schliesser’s reading of Walter Lippmann that government can create the required knowledge, either directly or indirectly through the “revolving door” of government and university? The Lippmanian story strikes me as fanciful, as does the notion that “data-mining of large administrative data-sets” might provide the causal knowledge, both theoretical and empirical, that deliberate policy success requires.10
NOTES

2. The first piece of advice that Professor Boettke ever gave me was something along the lines of “Never attack idiots or idiotic arguments. Only attack the best, most respected, people and arguments.” So, in defense of my apparent lack of gratitude for Boettke’s generosity, he asked for it!
3. Consider Zanotti and Cachanosky (2015), who argue that Fritz Machlup, in his debate with Terence Hutchison, offered an adequate defense of Mises’ apriorism in Lakatosian terms. As I show in Scheall (2017a), Zanotti and Cachanosky’s thesis is defensible only if one ignores the subsequent round of the Machlup-Hutchison debate, in which Machlup admitted that, indeed, Mises’ apriorism was more epistemologically extreme than could be defended on Lakatosian grounds.
4. On the various interpretations of Mises’ varying epistemological pronouncements, see Scheall (2017a).
5. Both Boettke, in other places (see Boettke 2015 and 2018), and Koppl, in his contribution to the current symposium, try to claim consistency between (their preferred interpretations) of Mises’ epistemology and Hayek’s own epistemology, a proposition that Hayek himself explicitly denied multiple times (see, e.g., Hayek 1978, p. 137, and Hayek 1994, pp. 72–73).
6. Of course, it remains open to me, as Koppl suggests, to simply bite the bullet and deny that *E. coli* bacteria and other simple organisms can possess knowledge. However, it would then be necessary to explain the relationship between varying degrees of biological complexity and epistemic capacity. It would be necessary to explain why human beings and, presumably, other higher animals, can possess knowledge, but bacteria, amoebae, and other simple organisms—which, despite their relative simplicity are capable of performing seemingly complex tasks—cannot possess knowledge.
7. Contra Dekker (and, to a more limited extent, contra Koppl), I do not offer anything like a constitutional proposal in the book. Chapter Six suggests a method of analyzing policymaker ignorance that—in some future state, after extensive further inquiry—might in principle lead to mitigating, not solving, the problem in real-world contexts, through constitutional reform, subject to the profound limitations described in Devins et al. (2015). Nothing could be further from my view than the notion that we occupy the epistemic position we would have to occupy in order to effectively engage in such constitutional reform. Indeed, I see no reason to think we will ever occupy the epistemic position that constitutional reform consistent with the qualifications of Devins et al. (2015), would require. Yet, the analytical method suggested in Chapter Six may prove fruitful in other respects regardless of how far it leads to positive reform.
8. I do not know what Dekker read in the book that made him think it is “within the mainstream welfare economics perspective.” I doubt that the book bears any implications for either of the fundamental theorems of welfare economics, much less for human welfare or well-being. I certainly make no methodological appeals to the use of social-welfare functions as policy instruments that might moderate or annihilate policymaker ignorance.
9. Professor Schliesser’s criticisms are “hedged,” because he recognizes that the argument of the book ultimately leads to Humean skepticism, not to the effective nihilism that would be implied if I actually thought policymakers ought not pursue goals they are too ignorant to deliberately realize.
10. Though I might miss my “own best insight” in the specific passage that Professor Schliesser quotes from page 140, that insight is emphasized repeatedly throughout the book. I note, for example, that a political-epistemological Utopia would not be a political environment in which policymakers were limited to goals they could deliberately realize on the basis of their knowledge and learning capacity, but “a world in which policymaking was most epistemically feasible subject to the constraint that the world itself be feasible on the basis of policymakers’ existing epistemic capacities and available spontaneous forces” (p. 92). Every political action is a sort of “transition problem” and it is always an open question whether policymakers possess the know-that and know-how required to deliberately effect the transition.
REFERENCES


Geoffrey Hodgson’s *Is There a Future for Heterodox Economics?* is both an interesting institutional assessment of the current state of affairs in heterodox economics and a valuable guidebook full of possible ways forward so that heterodox economics could have a valuable future. Over the last fifty years, and more particularly since the Great Recession of 2007–8, the community of heterodox economists has expanded, and its publications have proliferated in new heterodox journals. Nevertheless, the relevance of heterodox economists within economic departments and their impact on the leading journals has waned considerably. Geoffrey Hodgson’s (2019a) new book—which was launched almost simultaneously with his book on the feasibility of socialism¹—is an enjoyable account of this academic and scientific paradox.

Hodgson’s main and crucial point, which all heterodox economists need to take into account, is that the current academic strategy of heterodox economics has been a failure. Hence the heterodox community needs to deeply reconsider its academic-strategic options in order to develop a valuable and sustainable research agenda. The current state in mainstream economics and the dominance of conventional economics in the leading academic journals and universities show that it is no longer viable for heterodox economists to continue with business as usual and work as they have been since the 1970s.

By borrowing key insights from social epistemology, the sociology of scientific communities, and institutional analysis, Hodgson argues that:

the strategy of using “heterodox economics” as a banner to organize dissent from the mainstream has been at best of limited success . . . the problem in the past is that heterodoxy has been united more by (leftist) ideology than by analysis. This ideological orientation is ultimately damaging for the development of heterodoxy. It creates leftist ghettos that become ill-prepared to engage adequately with other communities outside. . . . We should be wary of communities of social scientists that congregate around a narrow range of political views. Such circumstances encourage groupthink rather than genuine pluralism and open debate. Analyti-
cal positions are dismissed simply because they are said to be “right wing”, rather than through critique of the analysis involved (Hodgson 2019a, p. 174).2

This critique may sound, to some heterodox economists, like a biased attack by some mainstream and “right wing” economist. But readers may be surprised that this critique comes from an economist that is part of their own community and thus coming from their own ideological and methodological trenches. Geoffrey Hodgson has long been a strong critic of contemporary orthodox economics, and he also defines his political views as left-leaning: “To put my political cards on the table, for 30 years or more I have not seen myself as a socialist, instead as a social democrat or a left liberal” (p. 9). Furthermore, he has also been an active participant within the heterodox community since the 1970s. In other words, this serious critique is coming from a friend and a fellow traveler of the heterodox community rather than a foe. This is important to acknowledge since if someone who comes from your own community and is a friend is severely criticizing your heterodox approach and your current academic strategy, then perhaps you should listen carefully. I believe that this is exactly the case, and all heterodox economists—both young and veteran, both left and right leaning—should read this thought-provoking book. Indeed, every economist or other social scientist interested in successful and unsuccessful academic strategies and all of those puzzled about the reasons why important heterodox contributions haven’t permeated the mainstream economic profession should read this book. The book may thus be relevant for Austrian economists, constitutional political economists, and other economists with evolutionary and institutional orientations.

This book certainly offers a compelling explanation of why heterodox economics has not been successful at retaining academic power and influence within the economics profession, and it explains the challenges that the heterodox community must face in order to successfully challenge the current orthodoxy. The book also closes with some important suggestions concerning viable academic strategies that heterodox economists could adopt in order to have a successful future.

Hodgson’s book can be broadly interpreted as critical institutional analysis and a critical survey of the unsuccessful academic strategies that different branches of heterodox economics have adopted over the last decades. The book also emphasizes two additional important points: First, the view—held by some heterodox economists—that mainstream and neoclassical economics are largely and necessarily an apologia for a free-market economy is utterly mistaken and inconsistent with the history of the discipline. In Hodgson’s words, “The neoclassical core of mainstream economics has been used to support socialism as well as capitalism” (p. vi). Second, the crucial problem that the heterodox economists face is that they do not agree on a theoretical core. Consequently, and regrettably according to Hodgson, the only thing that holds them together is ideology and politics. In Hodgson’s words, inside the heterodox community, “instead, a left ideology has prevailed throughout: despite major theoretical disagreements, ideology unifies the majority of its adherents” (p. vii). This strategy has generated only “leftist ghettos” that have been detrimental to the advancement of heterodox economics. As a result of this, in recent decades the global influence and power of heterodox economists in leading academic journals and departments of economics have faded.

Today, despite the growing network of heterodox economists and the discussions about the need to establish alternatives to the standard way of doing economics, it is more difficult than ever for a heterodox economist to get a job in a good department of economics or to publish in leading academic journals. This book brilliantly explains the institutional and academic-strategic reasons why heterodox economics faces this undesirable paradox, while it also proposes potential strategies for a sustainable future of heterodoxy.

Hodgson borrows from the philosophy of science, institutional economics, social epistemology, and M. Polanyi’s studies on the sciences as social systems to critically assess the scientific community of heterodox economists while shedding light also on the crucial paradox that heterodox economists currently face. The book consequently is rather critical—for good reason, in my view—of contemporary heterodox economists since they have followed an ideologized academic strategy that has led the community toward “an increasingly self-ghettoised sect of people who are stuck on a double-bind of resentments” (Mearman et al. 2019,
The book suggests that it’s going to be very difficult for the heterodox community to escape from its entrapment. Nevertheless, the book ends on a constructive note, suggesting eight possible strategies for heterodox economics and by emphasizing that ignoring how institutions of academic power currently operate is no longer a viable option for the heterodox community anymore.

As Hodgson informs the reader, this book is about “institutions, culture and habits of thought that can enable or disable the forces of change within economics. . . . This book is about academic power and powerlessness” (p. 3). In that regard, this book successfully accomplishes its mission by assessing the different aspects of heterodoxy in economics in six chapters.

Chapter 1 critically explores the strong influence of Cambridge economists and their non-neoclassical branch from the 1940s to the 1970s on modern heterodox economics. The major figures are Joan Robinson, Piero Sraffa, and Nicholas Kaldor, among others. The chapter explores the institutional and cultural setting in Cambridge in which their theories developed, and the role played by leftist ideologies that motivated the protagonists and influenced their academic decisions. Hodgson convincingly shows that most of the (negative) attributes of current heterodox economics today can be traced back to the attitudes and mindset of pre-1980s Cambridge heterodoxy.

Particularly, Hodgson identifies three detrimental attributes that sprung out of Cambridge heterodoxy: first, the failure to agree upon a unifying theoretical core; second, the strategy of using ideology to define heterodox economics as left wing and orthodox economics as right wing, pretending that all of the heterodoxy must be left-leaning economics and all of the orthodoxy must be, consequently, pro-market economics; third, an intellectual echo chamber and “leftist bubble” (p. 24) that led Cambridge economists to dismiss certain crucial heterodox critiques being put forth outside of the Cambridge citadel—such as Herbert Simon’s behavioral approach, some fundamental microeconomic issues related to price theory, and F. A. Hayek’s and Ludwig von Mises’s critique of central planning. The ideologically biased evolution of post-1980s heterodox economics around the world has unfortunately perpetuated the flaws stemming from the Cambridge “prestigious leftist bubble” (ibid).

This chapter is particularly interesting since it sheds light upon the detrimental neglect by the heterodoxy of the socialist calculation debate. Hodgson identifies the debate as “arguably the most important debate in economics in the twentieth century” (p. 39). Hodgson argues that this debate is crucial for heterodox economists in general not only since it deeply challenges the viability of socialist planning, but also because in that debate Hayek “developed a major critique of general equilibrium theory, which highlighted the nature and role of knowledge” (ibid). Yet, despite the fundamental significance of the debate, which severely questioned the feasibility of Cambridge’s socialist priors, Cambridge conveniently and entirely passed over it. Hence, the “Austrian critique was unanswered and generally assumed to be wrong” (ibid). Cambridge economics continued with business as usual by not questioning the feasibility of socialism and by dismissing fundamental economic issues—pointed out by Mises and Hayek—related to misaligned incentives, dispersed information, and tacit knowledge.

Consequently, “Cantabrigian heterodoxy failed to pick up this scent, partly because their socialist ideology prevailed over any spirit of critical investigation into this discrepancy. . . . The neglect of the debate betrayed major problems within Cantabrigian heterodoxy itself” (p. 40). This crucial intellectual neglect of the impossibility of monocentric socialism without extensive markets and private property “persisted among the rising post-Keynesian and heterodox movements” (p. 33). Thus, it is no wonder that contemporary heterodox economists are particularly acute at appraising the limits of markets but unable to grasp the inherent limits of planning and socialism.

In chapter 1 Hodgson is also critical of some of the ideological ramifications of Joan Robinson’s unapologetic socialism. Hodgson has no problem mentioning some of the political misjudgments of Robinson. For instance, he mentions Robinson’s strong support of the North Korean regime during the 1960s and, even more problematically, her impenitent support of Mao’s policies until his death in 1976. Robinson even claimed, in 1965, that “no one starved” during the Great Leap Forward, when in fact it had led to a fatal famine that produced—according to some estimates—about forty million deaths. As a consequence
of her strong ideological leanings, “Robinson simply took the viability of socialism for granted and she failed to discuss the twentieth-century theoretical debate on the economic feasibility of socialism” (ibid). Yet Hodgson correctly points out that “the severity of her economic and political misjudgments is rarely noted by her followers” (p. 34). Ultimately, we need to ask ourselves, “Why is Robinson so rarely criticized for her naïve and irresponsible credulity concerning Mao’s monstrous crimes? Perhaps this silence tells us something about the political dispositions of Cantabrigian heterodoxy” (ibid). Unfortunately, the Cantabrigians’ elitist and socialist political views deeply affected their theoretical research agenda by severely weakening their capacity for internal critical assessment and appraisal of important ideas from elsewhere that could have challenged their strong socialist priors. As a consequence, Cambridge heterodoxy’s hegemony over Cambridge’s Faculty Board “disappeared for good by 1988” (p. 45).

If young heterodox economists today can learn anything valuable from the intellectual “boom and bust” of the Cantabrigians that Hodgson so well scrutinizes, it is that non-neoclassical and nonmainstream economists need to take four fundamental points to heart: First, they must pay keen attention to microeconomic debates and price theory. Second, they must avoid politicizing economic rhetoric and avoid meshing economic theory with ideology. Third, they must avoid a ghettoized intellectual and ideological attitude that seeks to affirm confirmation biases, as it only generates groupthink, in which one “searches for evidence to confirm a view, rather than to challenge it. Whole areas of research . . . were neglected. The value of political viewpoint diversity in the academy was under-unappreciated” (p. 47). Fourth, they must never take for granted the economic and political feasibility of democratic socialism, and thus they must seriously study the debates within economics concerning the unviability of monocentric socialism. Special attention should be given to the epistemic and non-neoclassical arguments put forth by Mises and Hayek and contemporary Austrian economists such as Don Lavoie (1985).

Unfortunately, according to Hodgson, the post-Keynesian and heterodox economics that sprung out of Cambridge in the 1970s have perpetuated some of the ideological thinking of and academic mistakes made by Robinson, Sraffa, and other leading Cambridge scholars, which largely explains the irrelevance and powerlessness of the heterodox economists today in the academic world. Regrettably for the future of alternative approaches to economics, the “heterodoxy still bears the marks of its Cambridge origins” (p. 48), continuing the same neglect of the four crucial points aforementioned.

Chapter 2 attempts to define heterodox economics. Hodgson analyzes two key definitions: Tony Lawson’s and Frederic Lee’s. He critically assesses these definitions as unsatisfactory and incapable of capturing the core meaning of heterodox economics. Hodgson instead proposes a distinction between heterodox and orthodox economics centered on the use (or non-use) of the utility-maximization criterion. The aim of chapter 2 is to assess the current meaning of heterodox economics and its current feasibility as an organizing label. The chapter shows that there is very little consensus around its meaning. This obviously “creates a problem in using heterodox economics as a label to organize critics of orthodoxy. . . . it is unlikely to make substantial progress unless the criteria of inclusion and the raison d’être are agreed . . . Unfortunately, after several decades, such minimal agreement does not yet exist” (p. 49). In other words, beyond its demands for greater pluralism in economics and its opposition to the orthodoxy, it is still unclear what exactly binds heterodox economics together.

Hodgson starts chapter 2 by convincingly arguing that you cannot use political ideology to divide the orthodoxy from the heterodoxy. He agrees with Tony Lawson in rejecting any policy-based demarcations. Indeed, many prominent theories, such as general equilibrium theory and neoclassical analysis, can serve multiple and contrasting ideological positions. This is clear from the fact that a group of mainstream neoclassical economists led by Oskar Lange and Abba Lerner used general equilibrium theory in attempting to prove the feasibility of socialism. Thus, there is ample evidence showing that you cannot simply perform a one-to-one mapping of different theoretical positions in economics based only on policy views and ideological predispositions. Given this impossibility, Hodgson then explores Lee’s and Lawson’s definitions of the heterodoxy. This is probably one of the most interesting and valuable sections of this book.
Lee describes heterodoxy as an intellectual tendency to dissent from mainstream economics. And he describes the mainstream as comprising a core set of propositions such as the use of fictitious concepts, closed-system methodology, methodological individualism, and the concept of scarcity. But Hodgson correctly points out that Lee’s definition has several problems concerning definitions of the terms characterizing the orthodoxy. For instance, the notion of scarcity has very different meanings for different people. Obviously, there are things such as natural resources that are indeed scarce, while others such as benevolence, laughter, and love are not. The existence of intangible things that are not scarce does not negate the fact that the concept of scarcity is useful in economic analysis. The recognition that love is not scarce can hardly be “an argument to abandon entirely the notion of scarcity. It is an argument to be more precise about its possible meanings” (p. 54). Likewise, Lee’s claim that the rejection of methodological individualism is a necessary feature of heterodoxy is left unexplained; the “widespread heterodox rejection of ‘methodological individualism’ rarely clarifies what is being rejected” (p. 54). Different economists, both mainstream and heterodox, use very different conceptions of methodological individualism. Moreover, many advocates of methodological individualism, explicitly or implicitly, explain economic phenomena in terms of both individuals and social relations.

In sum, Hodgson shows that Lee’s definition of heterodox economics is not very useful since it uses imprecise and ambiguous terms to define the core characteristics of the mainstream; alas, there is no consensus around the meaning of those crucial terms. A final problem with Lee’s conception of heterodox economics is his ideological stance in portraying the heterodoxy as “not simply a scientific endeavor but also part of the global struggle against capitalism. . . . Hence Lee depicted mainstream economics as a defence of capitalism and heterodox economics as part of the struggle against it” (pp. 61–62). This, as Hodgson argues, is not only entirely wrong conceptually (see footnote 3 here) but also a highly questionable academic strategy. Thus, “Lee’s blunt account is of little help in guiding us through this mess” (p. 56).

Hodgson is also critical of Tony Lawson’s definition of heterodox economics even though he seems to consider it superior to Lee’s definition since, unlike Lee, “Lawson did not attempt to relate his definition of heterodox economics to any political position or policy stance” (p. 71). Lawson’s approach is broadly based on ontological considerations from which he draws methodological conclusions concerning the limits of mathematics in situations in which it is allegedly unsuitable. For Lawson, “the mainstream project of modern economics just is an insistence, as a discipline-wide principle, that economic phenomena be investigated using only certain mathematical-deductive forms of reasoning. This is the mainstream conception of proper economics” (p. 64). Hence Lawson’s novel and interesting conclusion is that the core essence of heterodoxy is ontological. Lawson’s ontological and methodological criteria generate an orthodoxy-heterodoxy demarcation in which the former imposes mathematical methods in complex situations that are inappropriate while the latter minimizes—but not necessarily negates—the use of mathematics as something that is applicable only under conditions resembling a closed system. Lawson seeks to identify the heterodox camp as making complementary (explicit or implicit) “ontological claims that imply that mathematical modelling is difficult or inappropriate. . . . different heterodox strands commonly resist the pull of inappropriate mathematical formalization” (p. 65).

Hodgson correctly points out several limitations of Lawson’s argument. The most relevant one is that Lawson based “his critique of formalism on an ontological mismatch between formal models and reality” (p. 66); however, he “made no argument, that the ontological characteristics of reality (such as complexity and openness) must be fully reflected somehow in the theory. . . . theory and reality are different things” (ibid). The point that Hodgson makes is well taken: “Lawson wrongly assumed that if reality is open (in some sense), then theories that are used to analyse reality must also be open. But it is neither possible nor desirable that theory should mirror reality in such a manner. Lawson provided no argument to sustain the proposition that ontological characteristics in reality should somehow be incorporated or reflected in a good theory. Even if we are concerned that theoretical assumptions should be as realistic as possible, then this does not mean that theory can be a mirror of reality” (p. 67).
Hodgson correctly reinterprets Lawson’s point as one in which the fundamental problem of mainstream economics is not necessarily the use of mathematics per se, but rather “its elevation of the development of formal techniques over the analysis and understanding of reality” (p. 68). Finally, if we follow Lawson’s criteria, then thinkers such as Coase, Hayek, North, Ostrom, and Williamson should be considered heterodox economists, yet these Nobel laureates are rarely considered heterodox economists by the heterodox community itself. Hence it is highly unlikely that his definition—as conceivable as it might be—will become widely accepted.

Ultimately, Hodgson’s objective in criticizing both Lee’s and Lawson’s definitions is to propose a definition of heterodox economics as a branch of economics that is in opposition to orthodox economics defined “in terms of the centrality of the assumption of utility-maximizing agents with [stable] preference functions, otherwise known as Max U. . . . This postulate of maximization dovetails with the widespread use of equilibrium analysis: maximization occurs when an equilibrium is reached. . . . Max U enabled a particular type of mathematical formalism in economics, often including calculus” (p. 78). In other words, Hodgson argues that the orthodoxy should be conceived of as “characterized by Max U: he invokes a crude one-dimensional view of human behaviour that cannot do justice to multiple facets of human motivation—particularly moral motivation—that are relational rather than purely individual” (p. 80).

Hodgson closes chapter 2 by proposing that “the best definition we have for heterodoxy is an economics that rejects Max U with his preference function” (p. 80). According to Hodgson, under this criterion, “evolutionary game theory and parts of the new institutional economics (which do not assume maximizing behaviour) may be regarded as mainstream but not orthodox” (p. 79). In this sense, under Hodgson’s definition, Coase, Hayek, North, and Ostrom should also be considered as part of the heterodox community even though they might be seen as mainstream economists.

Perhaps Hodgson’s arguments here could have been more persuasive and useful for heterodox and non-mainstream economists alike if he had incorporated other portrayals of this mainstream-versus-heterodox debate from other branches of the heterodoxy, such as Peter Boettke’s (2012) arguments for rekindling “the mainline of economics,” E. Ostrom’s (1998) plea for a behavioral approach to institutional economics and rational choice theory, and Vernon Smith’s (2019) attempt to reconceptualize economics along the lines of Adam Smith’s moral sentiments. I posit that both Boettke’s (2012) vision of mainline economics and Hodgson’s arguments against Max U could be fruitfully combined to carry heterodox economics forward into the future in a manner that rejects Max U while still focusing on purposeful but imperfect individuals—with different motivations—whose behavior and interactions are guided or affected by the institutions that determine both incentives and available information.

This alternative Boettke-Hodgson-Ostrom vision of heterodox economics could then fruitfully focus on a more complex theory of human motivation affected broadly by the institutions within which interactions takes place while allowing heterodox economists to escape ideological and political biases. Ultimately, the social order and human progress emerge not from perfectly rational individuals maximizing predefined utility functions, but from the attempts of imperfect and cognitively limited individuals to cooperate, exchange, and interact in a manner that might solve social dilemmas. Indeed, as Hodgson acknowledges, “the provision of a more complex theory of human motivation does not necessarily lead to anti-market policy conclusions” (p. 81). Hence, a fruitful research agenda for heterodox economists might indeed emerge from blending the powerful anti-ideological and scientific message from Hodgson with the insights of Peter Boettke, Elinor Ostrom, and Vernon Smith about the fundamental nature of economics and institutions.

A successful paradigm change in economics can only come about if heterodox economists are able to establish a consensus about heterodoxy’s identity, an “agreement on the nature of the required change,” and an “agreement on the kind of alternative theories that are to replace orthodox ones” (p. 82). I believe that a non-ideological synthesis of Hodgson’s, V. Smith’s, and Boettke’s visions of economics could shed some light on the way forward for heterodox economists. Nevertheless, as Hodgson clearly and pragmatically reminds us, “any viable school of thought should have a shared raison d’être” (ibid). This academic raison d’être can be defined, according to Hodgson, in terms of four possible complementary criteria: (1) the study
of a specific zone of inquiry; (2) the promotion of a particular theoretical approach; (3) the promotion of a particular set of analytical techniques; or (4) the promotion of policies in a defining problem area. The "raison d’être may consist of one of these, or a combination of more than one" (ibid). This list is highly relevant to those, such as Austrian economists and institutionalist economists, who want to build today a successful and heterodox academic strategy.

Chapter 3 critically assesses David Colander’s claims concerning the fact that “mainstream economics is moving away from the central assumption of utility maximization” (p. 12). Hodgson argues that those claims are exaggerated. In fact, the opposite appears to be true: Max U is gaining ever more relevance in economics, and even behavioral economics has reconciled itself with utility maximization. Thus, “rumors of the death of Max U are exaggerated” (p. 84). Hodgson acknowledges that “despite numerous important developments in recent decades, mainstream economics may prove much more difficult to budge from its enduring core assumptions” (p. 13). This strong entrenchment of the core Max U assumptions in economics “suggest[s] more pessimistic conclusions concerning the possibility of changing the core assumptions of economics and thereby establishing a new paradigm” (p. 87). Hodgson present journal-citation data to argue that “the Formalist Revolution of the 1950s led to a consolidation of the hard-core assumptions of Max U. . . . Max U has survived the 2008 crisis without any wounds, and he seems alive and well” (p. 95). This is largely because the core assumptions of Max U are unfalsifiable, making the model invulnerable to evidence, falsification, or scientific scrutiny.

Importantly for economics, "the unfalsifiability of Max U at the core of mainstream economics places this discipline in a position different from others. . . . the role of Max U in economics seems unique. His unfalsifiability and simplicity are reasons why he lives on, and why a Kuhnian scientific revolution in economics is unlikely” (p. 99). This Kuhnian point made by Hodgson is extremely relevant for academic strategies since the status of Max U makes economics entirely unique and unlike other sciences: its core assumptions and vision are ultimately unfalsifiable and thus not prone to traditional forms of scientific revolutions that occur in other sciences. Hence bibliometric evidence, the simplicity of the orthodox approach, and the unfalsifiability of utility maximization undermine the claim that mainstream economics is actually moving away from its “holy trinity” assumptions of perfect rationality, narrow maximizing selfishness, and equilibrium.

The uncomfortable conclusion is that “heterodox economists may have to choose to live with Max U, or to abandon the academic power structures of economics altogether” (p. 87). Economics certainly faces a huge challenge in moving the discipline along the moral, philosophical, and behavioral lines delineated by Elinor Ostrom, Amartya Sen, and Vernon Smith and thus in rehabilitating the moral-sentiments approach of Adam Smith. In order to challenge Max U, heterodox economists need to pay attention again to these moral and philosophical issues and construct an alternative and institutionally oriented explanation of human behavior.

Chapter 4 considers the narrow branch of the heterodoxy known as evolutionary economics. Hodgson uses evolutionary economics as a case study of the possible strategies that heterodox economics in general could adopt in order to deal with academic power to challenge the orthodoxy. Alas, the strategy adopted by evolutionary economists has been to simply move away from economics departments and become academic exiles in (mostly) management departments and business schools; there is also “little further agreement on a core theoretical approach” (p. 112). Clearly, as Hodgson recognizes, this is not a long-term successful strategy, since evolutionary economics has now “become fragmented and divided, by multiple specialisms and disciplinary boundaries” (p. 13), which has postponed the development of a shared core identity. As a consequence of this fragmentation and self-isolation, “evolutionary economics has failed to invest in a viable theoretical core or provide another suitable raison d’être. . . . The necessary degree of consensus is more difficult to sustain in such [fragmented] contexts” (p. 122).

Chapter 4 also presents some bibliometric, cluster, and co-citation analyses to show that there has been insufficient institutional investment in developing a theoretical core; thus “additional institutionalized incentives are necessary. . . . the narrower stream of evolutionary economics lacks an adequate theoretical
'hard core' in the sense of Imre Lakatos" (p. 123). Hodgson argues that evolutionary economics is moving toward the "silo effect," which "refers to an outcome of specialization and fragmentation, where sub-fields become less capable of reciprocal operation with related sub-fields" (p. 124). Inorganic and nonstrategic fragmentation has "not been matched by fruitful development of overarching theory, a common conceptual vocabulary, and common research questions" (ibid). Thus, evolutionary economists have been unable to propose their own academic raison d’être defined in terms of the four possible criteria identified by Hodgson above.

Chapter 5 seeks to bring together insights from the philosophy and sociology of science and from social epistemology in order to analyze the current state of the heterodox community as a scientific community. Following Michael Polanyi and Philip Kitcher among others, Hodgson argues that while pluralism is important to foster innovation, contestation, and creativity, a certain "degree of consensus over core issues is also necessary for sustained progress in science" (p. 13). Alas, such a degree of consensus is absent in heterodox economics. A crucial point in this chapter is that economics should be examined "not merely as a set of doctrines, but as a system of organized authority, requiring strategies of power to transform it” (p. 133). This point is crucial since it reminds us that we need to apply the analytical tools of science to understand the power of science itself. In making this point, Hodgson argues that the traditional emphasis of the heterodox community on pluralism is "one-sided" since "it overlooks the need to build up an empowered community of scientists sharing some common assumptions and with some agreement on key issues. . . . some kind of authoritative agreement is essential for science to move forward, and to create cumulative progress" (ibid).

The lack of focus on power and authority to sustain cumulative progress—"in addition to its chequered esteem in terms of quality”—partly explains the limited progress that heterodox economics has made since its consolidation in the 1970s (ibid). Ultimately, Hodgson crucially reminds us that "science is a system of power," and thus, to function effectively, "science must be organized" (p. 137). It requires a certain level of common understanding and "fundamental assumptions and with some consensus on key issues” (Ibid). This is as essential to ultimately exercising power and authority as to monitoring quality, maintaining standards, and weeding out low-quality research; otherwise scientific progress “is impaired by endless dispute over fundamentals” (p. 139).

Any school of thought requires establishing strong institutions and rules so that expertise and authority can help monitor quality and keep a stable consensus around some fundamental issues. Hence, a fundamental message of the chapter is that “all functioning sciences are elitist—they proceed by authority, consensus and constructive dissent, not by democratic votes. . . . Science combines competition with hierarchical authority, the enforcement of rules, and limited entry” (p. 138).

Moreover, any school of scientific thought needs to balance pluralism and dissent with coalescence around some form of authority; otherwise, warns Hodgson, the economic discipline, like sociology, may come to suffer from “excessive diversity and insufficient consensus” (p. 141). When this happens, a discipline develops flawed screening criteria that might degrade the quality and the credibility of a scientific endeavor, which impairs the development of the science. If the heterodox community wants to establish rival centers of academic power, it needs to institute a stronger "degree of consensus and a sustained research focus that are so far lacking in the theoretically diverse and fragmented heterodox community. . . . without attention to such problems, heterodox economics is likely to decline still further” (p. 149).

Chapter 5 is also extremely valuable to economists and other social scientists alike concerned with social epistemology and the social production of knowledge since Hodgson, building upon M. Polanyi, provides a valuable overview of how science functions and produces knowledge. In short, "human cognitive capacities are thus irreducible to individuals alone: they also depend upon social structures and material cues. This is true for science as much as any other organized human activity. Science becomes institutionalized, to create ‘epistemic communities’ and ‘machineries of knowing’. . . . Scientific knowledge is developed by institutionalized communities of investigators that scrutinize others in their area and play a crucial role in the advancement or rejection of particular approaches to understanding” (p. 136). Ultimately, "scientific
advance depends on particular institutions and incentives that channel and combine varied individual incentives towards the general growth of knowledge” (p. 141).

The issues explored in chapter 5—problems with quality control, lack of consensus about fundamental issues, and unrestricted tolerance of diversity—help to explain the limited advancement and the weak influence of heterodox economics within departments of economics. When unrestricted tolerance is greater than authority, as is the case in heterodox economic circles, anything goes, leading to a failure of quality control in heterodox research. When pluralism and tolerance go too far, scholars in heterodox communities lack a strong institutional environment that can provide rigorous criticism and skepticism in order to improve arguments and, consequently, enable them to publish in leading academic journals.

Ultimately, novel, persuasive, and high-quality ideas can only be developed “in a climate of intensely critical, expert scrutiny, based on teams of well-informed specialists. . . . Even if the opposition is right and orthodoxy is wrong, the opposition can be seriously impaired by low-quality allies in its ranks” (p. 151). This self-perpetuating problem—in which groups that oppose consensus are denied academic power, forcing them to cling together in the fringes, after which they attract marginalized and low-quality new scholars, which ultimately allows weak arguments to persist and marginalizes the group even further—is what Hodgson denominates as “the Joan Robinson Problem” (p. 152), and it seems to threaten any form of academic heterodoxy that seeks to challenge the established academic consensus. Heterodox economists need to establish rules and institutions to balance their excessive diversity, and they need to establish a core consensus to allow them to exercise authority and address the problems of quality control and ideology that have plagued heterodox economic endeavors since their Cantabrigian origins.

The final chapter (6) considers eight possible non-mutually-exclusive academic strategies that could be useful for heterodox economics in order to start building a sustainable and successful future. As Hodgson suggests, “After several decades, with an evident decline of influence of self-described heterodox and post-Keynesian approaches . . . it is time for a serious re-evaluation of current strategy” (p. 153). In doing such a re-evaluation, Hodgson (pp. 154-55) proposes that any successful academic strategy for heterodox or non-mainstream economists needs five elements:

1) An adequate raison d’être, or reason for being.
2) A specific institutional arena of engagement with the existing segments of organized science.
3) Good incentives to attract high-quality and young researchers to join the community, incentivizing them to both remain active members and producers of knowledge for sustained periods of time, and also to abide and accept some defined consensus position.
4) Strong institutionalized mechanisms to sustain quality control and for quality enhancement.
5) A well-established and clear degree of consensus that both limits and guides pluralism in the field, as to encourage cumulative yet novel and creative knowledge.

Hodgson is well aware that these five requirements can be satisfied by different academic strategies. Nevertheless, it should be clear by now that contemporary heterodox economics has substantial deficiencies in all of these five aspects: there is little agreement on the raison d’être, there has been no substantial effort to define the institutional arena of engagement, there has been little attention to academic incentives, there are insufficient mechanisms to assess and enhance quality control, and there is a lack of a strong consensus to serve as an intellectual foundation.

Given these deficiencies, Hodgson (pp. 156–73) explores the pros and cons of eight practical academic strategies that heterodox economists could adopt: (a) split economics into mainstream economics and political economy; (b) rekindle the engagement with mainstream economics; (c) try to unify the social sciences into a broader multidisciplinary umbrella; (d) enter survival mode and leave economics departments for business schools; (e) abandon a generalist mission to change economics and focus instead on specialist regroupment around a particular approach or subfield; (f) develop theoretical alternatives to Max U; (g)
move the intellectual focus on institutions from inside economics; and, finally, (h) focus on institutions from multiple disciplinary perspectives.

After assessing these eight strategies, Hodgson argues that a plausible strategy going forward is a combination of four of them: (c) unify the social sciences, (e) engage in specialist regroupment, (f) develop alternatives to Max U, and (h) focus on institutions from multiple disciplinary perspectives including economics. According to Hodgson, “These four preferred strategies are complementary rather than mutually exclusive” (p. 175). Considering my nonmainstream experience as a political economist and considering Hodgson’s excellent analysis concerning the strong entrenchment of Max U in economics and how it is ultimately unfalsifiable, I think that strategy (f) (develop alternatives to Max U) is highly unlikely to succeed in the near future. Hence, it should be considered a very long-term strategy to pursue after some other successful strategy succeeds in the near future. Thus, I propose here briefly a modification of Hodgson’s proposal.

I consider that a more plausible academic strategy is a combination of just three strategies, what I denominate here as the “heterodox triangle”: (c) establish horizontal relationships to build a unifying theme around political-economy and institutionalist groupings within political science, sociology, and economics; (e) engage in a specialist regroupment for a subset of heterodox economists that involves organizing “around a particular approach that has evident success and future potential” (p. 166); and (h) focusing on institutions and institutional analysis from multiple disciplines including economics.

This feasible, and hopefully successful, academic strategy of the heterodox triangle has already been pursued—with some success—in different academic settings such as the F. A. Hayek Program in economics at George Mason University; the Political Economy Department at King’s College London; some branches of new institutional economics; the Ostrom Workshop at Indiana University Bloomington; the World Interdisciplinary Network for Institutional Research; and, finally, some academic journals such as the *Journal of Institutional Economics* and the *Review of Austrian Economics*. These examples all show, in different manners, a creative combination of (c), (e), and (h) as a plausible strategy to regain some academic power while still being part of the heterodox community in economics.

The broad objective of these attempts is to congregate scholars from different disciplines, including economics, who “are called upon to collaborate in the study of the institutions (and organizations) of economic life, and to create some consensus over core concepts. This more inclusive strategy draws from disciplines such as law . . . from philosophy and social theory because these disciplines are essential to understand the ontology of institutions” (p. 173). The relative success of some of these endeavors—such as the *Journal of Institutional Economics*, which has seen a quick rise in the academic rankings and achieved higher impact factors than most long-established heterodox journals—suggests that the heterodox triangle could be a fruitful academic strategy going forward for heterodox and nonmainstream economists. Given that interdisciplinary research is much less prominent in the United States than in Europe, chances are that this strategy will be more successful “in Europe than elsewhere” (p. 174).

To conclude this review essay, as Hodgson points out throughout this book, in order to change economics for the better along institutional and heterodox lines, we need to “address the systems of power and authority within the organizations of science and academia. We also need to consider the ways in which that power is created and enforced including the habits and conventions of scientific communities. To do this we need to break from individualistic views of how science operates. . . . The fate of heterodoxy is about the extent of its institutional power and esteem, as well as about its ideas” (p. 5). It is no longer viable for nonmainstream economists to keep working in the fringes as they have been doing for decades. Heterodox economists can no longer afford to rely on isolated and de-institutionalized views of how science produces knowledge and establishes power relations. If they continue as they have been, nonmainstream alternatives will be marginalized and meet their demise.

The heterodoxy is no longer living in the cozy Cambridge of the 1950s or 1960s: the academic cost of being heterodox has increased, while the benefits have declined substantially. The heterodox community needs to consider alternative strategies in order to survive, and it needs to understand how science operates: the crucial role of scientific networks, the role of incentives and consensus, and the institutions that must be
in place in order to enhance quality control and establish centers of academic power that can influence the scientific community. The heterodox community needs to reassess its academic-strategic options in light of the eight possible strategies suggested by Hodgson and especially the heterodox triangle suggested here.

Finally, the success of heterodox economists in regaining academic influence depends “on the strategic manipulation of the institutions of science. Instead of merely promoting pluralism . . . attention must also be directed towards screening out low-quality research and ultimately changing the consensus discourse. This must entail a strategy to establish positions of power within academia. Heterodox economists have given far too little attention to these issues, upon which their future depends” (p. 7). We can only thank Hodgson for telling us what is wrong with our current nonmainstream efforts and for shedding light on the strategic ways to build a better future for heterodox economists. We have a lot of collective, academic, and institutional work to do, but now, and thanks to Hodgson, it is clearer where to direct our institutional efforts and attention. Now is the time “to offer something better to the scholars of the future” (p. 175).

NOTES

1 See Hodgson (2019b) Is Socialism Feasible? Toward an Alternative Future. Both books have something very important in common: both recognize that “by necessity both science and modern economies are decentralized coordination systems operating by devolved mutual adjustment among individual agents. These systems are both highly complex and rely on dispersed tacit and fragmented knowledge. Consequently, they cannot be adequately planned from the centre” (Hodgson 2019, p. viii). In both books the intellectual hero is the polymath Michael Polanyi, who greatly influenced F. A. Hayek and made crucial contributions to the social sciences, including the notion of tacit knowledge, the concept of polycentric systems, and the idea that markets and science are forms of spontaneous orders that cannot be centrally planned (Polanyi 1951).

2 For the remainder of this review citations without the authors’ name are all reference Hodgson (2019a).

3 Hodgson correctly points out that interpreting neoclassical economics and orthodox economics in general as a de facto defense of capitalism or markets is fundamentally wrong. Some of the most accomplished neoclassical economists have been either socialists or left leaning; Arrow, Stiglitz, and Krugman come to mind. Moreover, Hodgson (p. 158) points out that some crucial developments in general equilibrium theory in the 1930s–1940s were developed by Lange, Lerner, and others with the intention to prove—through neoclassical models—the feasibility of socialism and central planning. Similarly, you cannot define the heterodoxy as a left-leaning anti-capitalist endeavor once you recognize that there are several Austrian, neo-institutionalist, and evolutionary economists that are either pro market or, at the very least, acknowledge the fundamental role of capitalism and markets in economic development and progress.

4 Readers interested in the socialist calculation debate and the Austrian critique of central planning should consult Lavoie’s (1985) historical review of the debate. Hodgson correctly acknowledges that “the overwhelming and persuasive conclusions of these studies were that Lange and others had failed to answer adequately the criticisms of Mises and Hayek. Lange and others had not provided a satisfactory outline of a workable and dynamic socialist system” (p. 37).

REFERENCES


Like most twentieth-century French intellectuals, Aron’s development was profoundly shaped by German thought. But whereas Sartre, Aron’s exact contemporary, was steeped in Hegel, Marx, and Heidegger, and perhaps the most important member of the next generation, Michel Foucault, was indebted to Nietzsche, in Aron’s case, Adair-Toteff argues, the most important influence was Max Weber. Though Aron was indebted to Husserl for his reflections on the philosophy of history, it was from Weber that he absorbed the emphasis on ‘the need for political responsibility’ (p. 37).

A sense of political responsibility for Aron, according to Adair-Toteff, meant two things. First, it demanded that politicians (but also the electorate, and the intellectuals who exercised influence in public life) carefully consider the consequences of their actions (p. 42). Politics could involve life or death decisions and had to be treated with all the seriousness that entailed. Second, it required that politicians maintain a critical distance from themselves. Vanity was a particularly dangerous failing in a politician (p. 82). But while the list of requirements for political responsibility might have been short, putting them into practice was far easier said than done. Twentieth-century politics was, for Aron, largely the history of the pursuit of the illusions that had arisen because the requirements of political responsibility had been ignored.

As Adair-Toteff shows in his opening chapter, chief amongst those illusions was the historicist conception of history itself. Aron was resolutely against deterministic theories in which history followed a necessary course. The prime example of this was Marxism. In his work on the philosophy of history in the 1930s, Aron repudiated its progressivist assumptions, arguing that the relationship between historical events had to be understood probabilistically in a way that made prediction of the kind allegedly found in the natural sciences impossible. Aron was, in this respect, broadly in the neo-Kantian tradition that distinguished the human from the natural sciences, but was less inclined to take an a priori approach to either the theory of historical knowledge or to the nature of the historical process. Indeed, Adair-Toteff argues that Aron increasingly leaned towards sociology, giving less time to pure philosophy in favour of a discipline more oriented towards the problems of action (p. 143).
This shift was in keeping with Aron’s political realism, which rendered him immune to all forms of ideological temptation. He was repelled by Hitler, though this was less because he was Jewish than because of the blindness of the Nazi enterprise (p. 87). But he was equally disinclined to embrace any conservative nostalgia for the past (p. 152). This independent-mindedness made him an astute critic of the new bi-polar international order that emerged in the aftermath of the Second World War. This post-war world would not survive more than a decade after Aron’s death in 1983, but while it lasted, he was more attuned than most of his contemporaries to its logic. This is thanks partly, so Adair-Toteff shows, to Aron’s interest (also atypical for a French intellectual of his generation) in Clausewitz and Thucydides. One the one hand, Aron was clear-sighted about the oppressive nature of the Soviet Regime, which he regarded as built on lies (p. 154). But he was equally critical of Western intellectuals, particularly in France, who continued to support the Soviet Union long after its totalitarian character under Stalin had become clear.

Aron’s critique of revolutionary and utopian thinking, *The Opium of the Intellectuals* (1955), which argued that Marxism was to the French intellectual what Marx and Engels had proclaimed religion to be for the working class, put him at odds with almost all the leading thinkers of his generation. Sartre, for example, with whom he had once been on friendly terms, never forgave him for it. But Adair-Toteff argues that unlike his contemporaries, Aron was willing to engage with the new political realities that came along with the nuclear age. Sovereignty, for Aron, was in the last resort the power to decide to go to war, and to think that perpetual peace could be achieved by international institutions was a liberal illusion that was potentially as dangerous as the illusions of Marxism (p. 116). The nation-state, for Aron, remained the dominant political unit, and that meant admitting that some nation-states were far more powerful than others. The ultimate goal of all politics, national or international, had to be the negative one of trying to prolong the absence of war (p. 122).

This sceptical orientation stemmed from the fact that in Aron’s political thought, Adair-Toteff observes, freedom and power went together (p. 146). Like Montesquieu and de Tocqueville, he argues persuasively, Aron saw freedom as civil and moral; that is, it required government. But the classical liberal ideal of the rule of law that these thinkers had endorsed was no more than that for Aron; the realities of power were always liable to corrupt it in practice. Adair-Toteff does remark in the conclusion that he has some serious differences of opinion with Aron, and he is clearly so well-versed in Aron’s thought that the reader might wish that he had given more space to his own criticisms of it (p. 171). There are certainly some puzzles to do with both Aron’s philosophy of history and his political philosophy.

For example, Aron apparently disagreed with Hannah Arendt, another prominent critic of totalitarianism with whom he shared a secular Jewish background, over the identity between Nazism and Communism. Yet he apparently endorsed Hayek’s distinction between nomocratic (law-governed) and telocratic (goal-directed) regimes. If both Nazism and Communism are telocratic, it is not clear how Aron could maintain they were at bottom distinct (pp. 142, 153). Similarly, Aron wanted to argue that the aim of historical inquiry was to assign responsibility by tying events to intentions. But his view of historical processes was that while they were retrospectively intelligible, they reflected no grand design. The relationship between an understanding of events as contingent and probabilistic and as the product of intention was another feature of Aron’s thought that remained unclear (pp. 25, 30).

There is thus a suspicion that Aron’s turn from philosophy to sociology meant that he never quite got to precise focus the nature of the distinction between theory and practice which his own rejection of Marxism implied. This is somewhat ironic given Adair-Toteff’s portrayal of Aron as essentially Aristotelian in his political theorizing, because the distinction between participating in political action and comprehending it was fundamental to Aristotle, who can be read, just like Weber, as an advocate of political responsibility. Adair-Toteff is not quite as original as he claims in highlighting this theme in Aron’s political thought, because Tony Judt’s *The Burden of Responsibility Blum Camus Aron and the French Twentieth Century* (1998), contained an important discussion of it. But Judt’s book, which oddly goes unmentioned in *Raymond Aron’s Philosophy*, fully supports Adair-Toteff’s argument that responsibility was
A vital concept for Aron. Adair-Toteff also explores the theme in greater depth than Judt, and it is deserved praise for *Raymond Aron’s Philosophy* that the reader’s reaction to the unresolved issues it contains will likely be to want to read more by Aron himself.
AIMS AND SCOPE

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