paternalism, the practical consequence of the view, we are told, is not a legal and cultural revolution, but rather something that very much resembles the status quo in America circa 2015. The fact that, both culturally and legally, the contemporary United States is probably the least paternalistic society in the history of the world, suggests that Hanna could profit from some more serious reflection on the practical implications of his philosophical views.

Finally, it should be mentioned that a great deal of current interest in paternalism was sparked by the 'libertarian paternalism' defended by Richard Thaler and Cass Sunstein, and popularized in their book *Nudge* (2008). Those who come to Hanna's book hoping for engagement with these issues will be disappointed. He does include a chapter discussing the topic, but it is all rather perfunctory. Libertarian paternalism is of interest only because of the claim that it identifies a set of strategies, which states can pursue, that despite being superficially paternalistic, nevertheless do not violate the traditional strictures against paternalistic intervention. But if these strictures are not worth defending, as Hanna suggests, then there is no reason to confine oneself to being a 'libertarian' paternalist. Thus Hanna focuses instead on the complaint, made by some, that the 'nudges' recommended by Thaler and Sunstein are manipulative in an objectionable way. But again, Hanna's view on this question is too obvious to need stating. Anyone who sees no special problem with the state coercing its citizens in order to promote their interests is unlikely to see any problem with the state manipulating them to the same end.

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Where Economics Went Wrong: Chicago's Abandonment of Classical Liberalism, David Colander and Craig Freedman. Princeton University Press, 2019, xii + 267 pages
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David Colander (1992; Colander and Su 2018) has been writing about 'the lost art of economics' for years now, calling economists to task for rejecting two key assumptions held by their classical liberal predecessors. The first assumption is the classical liberal's recognition of the role normative analysis plays in economic theory. The second is their appreciation for policymaking as an art, rather than a science. Together these two assumptions led 19th-century liberals like John Stuart Mill to view democratic governance as a form of discussion, to which the political economist brought not only their scientific knowledge but also their normative commitments and their judgment regarding how to advance liberal society.

Now Colander has teamed up with Craig Freedman, whose past work has focused on the Chicago School (Freedman 2008). The focus on Chicago should not suggest to the reader that Colander and Freedman think American Keynesianism somehow escaped their criticism. Instead, the Chicago School is the focus here because the authors see it as the last strand of modern economics to be infiltrated by creeping scientism, and, perhaps because it let down the gates more slowly than elsewhere, one of the best places to study. But regardless of the reasons behind their interpretation of the problem, Colander and Freedman believe that economics went wrong long before its failure to predict recent global financial crises. The 21st century problems are the result of 20th century developments. The authors consciously draw a much larger circle around recent criticisms, arguing that the source of 'where economics went wrong' lies in the discipline's sublimation of the art of economic policy to its scientific counterpart. Readers interested in the 20th century history of economics will profit from the book's deep dive into parts of that history, as will philosophers and political theorists interested in the role of economic expertise and the art of policymaking in a democratic society.

The central historical argument of Colander and Freedman's book can be stated as follows:

> Classical liberalism, best represented by the economics of John Stuart Mill and the methodological pronouncements of John Neville Keynes, appropriately balanced classical political economy (in both its positive and normative aspects) with the art of policy-making. Twentieth-century economics, especially the mainstream encapsulating both Chicago and Keynesian economics, tipped the scales in favor of economic science (and in the process collapsed the positive and normative differentiation). Chicago economics, in particular, accomplished this unbalancing by combining the standard assumptions of methodological individualism and *homo economicus* with a new assumption – Milton Friedman's (1953) 'methodology of positive economics' in which predictive power was the sole criterion for good science. For Chicago economics, Friedman's methodology meant that simple models with few assumptions and strong predictive power were preferred to models with many realistic assumptions intended to describe the situation before making predictions. Friedman's approach turned policy-making into the equivalent of a mechanic finding the right tool to fix your car's engine (Friedman's analogy), or a dentist choosing the right tool to clean your teeth (see Keynes 1930). In this sense, post-war Keynesianism *and* the post-war Chicago School enshrined the 'economist as policy scientist' as the new archetype of a modern policy advisor.

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At the core of Colander and Freedman's argument, then, is *not* the claim that economists are too blinded by the beauty of markets to appreciate their problematic effects. Rather, their central argument is about the economists' *methodological* commitments. First and foremost, of course, are the combination of methodological individualism and self-interested rational choice. These assumptions are broadly shared in classical and modern economics. But for Colander and Freedman, Milton Friedman's 'methodology of positive economics' (1953) is the key that turns economics into an applied policy science. And since it was the Chicago School that enshrined the modern economist's methodological commitments, it is the Chicago School that draws much of the attention in the book. The argument draws heavily on the research that underpinned Freedman's previous book, *Chicago Fundamentalism: Ideology and Methodology in Economics* (2008). In *Where Economics Went Wrong*, much of the supporting material for the argument is contained in the book's extensive endnotes, which draw upon Freedman's many interviews with Chicago economists and are well worth reading.

One can appreciate and agree with much of the criticism of this narrowed vision of economics without raising critical questions. But since the point of a review is to identify weaknesses and issues that deserve more attention, I will draw attention to only a few key aspects of their argument. The first is the simple point that the average reader, seeing the book's subtitle and cover photograph, will assume that the book's message is that the inauguration of the Chicago School is where economics began to go wrong. We usually identify the beginning of the Chicago School with the arrival of Frank Knight to join Jacob Viner at the end of the 1920s; hence, we might expect the authors to use the 1920s as their starting point. But the reader would be immediately surprised to find that Colander and Freedman consider Knight and Viner to be on the 'good' side of the line; that is, *to come before* economics went wrong. A more likely starting date that would fit the authors' story better would be the late 1940s, when Viner left Chicago, Knight slipped toward retirement writing mainly about social philosophy, T.W. Schultz and company arrived from Iowa State, Schultz assumed leadership of the economics department, Viner and Knight gave up editorship of the *Journal of Political Economy*, Milton Friedman was hired, Aaron Director assumed responsibility from the late Henry Simons for teaching economics in the Law School, and the Cowles Commission was in residence. In other words, the postwar period is when the scientistic orientation took hold at Chicago (see Morgan and Rutherford 1998; Irwin 2018). Indeed, one of the puzzling aspects of Chicago economics in the post-war period that Colander and Freedman's story helps solve is the chilly reception that F.A. Hayek received from the free-market-oriented economists in the economics department during his 12-year stay in Chicago's Committee on Social Thought. Hayek's main project during those years was a historical and philosophical study of the Liberal tradition (Hayek 1960), but he launched his stay with the publication of *The Counter-Revolution of Science*, which called the creeping scientism of fellow social scientists an aspect of the modern 'abuse of reason' (Hayek 1952). No wonder he received the cold shoulder.

Two other aspects of the Chicago story, each pointing in different directions, combine to make another point about Colander and Freedman's book – they

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missed aspects of Chicago economics that complicate their story. For example, their argument would have been strengthened if they had realized the way methodology reinforced ideology in the Chicago School's adoption of what came to be known as the *de gustibus non est disputandum* assumption (Stigler and Becker 1977). The *de gustibus* assumption set the methodological rule that changes in tastes and values could not be used as explanations for changes in market outcomes. Combined with Friedman's rule, Stigler and Becker's rule required Chicago economists to explain outcomes in terms of changes in costs and/or institutional constraints. Saying 'people changed their values' was ruled out as an acceptable explanation of any action, individual or social. The *de gustibus* assumption is an integral part of Chicago's turn away from what Colander and Freedman call classical liberalism.

But having said that, it is also the case that Colander and Freedman's focus on the well-known names at Chicago (not just the Nobel laureates), who almost all increased scientism in economics, provides a misleading picture of some aspects of Chicago economics that move in other directions. Chicago continued to be highly diverse in methodological, theoretical and normative terms, some of which reinforced the scientism and some of which did not. One would be hard pressed to rank Ronald Coase and Robert Fogel, on one side, and T.W. Schultz and Harry Johnson, on another side, in the Friedman/Stigler/Becker methodological tradition. Coase and Fogel started intellectual movements that only bore fruit a couple of decades later with the birth of new institutional economics (some mention of Coase is made, focused on his pre-Chicago days in the Virginia School). Schultz and Johnson are much closer to the classical liberal science and art combination that Colander admires. And even though Deirdre McCloskey did fit into the Friedman approach during the early part of her career, her *The Rhetoric of Economics* article (McCloskey 1983), published shortly after she left Chicago, launched her on an intellectual journey that one might argue goes far beyond any other economic historian, and even beyond Hayek and Coase, in recreating classical liberalism for the 21st century. Thus, Chicago was far more varied than the standard story suggests.

The final three chapters of *Where Economics Went Wrong* suggests three movements that point toward what classical liberalism might look like in the near future. Two of those chapters deserve attention here; the chapter on 'argument for the sake of heaven' and the chapter on the Virginia School. The third, and final, chapter of the book, well worth reading but not the focus of attention here, looks at three living economists whose work embodies at least Colander's vision of a revived classical liberalism: Amartya Sen, Dani Rodrik and Paul Romer.

The chapter on the Virginia School is both insightful and frustrating. They call the School 'stillborn' because Coase and Buchanan left the University of Virginia in the early 1960s. Were Coase and Buchanan to have stayed closely linked, our authors argue, they might have created a blend of 'approaches to economic policy [that] would have been much closer to the Classical Liberal methodological tradition than that offered by the Chicago School' (118). True, but they provide no hint of the future merger of Buchanan's constitutional economics with the re-emergence of Austrian/Hayekian economics in America at George Mason University, and the eventual inclusion of insights from Nobel laureate Elinor Ostrom into an approach that provides a significant contemporary answer to the scientism they bemoan. The merger of Austrian/ Hayekian economics, Buchanan's constitutional economics, and Ostrom's commons governance provides a significant contemporary version of Classical Liberalism that has escaped scientism.

The lack of attention to Ostrom is particularly unfortunate for their argument in these last chapters, perhaps because Colander and Freedman are not as familiar with her work as they could be. Her work represents one of the best versions of Colander's notion of the combination of art and craft that currently exists in political economy. Theory informs the design principles for common resource systems, but sorting out which principles are relevant to any specific situation, and in what degree, is an art requiring judgement and the intuitive feel of a person who has spent years and years looking at common resource systems of various types (fisheries, forests, community banks, digital commons, even academic research teams). Perhaps not surprisingly, Ostrom's intellectual debts were to Adam Smith as much as Alexis de Tocqueville, and her work is an appropriate classical liberal response to the scientism of Garrett Hardin, international environmental committees, and ministers of the environment around the globe.

Finally, the chapter on the notion of 'argumentation for the sake of heaven' is a fascinating addition to the book, and deserves a longer discussion than can be given here. However, it presents a dilemma, because it stands in some tension with the 'art-and-craft' approach used elsewhere in the book and the authors' invocations of the notion studiously avoid the key question of how expertise in a democracy should function.

Starting with the need for judgement rather than scientific expertise may help the reader understand Colander's version of the 'for the sake of heaven' argument. While postwar Chicago economists were building the approach Colander and Freedman criticize, their older colleague Frank Knight was arguing that the relevance of economic theory to economic life is a matter of judgement that comes from experience, rather than the expansion of the scope of economic theory to be more predictive. Most of the knowledge needed, Knight argued, was already expressed in the core principles of economic theory. A student should study theory to gain appreciation of the relevance of those principles, but their application required other things as well, including ethics, awareness of the political and social setting in which policy was being made, and experience with the practical realities of economic life. Thus, what we need is not the further refinement of economics principles, but rather

> to respect all the principles, take them fully into account, and then use good judgment as to how far to follow one or another in the case in hand. . . . The truly right course is a matter of the best compromise, or the best or "least-worst" combination of good and evil. As in cookery [N.B. – a *craft*], and in economic theory, it calls for enough and not too much, far enough and not too far, in any direction. Moreover, the ingredients of policy are always imponderable, hence there can be no principle, no formula for the best compromise . . . (Knight 1951: 6)

Colander's 'art and craft' approach is exactly the kind of social discussion of principles, their relevance, and the art of going far enough, but not too far, that Knight described.

But the expression 'argumentation for the sake of heaven' also has a different meaning, springing from the Jewish tradition in which it was first used. There, the judgement made had nothing to do with discussion about the appropriate application of knowledge to a society's problems. Instead, it was used in the context of admitting that a community did not know enough yet to decide between competing claims about truth. For the sake of posterity, scholars from both sides of a disagreement over the meaning of a text would be brought together to give the best version of their view possible, refined through discussion with their opponents. No decision would be rendered; the arguments were being made 'for the sake of heaven'. Rendered in today's context, the Jewish notion of 'argumentation for the sake of heaven' concerns scientific discussion, rather than the art and craft of its application. 'Truth wins out', eventually, but only if we remember the history of our debates, and look for lost arguments that may, today, have relevance even though they were out of touch with their own time.

The Jewish notion of 'argumentation for the sake of heaven' accepted that the community's goal was *truth*. So how should we understand Colander's efforts to resurrect it today? How is this notion relevant to a modern democratic society, which accepts that social discussion about policy is not about truth? Democratic discussion is not built upon, nor does not produce, claims about how a society should truly live. Its products are compromises among competing demands, accommodations to various constraints, and incentivized adjustments to the plans of individuals, businesses and other organizations that emerge from the decisions made, mostly by bureaucrats. Even on Colander's terms, it probably isn't even a very pretty business.

The classical liberals Colander and Freedman suggest we remember would probably agree. They did think of democracy as a never-ending discussion, full of compromise and even ill-fated judgements. They had some hope that decision-makers were held in check by the court of public opinion. They understood that the science of economics provided a few general propositions about the functioning of markets, which society ignored to its peril. But figuring out how to adapt them to a specific society, and to a specific point in that society's economic development, required more than just the scientific truth-claims. One needed to know what normative objectives were to be advocated and what constraints the current state of society imposed on the process of accomplishing progress. Democracy had to be more than a vote between which group of experts – the Keynesians or the Chicago School – should be running the show. Fortunately, we do have a few examples in *Where Economics Went Wrong*, pointed to earlier, to look at to see how things might be different.

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Economic Statecraft: Human Rights, Sanctions, and Conditionality, Cécile Fabre. Harvard University Press, 2018, 214 pages.
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Over the course of two decades, six monographs and many articles Cécile Fabre has been a staunch defender of cosmopolitanism and individual rights. Her rights-based theory of cosmopolitan justice takes fundamental rights and their correlative duties as the starting point of moral reasoning. In her most recent book, *Economic Statecraft*, Fabre discusses what justice thus conceived implies for the use of economic foreign policy tools. She argues that the protection and enforcement of fundamental rights sometimes justifies the use of sanctions and the conditionality of aid and loans. Some fundamental rights are at first glance in tension with economic sanctions and conditional economic offers. Sanctions interfere with property rights and in particular with the right to trade. Conditional aid and loans interfere with the recipients' rights to some of the resources of the affluent. But Fabre shows that upon closer inspection some of those seeming tensions dissolve and where the tensions are real she presents solutions.

With *Economic Statecraft*, Fabre once again makes important contributions to philosophical debates on the protection of human rights. She can be commended for calling attention to sanctions, aid and loan conditionality; policy