chapter on Adam Smith and Latin American public initiatives to the final section on Smith, Locke, and Montesquieu.

The organizational issues, however, are secondary—readers are, after all, free to read the chapters in any order they like—and at the end of the day the assessment of the book falls on the quality of the chapters, which range from very good to excellent. I expect that readers of this journal will find most of the eleven chapters to be of significant benefit. Non-specialist readers may appreciate the three contemporary chapters the most but will still find much of value in the rest, due to the clear writing and argumentation therein. Although the book would be a more solid academic collection without those three chapters, which would fit better in another volume with other contributions in the same style, it is nonetheless a worthy addition to the growing corpus on the morality of the market.

Mark D. White
College of Staten Island/CUNY

David Colander and Craig Freedman, *Where Economics Went Wrong: Chicago's Abandonment of Classical Liberalism* (Princeton and Oxford: Princeton University Press, 2019), pp. 267, $27.95 (hardcover). ISBN: 9780691179209.

doi: 10.1017/S1053837219000270

This book argues a bold and seemingly simple thesis: that modern economics went astray when it abandoned the distinction between the science and art of economics, a change that happened in between the generation comprising Frank Knight and Jacob Viner and that represented by Milton Friedman and George Stigler. The book begins with a chapter on how economists came to conceive policy making as applied science rather than as "art and craft" (p. 1) followed by a chapter on what they call "Classical Liberal methodology" (p. 20). There are then three chapters on the course followed by Chicago economics, a chapter on welfare economics (central to the vision of economics as applied science), and a chapter distinguishing the Virginia School of Economics from Chicago. Conclusions are then drawn in two chapters.

The simplicity of the book's thesis is deceptive, for, as the title indicates, whilst the book is making claims about economics in general, the focus is on economics as practiced in the University of Chicago. A further complication is that "classical liberalism," a term usually understood as an ideological position, is used to refer to a methodology according to which a "firewall" is constructed separating economic theory from economic policy. And what of those who were not "classical liberals" but whose ideas nonetheless fed into modern economics? Given its complexity, the thesis requires precise definitions and careful marshalling of supporting evidence, but I was unfortunately left unsatisfied on both counts. Given that the book contains seventy-seven pages of endnotes, most readers, who will not read all the endnotes, might take the thoroughness of the documentation for granted. This would be a mistake, for most of the endnotes are, as the authors concede (p. xi), tangential to the main argument. As a reviewer, I vacillated between following the authors' advice to the "average reader" to skip the endnotes and searching the endnotes for (and usually failing to find) the evidence I felt was lacking in the main text.

Central to the book is the argument that the methodology of classical liberalism involved constructing a "firewall" between economics and policy. If a firewall meant what it means in a modern financial institution, this would imply that policy should be undertaken in schools of public policy, business schools, and similar institutions, whose members never talk shop to economic scientists, housed in separate departments. What, then, would be the purpose of economic science? David Colander and Craig Freedman presumably mean something much weaker than a "firewall," such as the recognition that policy depends on values as well as on scientific results. But how many modern economists would disagree with that?

Despite the book's focus on Chicago, Colander and Freedman label the approach that they are challenging "Samuelsonian." There is no doubt whatsoever that Samuelson attached great importance to being scientific, but I see no evidence that Samuelson failed to recognize that policy needed to take account of values as well as scientific evidence. To the contrary, the Bergson-Samuelson social welfare function makes this explicit: he repeatedly argued that normative conclusions required ethical judgments. He was skeptical about the policy conclusions that could be drawn directly from economic theory alone. For a start, most theory focused on perfect competition, whereas he saw real-world markets as involving oligopoly and other non-competitive market structures characterized by the rivalry and coalition formation analyzed by John von Neumann and Oskar Morgenstern. Yet he was not an enthusiast for the game theory that would be needed for what Colander and Freedman describe as "Samuelsonian" methodology. I see no evidence that Samuelson supported this, and, given that they draw on Abba Lerner's *Economics of Control*, perhaps "Lernerian" would be a more apt term than "Samuelsonian."

A major problem with the book is that it discusses very few examples of economic policy. By this I mean not broad discussions of policy in general, such as John Maynard Keynes's views on laissez-faire and the role of the state, but specific instances of economists who have used theory to support policy conclusions. For example, Samuelson used his consumption-loan model to argue that a pay-as-you-go system of national insurance could create higher welfare than a fully funded system, and Joseph Stiglitz has drawn on his models of asymmetric information to argue that some central bank policy responses have been inappropriate. Or take the example of James Tobin, accused, without a shred of evidence, of "violating the science/policy firewall" (p. 10). How was Tobin to analyze the effects of "Regulation Q" without constructing a model of the financial system containing a sufficiently detailed model of the banking system? Economic science was necessary to provide the advice that policy makers needed. Colander and Freedman's "firewall" between economic science and policy would have stopped this important advice being offered. My conjecture is that Samuelson, Stiglitz, and Tobin knew full well that care has to be taken in applying theory and that they do their best to engage sensitively in what Colander and Freedman call the "art" of policy.

I hope that non-British readers will forgive a parochial example that may no longer be topical when this appears in print: the problem of "Brexit" provides an ideal illustration. There is no doubt that the decision to leave the European Union is not just an economic problem: it involves questions of identity, attitudes towards reneging on international

https://doi.org/10.1017/S1053837219000270 Published online by Cambridge University Press

agreements, political judgments, and much else. A strong case can be made that it would be helpful to keep these issues separate so as to achieve the clarity needed to reach a consensus. If supporters of Brexit were to say that the economic costs were a price worth paying for other gains, that would be a legitimate argument. However, if, as is the case, the claim is made that Brexit will produce economic gains, economists surely have a duty to analyze those claims with all the scientific rigor they can command. A "firewall" would appear to be a ridiculous metaphor for the need to be alert to the limitations of economic theory and empirical work.

As the book is about economics in general and the methodogy under criticism is described as "Samuelsonian," my choice of examples is legitimate. Readers may expect examples involving Friedman and Stigler. Colander and Freedman may be right in arguing that they apply scientific economics without regard for the wider considerations involved in the art of economics. However, in the case of Friedman, two things give me cause for concern. Friedman repeatedly referred to the quantity theory of money as a "framework" rather than a theory, which must blur the distinction between science and art. Furthermore, as Colander and Freedman recognize at one point, Friedman believed that there was general agreement on values, and that most disagreements could be traced to disagreements over positive economics. Is it therefore not possible that he agreed with Colander and Freedman but chose to focus on the place where he believed there was disagreement?

Though I offer this as no more than a conjecture, is it possible that the evidence Colander and Freedman present on Chicago economics could be better explained by Chicago economists' having a commitment to what we might call "classical liberal" ideology: that their ideological commitment shaped their policy advice and created blinders that resulted in their scientific work supporting that policy advice? (Although expressed differently, this would be consistent with Melvin Reder's "tight prior equilibrium" interpretation of Chicago economics.) The disappearance of the much more nuanced policy positions held by "classical liberals" (many liberals supported what would now be considered extensive state intervention) could then be explained not by Friedman and Stigler having ignored the difference between the science and the art of policy, but by their ideological commitment. Perhaps it is what Colander and Freedman mean by "Painting policy by the numbers" (the subtitle of chapter 5).

No doubt there are, among the thousands of American economists, some or even many who believe that policy can be derived directly from scientific economics. Perhaps that is true of Chicago economists. Everyone will have their own examples of economists who have been overconfident in proposing policies that turned out to be misconceived, but I am not convinced that applies to the best, or even to most economists. Though I suspect their argument cannot be substantiated, the problem is that making Colander and Freedman's case requires a different sort of book: one that looks carefully at what happens when economics is applied to policy. To be convincing, in my view, the picture needs to be be painted with a much finer brush than the one used here.

The endorsements on the dust jacket praise the book for reminding economists of the need for greater humility (Diane Coyle). No one should disagree with that, or the claim that economists should treat their opponents' arguments with respect (Deirdre McCloskey), or that economists should remember that their science cannot produce clear and unambiguous policy advice (Dani Rodrik). However, surely the main danger today is not economists' overconfidence but the disdain in which "experts" are held. It may be important to acknowledge that "experts" do not hold all the answers, but it is dangerous when that shades into disdain for evidence-based policy or when skepticism about experts results in policy being based on what Keynes called the ideas of some defunct economist. Simple ideas about supply and demand may be able to capture the public imagination and in some settings they are very useful, but, in a world of asymmetric information and significant transactions costs, they may sometimes give harmful advice. Colander and Freedman's call for the imposition of a firewall between science and policy is therefore potentially dangerous. Claiming that their argument needs to be stated with greater precision and backed up with stronger evidence is, I would contend, much more than the nit-picking of someone who sees merit in historians being more cautious in drawing normative conclusions from the history of economics.

Roger E. Backhouse
University of Birmingham and Erasmus University Rotterdam