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**ALAN EBENSTEIN, HAYEK'S JOURNEY: THE MIND OF FRIEDRICH HAYEK**  
(Palgrave MacMillan, 2003)

Review by Israel M. Kirzner, Professor Emeritus of Economics, NYU.

Friedrich Hayek (1899-1992) was an extraordinary social philosopher whose work has fascinated many younger scholars, especially since his death. Often seen as the twentieth century's preeminent intellectual exponent of classical liberalism, Hayek began in pure economics, the discipline that arguably shaped his life's work in many other fields. By the end of his life, he had published an enormous volume of scholarly work, not only in economics (which he never entirely left), but also in political philosophy, psychology, pure philosophy, and social theory. In economics, his work is seen as central to the development of the Austrian Economics tradition during the first half of the twentieth century, and especially as indirectly contributing to the revival of that tradition during that century's latter decades. In economics Hayek's work dealt with the history of thought, monetary theory, capital theory, trade cycle theory, and, perhaps most fundamentally, the role of the market's competitive process in communicating knowledge through the price system. In this latter work he made important contributions to the economic critique of socialism—a critique pioneered by his mentor, renowned Austrian economist Ludwig von Mises. A strong case can be made that it was this extension of the Austrian understanding of the spontaneous market process which led to Hayek's later work on the unplanned development of social and political institutions.

Many books and probably hundreds of papers on Hayek's thought—a number of them brilliant treatments of their subject—have emerged in the dozen years since his death. Alan Ebenstein published his highly praised *Friedrich Hayek, A Biography*, in 2001. He now presents us with a companion volume that attempts a full-scale “complex intellectual portrait” of Hayek by offering “an in-depth look at the evolution of his thought.”<sup>1</sup>

The requirements for such an ambitious undertaking are daunting in the extreme. It is not sufficient to be thoroughly familiar with Hayek's six decades of work, with the history of economic thought, and Austrian Economics in particular over the past century and a half. In addition, the author would have to be completely at home with the twentieth century developments in each field to which Hayek contributed. To put it somewhat differently, a successful intellectual biographer of Hayek, writing over a period of, say, three years, would need to have at his fingertips everything Hayek himself learned during his entire seventy-year intellectual life. An optional, additional requirement (which Ebenstein apparently also believes is required) would be the ability to report on contemporaneous and subsequent scholars' assessments of Hayek's work. To judge Ebenstein's book to have failed to achieve this objective is thus hardly a devastating criticism of its au-

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<sup>1</sup> ALAN EBENSTEIN, HAYEK'S JOURNEY: THE MIND OF FRIEDRICH HAYEK (2003) (quoted from jacket copy)

thor or the book itself. And, indeed, a fair-minded critic cannot fail to be impressed by the industriousness with which the author has canvassed Hayekian literature in attempt to achieve completeness and balance in his coverage and assessments. This is a work which reflects dedicated scholarly labor. Nonetheless, a reviewer has the responsibility not only of recognizing this dedication, but also of pointing out shortcomings in the work.

What follows discusses (1) some of the strong points in Ebenstein's work; (2) some of its specific (not necessarily major) weaknesses; and (3) what this reviewer sees as a failure in the delineation of the larger picture of Hayek's work that Ebenstein set out to accomplish.

(1) Ebenstein has examined an enormous volume of Hayek's writing and commentary about Hayek. He has, from first- and second-hand sources, familiarized himself with the major intellectual issues arising during various phases of Hayek's intellectual lifetime. Virtually everything that should be mentioned in a definitive intellectual biography of Hayek is mentioned in this book. Someone wishing to acquire direct knowledge of Hayek's work and of Hayekian literature (provided he steels himself against Ebenstein's judgments) will find this work a valuable bibliographical guidebook. In all this, the book nicely complements Ebenstein's earlier purely biographical work on Hayek. And for this, Ebenstein deserves full credit.

(2) At the same time (and apart from my critique, offered below, of the overall picture of Hayek's work presented in this book) one must comment on numerous small errors, or at least infelicities, which qualify admiration for the book as a bibliographical guide to Hayekian literature. Each of these errors in itself hardly constitutes a serious defect, but in their totality they cast a troubling shadow. Some examples:

(a) Ebenstein's treatment of several key economic concepts, especially in the history of Austrian Economics, is imprecise or unclear: The treatment of diminishing marginal utility,<sup>2</sup> opportunity cost,<sup>3</sup> Mises's version of the Austrian trade-cycle theory,<sup>4</sup> the relationship between Hayek's celebrated papers on the role of knowledge in market processes and his work on *The Abuse of Reason* in the history of social sciences,<sup>5</sup> and his references to the Salerno-Rothbard thesis on the "dehomogenization" of Mises and Hayek.<sup>6</sup>

(b) Many admittedly trivial, but troubling, infelicities also mar the book. Ebenstein seems unaware that economic history is not identical to the history of economics. Historians of economic thought are, in economics literature, not generally referred to as "economic historians" as they are throughout this book.<sup>7</sup>

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<sup>2</sup> See *id.* at 30-31.

<sup>3</sup> See *id.* at 31.

<sup>4</sup> See *id.* at 50-53.

<sup>5</sup> See *id.* at 108-22.

<sup>6</sup> See *id.* at xiii, 50.

<sup>7</sup> See *id.* at 19, 24, 29, 32, 33, 34, 37.

Shackle's name is misspelled both in the text<sup>8</sup> and in the Index. A number of chapter titles in the book are highly inept indicators of those chapters' content.

(c) A number of the judgments scattered across the book seem at best arbitrary. Admittedly, such judgments reflect the subjective assessments of the writer and, therefore, can hardly be definitively rebutted. Yet some do raise eyebrows, and at least one can be described only as quite astounding, if not bizarre. Ebenstein comments egregiously that one of the "paradoxes of Hayek is that he wrote better than he thought," explaining this to mean that "Hayek's thought was not as profound or stimulating as the writing based on it."<sup>9</sup> Ebenstein throws out this sweeping judgment almost casually, without further explication and without providing any illustrations. Presumably, Ebenstein saw the dozen years of hindsight since Hayek's death as enabling him to look down on Hayek's life's work from a magisterial vantage point and pronounce such a dismissive judgment.

(3) This reviewer's professional familiarity with the Hayekian literature is largely confined to economics and, in particular, to the context of the Austrian Economics tradition in which Hayek was trained and to which he made fundamental contributions. Yet Ebenstein's treatment of this aspect of Hayek's work has prevented him from providing a coherent overall narrative of Hayek's intellectual journey. And because of the centrality of economics in Hayek's lifework, any shortcomings in the discussion of Hayek's economics necessarily and unfortunately limit the clarity and cogency of the remaining story.

Granting that different students of Hayek's work may arrive at different understandings of his intellectual "journey," I must nonetheless express my conviction that Ebenstein has not succeeded in giving us the "big picture." Perhaps the very comprehensiveness of his attempt is responsible for this failure. The jacket cover of the book contains a picture of Hayek seemingly made of a large number of small, distinct segments. But Ebenstein's hundreds of direct quotes from Hayek's own writings, as well as his large number of direct quotes from secondary sources (not all of them wisely chosen), do not add up to an integrated story. To make reference to virtually every issue touched upon in Hayek's writings or Hayekian literature does not necessarily provide an intellectually satisfying sense of continuity; the dots have not been connected (although Ebenstein does link the emphasis on evolutionary processes in Hayek's later work to the Darwinian intellectual milieu of Hayek's youth). One might perhaps claim that Hayek's lifetime work does not constitute a coherent narrative, but Ebenstein does not make any such claim. One is left with a certain sense of dissatisfaction. The reader expects a book-length story; it is only a slight exaggeration to say that instead he has received a volume of discrete chapters (sometimes even discrete paragraphs). I believe that a coherent story does exist (and therefore my critical judgment here may reflect the bias of my vested intellectual interest): Hayek's economic insights in the 1930s and 1940s concerning the spontaneous manner in which dispersed crumbs of knowledge come to

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<sup>8</sup> See *id.* at 72.

<sup>9</sup> *Id.* at 118-19; see also *id.* at 185.

be deployed in markets in socially relevant patterns—insights that emerged from the socialist calculation debate—led him to perceive cognate patterns of spontaneous social coherence in the broader contexts of societies’ legal and other institutions.

There is much to admire in this book. That it falls short of being *the* authoritative, sweeping survey of the evolution of Hayek’s thought need not prevent us from recognizing the contribution it makes to the rapidly burgeoning field of Hayek Studies.

**BRUCE CALDWELL, HAYEK'S CHALLENGE: AN INTELLECTUAL BIOGRAPHY OF  
FRIEDRICH HAYEK** (University of Chicago Press, 2004).

Review by Alan Ebenstein

Professor Bruce Caldwell is a fine scholar who in previous work has contributed to our understanding of important historical schools of thought.<sup>1</sup> I cannot see, however, that in the long run his most recent book<sup>2</sup> will prove an important scholastic contribution, though it likely will be met by encomia from various scholars and academics in the short run. It is time, in Milton Friedman's words, for a "methodological sermon."

Professor Caldwell is neither an Austrian nor a positivist. His greater criticism is of positivism rather than Austrianism, although he does comment that "the specific [business] cycle theory developed by Hayek must now be viewed as chiefly of antiquarian interest."<sup>3</sup>

*Hayek's Challenge* was not intended as an intellectual biography of Hayek, and it is not—a statement with which I should think Professor Caldwell would agree. Rather, the subtitle was suggested to him by the publisher. The original working title was "F.A. Hayek and the Limits of Social Science," and it is in this area that this work makes its greatest contribution.

I personally cannot agree with Professor Caldwell's opposition to positivism, and I think this opposition undercuts many of his conclusions in the book's epilogue. Caldwell inveighs "presuppositionless" theory and states that all observation is "theory laden." This reviewer is more optimistic about the possibility of achieving agreement on sensory, or empirical, issues.

From a methodological perspective—and now the sermon commences—prediction is the most vital component of scientific discourse. For the philosophy of science, where there is no prediction, there is no science. By contrast, the demarcation line of science suggested by Karl Popper, on whom Professor Caldwell has also written, is inadequate. In essence, Popper's idea of falsification is that there is no such thing as absolute proof. There is then, indeed, no such thing as absolute falsification. Our knowledge is always incomplete and provisional. This does not mean that we cannot come to relative agreement with respect to activity in the sensory, material world. This is why Professor Caldwell's criticism of positivism fails.

During the twentieth century, our knowledge of economic activity expanded substantially. We learned that the Great Depression was not caused by inevitable deficiencies in capitalism, but by mistakes in monetary policy. We learned that command economies are less economically productive than free private property capitalism and that free markets are economically dynamic. We learned that inflation is a monetary phenomenon. We learned that democracy is

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<sup>1</sup> See generally BRUCE CALDWELL, *BEYOND POSITIVISM* (1982); CARL Menger and His Legitimacy in Economics (Bruce J. Caldwell ed., 1990)

<sup>2</sup> BRUCE CALDWELL, *HAYEK'S CHALLENGE: AN INTELLECTUAL BIOGRAPHY OF FRIEDRICH HAYEK* (2004).

<sup>3</sup> *Id.* at 325.

not compatible with command economies and that free private property capitalism is. These were major lessons.

Thus, I cannot agree with Professor Caldwell's pessimistic outlook on the future study of the history of economic thought. I predict this study will expand in the years ahead.

The ironic aspect of Professor Caldwell's perspective that "measurement without theory" is useless (or not possible) is that it is in some ways close to that of econometricians, who do not appear much in his work. Prediction is important in discourse with scientific aspirations because academics and others who typically aspire to scientific discourse often have excellent verbal skills. But it is not enough merely to write well. To make a contribution, there must also be substance in what one writes (which is not to say there is no substance in Professor Caldwell's work). This is where prediction enters the methodological picture. Two academics who both write well, with ample information and discussion, claim validity for their respective ways of looking at the world. How do we know which is correct?

The positivist predictionist view (predictive positivism), which is this reviewer's own, is important because at least one criterion for many models or theories of economic activity is or should be whether they tell us anything about the future. If not, then it would be hard to see that they would be of much, or at least of complete, worth. On the other hand, if the answer is yes, then we have at least one way to discern two theories or models of economic activity and to evaluate them.

In an interview with this reviewer in 1995, Professor Milton Friedman expressed views along this line. When discussing his methodological outlook in contrast to that of Hayek, Friedman said: "What we do in science is offer hypotheses about the consequences of certain events, and if we disagree, we test those by trying to seek empirical evidence . . . . [I]f you and I disagree, we have . . . [a] way to solve our problems, resolve our differences. I say to you, 'what facts can I find that will convince you I was right and you were wrong?' You say to me, 'what facts can I find that will do the opposite?' Then we go out and observe the facts. That's how science progresses." End of sermon.

To return directly to *Hayek's Challenge*, Professor Caldwell focuses on Hayek's methodological work in the essays collected in *The Counter-Revolution of Science* (1952). It is not this reviewer's opinion, as it is apparently Professor Caldwell's, that this was Hayek's most important work. The essays in *The Counter-Revolution of Science* are relatively turgid and not as interesting as other parts of Hayek's work. Much of *The Counter-Revolution of Science* is a history of the development of social ideas during the nineteenth century which, while interesting to specialists, is not of much theoretical interest. Rather, the important theoretical part of *The Counter-Revolution of Science* is its first part, where Hayek brilliantly expresses that there are limits to reason and that attempts to make reason do more than it can are condemned to failure.

I entirely agree with this sentiment, and there are some nice passages in the first part of *The Counter-Revolution of Science* where Hayek states it. But there seems

to be little more of value in the first part of this work. To accept that there are some limits in the social sciences is not to say there are total limits. To say that mankind should not attempt to make reason do more than it can is not to say what reason can do. I am considerably more optimistic than Professor Caldwell that the limits of social science are not so extensive.

Furthermore, Professor Caldwell does not address why Hayek made such a mistake about what he spent his time on during the last forty years of his career. Hayek thought this work of utmost relevance. When he wrote them, he considered both *The Constitution of Liberty* and *Law, Legislation and Liberty* to be possibly his most important works. This became even more so his view with respect to *The Fatal Conceit*.

Most of the extensive historical background and discussion in *Hayek's Challenge* revolves around methodological issues. Relatively little attention is directed toward Hayek's work in political philosophy and technical economic theory.

This reviewer once did a survey of the authors to whom Hayek referred the most in his four major works of political philosophy over the last four and a half decades of his career—*The Road to Serfdom*; *The Constitution of Liberty*; *Law, Legislation and Liberty*; and *The Fatal Conceit*. The top ten were, in order of number of references, David Hume, Adam Smith, John Stuart Mill, Karl Popper, Ludwig von Mises, Edmund Burke, Lord Acton, John Locke, Aristotle, and Jeremy Bentham. It says much about *Hayek's Challenge* that five of the figures most cited by Hayek (Burke, Acton, Locke, Aristotle, and Bentham) do not appear in it at all. Hume and Mill barely appear. In addition, Hayek considered Immanuel Kant on some occasions, who barely appears in *Hayek's Challenge*.

*Hayek's Challenge* also does not cover the evolution of most of Hayek's economic thought. In earlier work, particularly *Hayek and Socialism* (1997), Professor Caldwell demonstrated that he can wonderfully recreate the intellectual milieu in which Hayek's thought evolved. He could do similar work with respect to the development of Hayek's technical economic theory.

In the second appendix of *The Pure Theory of Capital* and elsewhere, Hayek identified the line of British, German, and other thinkers from which his trade-cycle theory developed. He did not emphasize forbearers in the Austrian school (with the important exception of Mises) for the simple reason that Carl Menger, Eugen von Bohm-Bawerk, and Friedrich von Wieser did not write much on trade-cycle theory. Nor did the German historical school. They were of earlier generations than most of the development of trade-cycle theory.

Excellent intellectual biography could be written on this development in Hayek's thought, and Caldwell would be just the one to do it. Among the intellectual forbearers of his technical economic work that Hayek identified, in addition to Mises, were Karl Marx, M. v. Tugan-Baranovsky, Gustav Cassel, and Arthur Spiethoff. It would be a considerable contribution to our knowledge of Hayek if the relationship between the work of these men and Hayek's economic thought were explored in greater depth.



In addition, it would be a significant contribution to our understanding of Hayek's work if other figures he identified as so important to him—particularly Hume and other Scottish writers—and their intellectual relationship to his work were explored in more depth. This is not, however, the purpose of *Hayek's Challenge*. Rather, it mostly considers from a historical perspective such figures as Max Weber and the German historical school, whom Hayek hardly considered at all and did not say were at all important to him.

Because knowledge is decentralized, it is not possible to plan an economy or society centrally. Hayek's work is of significant continuing relevance, and it is for this reason vital that discussion of his work continues and expands. I should like to close, accordingly, by thanking Professor Caldwell for his thoughts on an earlier version of this review and his willingness to engage in this dialogue.

#### Response to Alan Ebenstein by Bruce Caldwell

I welcome the opportunity to comment on Alan Ebenstein's review of my book, *Hayek's Challenge*, not least because I disagree with so much that he has said.

In the first instance, contrary to what Ebenstein asserts, the book *is* intended to be an intellectual biography, in the sense that I tell the story of the development of Hayek's ideas, and only peripherally deal with his personal life. To be sure, I do not cover all of his ideas in equal depth, but neither is the book as narrowly focused as Ebenstein suggests. I have chapters on his early monetary theory; on his inaugural lecture at the London School of Economics; on his famous paper "Economics and Knowledge" and its links to the socialist calculation debate, where Hayek first began developing his views on the implications of knowledge dispersion; on the Abuse of Reason project (where I emphasize his "Scientism" essay, which has received multiple interpretations in the secondary literature); on Hayek's often-neglected writings on psychology; and on his work from the 1950s onward on political philosophy and social-science methodology. Because I do not treat all topics with the same depth, I generally tried to point the reader to the relevant existing secondary literature when my treatment of an area was lighter.<sup>1</sup> As such, I feel that anyone who reads my book will have a solid understanding of what Hayek thought and how he came to hold his views. Given that I take pains to alert the reader about, and to justify, my particular emphases in the book's introduction, Ebenstein's complaints here are at a minimum misleading.

Indeed, much of his review is about what I did *not* write, which is always an available strategy for a reviewer ("You did not write the book I wanted!"), but not helpful to the reader. Given the space Ebenstein gives to this, I will at least respond to his "citation count" complaint. First and most evidently, a citation count is not an argument. If Ebenstein thinks I made interpretive errors due to my neglect of the individuals he mentions, he should offer details. Next, his list is drawn

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<sup>1</sup> See, e.g., BRUCE CALDWELL, *HAYEK'S CHALLENGE: AN INTELLECTUAL BIOGRAPHY OF FRIEDRICH HAYEK*, 11 n.11, 176 n.10, 347 (2004).

only from Hayek's works in political philosophy, an area I admittedly did not emphasize (though I did not emphasize it, I did treat it, and I certainly never said I thought that Hayek's work in the area was a mistake and cannot imagine how Ebenstein manufactured that idea). Furthermore, Hayek's reference over the years to some of the ten figures Ebenstein mentions was not consistently substantive. For example, Hayek frequently quoted Acton as a representative of classical liberalism, usually in epigraphs to illustrate points. He did not use Acton's specific ideas in the way that he used those of Mises or Hume. Finally, I will note that Ebenstein has his own book purporting to discuss "the mind" of F.A. Hayek.<sup>2</sup> Taking the five figures he faults me for not mentioning, Ebenstein has no citations to Burke or Bentham, Acton gets only one, Aristotle three, and Burke four. If these are essential figures for explaining Hayek's "mind," how does Ebenstein account for his own slighting of them?

Let us move from what I do not say to what I do. I begin the book by noting that interpreting Hayek poses many challenges, such as why he kept switching research programs. As my brief summary above indicates, his intellectual journey was peripatetic, ranging from economics (and various areas within economics) to social science methodology, psychology, and political philosophy and theory. How can we explain this? No one has tried to answer this question before. In trying to answer it myself, I decided to focus on Hayek's changing views on methodology as a unifying theme.

Hayek's work nearly always contained a methodological component. In his early monetary theory, for example, he mouthed the earlier Austrian position regarding the role of empirical and theoretical work in economics. In the 1930s, in the course of the socialist calculation debate, he began to develop his own ideas, questioning the ability of the standard equilibrium theory of his day to capture the workings of a competitive market process—work which led him to his insights on the knowledge problem. In the 1940s, Hayek tried to explain why so many of his opponents could not understand the points he was making, and again he turned to methodological argumentation: his opponents all embraced some variant of "Scientism." One variant was objectivism—for example, behaviorism in psychology—and this is the reason (or so I argue) that after the war Hayek decided to produce *The Sensory Order*. Finally, as he began to make contributions to political theory and philosophy in the 1950s, Hayek again began changing his views on methodology, distinguishing the sciences not by the traditional social-science/natural-science split, but according to the complexity of the phenomena they investigate. His later emphases in his political work on rules, complex orders, and evolution are all illuminated by an understanding of these developments. The range and apparently disparate nature of Hayek's contributions are daunting, but by focusing on the development of Hayek's methodological views and the battles he fought over the proper methods of the social sciences, one can more effectively understand the trajectory of his intellectual journey.

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<sup>2</sup> ALAN EBENSTEIN, *HAYEK'S JOURNEY: THE MIND OF FRIEDRICH HAYEK* (2003).

That the emphasis on methodology underlines Hayek's role in a much larger drama even further recommends this emphasis. In this sense, the book is about more than just Hayek, which is why its initial working title was "F.A. Hayek and the Limits of Social Science." Though positivist philosophy had nineteenth-century antecedents, in its modern instantiations it was Viennese in origin, and from there it spread to the world. Within the philosophy of science, positivist ideas were superseded in the 1960s and 1970s, but their influence on the rhetoric and self-image of social scientists remained strong much longer and arguably lasts even until today. Anyone who knows this story (and presumably this would include those who have read my earlier book, *Beyond Positivism*, where I tell the story) would, I think, find it fascinating to see how the Austrian economists had much earlier fought many of the same intellectual battles that later replayed in the philosophy of science and the methodological writings of the special sciences over the course of the twentieth century. As in so many things, the Austrian economists got there before anyone else. I wanted to explain how this came about, which is one reason I devoted considerable space at the beginning of the book to the *Methodenstreit* and the debates over socialism and positivism that preceded Hayek's own contributions. But there was another reason: though they participated in the founding of modern microeconomics, it is evident that the Austrians are in many ways different from mainstream economists, and the most important of these differences are methodological. If we are to understand how and why the Austrians separated themselves from the mainstream, we must delve into the origins of these views.

As I noted above, in philosophy positivism is dead, but it still influences how economists view their own practice. Hayek was a critic of positivism. In the last section of the book, I argue that his view is the more persuasive, and that by adopting it we better understand how economics developed as a discipline in the twentieth century. Had the positivists been right, we would have expected economics to have developed very differently. Hayek's challenge to positivist ideas, then, is his final challenge to the discipline of economics.

This is one area where Ebenstein and I differ most strongly, and I think it is an area in which honest disagreement is possible—Hayek and Milton Friedman, for example, appear to take different views about prospects for positivist ideas in economics. However, it would be dishonest if I did not state that, in my view, Ebenstein's characterization of the issues is often naïve and at times borders on incoherent.

An example of the former is his bald statement that "where there is no prediction, there is no science." The issues at stake are more nuanced. Hayek believed that when dealing with complex phenomena, the best we can do is to provide broad pattern predictions or provide the principles that underlie an explanation. Positivists, on the other hand, believe that the ability to make increasingly precise predictions is the best measure of scientific advancement. Given these two accounts, which makes the best sense of, say, evolutionary theory, which most observers would label a scientific theory? The theory of evolution explains speciation using the principles of variation, heritability, and selection; it explains the princi-

ples by which it takes place. While this allows evolutionary theory to explain speciation after the fact, it cannot predict the specifics of species change, because natural environments are too complex. Hayek often called evolutionary theory a paradigmatic example of a science that studies complex phenomena. He repeatedly emphasized that when dealing with such complex phenomena, our ability to predict is limited. Ebenstein's simplistic statement leaves out everything that is important in analyzing such a case. On his reading, one presumes, evolutionary theory is not science.

I can make no sense of some of Ebenstein's other remarks. Hayek agreed with Popper that knowledge is incomplete and provisional; they were fallibilists. But neither Hayek nor I ever concluded from this, as Ebenstein seems to think, that humans cannot reach relative agreement about the sensory order. Indeed, in the "Scientism" essay, Hayek asserted that the "common structure" of the human mind helps explain why humans experience things in a similar way, and he noted that our ability to communicate with one another was further evidence for it. Ebenstein seems to think that Hayek's or my criticism of positivism rests on this premise. Hayek's criticisms of positivism rest on the belief that the best we can do is to make pattern predictions in economics, because the field examines complex phenomena. He made this claim well before the economics profession developed an impressive array of econometric techniques and computing power that, had the positivists been right, would have allowed them to make more precise predictions. My claim is that Hayek was prescient about the prospects for positivism, and I think that the history of economics in the twentieth century supports this claim.

Ebenstein lists some "knowledge" that we have gained in the twentieth century. His examples, however, can be used to prove the opposite of what he claims. For example, economists have known for centuries that changes in the money supply affect the price level in the (unspecified) long run, and that in the (again, unspecified) short run they can cause changes in real economic activity. This is a prediction, but not a very precise one; it is a pattern prediction. For the past sixty years, one of the most studied of all empirical relationships has been that between changes in the money supply and a range of economic variables. Has this allowed us to move beyond the standard pattern prediction? Unless you take the statement that "changes in the money supply can affect real economic activity in the short run with lags that are variable in length and intensity" as a point prediction, the answer is no. Similarly, Ebenstein says "we all know" now that command economies are less efficient than private property capitalism. I agree that is a common sentiment, and it is one I share. But what empirical studies brought us to the conclusion? "We all know" this in precisely the same way that "everyone knew" in the 1930s that capitalism was doomed; these conclusions are based on broad interpretations of the state of the world. They are not the results of careful empirical studies undertaken by economists over the years. Indeed, for much of the twentieth century, many economists thought the contest between free-market economies and command economies on efficiency grounds was a draw. When you study complex phenomena, "the facts" do not speak unambiguously.

I will not continue to rebut Ebenstein's specific claims here. A far more formidable opponent would be Milton Friedman himself, and part of my final chapter is an attempt to argue against his views on the role of empirical work in economics. As Friedman himself has admitted, the results have not matched economists' aspirations.<sup>3</sup> I urge the reader to have a look at my last chapter and decide who has better arguments. Part of the chapter is also a lament for the passing of the history of economic thought from graduate curricula in economics. This is happening at school after school, and I consider it a final and malefic legacy of positivism. As it occurs, the history of my discipline will increasingly come to be written by those who are trained neither as economists nor as historians. I am saddened by this development, and perhaps the readers of this review will now better understand why.

#### Reply by Alan Ebenstein to Professor Caldwell's Response

In his comment on my review of *Hayek's Challenge: An Intellectual Biography*, Professor Bruce Caldwell urges the reader to look at its last chapter,<sup>1</sup> so it is to this chapter that I turn in this brief response. Professor Caldwell poses, in my mind, a false dichotomy: either one is an econometric positivist or a Hayekian pattern predictionist. This reviewer is neither. While I think Professor Caldwell offers some good thoughts with respect to the difficulty of prediction in economics, they are incomplete.

Prediction is, in its nature, indefinite in much of the realm of experience. Particularly once the area of prediction passes to the actions of men and women, precise prediction apparently becomes increasingly difficult. This does not mean that only prediction of a pattern or explanation of a principle is possible. Very often, prediction takes the form that there is a 10 percent chance one phenomenon will occur, a 20 percent chance that another will, and so on. The concept of pattern prediction, as presented by Professor Caldwell, neglects this crucial indeterminate aspect of prediction and instead focuses on the necessary generality of prediction. The distinction here is something akin to that between risk and uncertainty in Frank Knight's work.

I should not, then, argue that because the conception of prediction presented by Professor Caldwell is incomplete, the view this reviewer represents is therefore some form of econometrics, for it is not. Indeed, in writing the original review, I was struck by the similarity between some of Professor Caldwell's ultimate philosophical positions and those of the econometricians he criticizes. Like them, he believes that "measurement without theory" is not possible or is without value. This is not my view. I am much more of the opinion that facts, even without theory, are of the greatest worth.

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<sup>3</sup> See CALDWELL, *supra* note 1, at 379-82.

<sup>1</sup> Bruce Caldwell, *Reply to Alan Ebenstein*, 1 NYU J. LAW & LIBERTY \_\_, \_\_ (2004).

I do not agree with Professor Caldwell's assertion in the final chapter of Hayek's *Challenge* that Professor Milton Friedman's 1953 essay "The Methodology of Positive Economics" is, "in many ways, the perfect expression of the optimism concerning the prospects for empirical work that reigned during the positivist era."<sup>2</sup> Indeed, while recently researching the origins of Professor Friedman's methodological views, I was struck by the circumstance that these developed largely during the time he was in most strident contact with the views of econometricians at the University of Chicago associated with the Cowles Commission there, from which views he strongly dissents.

Professor Caldwell's basic message is that there are limits to prediction. I agree with this position, but do not believe it to be the essential one. Rather, as Hayek wrote, endorsing a "hypothetico-deductive" approach in science, including in the social realm: "Its basic conception lends itself to a[n] ... interpretation according to which the essence of all scientific procedure consists in the discovery of new statements ... from which testable predictions can be derived."<sup>3</sup>

In addition, Hayek importantly remarked that "we shall clearly not be able to control developments further than we can predict the results of our action. A limitation of prediction thus implies a limitation of control."<sup>4</sup> The significance of prediction is that prediction precedes control. The important question is not "the limits of social science," but its extent.

It is true that Hayek also wrote that in the social field "individual events regularly depend on so many concrete circumstances that we shall never in fact be in a position to ascertain them all; and that in consequence . . . the ideal of prediction and control must largely remain beyond our reach."<sup>5</sup> But to the extent that even in the social realm prediction and control are in our reach, we should grasp them.

This reviewer is considerably more optimistic than Professor Caldwell with respect to the future development of economics as a field of human inquiry and knowledge. Much of knowledge is, in a sense, the capacity to predict. As our capacity to predict continues to increase, so should interpersonal agreement and social harmony. The future of economic theory should be bright. The history of economic thought should flourish in the years and decades ahead. Predictive positivism is, to use the title of one of Professor Caldwell's works, beyond positivism.

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<sup>2</sup> BRUCE CALDWELL, *HAYEK'S CHALLENGE: AN INTELLECTUAL BIOGRAPHY OF FRIEDRICH HAYEK* 379 (2004).

<sup>3</sup> F.A. Hayek, *Degrees of Explanation*, in *STUDIES IN PHILOSOPHY, POLITICS, AND ECONOMICS* 3, 4 (1967)

<sup>4</sup> *Id.* at 18 n.16.

<sup>5</sup> F.A. Hayek, *The Theory of Complex Phenomena*, in *STUDIES IN PHILOSOPHY, POLITICS, AND ECONOMICS* 22, 34 (1967).

## F.A. HAYEK, THE ROAD TO SERFDOM IN CARTOONS

When Hayek began writing *The Road to Serfdom* in September 1940, it was an unlikely book. The West was embroiled in war and just emerging from the Great Depression. Conventional wisdom held that free markets were the malady, and central planning the cure. Capitalism was nearly a dirty word. The prospects of a book critical of planned economies were dim.

Routledge published the first edition of *The Road to Serfdom* in March 1944. Among intellectuals its thesis approached heresy. Apart from a few outliers, like the iconoclast George Orwell, the academy roundly—even hysterically—rejected Hayek’s arguments. Ironically, among Hayek’s few champions was his friend John Maynard Keynes, the doyen of planning himself.

But as much as Hayek’s peers hated the book, the public loved it. After an avalanche of positive reviews in the popular press, *The Road to Serfdom* was wildly successful. Demand was so great that “it soon became known as the book nobody could get.”<sup>1</sup> Despite the war and paper rationing, by June 1945 *The Road to Serfdom* was already in its fifth printing. It caught on with British politicians as well. Churchill was the first of many to draw on *The Road to Serfdom*, citing it in his unsuccessful 1945 campaign. Hayek was also a key influence on Margaret Thatcher, who famously slammed a copy of *The Constitution of Liberty* onto a table and announced, “This is what we believe in!”<sup>2</sup>

If *The Road to Serfdom* was a success in Britain, in the United States it was a phenomenon. The University of Chicago Press published the first American edition in early 1945, but Hayek’s sudden rise to fame in America owed more to the rapid embrace of his ideas in the popular press. A condensed version of *The Road to Serfdom* opened the April 1945 edition of *Reader’s Digest*.<sup>3</sup> Despite his initial skepticism of the wisdom of the project, Hayek was ultimately “astonished and pleased” with the quality of the condensation.<sup>4</sup> The issue of *Reader’s Digest* appeared while Hayek was en route to New York. When his ship arrived, he discovered 3,000 eager admirers and a battery of microphones awaiting him. His talk in New York began a nationwide speaking tour and his transformation into a public figure. *The Road to Serfdom* was even a Book of the Month Club selection.

Thirty years later, in 1974, Hayek won the Nobel Prize in Economics, but he remains best known for *The Road to Serfdom*, the book that forever discredited central planning and catalyzed the revival of free-market ideas. The following cartoon version first appeared in the February 6, 1945, issue of *Look* magazine. Later, General Motors reprinted the cartoon as a booklet in the *Thought Starter* series.<sup>5</sup>

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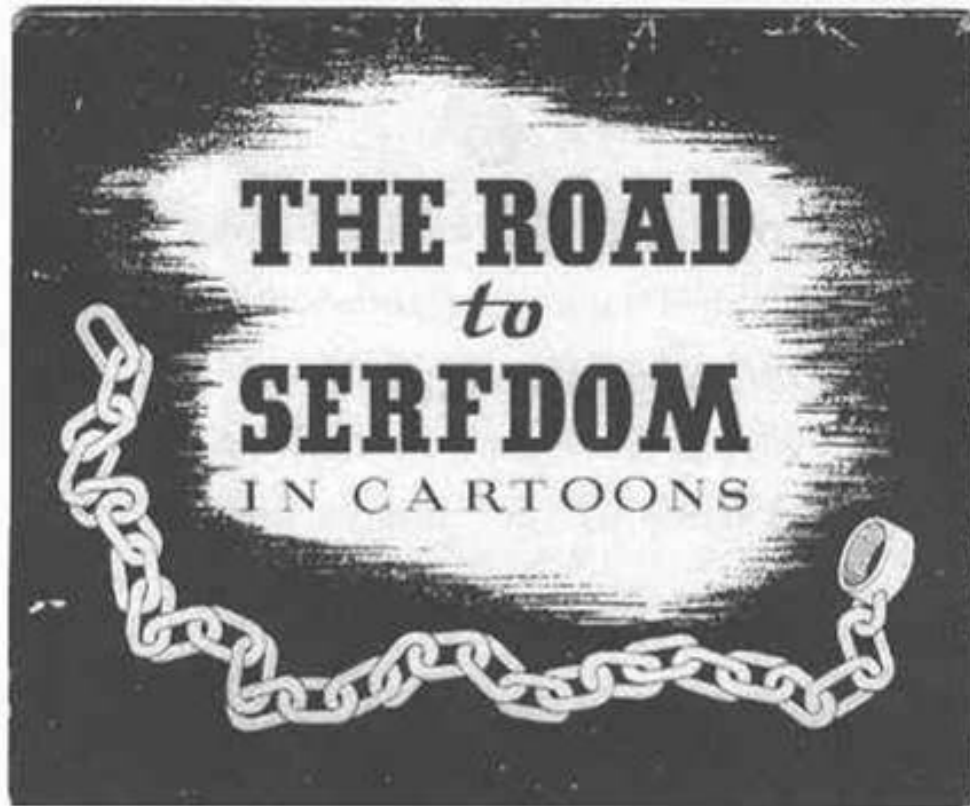
<sup>1</sup> John Blundell, *The Road Less Traveled*, WALL ST. J. ONLINE, Mar. 10, 2004 (subscription required).

<sup>2</sup> *Id.*

<sup>3</sup> See THE READER’S DIGEST CONDENSED VERSION OF THE ROAD TO SERFDOM (Inst. for Econ. Affairs 2001) (1945).

<sup>4</sup> See Blundell, *supra* note 1.

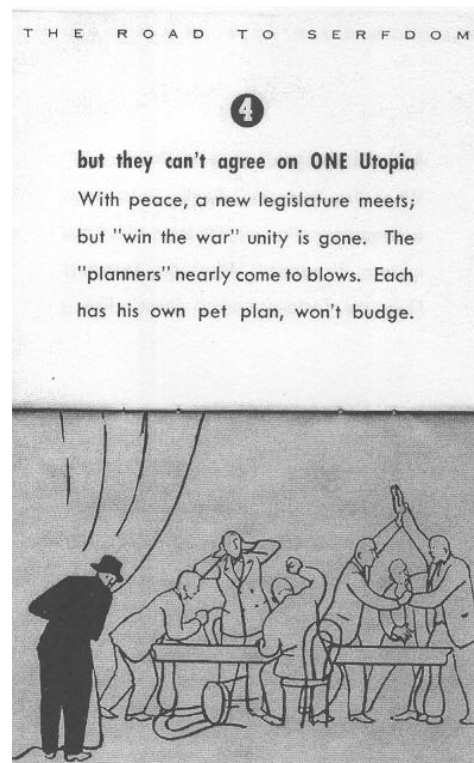
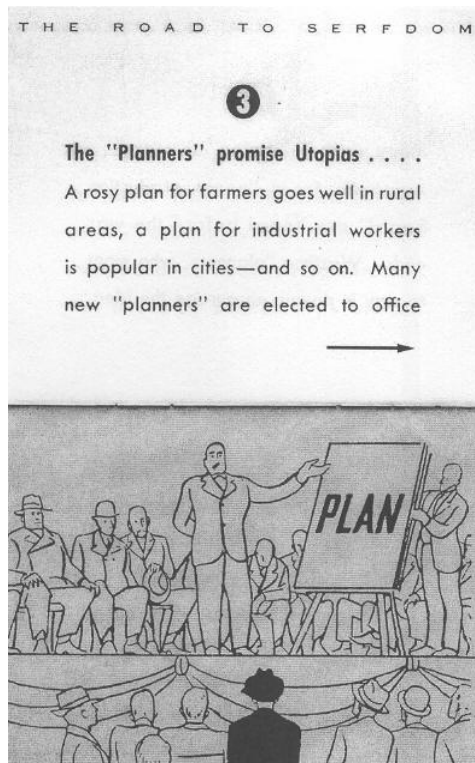
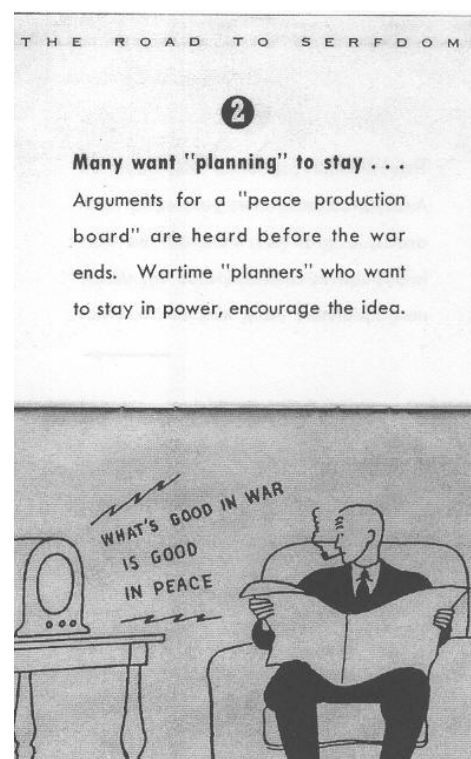
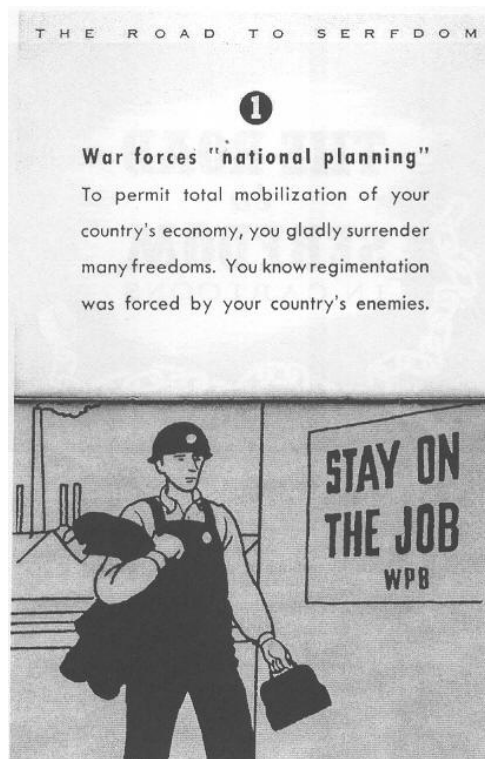
<sup>5</sup> *The Road to Serfdom in Cartoons*, THOUGHT STARTER No. 118.



Originally published in *Look* magazine

Reproduced from a booklet published by  
General Motors, Detroit  
in the 'Thought Starter' series (no. 118)





## THE ROAD TO SERFDOM

5

**And citizens can't agree either . . .**

When the "planners" finally patch up a temporary plan months later, citizens in turn disagree. What the farmer likes, the factory worker doesn't like.



## THE ROAD TO SERFDOM

6

**"Planners" hate to force agreement . . .**

Most "national planners" are well-meaning idealists, balk at any use of force. They hope for some miracle of public agreement as to their patchwork plan.



## THE ROAD TO SERFDOM

7

**They try to "sell" the plan to all . . .**

In an unsuccessful effort to educate people to uniform views, "planners" establish a giant propaganda machine—which coming dictator will find handy.

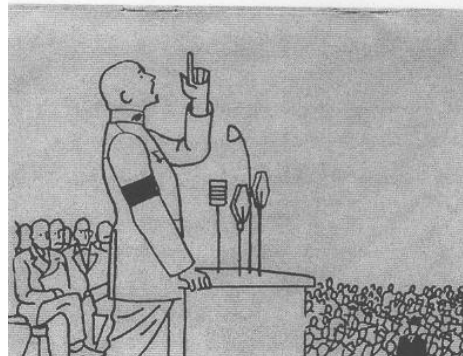


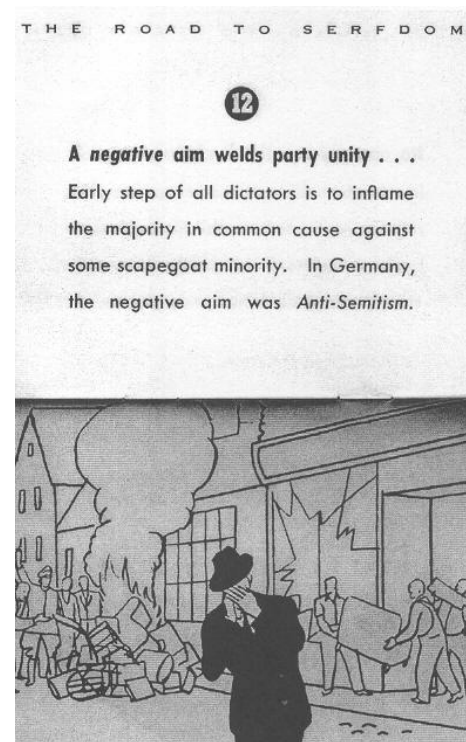
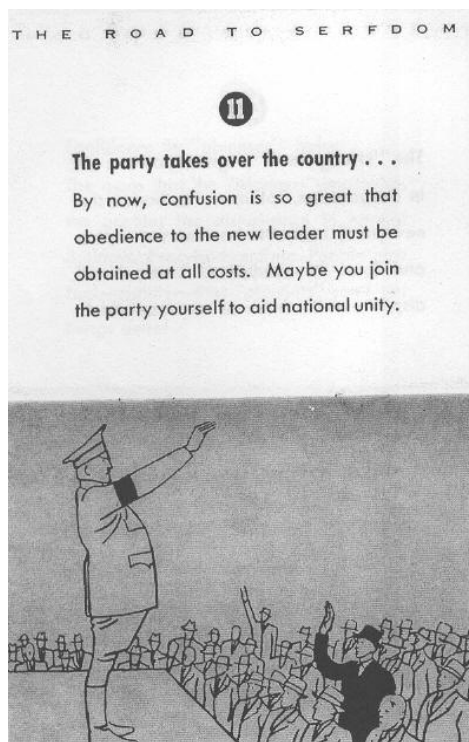
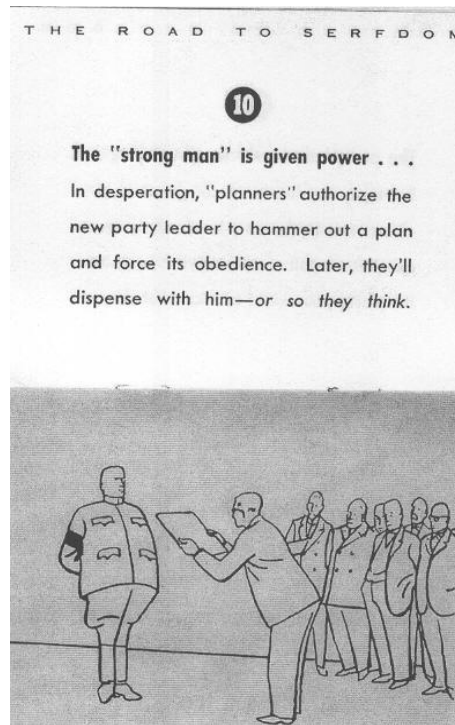
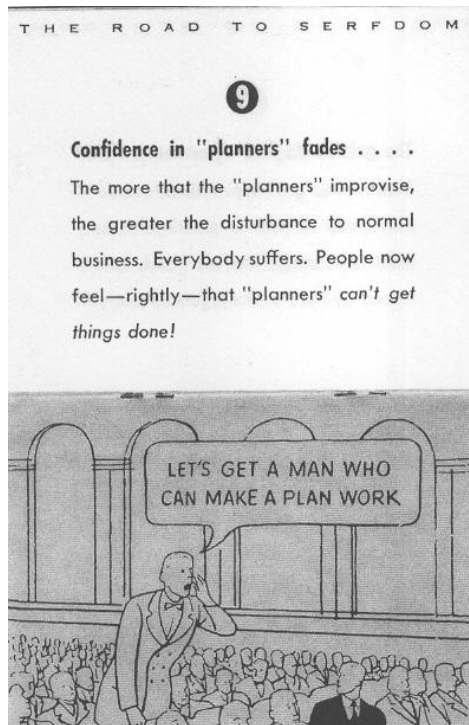
## THE ROAD TO SERFDOM

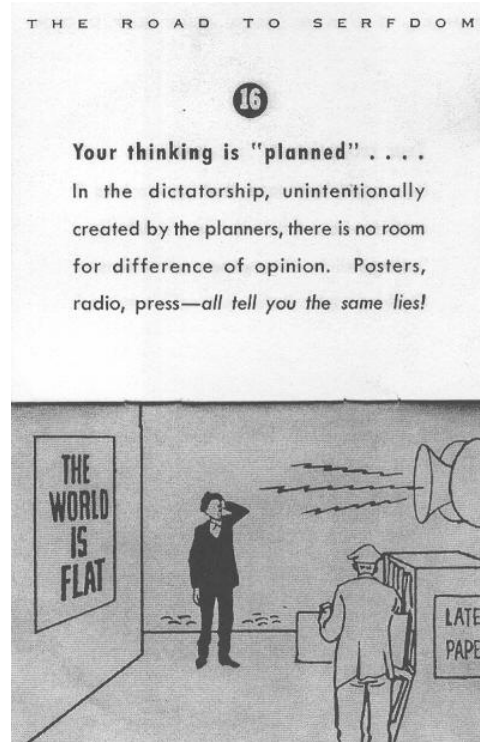
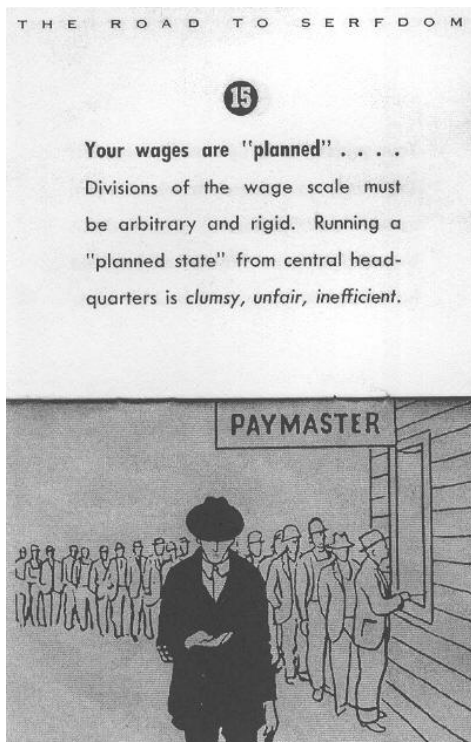
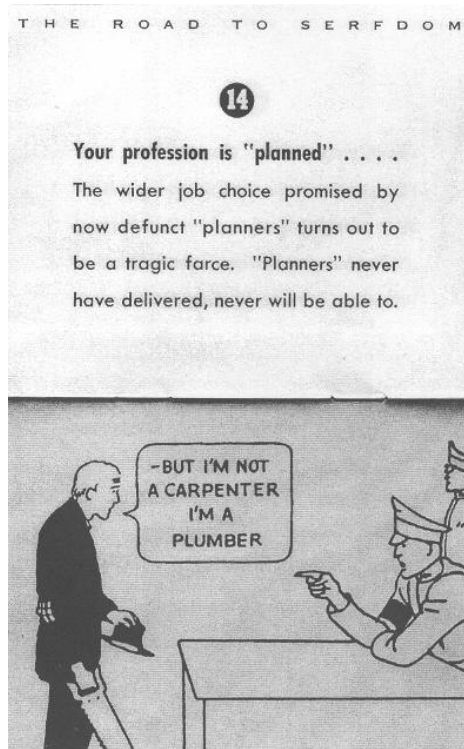
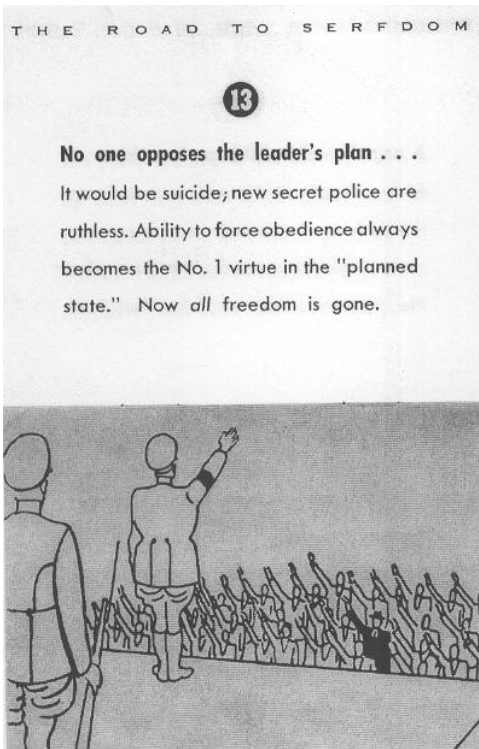
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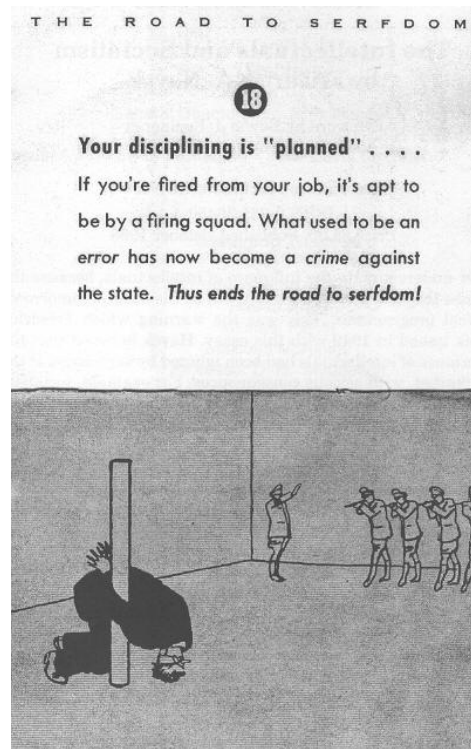
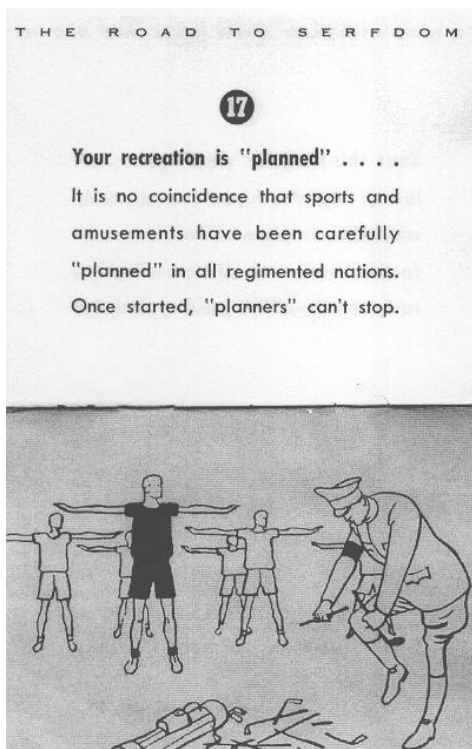
**The gullible do find agreement . . .**

Meanwhile, growing national confusion leads to protest meetings. The least educated—thrilled and convinced by fiery oratory, form a party.









### Book Summaries

JAMES SUROWIECKI, *THE WISDOM OF CROWDS* (2004)

*The Wisdom of Crowds* synthesizes and presents for lay readers a body of research that author James Surowiecki cogently summarizes as showing that “under the right circumstances, groups are remarkably intelligent, and are often smarter than the smartest person in them.”<sup>1</sup> It is a Hayekian lesson.

The book is filled with numerous examples of groups both big and small solving, in a decentralized and often spontaneous way, a wide variety of problems that would befuddle an individual expert. The most common type of example illustrates how the aggregation of individual predictions can produce a collective estimate far more accurate than any individual, however expert and intelligent, could consistently achieve. The array of examples is indeed entertaining. Some are as simple and familiar as guessing the number of jelly beans in a jar; others are as seemingly complex as the operation of futures markets or the search for pollen by hives of honeybees.

Of course, the collection of anecdotes only serves to illustrate the theoretical explanation that forms the core of the book. Surowiecki splits the problems facing groups into three categories—cognition problems, coordination problems, and cooperation problems—devoting a chapter to describe each. Cognition problems involve situations where an answer is sought to a question, such as how many jelly beans are in a jar, or who is going to win the next Super Bowl. Coordination problems arise where interactions of individuals might create chaos rather than order if not guided by rules (however created). Cooperation problems, also known as collective-action problems, arise where cooperation is at odds with individuals’ self-interest. The author’s task is to relate under what conditions groups are able and likely to solve these varieties of problems without requiring centralized orchestration, i.e. under what conditions crowds can be wise.

Those conditions are diversity of opinion, independence, decentralization, and “some method for [aggregating] private judgments into a collective decision.”<sup>2</sup> Diversity of opinion means variation in people’s life experience, knowledge, cognition, psychology, expertise, etc., which produces more possible solutions to any given problem and more perspectives on elements of a solution set. Independence of the individuals making up a group enables that group to avoid the potential herding effects of “information cascades.” When individuals in a group make decisions sequentially, their scope of knowledge likely includes what others have previously done. This can add to the wisdom of each successive decision if independent judgment is still brought to bear on the question at hand. But if individuals rely entirely on others’ choices in deciding what to do, they are adding little to the collective wisdom; as more individuals act this way, the crowd behaves like a herd—it stops being wise. Decentralization and an aggregative method go hand-in-hand.

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<sup>1</sup> *Id.* at xiii.

<sup>2</sup> *Id.* at 10

Decentralization of decision-making encourages independence and specialization, which increase the potential for collective wisdom, but decentralization is often only useful if there is a method for aggregating dispersed individual assessments into a collective judgment. Hayek's realization that the market price mechanism serves this role is so well-accepted today that Surowiecki mentions it in passing as "obvious[]." <sup>3</sup>

After describing the theory, Surowiecki presents a number of case studies exemplifying the wisdom of crowds or the lack thereof. The last chapter of the book touches on the theory's relevance to democratic government. A law- or politics-minded reader will likely be disappointed by the brevity of this last chapter, but, as noted above, the book is written for lay readers, and the endnotes provide valuable pointers to the literature for those who want to delve further into any of the areas of research covered by the book.

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<sup>3</sup> *Id.* at 74.