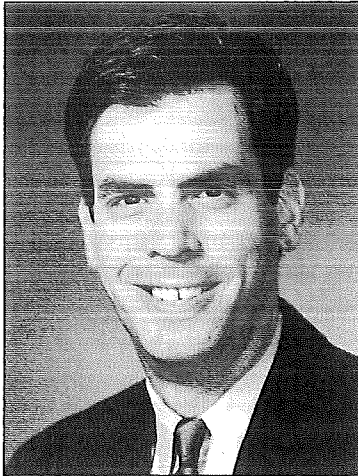


Austrian

Economics Newsletter



Peter G. Klein: Sticking with Economics

Peter G. Klein, an emerging star in the economics profession, is doing pioneering work squarely within the Austrian tradition. A graduate of the University of North Carolina, Chapel Hill (BA), and the University of California, Berkeley (PhD), Klein is assistant professor of economics at the University of Georgia. He is also an adjunct scholar and a former Mises and Hazlitt fellow of the Ludwig von Mises Institute, and a faculty member of the Institute's summer instructional program, the Mises University.

Klein is editor of F.A. Hayek's *The Fortunes of Liberalism*, volume four in Hayek's collected works from the University of Chicago, an associate editor of the entire series, and author of "Mergers and the Market for Corporate Control" in *The Elgar Companion to Austrian Economics*. Professor Klein conducted the Austrian Economics Workshop at Auburn University in Summer 1995, and was interviewed by the editors of the *AEN* following a session on the classical economists.

AEN: How were you introduced to the Austrian School?

KLEIN: I first heard of it in high school. A friend gave me Mises's *Anticapitalistic Mentality*. I had no well-formed economic views, but I was instinctively

pro-market. This book impressed me. By the time I went to college, I was fairly widely read in the Austrian and free-market literature.

This was long before I had my first economics class, which was Samuelson-style Keynesian macroeconomics. What a shock. I immediately saw the problems, but I learned the material diligently and repeated it at test time. Economics appealed to me as the most analytical of the social sciences, but without the Austrian literature, I don't think I would have majored in it. It was the Austrian School that made me an economist and kept me intensely interested in the subject all through school.

AEN: For you, then, the problem of being an Austrian in the midst of non-Austrian mainstreamers began very early.

KLEIN: It's a constant problem for any Austrian-oriented graduate student or even professor. You have to maintain enough interest in mainstream thought to succeed in your program, even while your real passion is with this radical alternative. It's something every graduate student who reads this literature faces.

For me, the Austrian School has been a professional plus. It maintained my interest in economics in the midst of the tedious mathematical modeling that passes for economics today. It provided me an organizing framework into which fit the pieces of the rest of mainstream thought. When I read the textbook version of theories, the Austrian School made me interested in where a theory came from, where it is headed, and where it goes wrong.

Also, the Austrian literature helped me raise interesting questions. As an undergraduate, my courses were mostly straight textbook stuff, with multiple-choice exams and diagrams to draw. But I had a few classes where I had to write papers and essays. Now, the typical economics undergraduate has a hard time writing a paper in economics. That's where I had an advantage.

The general dumbing-down of undergraduate education has forced this textbook-style learning. But, ironically, this provides an opening for Austrians. The textbooks don't raise enough issues to get students who enjoy economics to really think. Good students demand more, and they begin to look elsewhere.

AEN: Given professional hostility, is it possible for interest in the Austrian School to throw a person off the track of being a successful student?

KLEIN: Not the School as such, but some students who read this literature tend to forget their common sense. You don't badger the professor or point out his errors. You have to be careful and judicious.

But unlike other students, one who studies the Austrian School can step back and say: "doesn't that model assume such and such, and isn't this in dispute?" It turns out, that at the highest levels, many of these assumptions are controversial. But unless you had asked the question, the professor would never have mentioned it. When a professor finds an undergraduate student who is actually interested in such questions, he usually appreciates it. Such discussions can stimulate his own research.

AEN: Did you train under any Austrians in school?

KLEIN: No. My training came from my own reading and outside conferences and seminars offered by groups like the Mises Institute. But I had one professor who was himself a heterodox economist, interested in a wide variety of areas, and he encouraged me to pursue my own interests outside the mainstream. He was more accepting of ideas that question the dominant orthodoxy, even if he wouldn't have questioned them in the same way.

AEN: How does an Austrian avoid developing a disdain for the mainstream too early, such that it causes classes to be drudgery?

KLEIN: The key is that if you are going to be an economist, you have to have an interest in the economy. I never developed that disdain because most standard economists, whatever their faults, are analyzing things that go on in the world. Even if the answers are wrong, the subject matter still provides an opportunity for learning.

In recent years, some Austrian graduate students have become interested not in economics but in "metaeconomics": deconstructionism, hermeneutics,

learning, or whatever. They seem to see Austrian economics as a way of avoiding what they are supposed to be doing: economics.

If you are going to be an economist, you have to be interested in the way the world works. By the way, this is not only a criticism of the so-called radical subjectivists. It also applies to the extremist mathematical modelers. Many of them are not interested in the economy either.

Austrians must realize that if they ever stand a chance of influencing the profession, they need to provide a persuasive rationale for how the world works. That requires being more interested in General Motors than in Wittgenstein.

AEN: This problem might be related to the type of Austrian economics people are introduced to early on.

KLEIN: Exactly. Reading and being persuaded by Roger Garrison's capital theory is not going to derail anyone. And look at Mises. He was primarily an economist, and an applied one at that. In his day job he was chief economic adviser to the Austrian government. He was dealing with practical, applied economic problems every day. If it weren't for that, he would have been much less likely to come up with the more general, theoretical insights.

Of course, Mises was also learned in philosophy, law, and history. But he was able to bring these disciplines to bear on economics because he actually did economics. Every time I see an Austrian article called "A Subjectivist Perspective on X," I'm much less inclined to read it. The person is not often trying to explain the phenomenon in question. He is explaining how he would go about explaining it if he ever actually did attempt to explain it.

AEN: Isn't there also a mistake in hiding one's Austrian views?

KLEIN: Yes. You have to say where you are coming from, explain whose work you are building on, and what tradition of thought your topic comes out of. There's room enough in an increasingly eclectic profession for the Austrians.

But that doesn't mean that all Austrian writings have to be internal to the school. Mises's and Hayek's discussions of the Austrian School *per se* appear when they are doing history of thought, explaining where a particular economist fits in, or commenting on their predecessors. Remember that it wasn't the Austrians who came up with the term "Austrian." It was the German historicists who used the term to denigrate those guys over in Vienna, referring mainly to the Böhm-Bawerk/Wieser generation.

AEN: Then you see two strategic errors: extreme segregationism and extreme integrationism.

KLEIN: There are disadvantages to both. Rothbard often talked about this with younger scholars. You can't play games in which you pretend to believe things you don't. An Austrian might begin by promising that he

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A u s t r i a n
Economics Newsletter

Volume 15, Number 2

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The Austrian Economics Newsletter is published periodically by the
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(334) 844-2500; fax (334) 844-2583; internet:
lvmises@mail.auburn.edu.; <http://www.auburn.edu/~lvmises>
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will do mainstream work only until his dissertation is approved. But then he has to get a job, so he pretends again. But then he has to get tenure, so he pretends again. By the time all the constraints are gone, he's stopped pretending. He's forgotten what it was about the Austrian School that interested him in the first place.

It's just not feasible to shift gears in the middle of your career. Research is hard to do. Writing a really good article—whether Austrian or mainstream—requires tremendous work and thought. You can't casually jump around. We're in this business because we're seeking to understand real-world economic phenomena. Part of our goal as Austrians should be to show that we can do this better than others because we have better tools.

AEN: One of many intriguing aspects of your path is your choice of graduate schools. Berkeley isn't exactly known as an Austrian hotbed.

KLEIN: It was a difficult decision. But I concluded that I wanted maximum exposure to mainstream literature, and hoped I could keep up with the Austrian literature at the same time. Berkeley is a large and anonymous department. We had 150 PhD students in economics and a large faculty. Most of the professors don't take an interest in you. The advantage is that no one is putting pressure on you to do research of a particular type; you're left on your own. On the other hand, I missed the fellowship of people who shared my interests.

It's not for everyone, but it worked for me. Also, it might not have been possible apart from the Mises Institute's summer instructional program, which allowed me to catch up on what I was missing in the Austrian literature. It also provided camaraderie. Thanks to the Institute, it's possible for Austrian students at non-Austrian programs to keep up.

I applied for a fellowship from the Institute in my first days in the PhD program at Berkeley. That connection allowed me to meet Austrian professors, get research help, and make the contacts in the profession with people who share my interests. I worked for the *Austrian Economics Newsletter*, wrote reviews, and helped with indexing and editing. Otherwise I would have had a lonely student life.

AEN: Tell us about your dissertation.

KLEIN: It is an applied study of corporate diversification. I looked at the conglomerate merger boom of the 1960s, trying to understand its nature, causes, and

consequences. It's important because many people see the takeover wave of the 1980s as a direct outgrowth of it.

During the 1960s, when the government enforced tight antitrust restrictions on horizontal mergers, firms could not expand much in their own industries. As a result, they expanded by acquiring firms in totally unrelated industries. There were single firms with interests in aerospace, meat packing, insurance, and rental cars, for example.

In the 1980s, many of these acquisitions were liquidated by takeover specialists. Economists tend to view the 1960s mergers as a mistake, as empire building, as market failure on a grand scale. This is the argument of David Ravenscraft and F.M. Scherer in their book *Mergers, Sell-Offs, and Economic Efficiency* (1987). They say the conglomerate boom was market failure, and they conclude that government authorities should have more scope to regulate mergers and corporate activity in general.

The problem is that this story is based on ex post evidence. Just because 1980s bust-ups were value creating doesn't necessarily mean that the previous mergers were value destroying. It turns out that there was considerable rationale for firms being highly diversified during that time. Changes in external circumstances—like changes in antitrust enforcement and takeover laws—explain the trend away from diversification much better.

AEN: Is the conventional view of conglomerates that capitalists made errors or that there was a welfare problem?

KLEIN: Both. But in my view, the reorganization of industry in the eighties shows that the market is capable of adjusting to changes in circumstances, not that it failed. The fact that, *ex post*, an acquisition turns out not to be profitable does not mean that the acquisition was a mistake at the time.

The people who see market failure have an agency explanation for mergers. Then they switch to an efficiency explanation for divestitures. If managers run the firms, and they make bad acquisitions, then why do firms ever break up?

AEN: How do you explain the breakups?

KLEIN: Particularly important was the deregulation of financial markets. In the 1960s, financial markets were tightly controlled. Brokerage houses were highly regulated and their commissions were fixed.

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There wasn't much competition among financial-service providers on Wall Street.

Financial deregulation made capital markets more efficient. Much of the resource allocation that was done inside the large, diversified firm could now be done externally, with greater efficiency. The obvious Austrian tie-in is with the calculation debate, and, in particular, Rothbard's application of it to the theory of the firm.

AEN: You are attempting to contribute to a general theory of the firm as well?

KLEIN: It's a natural area for Austrians. A function of any corporate office is to act as a private central planning board, directing cash flow in and around the enterprise, taking resources out of low-growth divisions and shifting them into high-growth divisions.

Now, why should any of this take place in a firm and not in markets? Why shouldn't each division be a separate firm? For that matter, why does the market not consist entirely of a network of independent contractors? Why do we see firms at all, and why do we see large firms that do a lot of resource allocation internally?

The evidence suggests that there can be advantages to resource allocation within organizations, even within conglomerate corporations. There are reasons why entrepreneurs can sometimes organize transactions inside a firm better than two independent entities could by contracting between them.

There's a group of economists who believe they have taken market-process theory and applied it to the theory of organizations, claiming that any sort of planning within an organization is somehow mysterious. This is not the case at all. So long as organizations exist within a competitive market, there are many reasons why activity can be directed by private planners, so to speak, within the organization itself.

There is nothing anti-market about hierarchies or structures of authority, whether in families, clubs, churches, or firms. The ability to plan and manage is a skill valued by the market. When internal allocation becomes inefficient, such organizations or firms will suffer and the market will work to correct the error.

AEN: In your theory, are you drawing more on the Misesian calculation argument or Hayekian knowledge dispersion?

KLEIN: The calculation angle is more fruitful. There's a myth that real-world socialist planners

attempted to create prices based on costs of production. In fact, the Soviets simply opened up the *Wall Street Journal* and checked world market prices. While pure socialism would be impossible, Soviet socialism was possible because it existed within a world market system.

It's the same way with the firm. Look at transfer pricing, a subfield within the theory of the firm. Imagine a large firm with different divisions that are semi-autonomous. If one division buys an input from another division within the firm, how is this commodity to be "priced"? It's not really a market transaction, of course; it's an accounting function. But how does the firm know whether it should be producing that thing in-house or outside? Which division, if either, is earning a profit?

The transfer pricing problem, is the key to understanding the limits of the firm. Rothbard makes this point in *Man, Economy, and State*. The firm cannot become so large that there are no longer market prices outside the firm for intermediate goods transferred within the firm. A firm can never be the sole buyer and the sole seller of a single intermediate component. It would have no way to calculate the correct price of that component and no way to make decisions about its use.

AEN: Under that scenario, why can't the firm simply calculate the marginal cost of the inputs instead of relying on outside prices?

KLEIN: The first theoretical article on transfer pricing, by Jack Hirshleifer in 1956, suggested precisely that. He said the problem is simple: set the price equal to marginal cost. Well, this doesn't help any more than does Lange's "solution" to the problem of socialist calculation. Lange said: instruct the plant-level managers to price at marginal cost and we'll have efficiency. But how do these managers know their true marginal cost? Without knowing the real opportunity costs, this is merely an accounting trick that leaves the essential economic problem unaddressed.

AEN: So this Rothbardian approach explains the upward limits of the firm, but what explains the downward limits?

KLEIN: Rothbard himself cites the 1937 article by Ronald Coase on the nature of the firm. In Coase's early work—which I find much more interesting than his later articles—he showed that using the market is not costless. There are costs to discovering products,

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haggling, writing contracts, and specifying enforcement mechanisms, among other costs of using the market mechanism.

Coase says that firms provide a way for entrepreneurs to overcome these costs. That insight has been generalized into a transaction cost framework by which we look at the make-or-buy decision. You have to weigh the costs of using the market (buying) against internal bureaucratic costs (for making), and the tradeoff determines the firm's boundaries.

AEN: Is the upper limit determined by the transaction costs, the calculation problem, or are they essentially the same problem?

KLEIN: In Coase's framework, as developed by Oliver Williamson, transaction costs can be any costs other than the actual production costs. You could include as internal transaction costs bureaucracy, politicized decision making, etc. But the ultimate internal transaction cost is the need for internal calculation.

Incidentally, some Austrians have been critical of the Coasian framework. They say it is too much of an equilibrium model. I don't fully share that view. What is lacking in the Coasian model is a full explanation of the limits to organization. Coase speculated that there must be limiting factors, but he didn't specify them. The Austrian calculation argument can be extended to show the serious implications of the transfer pricing problem, which can help us understand the limits to the size of the firm.

AEN: Do you foresee any troubles in weaving Coase, Mises, and Rothbard together in a single theory?

KLEIN: No, but I'm staying away from the law and economics literature. Williamson points out, with some pleasure, that in Coase's acceptance speech for the Nobel prize he talks about his work on the firm, but says almost nothing about the so-called "Coase theorem." People like Posner are always citing Coase as their intellectual forerunner, but Coase himself has distanced himself somewhat from the Chicago approach.

In the theory I'm working through, the Mises-Rothbard framework is indispensable in its emphasis on financial markets. The stock and bond markets, not the labor or product market, are the hallmark of a capitalist economy. The capital markets are the key, which is why a socialist economy cannot work. There are no markets for productive factors, i.e., no financial markets.

AEN: Some Austrians have emphasized the role of uncertainty in the theory of the firm.

KLEIN: A school of thought in management and economics, the "core-competence" school, emphasizes uncertainty, especially as in the work of Edith Penrose, G. B. Richardson, and David Teece, as well as in Nelson and Winter's book *An Evolutionary Theory of Economic Change*. They think uncertainty is the key to understanding the boundaries of the organization. I am more interested in the practical applications of Austrian price theory.

AEN: How did you come to be one of F.A. Hayek's editors?

KLEIN: Providence, I suppose. In graduate school, I was looking for a summer job, and through my contacts with the Mises Institute I hooked up with W.W. Bartley III at the Hoover Institution, who was at that time the general editor of *The Collected Works of F. A. Hayek*.

The first volume in the series, *The Fatal Conceit*, had been published in 1989. I worked as Bartley's research assistant on what became volume three in the series, *The Trend of Economic Thinking*. It was through that work that I got involved in volume four, *The Fortunes of Liberalism*. Bartley was a trained philosopher, a student

of Karl Popper. He wanted an economist to help him with the volume. We were going to co-edit the volume, but he died unexpectedly before we really began serious work on it. His successors at the *Collected Works* asked me to edit the volume by myself. Now I am an associate editor of the project itself and will be editing more volumes in the future.

AEN: How does the volume you edited contribute to our knowledge of the Austrian School?

KLEIN: It collects all of Hayek's writings on Austrian economists and the Austrian School. It includes an expanded version of Hayek's 1968 entry in the *International Encyclopedia of the Social Sciences*, which is a very good introduction to what we are all about. It also contains Hayek's long essay on Carl Menger, which has a wealth of information. There's also a memoir about the economics of the 1920s as seen from Vienna. In this, Hayek recalls his student days, when he took a trip to the U.S. and met the big economists of the time. I recommend this essay, especially his very funny comments on Veblen.

The second part of the volume contains Hayek's writings on the rediscovery of freedom in postwar

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Europe, with Hayek's opening address to the Mont Pelerin Society, along with other essays on people like Eucken and Röpke, and a strategy for economic reform. It discusses the relationship between Austrian economics and free-market policy conclusions, a subject which is being continually debated.

AEN: What's the biggest surprise to come out of the series?

KLEIN: I would say the volume called *Hayek on Hayek*. It's a wonderful volume that weaves together Hayek's unpublished autobiographical notes with transcripts from interviews. It's nicely done. It reveals that Hayek is more consistent on economics than he appears at first, but less systematic on philosophical matters. Also, Hayek drops a few bombshells in this volume—though I won't mention any names.

AEN: This summer [1995], you are conducting a seminar on Rothbard's *History of Economic Thought*. Rothbard attributes a great deal of significance to the role of philosophy and religion in the history of thought. Do you see this in Hayek?

KLEIN: The effect of religion and philosophy on Hayek is much debated and will continue to be. His views on society and evolution are bound up with that question. Austrians have also wondered whether his later views on society and knowledge mark a sharp divergence from his earlier work or the core framework for his technical economics. Is there a Hayek I and II or is Hayek just Hayek? Rothbard accepted the theory that there were two Hayeks.

I can't endorse the schism theory, but I do think Hayek is at his best as an economist. One reason there appears to be a schism on paper is that Hayek didn't write any major works on economics after the Second World War. Part of the reason might be that he couldn't get an economics position at any university.

His later work on the broader social issues is interesting and provocative, but in my view somewhat overrated. Some younger Hayekians have taken up these later themes because they are trendier and of greater interest to people in exotic disciplines outside economics. Maybe they think this is the way to introduce non-Austrians to the theory. But it seems to me that we are better off developing the core areas of economic theory. This goes back to the fact that some of these people are not really interested in economics.

AEN: What do you think Rothbard's *History of Economic Thought* does for his place in history?

KLEIN: Only somebody like Rothbard could have written *Man, Economy, and State*. Just read the footnotes and you'll know more about economics than is taught in any undergraduate degree program. The *History of Economic Thought* offers a similar experience. The bibliographic essays alone are remarkably helpful.

It's true that Rothbard was a genius. But there's more to it than that. The Austrian program provided

him with an approach to understanding intellectual history that yielded more fruit than the mainstream approach. He teaches us to see economics as groups of ideas advanced by individual writers that must be tested against logic and certain methodological standards.

Part of the Misesian approach to history says that there's not much you can do with facts unless you have a coherent theory. It's the same with history of thought. Before Rothbard began his research, he didn't know what he would find, but he knew what he was looking for. That's how he was able to dance through so much of recorded history and tell such a remarkable story of thinkers, personalities, and purposes. This makes for great reading and is much more insightful than the usual Whig approach in which ideas float from age to age, detached from person and circumstance.

When reading a history of thought, what are we interested in? Not only the details about the theories. We want to know the meaning of it all. Rothbard gives us that. If you only want to know what's in Ricardo, you should read Ricardo. But if you want to know about the development of ideas, read what someone like Rothbard says about Ricardo and his place in history.

AEN: But is an explicitly Austrian work like this going to make inroads in the profession?

KLEIN: It's an interesting strategic issue that Murray and I used to talk about. If the Kuhnian view of the history of science is correct—that paradigms shift through attrition and not refutation—then why should Austrians spend all their time trying to persuade colleagues that their own views are correct? We are better off developing our own paradigm and convincing young people of its merits.

My own view is that debating exercises with the mainstream are not very fruitful. In my experience, when you approach someone as an economist interested in a particular issue or problem, that person does not ask: Are you a neoclassical? A Post Keynesian? An Austrian? If you are interested in, say, mergers, then other people with an interest in mergers will want to hear what you have to say, presuming you have studied the topic.

There's a myth out there that the mainstream has fully worked-out views on the proper methodological foundations of the discipline. In fact, the average economist doesn't have the slightest interest in method. He has never thought about these issues at all. He is doing economics the way he is doing it because that's all he knows, and he thinks he is getting results.

Austrians need to learn to say to their colleagues: hey, if you really want to understand this particular empirical phenomenon or problem that you're interested in, you have to look at it this way. Look at the results you get. Our best hope is for Austrians to help mainstreamers

understand the economy better. If Austrians focus on metaeconomics, and try to force mainstreamers to re-think abstract issues of epistemology, we'll go nowhere.

AEN: Are you suggesting that Austrians drop all discussion of method?

KLEIN: Not at all. I'm suggesting that it be put in perspective. I wrote an undergraduate thesis on methodology, with a detailed analysis of some historical debates. I suppose I have my regrets about that now. When you're young, you think you can go out and show the entire profession the right way to do things. In fact, it's very rare for anyone at that stage of life to produce anything of value, much less redo the entire discipline. It's far better to simply start doing economics as the Austrians have understood it.

The point of a research article in economics should be to understand some real problem, and not simply to illustrate the use of a particular method. Again, the same criticism can be applied to some neoclassical economists. Mathematical models on their own rarely tell us something that can't be described in plain terms. A lot of game theory, for example, has consisted of endless variations of similar models with very few novel conclusions.

How can an economist tell if he is doing something worthwhile? I think he should ask himself, with regard to a piece of research: Could I provide a rough explanation of this to my mother in a few sentences? If he can't do that, it's probably not a good piece of research in economics.

AEN: But doesn't an Austrian tend to draw others into debates that are about methodology, and isn't that unavoidable?

KLEIN: To an extent, that's true. Sometimes economists will say to you: where are your regression coefficients? In that case, nothing much is going to come out of a dialogue. I don't suggest Austrians engage in long philosophical debates with people who think that Austrians have nothing to offer.

But the truth is that many economists won't ask such a question. Those who will are a much smaller percentage of the profession than we think. There are plenty of mainstream economists and fellow travelers of Austrians who do good research and generate good work. In the past, many Austrians have developed a defensive posture that is not necessary. Rothbard didn't have that problem.

You hear two contradictory complaints about Rothbard. A recent book on modern Austrian economics implies that he's too mainstream, too neoclassical. Others say his economics was so anti-mainstream that it didn't engage the profession enough. Both can't be true.

The truth is that Rothbard attempted to build bridges where he could. He wrote for mainstream journals when they were open, but when they began to shut out Austrian articles, he founded *The Review of Austrian Economics*. He intended the journal to foster internal growth and help make Austrian ideas mainstream on their own terms. His goal was to keep the school in play.

AEN: Does his *History of Thought* fit into your strategic vision of persuading others by example?

KLEIN: Yes it does. It has the potential to have the impact that Schumpeter's history had. The reviewers, who have

been mainly non-Austrians so far, have been impressed. I would hope this will provide some research topics, given that it is a large survey work. His interpretation of each major figure, especially his revisionist insights about the classical economists, could become a fixture in the literature.

But there's also a danger that Rothbard's work will get younger Austrians too excited about the possibilities of the history of thought. If you want to specialize in the history of thought, you have to be very careful. It comes with a tremendous cost: namely, your chances of getting a job are very small. It was Rothbard's last major professional project.

AEN: What type of research do you see yourself doing in five years?

KLEIN: It's hard to say, but whatever it is, I feel sure it will be about the economy. I enjoy Austrian work in comparative systems and how it relates to industrial organization. There are many applied topics that come out of that, including the economics of transition, from socialism and firm decision-making under interest-rate distortions and periods of the business cycle.

AEN: You and your wife Sandy met at a Mises Institute seminar. What's it like for two Austrian economists to be married?

KLEIN: We have great discussions and share notes. But if a husband and wife had nothing in common but economics, they would surely have a difficult time of it. ▲

REVIEWING A BOOK WITHOUT READING IT

by Gottfried Haberler[†]

The nineteenth-century German philosopher Arthur Schopenhauer once wrote in a preface to one of his books that nobody could understand or enjoy the book who was not also familiar with Kant, Hegel, Plato, and several others. Schopenhauer admitted the reader might feel cheated, having bought a book and then being told to buy six others to enjoy it. There was a consolation, however: The reader could enjoy the book without reading it. He could have it nicely bound and displayed, he could present it as a gift, and so on. Finally, if necessary, the reader could review it.

This miracle of reviewing a book without reading it, we should recall, was performed by no less than the great British economist and statesman John Maynard Keynes. In the *Economic Journal* of September 1914, Keynes wrote a three-page review (reprinted in Keynes's *Collected Writings*, vol. 11) of Ludwig von Mises's *Theorie des Geldes und der Umlaufsmittel* (translated in 1936 as *The Theory of Money and Credit*).

"Dr. von Mises's treatise is the work of an acute and cultivated mind," wrote Keynes of the German manuscript, "but it is critical rather than constructive, dialectical and not original. The author avoids all the usual pitfalls, but he avoids them by pointing them out and turning back, rather than by surmounting them. Dr. Mises strikes an outside reader as being the very highly educated pupil of a school, once of a great eminence, by now losing its vitality. There is no *lift* in his book, but on the other hand an easy or tired acquiescence in the veils which obscure the light, rather than rending away of them. One closes the book, therefore, with the feeling of disappointment that an author so intelligent, so candid, and so widely read, should, after all, help one so little to a clear and constructive understanding of the fundamentals of his book."

Sixteen years later, Keynes wrote in a footnote to his *Treatise on Money* (p. 199) that "I should have made more references to the works of [German and Austrian] writers if their books, which have only come into my hands as these pages are being passed through the press, had appeared when my own thought was at an earlier stage of

development, and my knowledge of the German language was not so poor (in German I can only clearly understand what I know already!—so that new ideas are apt to be veiled from me by the difficulties of language)."

Keynes evidently forgot he had reviewed Mises's book, but we may pardon him for his lapse of memory in view of the enormous volume of his writings.

Gottfried Haberler (1900–1995) is the author of *The Meaning of Index Numbers* (1927), *The Theory of International Trade* (1933), *Prosperity and Depression* (1937), and "Money and the Business Cycle" (1932), an important article on Austrian theory reprinted in *The Austrian Theory of the Trade Cycle and Other Essays* (Richard Ebeling, ed., The Mises Institute, 1983).

As a friend of Mises's, he was in Mises's wedding party in Geneva, Switzerland. In his early years, he was closely identified with the Austrian School, but less so at the height of his career. In 1957, he was appointed Calen L. Stone professor of international trade emeritus at Harvard University, and in 1963 he was made president of the American Economic Association.

He also served as president of the International Economic Association. After leaving Harvard he was a Resident Scholar at the American Enterprise Institute. He died May 6, 1995. This note was Dr. Haberler's last article, and he wrote it for the *Austrian Economics Newsletter*.

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