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HES PRESIDENTIAL ADDRESS HAYEK: RIGHT FOR THE WRONG REASONS?

BY

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The controversial question of whether the millennium began on January 1, 2000 or on January 1, 2001 weighed heavily on my mind as I composed my talk. The dilemma is, I should think, evident: Should a historian of thought want to give the last address of the old millennium, or the first address of the new one? I couldn't make up my mind about this, so I decided to consult my friends, particularly those whom I felt had given the question their considered attention. I asked David Colander, and he assured me that my preferences didn't matter, since he in fact had certainly given the final presidential address of the last millennium when he spoke in Greensboro the previous June. I then asked John Davis, and he assured me that my preferences didn't matter since he, in fact, was certainly going to give the first one of the new millennium, in 2001. Now being not nearly so stupid as I look, it occurred to me that if I listened to both of these individuals, there was a chance that I was going to give neither the last address of the old nor the first address of the new millennium. Feeling that this was, to put it mildly, an unacceptable outcome, I decided to disregard the ill-considered advice of my so-called friends and take firm control of the situation. Ladies and gentleman, I am very proud to be before you here today, delivering both the last presidential address of the old millennium and the first of the new one.

The title of my talk tonight emphasizes Hayek, but it will also be about methodology. The question that motivates my talk, while perhaps not millennial, is certainly centennial in scope. It is: Why did economics turn out the way that it did in the twentieth century? (If you sense an unvoiced criticism of economics in the question, your senses do not deceive you.) Hayek turns out to be an excellent vehicle for approaching this question, not only because his life spanned the century, but also because he *disagreed* with most of the developments within economics over that long period. For example, having first helped introduce general equilibrium economics to English-speaking economists, he ended up challenging its relevance for understanding the workings of a market system. He had no truck with the Keynesian framework or with econometrics. He railed

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against positivism, and that, during a period when everyone from socialist planners to Milton Friedman attached themselves to the label. He pursued interdisciplinary investigations when most other social scientists were becoming ever more specialized. This was a man who was more than out of step; he was openly hostile towards much of what the rest of the discipline was doing. So if one's problem is: Why did economics turn out so strangely in the twentieth century? looking at Hayek, who fought each development every step of the way, allows one to address the question, contrapuntally, as it were.

To explain my choice of title, I will invoke perhaps the most famous episode in the Austrian School's history, the socialist calculation debate. Peter Boettke (1998) has, and I think rightly, called the multi-pronged Austrian critique of socialism *the* major Austrian contribution to economics. For a long time, though, this decidedly did *not* reflect the view of the majority of the profession. Until very recently, most economists viewed the Austrian critique of socialism as simply wrong. With the fall of the Berlin Wall and the collapse of the eastern bloc, however, opinions suddenly changed. The failures of central planning became everywhere apparent, and both Hayek and Ludwig von Mises were praised for having seen the problem from the outset. The magnitude and the sources of praise have been at times astonishing. If everyone was shocked in the late 1960s to hear Richard Nixon claim that "we are all Keynesians now," certainly it was nothing compared to witnessing a columnist in *The New Yorker*, of all places, writing that the twentieth century would come to be known as "the Hayek century" (Cassidy 2000, p. 45). Amazing.

So, are we all Hayekians now? Well, I think not ... for if one reads carefully the assessments written by, say, Robert Heilbroner or Jànos Kornai, there is a decided back-handedness to their compliments. We find in their writings such statements as, for example, "the warnings of a Mises or a Hayek about market socialism" are best viewed as "brilliant guesses" rather than as "scientific propositions" (Kornai 1993, p. 63); or that "the successes of the far-sighted seem accounted for more by their prescient "visions" than by their superior analyses" (Heilbroner 1990, p. 1098). To put their point in terms of my title, these writers are saying that the Austrian critique of socialism was right, but for the wrong reasons; that the Austrians, responding from their gut, seem in retrospect to have gotten it right, but they didn't get their results *scientifically.*

Now there are two questions here. The first one, the one everyone focuses on, is whether the Austrians *were* right about the various forms of socialism. I will not spend any time on that question here today. Instead, I will focus on the minor clause, and I am prepared finally to state my thesis: whether Hayek was ultimately right or wrong about socialism, it is my contention that his *reasoning* was right. I disagree with those who say that Hayek must have just gotten lucky, or that his approach did not qualify as "Real Science." To put my thesis more positively, I believe that Hayek had a better handle on the nature of social phenomena than did the vast majority of his critics, that he understood the subject matter of economics better, and that as a result he was actually the better social analyst. In my remaining time, I will advance this thesis by arguing the following points.

First, Hayek developed his ideas about economics in his debates over socialism.

They are separable however, from those views. His principle claim is that economics is a science that studies complex phenomena.

Next, Hayek's claims have implications about the sorts of progress we might reasonably expect to see occur in economics. Though his claims are not directly testable, they may be indirectly tested by seeing what sorts of progress have in fact occurred in economics.

Third, I will argue that Hayek's claims do help us to explain why economics ended up looking as it did in the twentieth century.

Finally, Hayek's claims, together with a little help from his friend Karl Popper, also may help to explain why so little of this has entered into the consciousness of mainstream economists.

I'll begin with a quick sketch of the development of Hayek's views on the proper methods of economics. Though Hayek's earliest methodological writings, which consisted principally of a qualified defense of equilibrium theorizing, date from the late 1920s, the significant and transforming moment came in the mid-1930s. It seems to have first surfaced in his duly famous essay, "Economics and Knowledge" (Hayek 1937), about which Hayek later was to recall, "I wrote that lecture in a certain excitement. I was aware that I was putting down things which were fairly well known in a new form, and perhaps it was the most exciting moment in my career when I saw it in print" (Hayek 1983, p. 426). In retrospect, one might characterize what resulted as Hayek's response to the abduction by the socialists of equilibrium theory. That abduction, by the way, was a move that was fully as brilliant as Milton Friedman's abduction from the socialists of the rhetoric of positivism a generation later.

I call it an abduction because Hayek, along with Lionel Robbins, had, after all, helped introduce general equilibrium theory to economists in Britain. In the early 1930s socialist economists there began arguing that since a market system and a socialist system could both be formally represented by a general equilibrium system of equations, von Mises's claim that rational calculation under socialism was impossible had been refuted. In a famous article H.D. Dickinson had even proposed that just such a mathematical system be set up and solved. Hayek responded, quite sensibly, that any such attempt would run into enormous practical difficulties. This provided the opening for Oskar Lange's proposal that a trial and error process be used, under which socialist managers would simply move prices up and down whenever excess demand or supply appeared. Lange seemed to have offered a practical solution to the problem of having to solve a gigantic system of equations. For most observers, Lange's proposal meant that both Mises's claim about the logical impossibility and Hayek's about the practical impossibility of socialism had been refuted.

Now, as it turns out, Hayek had actually anticipated the trial and error approach in his 1935 book on *Collectivist Economic Planning*, and he also later offered a specific critique of Lange's proposal in a book review (Hayek 1940). Hayek believed that practical problems would plague trial and error schemes, as well. Under Lange's plan, for example, prices get fixed for a period and then get adjusted to equate demand and supply. Lange didn't seem to recognize that each price adjustment would cause the behavior of, in some instances, millions of market participants to change, which would make further adjustments necessary, which would cause further changes, and so on. How could Lange miss this? From Hayek's perspective, his opponent had been misled by his reliance on a static theory that emphasized given data and final equilibrium states.

In the real world, though, data aren't "given," they have to be discovered, or as Hayek was later to put it: "which goods are scarce goods, or which things are goods, and how scarce and valuable they are—these are precisely the things which competition has to discover" (Hayek 1978, p. 181). Furthermore, equilibrium theory suggests, falsely, that once all the adjustments are made, a final equilibrium is reached. But in the real world there is no final equilibrium, but rather a continuous and continual process of adjustment. Equally important, things that happen outside of equilibrium are what drive the whole process. When markets are out of equilibrium, errors are being made, so profit opportunities exist. Rivalrous entrepreneurs act to seize such opportunities, and that is what helps to eliminate errors and thereby to move markets "in the right direction."

Another crucial assumption made by Lange was that prices were simply "terms on which alternatives are offered" (Lange 1938, pp. 60–61). For the Austrians, money prices formed in markets are truly social phenomena. Such prices reflect the estimates of future market conditions made by thousands of market participants, some of whom are exploiting local or tacit knowledge. Such prices have information in them, information that would not be there in prices simply set by planners.

Now, whether or not you believe Hayek's portrayal of the market process, what is clear is that he looked at the world and believed that the models being used to describe its essential features were misleading. This is a key point. Hayek was not complaining about mathematics or abstraction or the use of theory in economics. It was the Austrians, remember, who had defended theory over and over again in their battle with the German Historical School economists, and Hayek was very much a part of that tradition. The point was, rather, that there is an underlying causal process taking place that general equilibrium theory obscured. If we put his point into the language of the scientific realist Uskali Mäki, Hayek was saying that, although all models abstract from the truth, static equilibrium theory abstracts from "significant truth" (Mäki 1998, p. 312). The evidence of this for Hayek was that the models could not distinguish the *essential* differences between a socialist system and one in which freely adjusting market-formed money prices are allowed to operate.

By the 1940s Hayek was searching for a way to generalize his insights. I think of his "Scientism and the Study of Society" essay as a first attempt to do so. In the essay, Hayek had a foot in two worlds. On the one hand, in trying to write explicitly about methodology, he returned to the original Austrian source on matters methodological, Carl Menger. The result was that parts of his essay read like a battle against Menger's old enemies, like the historicists. But there are clearly some new foes, too: behaviorists, and positivists like Otto Neurath. Here and there he also makes some passing references to the proper role of psychology. And, finally, he makes some methodological pronouncements—about how, often in the social sciences, the best that we can do is to make pattern predictions, or to offer explanations of the principles at work behind phenomena. There is a lot there in the "Scientism" essay, but it also seems to me that at times Hayek was groping to find the clinching argument.

Then suddenly in the summer of 1945, another transforming moment came. Hayek pulled out a manuscript on psychology that he had written in his student days and began reworking it. It was done by the late 1940s, though not published until 1952. He titled his contribution *The Sensory Order*, and it carried the subtitle *An Inquiry into the Foundations of Theoretical Psychology*.

I have argued elsewhere (Caldwell 2000) that working on *The Sensory Order* and then generalizing his finding to other areas is what started Hayek moving towards a more evolutionary approach to social phenomena. He came to the conclusion that many natural and social phenomena were examples of self-organizing complex structures and, indeed, he spent a lot of time simply pointing out the ubiquity of such structures in a variety of fields. This led Hayek to develop what he came to call a theory of complex phenomena, and this served as the foundation for many of his methodological conclusions. Among the conclusions he reached, those of interest to economists include the following:

1. Many of the phenomena that economists study are, in fact, examples of complex phenomena.
2. When we deal with complex phenomena, precise predictions will be impossible. (This, then, is a fairly straightforward claim about the empirical limitations of economics.)
3. When we *theorize* about complex phenomena, usually the best that we are able to do is to offer explanations of the principle by which the phenomena occur. Though we may be able to predict broad patterns of behavior, and thereby rule out certain outcomes, our ability to falsify theories is also diminished. As Hayek put it: "The advance of science will thus have to proceed in two different directions: while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena" (Hayek 1967, p. 29).
4. Hayek's ultimate conclusion about economics might be summed up in the following aphorism: "what we can know in the field of economics is so much less than people aspire to" (Hayek 1983, p. 258).

Viewing economics as a science that studies complex phenomena, Hayek's main message concerned the limits that such a science would face. Though a message like that is never going to be popular, it was particularly repungnant during the positivist era in which he wrote. The real question, though, is not how his message was received, but whether or not he was right. So: Was Hayek right about the limits of economics? How might we go about testing his claims?

It should be immediately apparent that there is no way to *establish* the claims, since that would involve proving a negative: the limits we face today may be gone tomorrow. But we can ask: If Hayek were right, what would the history of economics in the twentieth century look like? Had the positivists been right, the answer to that would be easy: there would have been steady, indeed cumulative progress, as economic laws were discovered and multiplied, allowing ever more precise predictions to be made, and errant theories to be falsified and discarded.

If Hayek were right, on the other hand, economics would look very different. This is not to say that there would be no progress, just that the type of progress we might expect would be of a different sort from that which a positivist might foretell. So in a way it all comes down to a methodological question—what *kind* of empirical and theoretical progress was there in economics during the last century? This topic has been much studied of late, and indeed it was the theme of the 4th Annual Conference of the European Society for the History of Economic Thought held in Graz, Austria, in 2000. The question is obviously a difficult one, not the least because there is no evident criterion by which progress is to be gauged. So what follows should be taken as it is intended—as an argument sketch. With that very appropriate caveat in mind, I will suggest that many features of economics today are at least consistent with the limitations Hayek identified.[1]

Let us think first about empirical work. There has obviously been a considerable amount of progress here. The profession has at hand better statistical techniques, better data, and more powerful computers. I work in a largely empirical department of economics, and the econometric techniques that people of my generation learned as Ph.D. students are eclipsed by what we teach our Master's students today. Huge data sets are now available, panels of data tracking all sorts of variables across households and through time, and the techniques to squeeze information from them seem to multiply no less promiscuously. The transformations wrought by ever more powerful computers are everywhere evident. If the positivists were right, the end result of all this would have been more precise predictions, the discovery and establishment of empirical laws, and perhaps even a policy payoff—a better ability to fine-tune (to use the phrase of the 1960s) or to command and control our national economies.

I don't think that I am being overly provocative if I assert that this has not been what has happened. Virtually every person who has written a methodological piece about empirical work in economics in the past century, and no matter *when* the person was writing, has lamented the lack of progress of the sort that was thought possible in the positivist era. Rather than provide a laundry list (but see, e.g., Hutchison 1977, Blaug 1992, Goldfarb 1997, and citations therein for some evidence) I will simply quote from one of the best of the more recent contributions, Roger Backhouse's book *Truth and Progress in Economic Knowledge*. Backhouse concludes his lengthy and careful examination of empirical progress in economics with these words: "despite the immense effort, undreamed-of increases in computing power, and the development of vastly more sophisticated statistical techniques, econometrics has failed to produce the quantitative laws that many economists, at one time, believed it would" (Backhouse 1997, p. 136). There is not space here to rehearse the evidence that Backhouse marshals for his claim, so I will merely append a brief anecdote that

[1] I am painfully aware of how dangerous it is to try to characterize what "the economics of today" looks like, even if I had more space. At the end of the paper I suggest that more case studies might help us to make more progress in this area.

I think illustrates his point well. Our departmental econometrician recently attended a conference in the Netherlands where a number of different estimating techniques were compared against one another according to how well they handled a given data set. The data set consisted of a number of variables used to track the Dutch economy. When I asked our econometrician which estimating procedure provided the best forecast, he replied that that was not even one of the criteria they used in assessing the various techniques! Surely that would not have been the case during the positivist era.

Let me reiterate that I am not denying that there has been empirical progress in economics—as Roger mentioned, we have better statistical methods, better computers, and better data. We certainly can describe various aspects of the economy far better than we could two generations ago. We have, in a sense, better snapshots of what the economy looked like in the very recent past. And indeed, one of the themes of David Colander's presidential address last year was that what he called "applied policy modeling" may be taken as the very hallmark of the economics of today. But none of this, I submit, has allowed us to make more precise forecasts or come up with clean tests of hypotheses, and that is because with complex phenomena, pattern predictions are the best we can hope for. I would finally add that, even if "applied policy modeling" is a term that can be used to describe modern economics, most of the limitations I have noted seem to have seeped into the consciousness of the actual policymakers. We don't hear much about fine-tuning, about activist demand management policy, or about social control anymore, at least not in the U.S.

What about theoretical work? When Hayek was writing about "explanations of the principle" back in the 1950s, most of his examples, alas, were drawn from fields outside of the social sciences, such as biology. Indeed, his whole point in many of these writings was to show his readers that other sciences, in fact, study complex phenomena, so that they would accept it as a legitimate model for economics. But I think that if we consider the microeconomic theory of his day, which in its partial equilibrium instantiation is the stuff of our own present day undergraduate microeconomics textbooks, we will see what he was talking about. So, for example, when we tell our students that price ceilings will result, *ceteris paribus*, in excess demand, black markets, quality deterioration, and non-price rationing, we are making a pattern prediction. When we tell them that, *ceteris paribus*, the incidence of taxation depends on the elasticities of demand and supply that a good faces, we are explaining the principle under which a tax burden gets distributed. When we enumerate the conditions under which we would expect third degree price discrimination to emerge, or for a cartel to persist, we are providing explanations of when to expect certain patterns of economic behavior, rather than others, to come about. In such exercises we are identifying the sorts of variables that are important in explaining the phenomenon in question. The *ceteris paribus* clause is crucial, for it is there to remind us how hard it is to pass from such theoretical explanations to precise numerical predictions about such phenomena as they exist in the real world.

All of this is simply to say that Hayek's ideas of "pattern prediction" and of "explanation of the principle" fit the standard microeconomic theory of his day pretty well. But what about more recent developments in microeconomics? Here we are able to confront Hayek's claim that progress will necessarily involve the development of less falsifiable theories, since *that*, as he put it, "is the price we have to pay for an advance into the field of complex phenomena" (Hayek 1967, p. 29). If we were asked to name some of the major theoretical developments in microeconomics in the past half-century, three possible candidates would be the theory of information, game theory, and transaction cost economics. To some extent all three started out as critiques of mainstream theory, though each has by now been integrated into it. Each allows us to provide a theoretical analysis of economic phenomena that did not fit well into the previous theory, from the incentive structures that we might expect to arise under different informational regimes, to the form that labor and insurance contracts might then be expected to take, to descriptions of alternative forms of behavior in different strategic situations, to explanations of the existence and internal structure of firms.

All of these are theoretical advances, for they allow us better to understand and explain economic phenomena. But each of them, it seems to me, also makes microeconomic theory less falsifiable. Let us consider game theory for just a moment. To be falsifiable, a theory must predict certain specific outcomes and prohibit others. I remember studying game theory in graduate school in the 1970s, mostly in applications involving oligopoly, and I can distinctly remember my professor complaining about how the theory didn't allow us to make firm predictions about market outcomes, like the theories of perfect competition and monopoly did. That was the positivist era, and predictive impotence was viewed as a very severe limitation. In more recent years, it is often recognized (and still sometimes lamented) that whenever game situations of any complexity are entertained, a proliferation of solution concepts typically emerges. A theory that allows more solution concepts is one that prohibits fewer outcomes. So though these game theoretic models are better able to capture the complexity of social phenomena, they are also less falisifiable—they allow us to make less precise predictions. This is just what Hayek anticipated. In a sense, this particular advance specifically highlights the limits of our knowledge. And it is my contention that similar things may be said about transaction cost economics and the theory of information.

Well, obviously, given my time constraints I am dancing pretty fast here, and I am sure I have not convinced any doubters in so short a time. My objective, though, was simply to point out that some of the developments we have witnessed in economics in the century just past seem to confirm what Hayek had to say about the nature of the discipline.

Another way of testing Hayek's claims is to note that, if he were right, others certainly would have noticed some of the same things he did. Here the evidence is more mixed. Many economists, especially those interested in methodology or the history of thought, have made complaints roughly similar to Hayek's. For example, the indictments made by Terence Hutchison (1988) and Mark Blaug (1992, p. 111) that most economists fail to engage in anything much better than "innocuous falsificationism" are obviously consistent with Hayek's position. So is Dan Hausman's (1992, p. 253–54) argument that the data that economists have been using offer little hope for crucial tests of their theories. Where these analysts *differ* from Hayek is in their response to what they see happening in economics. Whereas Hayek might take such observations as confirming the limitations we face when we study complex phenomena, each of them urges economists to try harder: try harder to falsify, try harder to get better data. That is to say, they see the limits, but they hope to overcome them. In a like manner, though Alex Rosenberg's (1992) observation that there has been virtually no improvement in the predictive powers of economic theories mimics Hayek's views, Hayek would not share his conclusion that economics is therefore not a science. For Hayek, the outcome is just what one expects from sciences that study complex phenomena. And yes, even Deirdre McCloskey's (1985) claim that the argumentation of economists is principally rhetoric is, in its own way, consistent with Hayek's views. I doubt the claim would ever have been made had economics shown the sort of progress that positivists confidently envisaged a half a century ago. Both the birth of the rhetoric movement and the revival of professional interest in methodology are in many ways the direct result of the failure of economics to deliver on the promises of positivism.

But if many who study methodology have come to conclusions very similar to Hayek's, the same cannot be said about the rest of the profession. And so we come at last to the $64,000 Question: If what has been said here is so evident to so many of us, what accounts for the myopia of the rest of the profession? What accounts for the success of mainstream economics, and why are so many mainstream economists so self-satisfied? This has got to be one of the key mysteries of twentieth century economics, and indeed, a number of economists have tried to figure it out (a representative sample might include Mayer 1993, Reder 1999, and some of the articles in Colander and Brenner 1992). Explanations range from the pedagogical to the sociological to the conspiratorial to the cultural, and as for myself, I have yet to come up with a fully satisfactory answer. I will share with you, though, two observations that I think must play a role in any final explanation.

The first is simply to recognize the remarkable resiliency and adaptability of neoclassical microeconomic theory. In the American South we have the legend of the Tar Baby. It is the fate of every fool to think that he can defeat the innocent looking, diminutive Tar Baby by force. The reason one cannot is simple—the harder one hits the Tar Baby, the more deeply one gets stuck in the tar. Well, in my opinion, neoclassical microeconomic theory is the quintessential Tar Baby. A huge number of theories—monopolistic competition, game theory, information theory, bounded rationality, and the list goes on and on—started out as challenges to mainstream economics and ended up being incorporated into it. Why is this so? I think that if one views mainstream microeconomic theory as being a variant of Popper's situational analysis, the answer becomes evident. Most of the innovations mentioned above consist simply of reconfigurations of certain of the initial conditions of a situational analysis. Situational analysis is very adaptable, and by following its strictures economics has been able to adapt to the changing times. It is, of course, the supreme irony that Popper, whose name is forever linked with the notion of falsificationism, seemed unable to recognize that the method of explanation he thought applied to the social sciences would result in a theory that became ever less falsifiable. It took his friend Hayek to see that.

Situational analysis, then, is a powerful tool for understanding the structure and the apparent successes of economics. But when this tool gets tied to the rhetoric of positivism, it fosters false hopes and permits self-delusion. For then the nature of what kind of progress is possible is misunderstood. Positivism is what misleads us into thinking that we can, and indeed that to be scientific we must, improve the predictive adequacy of our theories. This sort of false consciousness is what drives the economics profession's refusal to acknowledge the limitations of economics. And, I would contend, its shadow is also there in the writings of people like Mark Blaug, Terence Hutchison, and Dan Hausman, all of whom hold out the hope that if we just try harder, "real" empirical progress will be possible. It also is there in Rosenberg's charge that our reliance on folk psychology prevents us from becoming a "real" science. There is a science of complex phenomena, it's just that when one practices it, one had better lower one's expectations about predictive adequacy.

It has now been almost fifty years since Milton Friedman, writing in the heyday of positivism, performed his abduction and enshrined prediction as the goal of positive economic science. We knew a lot less then than we do now about the prospects for such a program's success. To fail to acknowledge its limits at this late date is nothing short of a scandal. As Hayek himself put it in an interview late in his life: "You know, one of the things I have often publicly said is that one of the things I most regret is not having returned to a criticism of Keynes's treatise,[2] but it is as much true of not having criticized Milton's [*Essays in*] *Positive Economics*, which in a way is quite as dangerous a book" (Hayek, 1994, p. 145). The second half of the twentieth century saw the profession move beyond the message of *The General Theory*. It is high time that we moved beyond *Essays in Positive Economics* as well.

I will close with a final question—I have emphasized Hayek's criticisms, but: Is there a (not positivist but) positive program here? I think that there are a number of them. One is to articulate more fully the theory of complex phenomena, which provides the underpinning for Hayek's methodological claims, but which (it must be admitted) he never really fully developed. Another, and one that historians of thought can participate in more directly, is to test the theses offered here by examining the historical record to see exactly what sorts of progress actually have occurred in economics. A third challenge is to describe more fully the limits of economic analysis. Interestingly enough, economists both within the mainstream and outside of it, in various research programs, have begun to make contributions to all three areas. If such work continues, it may well be that the economics of the twenty-first century is more Hayekian than that of the twentieth. Now *that* would be something for *The New Yorker* to write about!

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[2] Though Hayek refers to Keynes's book as a treatise, he is talking about The General Theory, not A Treatise on Money.

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