

Why Didn't Hayek Review Keynes's *General Theory*?

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Two problems of historiography are highlighted in this article about F. A. Hayek, John Maynard Keynes, and *The General Theory*: the sorts of questions historians typically ask and the reliability of self-reports from major historical figures.

Historians, of course, usually try to explain why something happened, but not always. Occasionally the more interesting question is, why did something that apparently should have happened, not happen? This question has captured the attention of many historians of economic thought who typically have asked it in the hope of solving some riddle in the past.

We know, for example, that while lying on his deathbed Adam Smith instructed two of his close friends to burn sixteen volumes of his letters, lecture notes, and manuscripts, a practice that was, alas, not unusual in his time. What is curious is that Smith chose to hold back from the fire a small collection of incomplete essays. Vernard Foley (1976) asks, Why

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didn't he burn these papers, too? Are they, rather than his other more famous works, the key to his thought?

Carl Menger, a founder of the marginalist revolution, was also for a time the economics tutor for Crown Prince Rudolf of Austria. In his introduction to a book in which the Crown Prince's lecture notes are reproduced, Erich Streissler (1994) questions why Menger didn't use this golden opportunity to promote his own marginalist views. Why did he teach his young charge classical economics à la Adam Smith instead?

In his book *Stabilizing Dynamics*, Roy Weintraub (1991) follows the same pattern when he asks why Paul Samuelson, in his work on stability analysis, chose not to make use of the readily available Liapunov method, a step that would have solved certain problems that took much longer to address without it. Weintraub (54) likens his query to the problem of "the dog that did not bark" in the Sherlock Holmes story "Silver Blaze." Indeed, such investigations seem invariably to result in historical reconstructions that read a lot like mystery stories.¹

Despite their intrigue, "nonbarking dog" stories are not particularly common, for they pose a number of problems for historians. First, there is the simple fact that for every event that actually occurs, there is virtually an infinite number of events that might have occurred. The historian's first job is to convince the reader that only one event (out of all the possible events) that didn't happen, should have. Once that is accomplished a second problem emerges: now the historian must reverse gears to show why that which should have happened in fact did not. A historian pursuing a nonbarking dog story shoulders a double burden of proof: The more convincing he or she is in advancing one argument, the harder the second argument is to establish. These problems are particularly nettlesome when, as in the examples above, what is at issue are a person's motivations and when there is no written record (letters, diary entries, and so forth) explaining or rationalizing whatever did take place.

Another nonbarking dog question in the history of economic thought is the subject of this article: Why didn't Hayek write a review of Keynes's *General Theory of Employment, Interest and Money* ([1936] 1973)? After providing some background information on the protagonists, I

1. Interest in "nonbarking dog" questions is not limited to economists. For example, the "Silver Blaze" metaphor is used by the social historian Kevin McAleer (1994, 197) in reference to the *Sonderweg* (separate path) question of German history: Why didn't the development of the German Empire mirror those of other European nations? Why was its path so different?

will show that Hayek might reasonably have been expected to have written a review. I will then examine some reasons why it did not happen. This particular instance is unlike the examples above, however, in a remarkable way, for it turns out that Hayek himself wrote frequently about why he never wrote a review! As such our explanatory task is simplified considerably, or so one might at first surmise. But even with Hayek's assistance, a new (though ancient) problem emerges: that of the reliability of self-reports. Although in the end I will proffer an explanation of why no review was written, we will see that the problem of self-reporting appears in this case to be a real one. The "riddle of the review" may well remain an unsolved mystery in the history of economic thought.

1. Some Background on Keynes and Hayek

Friedrich A. Hayek received degrees from the University of Vienna in 1921 and 1923, then spent about sixteen months in the United States. While there he studied the workings of the Federal Reserve system and learned some basic statistical smoothing techniques. In 1927 the Austrian Institute for the Study of Business Cycles was created, and Hayek was appointed its first director. In this capacity he traveled to London to attend a meeting of European institutes that were gathering statistics and doing research on the trade cycle. It was there that he first met Keynes, an encounter he would recall in a paper published in 1966:

I first met him in 1928 in London at some meetings of institutes of business cycle research, and though we had at once our first strong disagreement on some point of interest theory, we remained thereafter friends who had many interests in common, although we rarely could agree on economics. He had a somewhat intimidating manner in which he would try to ride roughshod over the objections of a younger man, but if someone stood up to him he would respect him forever afterwards even if he disagreed. (Hayek [1966] 1995, 240)²

Keynes by then had achieved international fame for *The Economic Consequences of the Peace* ([1919] 1971), in which he offered an economic and moral appraisal of the terms of the treaty as well as a dev-

2. In this and subsequent Hayek 1995 references, the date in brackets represents the year in which the cited material first appeared.

astating account of the political maneuverings he had observed at the Paris Peace Conference. In Britain he was best known as a commentator on current economic issues. More important for our purposes, Keynes in the late 1920s was working on a book on theoretical economics, one that he hoped would establish his reputation as a monetary theorist. After much delay, *A Treatise on Money* was finally published on 31 October 1930.

Hayek, sixteen years Keynes's junior, was working in the same area. He had begun his own volume on the history of monetary theory and policy of which four chapters, covering roughly from 1650 to 1850, had been completed. In 1929 he published another work, *Geldtheorie und Konjunkturtheorie* (see Hayek [1933] 1966). The book contained Hayek's version of the Austrian (or "Mises-Wicksell") theory of the origins of the trade cycle. In the same year Hayek's critique of the underconsumptionist theories of the American writers Waddill Catchings and William Trufant Foster appeared in a German economics journal.³

Hayek's writings caught the eye of Lionel Robbins, the recently appointed head of the economics department at the London School of Economics and at age thirty the youngest professor of economics in England. In the spring of 1931 Robbins invited Hayek to the LSE to give four lectures. Published later that year as *Prices and Production*, the lectures were a big hit. Hayek was offered a visiting position for the following academic year and, soon thereafter, the Tooke Chair of Economic Science and Statistics.

In short order Hayek's battle with Keynes began. In the August 1931 issue of the journal *Economica*, Hayek published the first half of a lengthy critical review of Keynes's *Treatise* (Hayek [1931] 1995, chap. 3). One of his biographers tells us that "Keynes was obviously very unhappy with the August part of the review, for his copy of that issue of *Economica* is among the most heavily annotated of the surviving copies of his journals, with no less than 34 pencilled marks or comments on the 26-page review" (Moggridge in Keynes 1973, 243). Keynes's "Reply" was published in the November issue, before the second half of Hayek's review appeared in February 1932 (Hayek 1995, chap. 4 and 6).

3. Hayek's previously unpublished chapters on English monetary theory comprise chapters 9 through 12 of Hayek 1991. The English translation of his book is *Monetary Theory and the Trade Cycle* ([1933] 1966). The journal article, translated as "The 'Paradox of Savings,'" is reprinted as chapter 2 of Hayek 1995.

Keynes used about as much space in his reply attacking Hayek's book as he did defending his own. As the following passage indicates, Keynes's low opinion of *Prices and Production* comes across very clearly:

The book, as it stands, seems to me to be one of the most frightful muddles I have ever read, with scarcely a sound proposition in it beginning with page 45 [Hayek provided historical background up to page 45; after that came his theoretical model], and yet it remains a book of some interest, which is likely to leave its mark on the mind of the reader. It is an extraordinary example of how, starting with a mistake, a remorseless logician can end up in bedlam. (Hayek [1931] 1995, 27–28)

The dispute on the pages of *Economica* generated a correspondence between Hayek and Keynes: they ultimately exchanged twelve letters between 10 December 1931 and 29 March 1932. Piero Sraffa's harsh review of *Prices and Production* in the March 1932 issue of the *Economic Journal*, and the comment and reply that followed in June, must also be considered as part of this ongoing contest between rival theories of the workings of a monetary economy.⁴

Beginning probably sometime in the spring of 1932 Keynes began to revise his theory. One historian argues that although by his Easter term lectures (in April and May 1932) Keynes was still using the analytical tools of the *Treatise*, the voice of *The General Theory* had finally begun to emerge (Patinkin 1976, 72). The process whose ultimate conclusion was the publication of Keynes's masterwork in February 1936 has become one of the most studied episodes in the history of economics, partly because of its intrinsic interest, and partly because of the abundance of materials that has survived, including Keynes's own notes, draft tables of contents, correspondence, the notes of students, and related publications. As a biographer has noted, "Although scholars will always hope for more, it is probably the case that with the *General Theory* they have the most voluminous record surrounding the creation of any classic work in economics" (Moggridge 1992, 557).

Hayek was also busy. But he did not prepare a review of *The General Theory*. Why not?

4. The correspondence is reprinted as an addendum to chapter 5 in Hayek 1995, and the exchange between Sraffa and Hayek appears there as chapters 7 through 9.

2. Is It Reasonable to Expect that Hayek Would Have Reviewed *The General Theory*?

There are a number of reasons why it is reasonable to expect that Hayek would have reviewed *The General Theory*. First, one of the principal reasons that Hayek initially came to England from Austria was, as one commentator has put it, "to provide a counter-attraction to Keynes" (J. Robinson 1978, 2). Lionel Robbins was the person behind the scenes in this matter. Robbins probably first began thinking of Hayek as a candidate for "counter-attraction" when he read the German version of "The 'Paradox of Savings,'" published in 1929. In the late summer of 1930, Keynes invited Robbins to join the Committee of Economists, a small group of specialists that had been charged with writing a report on policies to combat the Depression. Keynes hoped to use the group to disseminate his own views, and Robbins soon found himself disagreeing with the others, both about the origins of the downturn and about appropriate remedies. Robbins ultimately wrote a dissenting report.⁵ He also tried, unsuccessfully, to get the committee to hear evidence from economists whose views were closer to his own. Hayek was among the names he had submitted. Very soon thereafter came the invitation for Hayek to lecture at the LSE.

In an interview a half century later Hayek recounted the event and how serendipitous it had all seemed. (Recall that he had just completed four chapters of a history of monetary theory, which is why he could produce such an original lecture.)

When I gave in Vienna my initial lectures as Privatdozent, I chose for my subject this kind of underconsumption theory which had then become acute in England. And Robbins could read German. That's an almost unique feature, an English professor who could read German literature—that's good luck, that he pounced on my subject: This is the thing we need at the moment, to fight Keynes. So I was called in for this purpose, produced of course a lecture which was original, which suggested more knowledge of the history of English theory than anybody else. Was sympathetically received by Robbins, who had been influenced by the Austrian school. We at once understood each other. This combination of accidents led to my

5. This episode is recounted in Howson and Winch 1977 (46–72).

appointment in London. It was luck from beginning to end. (Hayek 1994, 77)

It is also significant that Hayek had successfully challenged Keynes. As previously noted, the rivalry between the two was established early on. It is arguable that Hayek had the better part of the initial encounter. Both of their theories built on a framework that had been created by the Swedish economist Knut Wicksell. Wicksell's work was well known within the Austrian tradition. His first book, published in 1893, was an attempt to integrate the Austrian theory of capital with marginal productivity theory, all within a general equilibrium framework. Only after having tried to provide such "microfoundations" did Wicksell turn to a discussion of a monetary economy in *Interest and Prices* ([1893] 1965). In the *Treatise* Keynes completely ignored Wicksell's earlier contribution, borrowing only from *Interest and Prices*. Not surprisingly, Hayek in his review targeted the absence of a capital-theoretic foundation in Keynes's book.⁶ In his reply Keynes more or less granted Hayek's point. This was probably why he felt it necessary to take the extraordinary step of hatcheting Hayek's *Prices and Production* in his reply. It was as if Keynes were saying, "Well, yes, my theory has its problems, but they are nothing compared to those in Hayek's work."

This is not to say that Hayek won their *rencontre* outright. Because it had capital-theoretic foundations, Hayek's theory *appeared* the more well developed and consistent of the two. But his framework was unfamiliar to his English readers, and many observers still found it hard to judge between them. All that was clear was that a contentious dispute had started. John Hicks, writing during the heyday of the Keynesian revolution, recalled it this way: "It is hardly remembered that there was a time when the new theories of Hayek were the principal rival of the new theories of Keynes. Which was right, Keynes or Hayek?" (Hicks 1967, 203).

Despite Hayek's early successes, for a variety of reasons the tide had begun to turn in Keynes's favor by the middle of the decade. Hayek himself became engaged on a number of new fronts. The exchange between Hayek and Sraffa that began with Sraffa's review of *Prices and Produc-*

6. The relevant books are Wicksell [1893] 1970 and [1898] 1965. Both of Wicksell's books were translated from German into English only *after* Keynes had finished the *Treatise*. Indeed, in a footnote in the *Treatise*, Keynes admitted that "in German I can only clearly understand what I know already!" (Keynes [1930] 1971, 5:178 n. 2), so Hayek had a distinct advantage over Keynes in this area.

tion was, if anything, even more obscure than the earlier one between Hayek and Keynes, but few could mistake Sraffa's scathing tone. In 1933 Frank Knight began his assault on Austrian capital theory which Hayek was to answer in 1936. The year before he had been busy editing two essays to be contributed to *Collectivist Economic Planning*, a volume that set off the British version of the socialist calculation debate.⁷ These new battles not only dissipated Hayek's strength, they suggested to the neutral observer that very few economists outside of the LSE (and indeed, not even all of those who were within it) agreed with Hayek's ideas.

Meanwhile, Keynes's star continued to rise. He was frequently in the public eye, offering policy advice, delivering speeches, and writing articles. Keynes also made it clear that he was developing (with the help of a bevy of brilliant disciples at Cambridge) a new theory of aggregate income, one that would correct the deficiencies in his earlier work. All of this created a general anticipation for *The General Theory*. And it wasn't just Keynes's own self-promotion; members of the Cambridge Circus were doing their able best to change the minds of any recalcitrant Hayekian devotees. Abba Lerner, Nicholas Kaldor, and G. L. S. Shackle were among the more prominent of their new recruits.

All of this suggests that the stakes had increased by 1936. Hayek had experienced an early victory, but, having been challenged simultaneously from a number of directions, he was losing ground fast. A critical review of his chief rival's new theory never could have been more important. Instead, no response came; at least, not until much later, in 1963, when Hayek first publicly stated his regret at not having reviewed Keynes's book: "I have to the present day not quite got over a feeling that I had then shirked what should have been a plain duty" (1995, 60).

Hayek not only shirked a duty, he missed a golden opportunity. Why?

3. Hayek's Explanation(s) of Why There Was No Review

I have identified five different places where Hayek reflects on why he did not review *The General Theory*.⁸ The first is an essay that was part

7. Hayek's papers on socialism are reprinted in Hayek 1997, and Hayek 1936 is his response to Frank Knight (1933, 1934, 1935).

8. Others exist. Hayek gave interviews to newspaper reporters, was interviewed for articles in journals, and replied to queries from scholars. His relationship with Keynes was a popu-

of a series of lectures delivered in October 1963 at the University of Chicago, but was not published until 1995. The second is an article that appeared in 1966 (Hayek 1995, chaps. 1 and 12). The third rather brief reference to the episode occurred in a series of interviews that Hayek gave in the late 1970s and published in 1983. He returned to the topic twice in the 1980s, first in an article in *The Economist* (Hayek 1995, chap. 13), and second in an interview with the philosopher and, at the time, his prospective biographer, W. W. Bartley III (Hayek 1994, 88–98).⁹

In the essays from the 1960s, Hayek offers two reasons for not having written a review, and hints at two others. The first reason involved his experience with Keynes following his extensive review of the *Treatise*:

The somewhat disappointing upshot of all the pains I had taken with the *Treatise* was that not long after I had published the second part of my review it became known that Keynes had himself fundamentally altered his theoretical framework and was preparing a new and very different version. I must confess that it was partly due to this experience that when *The General Theory* appeared I did not return to the attack—out of a feeling that before one could complete a systematic examination, Keynes would probably again have changed his views. (Hayek [1963] 1995, 60)

This reason, that Keynes would probably soon change his mind again, appears in all five of Hayek's retrospective accounts.

The second reason was that, after reading *The General Theory*, Hayek got a vague but persistent feeling that he was opposed not so much to Keynes's theory, but to his whole approach and, in particular, to Keynes's use of aggregates, which hid from view changes in relative prices:

I did feel from the beginning, though I did not see it then as clearly as I do now, that the difference did no longer concern particular points of analysis but rather the whole method of approach. . . . As I saw it, an examination of the validity of *The General Theory* would

have made it necessary to take issue with the whole macrodynamic approach, the treatment of the economic process in terms of aggregates and statistical totals, a theory which was concerned only with price levels and total income streams and in effect took the whole structure of relative prices for granted and provided no tools to explain changes in relative prices or their effects. (Ibid.)

Hayek offers this explanation in both the 1963 and the 1966 accounts, but in the latter he adds that it “in retrospect appears to me the decisive one” ([1966] 1995, 241).

There are some differences between the 1963 and 1966 accounts. In 1963, after elaborating the “Keynes would change his mind” thesis, Hayek says: “But there was another reason than mere tiredness of controversy which made me hesitate” ([1963] 1995, 60). Now, “tiredness of controversy” is evidently not the same thing as thinking that Keynes would change his mind: one might wish to avoid wasting one's time, and yet not be tired. Therefore, this constitutes another possible reason. In the same transition place in the 1966 paper Hayek says, “Though he had called it a ‘general’ theory, it was to me too obviously another tract for the times, conditioned by what he thought were the momentary needs of policy” ([1966] 1995, 241). This suggests that Hayek thought that the theoretical edifice in *The General Theory* was designed to promote a specific set of policy views. As such, this might be viewed as an *additional reason* for why he thought Keynes would change his mind: it was suggested not only by Keynes's past behavior, but also by the type of book that was *The General Theory*.

Hayek never mentions “tiredness of controversy” again. His allusion to *The General Theory* as a “tract for the times” is a repetition of a charge that he had made in a brief piece in a symposium on the work of Keynes published in 1959 in *The Christian Science Monitor* (Hayek 1995, 237–39), but it is not repeated in subsequent accounts.

In the fall of 1978 Hayek was interviewed as part of the Oral History Program at the University of California at Los Angeles. Interviewers included, among others, Armen Alchian, Robert Bork, James Buchanan, Axel Leijonhufvud, and Leo Rosten. The question of the review comes up twice: in his discussion with Alchian on 11 November and again in the interview with Rosten on 15 November. In both instances the reason Hayek gives is that soon after he had completed his review of the *Treatise* Keynes had told him, “Well, never mind, I no longer believe that” (Hayek 1983, 408; compare to 114–16).

lar topic on such occasions. None that I have seen, however, adds anything of substance to the accounts listed here.

9. Bartley was also to have been Karl Popper's biographer, but he predeceased both of them.

Hayek discusses the review twice in the 1980s and tells essentially the same story in both instances. Interestingly, however, he modifies his earlier account:

It was not merely (as I have occasionally claimed) the inevitable disappointment of a young man when told by the famous author that his objections did not matter since Keynes no longer believed in his own arguments. Nor was it really that I became aware that an effective refutation of Keynes's conclusions would have to deal with the whole macroeconomic approach. It was rather that his disregard of what seemed to me the crucial problems made me recognize that a proper critique would have to deal more with what Keynes had not gone into than with what he had discussed, and that in consequence an elaboration of the still inadequately developed theory of capital was a prerequisite for a thorough disposal of Keynes's argument. ([1983] 1995, 251–52)

The meaning of Hayek's somewhat opaque final sentence is clarified in his interview with Bartley:

I had been criticized for the fact that in *Prices and Production* I had a very inadequate theory of capital; that in this crude Böhm-Bawerlian form of an average period of production, it was inadequate. So I had started writing a great book on capital and money, which ultimately dealt with the money phenomenon. It took me very much longer than I thought; I worked seven years on the thing. I was dead tired of the subject before I got to the monetary aspects. (Hayek 1994, 90–91)

The book to which Hayek refers is *The Pure Theory of Capital*, which was published in 1941. As Hayek points out, the book did not contain a well-worked-out alternative to Keynes's theory of a monetary economy. Of course, Hayek could still have launched a separate (if belated) attack on Keynes. But he goes on to note that in 1940 he had joined forces with Keynes in an attempt to avoid policies that would create either rationing or inflation during the wartime mobilization. Because they were then on the same side, Hayek decided to postpone his attack:

The main cause for this postponement was that I soon found myself supporting Keynes in his struggle against wartime inflation, and at that time wished nothing less than to weaken his authority. Although I regard Keynes's theories as chiefly responsible for the inflation of the past quarter of a century, I am convinced that this was a devel-

opment which he did not intend and which he would have endeavored with all his strength to prevent. ([1983] 1995, 252)

It should also be noted that Keynes and Hayek became personally closer during the war. At the beginning of the war the LSE was evacuated to Cambridge, and Keynes helped Hayek get rooms at Kings College.

What about after the war? Hayek apparently was biding his time in the hope that Keynes, who had revealed his anti-inflationist tendencies, would oppose the policies being promoted by some of his followers. Again fate intervened. In 1952 Hayek recalled their last conversation, in which he asked Keynes

whether he was not concerned about what some of his disciples were making of his theories. After a not very complimentary remark about the persons concerned, he proceeded to reassure me by explaining that those ideas had been badly needed at the time he had launched them. He continued by indicating that I need not be alarmed; if they should ever become dangerous I could rely on him again quickly to swing around public opinion—and he indicated by a quick movement of his hand how rapidly that would be done. But three months later he was dead. (Hayek [1952] 1995, 232; see also Hayek 1994, 92)

To summarize, Hayek did not write a review of *The General Theory* for the following reasons:

1. He thought, based on his experience with the review of the *Treatise*, that Keynes would change his mind again. (Mentioned five times, but de-emphasized in the last two accounts.)
2. He thought, because *The General Theory* was “a tract for the times,” that Keynes would change his mind again. (Mentioned once, in 1966.)
3. He had the vague feeling that a proper critique would require him to challenge Keynes's entire macroeconomic approach. (Mentioned four times; cited as the “decisive” reason in 1966, but de-emphasized in the last two accounts.)
4. He was “tired of controversy.” (Mentioned once, in 1963.)
5. He was then engaged in preparing a model, one with its own improved capital-theoretic foundations, which he hoped would soon be available as an alternative to Keynes's model. (Emphasized in the last two accounts.)

6. The alternative model never was put forward. By the time *The Pure Theory of Capital* came out Hayek and Keynes were fighting on the same side against wartime inflation, so Hayek didn't want to weaken his influence. (Emphasized in the last two accounts.)
7. Finally, Hayek held back criticism at war's end because he had hopes that Keynes would come out against the policies of his followers. But Keynes died before this happened. (Their final conversation; first mentioned in 1952, repeated in the 1980s.)

Note that the last two reasons explain why Hayek did not attack Keynes during the war years. If we restrict ourselves to the reasons why no review was written in the 1930s, only statements 1 through 5 are germane.

4. Some Problems with Hayek's Explanation(s)

Hayek's explanations are not unambiguous, and indeed, there are good reasons to treat his stories with some skepticism. Hayek didn't start writing about the question of the review until the halcyon days of the Keynesian movement. Given this timing, and the fact that he was completely opposed to the Keynesian approach, his statements about it must be considered with caution.¹⁰

Next, Hayek's story changes rather substantially through time. He emphasizes two reasons in 1966 and calls the second of these (his vague feeling of unease about macroeconomics) "decisive." This seems clear enough, but in the 1980s both major reasons are downgraded, the two that were mentioned in passing (Hayek's "tiredness of controversy" and Keynes's book being a "tract for the times") are wholly ignored, and two new reasons are offered as most important. Which accounts are we to believe?

Perhaps most damning is the tendency of Hayek's stories to leave the reader with the feeling that his memories are just a bit too consistent. Look, for example, at three of Hayek's descriptions of his first meeting with Keynes in London (these descriptions may be compared to a pertinent quotation at the start of this article):

10. Thus Hayek's constant reiteration of Keynes's propensity to change his mind might simply have been designed to discredit Keynes and to remind a later and more worshipful generation of one of their hero's most obvious foibles.

We at once had our first theoretical clash—on some issue of the effectiveness of changes in the rate of interest. Though in such debates Keynes would at first try ruthlessly to steamroller an objection in a manner somewhat intimidating to a younger man, if one stood up to him on such occasions he would develop a most friendly interest even if he strongly disagreed with one's views. ([1963] 1995, 59)

There we had our first difference on economics—on the rate of interest, characteristically—and he had a habit of going like a steamroller over a young man who opposed him. But if you stood up against him, he respected you for the rest of your life. (1983, 114)

We at once had a conflict, very friendly, about rate of interest. Inevitably. He did, in his usual manner, try to go like a steamroller over the young man. But the moment—I must grant him this—the moment I stood up with serious arguments, he took me seriously and ever since respected me. (1994, 89)

These remarkably similar accounts span three decades. Anyone who has viewed the requisite number of television courtroom dramas can immediately see the problem: rather than providing a spontaneous recollection of what actually happened, the suspect seems to have memorized a carefully constructed script. Is this what happened? At a minimum it would seem that, doubtless due to its frequent repetition, Hayek's account had taken on a life of its own.

5. A Proposed Interpretation

Some might want to throw out all the information Hayek provided and just leave the riddle of the review unsolved. I will nevertheless propose here an interpretation. Under the circumstances, it would be disingenuous to say anything more than that I think that this account makes sense of the evidence. My approach will be to try to assess the veracity of the various reasons which Hayek has offered.

Before doing so, however, we will first examine a simple, even obvious, explanation, one that Hayek himself never offered, but which occurred to several early readers of this paper: maybe no one asked him to do a review!¹¹ There is no evidence that Hayek was asked by

11. Martin Bronfenbrenner, Ross Emmett, and Maria Marcuzzo all came up with slightly different variants of this reason. Among those who reviewed *The General Theory* for the professional journals were: A. C. Pigou (*Economica*), John Hicks (*Economic Journal*), Brian Red-

any of the major journals to do a review. We do know that Keynes sent him an advance copy of the book. Hayek sent back a letter of thanks in early February, noting that he was puzzled by some parts and troubled by others, and concluding, "If my present doubts remain I shall probably ask for your hospitality for some notes on particular points in the E.J." (Keynes 1979, 208).¹² This indicates that Hayek may initially have intended to do a review. Perhaps by the time he got around to asking for space, the *Economic Journal* had already promised the review to John Hicks, and Hayek simply decided not to pursue the matter further.

Though it is possible to reconstruct plausible scenarios along these lines, my inclination is to discount this as an explanation. Were this really the reason Hayek didn't write a review, why didn't he simply say so? And the answer is clear: Even if no one had asked him to write a review, Hayek could have done one if he *had* wanted to. As Donald Moggridge has noted, "The book was reviewed everywhere—in the daily press, the quality weeklies and monthlies, the professional journals" (1992, 592). There were many outlets for reviews, and economists (Hayek among them) frequently made use of them.¹³ Hayek was not *prevented* from doing a review; he *decided* not to do one. It is his decision that must be explained. Let's turn to the reasons he provided.

A.

Intriguingly, the part of Hayek's story that is the hardest to evaluate is that which he repeated the most often: the "Keynes would just change his mind again" thesis. It is certainly a plausible reason. Keynes's quick-

daway (*Economic Record*), Jacob Viner, with additional "Comments" by Frank Taussig, Dennis Robertson, and Wassily Leontief (*Quarterly Journal of Economics*), Alvin Hansen (*Journal of Political Economy*), C. O. Hardy (*American Economic Review*), and Abba Lerner (*International Labour Review*).

12. Keynes was the editor of the *Economic Journal*, and Austin Robinson the review editor. The latter tells the following story: "I was responsible for the review section of the *Journal*. I was given a ration of pages. We settled anything that needed to be settled on a Sunday morning, myself seated among the Sunday papers and the proofs of the *Journal* at the foot of Keynes's bed in his room at King's. . . . I remember telling him that I proposed to ask John Hicks to review the *General Theory*. I thought he was sufficiently detached from its genesis, though he was by then in Cambridge. He thought it a good idea" (A. Robinson 1990, 166).

13. The reviews reprinted in Hayek 1997, for example, are taken from professional journals, but also from the *Spectator* (a weekly magazine) and the *London Times*.

silver variability was well known. The public perception was not based on his switching of theoretical frameworks when he moved from the *Treatise* to *The General Theory*. Rather, it was due to his pragmatic (and hence often inconsistent, given the British economic turmoil of the interwar years) approach to policy. The most famous of his apparent flip-flops occurred in the spring of 1931. Hoping to forestall Britain's abandonment of the gold standard, Keynes came out in support of a revenue tariff, a move that reversed his longstanding and vocal support of free trade. When England left gold that summer, Keynes dropped his support for the tariff. This earned him the sobriquet of "the boneless man" in the press and led to the following wonderful twist on the old joke about how economists never seem able to agree: "Where five economists are gathered together there will be six conflicting opinions, and two of them will be held by Keynes."¹⁴

It is easy to imagine Hayek simply latching on to this widely held image of his rival as his excuse for not writing a review. If Hayek had simply said, "It was Keynes, after all, who authored *The General Theory*; why should anyone expect him to believe what he had written six months hence? no one would have contradicted him." What complicates things is that Hayek did not say this.

A careful reading of Hayek's accounts reveals that he does not play on the popular image of Keynes's variability. Rather, he keeps referring back to his personal experience in reviewing the *Treatise*. And what he may well have had in mind was the last of the twelve letters that he and Keynes exchanged during their controversy. Recall the sequence of events: the first half of Hayek's review appeared in August 1931. Keynes's aggressive reply (as well as a reply to the reply from Hayek) followed in November. Then began a lengthy correspondence that did not conclude until March 1932, the month after the second half of Hayek's review appeared in *Economica*. Keynes ended his final letter, dated 29 March, with these words:

Having been much occupied in other directions, I have not yet studied your *Economica* article as closely as I shall. But, unless it be on one or two points which can perhaps be dealt with in isolation from the main issue, I doubt if I shall return to the charge in *Economica*. I am trying to reshape and improve my central position, and that is

14. See Johnson and Johnson 1978 (17) on "the boneless man" and Jones 1954 (19) on the joke. See Moggridge 1992 (640–41) for another example of Keynes's versatility.

probably a better way to spend one's time than in controversy. (Hayek [1932] 1995, 172–73)

The last sentence seems a likely source of Hayek's claim that Keynes had changed his mind.

This, then, is the dilemma. Hayek really had experienced firsthand, and in a not particularly pleasant episode, Keynes's propensity to change his mind. But at the same time, to simply say that Keynes would probably just change his mind again has the ring of a handy excuse; a reason that would be immediately understood if given to a contemporary. Which was it; reason or excuse? Neither can be ruled out, and indeed, it may ultimately have been both: Hayek may have convinced even himself that Keynes's theoretical inconstancy was reason enough not to bother with writing a review.

We will never know, I suspect, how important this reason was in Hayek's failure to act. My own inclination is to take Hayek at his word in his later accounts: though it may have played a role, Keynes's variability was not the principal factor behind Hayek's decision. My justification for reaching this conclusion is pretty weak: it just seems to me that *other* reasons seem *more* important.

B.

Hayek's second claim was that *The General Theory* was merely "a tract for the times" that would surely soon be revised and hence not something to be taken seriously. This claim might seem plausible to a reader in the 1930s, for whom Keynes was known principally as a witty, caustic, and often brilliant commentator on the current economic and political scene, the author of both *The Economic Consequences of the Peace* and "The Economic Consequences of Mr. Churchill." Such a reader might well think of *The General Theory* as "Keynes's latest book," rather than as a major theoretical work.

But there are reasons to question this explanation, too. Keynes thought he was writing, and claimed to others to be writing, a seminal work in economic theory.¹⁵ In his obituary on Keynes, Paul Samuelson assessed the effort as follows:

15. Note this from Keynes's letter to G. B. Shaw on New Year's Day 1935: "To understand my state of mind, however, you have to know that I believe myself to be writing a book on economic theory, which will revolutionize—not, I suppose, at once but in the course of the next ten years—the way the world thinks about economic problems" (Keynes 1982, 42).

Herein lies the secret of *The General Theory*. It is a badly written book, poorly organized. . . . It is not well-suited for classroom use. It is arrogant, bad-tempered, polemical, and not overly generous in its acknowledgements. . . . Flashes of insight and intuition intersperse tedious algebra. An awkward definition suddenly gives way to an unforgettable cadenza. When finally mastered, its analysis is found to be obvious and at the same time new. In short, it is a work of genius. (Samuelson [1946] 1950, 148–49)

The General Theory was many things, but it can hardly be mistaken for a policy pamphlet. If one compares it to Keynes's other writings, it is best considered the heir of *A Treatise on Money*; it certainly has little in common with any of his writings on policy. Hayek's claim might resonate with certain readers in the 1960s, but only because at that time the "neoclassical synthesis" view that Keynesian analysis was nothing more than "a special case" (albeit a highly relevant one for policy) was popular.

The claim that *The General Theory* was "a tract for the times" is best viewed as an *accusation* by Hayek rather than as a reason for his failing to write a review. Hayek is retrospectively faulting Keynes for calling what (in Hayek's opinion) was no more than a tract a *general* theory and chiding a later generation for lionizing the perpetrator. Indeed, had Hayek actually thought this about the book in 1936, it would have provided a perfect *entrée* for a critique. One can imagine the opening sentence: "Mr. J. M. Keynes has written a tract for the times which he, for reasons known only to God and himself, has chosen to call a *general* theory. . . ."

C.

Hayek's third reason is his vague but persistent feeling that in order to write an adequate review he would have had to oppose the whole macroeconomic approach. This reason was mentioned twice by Hayek in the 1960s, and the second time he called it "decisive." On the other hand, in the 1980s he downplayed it.

This explanation, too, should probably be discounted. What is least believable about it is Hayek's claim that he had had a *vague* feeling of unease about macroeconomic approaches. There should have been nothing vague about Hayek's reaction to an approach that employed

aggregates. Opposition to the use of aggregates is a long-standing methodological principle among Austrians. In a work published in the 1920s on the U.S. economy, Hayek himself explicitly criticized the notion of a general price level (Hayek [1925] 1984).

The source of the Austrian opposition is that aggregates mask the movement of relative prices, and relative price movements are an essential component of all Austrian theory. Changing relative prices play a key role in the Austrian theory of the cycle, they underpin the Austrian critique of central planning, and they are central in Hayek's own (just then developing) vision of the informational function of market prices. Even more, in one of the few places that Hayek does offer a criticism of Keynes's *General Theory* during this period, he points out that Keynes's "economics of abundance" ignores relative price movements (Hayek [1941] 1975, 373–76).

In the 1960s Hayek had developed a deep interest in methodological work. Furthermore, Keynesian macroeconomics was at its apex. It is understandable that when reminiscing during this period he might link his failure to write a review to a methodological criticism of macroeconomics. But it seems unlikely that a "vague feeling of unease" about aggregates was a causal factor in his decision in the 1930s.

D.

We can now address together the two reasons that in my opinion make the most sense; Hayek's own "tiredness of controversy" and the fact that he was working on his own model. The two explanations go hand in hand.

There were at least two senses in which Hayek could have been tired. He was doubtless both physically and mentally tired after having fought a running battle with the likes of Keynes, Sraffa, Knight, and a disparate group of socialist planners. But in addition, Hayek may well have been tired of what surely must have struck him as *pointless* controversy. Hayek was reserved, even a bit austere, and always the perfect gentleman; Joseph Schumpeter once chided him for his habit of giving his opponents the benefit of the doubt (Schumpeter 1946, 269). Hayek must have been disconcerted by the debating club antics, the offhand but razor-sharp insouciance of Keynes and his well-bred Cambridge clique.

In the last of his letters to Hayek regarding the *Treatise*, Keynes hinted that he, too, might be growing weary. His last sentence read, "I am try-

ing to re-shape and improve my central position, and *that is probably a better way to spend one's time than in controversy*" (Hayek [1932] 1995, 173; emphasis added). It is interesting that Hayek chose the word "controversy," the same one used by Keynes in his letter, in describing his reticence to do a review.

But Hayek was not just tired of controversy. Soon after he had concluded his initial confrontation with Keynes he came to the stark realization that his own model had some severe problems. The problems lay with the Böhm-Bawerkian notion of an "average period of production"; that is, with the capital-theoretic foundations of his model. This was bad news indeed. Hayek had scored most of his points against Keynes precisely because of the absence of such foundations in the *Treatise*. This had also been the one place that Keynes had expressly admitted that Hayek was right. Were Hayek to give Keynes another chance to reply to one of his critical reviews, it is not difficult to imagine how Keynes might use the opportunity: "The reader will recall that Professor Hayek earlier faulted my *Treatise* for its lack of capital-theoretic foundations, but what is now clear is that my book lacked, unlike his, the *pretense* of a foundation. . . ."

Now this is not to say that Hayek accepted the criticisms of the likes of Keynes and Knight.¹⁶ But he did recognize that problems existed, and he was in the middle of an attempt to solve them. Rather than interrupt his effort in order to spar with Keynes, he decided to continue working on his own model. He thought (wrongly, it turned out) that he was near enough to completion that the profession would soon have before it two models of a monetary economy, and that his would be judged to be superior. This, in my opinion, was Hayek's reasoning, and is the principal reason why he decided not to review *The General Theory*.

Before concluding, let us return to one of Hayek's earlier reasons, his stated "vague sense of unease" about macroeconomics. Though we have raised questions about this explanation, there were other reasons that Hayek may have experienced some vague uneasiness in the 1930s. As I have argued elsewhere (Caldwell 1988), in the mid-1930s Hayek began questioning the ability of static equilibrium theory to accurately portray how markets worked. The equilibrium theory of his day assumed

16. Keynes quotes Henrik Ibsen in attacking Hayek in *The General Theory*. The passage is at once lyrical and nasty; that is to say, vintage Keynes: Hayek is likened to a "wild duck" who "has dived down to the bottom—as deep as she can get—and bitten fast hold of the weed and tangle and all the rubbish that is down there" (Keynes [1936] 1973, 183).

that agents possess full and objectively correct information, that is, that all the necessary data is already given to them. For Hayek, this approach ignores a fundamental question: How, in a world of dispersed and subjectively held information, do the plans of agents ever become coordinated? Hayek eventually favored replacing the equilibrium story of the workings of the market mechanism with what Austrians now call a market process story. In the latter, free markets are one of a number of institutions that can assist the process of social coordination. In good Austrian style, a central informational role in this story is assigned to freely adjusting relative prices.

This market process view provided the foundation for Hayek's critique of central planning. But it also had consequences for his own work. In a market process world, it might be possible to predict broad patterns of behavior, but not the sort of precise sequence of changes in relative prices that Hayek had outlined in *Prices and Production*. Hayek may have begun to recognize that the line of thinking he was developing in the 1930s posed problems for virtually any theoretical approach to social phenomena, including his own earlier efforts. If so, his vague but persistent feeling of unease may have been over this, rather than over the fact that Keynes was doing aggregate analysis.

6. Conclusion

Historical reconstructions are always provisional. Stories change as more evidence is discovered. In constructing our narrative we have been forced to put our trust in a famously unreliable source: the self-reports of one of the principals.¹⁷ What are the chances that new, more trustworthy materials might surface in this case?

The chances are probably slight. Not much correspondence from the 1930s remains in the Hayek archives at Stanford. In this regard, Keynes is a much more attractive figure for historians than is Hayek. Keynes was continuously on the move between Cambridge, London, and his vacation home at Tilton, and was an excellent correspondent; he was always writing to whomever was left behind at the other two places. The resulting documentary trail is massive. Hayek, on the other hand, spent most of his nineteen years in England (with the

17. We are fortunate in this case to have a number of self-reports by Hayek. Because the story was retold many times, its inconsistencies became clear. Oral historians who get a story only once should be particularly careful about the inferences they draw.

exception of the war period) in only one place: London. He would talk to people like Robbins (who was both a colleague at the LSE and a close neighbor in the Hampstead Garden suburb), rather than send letters. Throughout his life Keynes never left the Cambridge-London-Tilton axis for long, so there were fewer chances that materials might be lost. Hayek made a number of large-scale moves—Vienna to London in 1931; London to Cambridge and back in wartime; London to Chicago in 1950—during which materials might easily have been lost or discarded.¹⁸ Finally, few individuals could match Keynes's penchant for squirreling items away; even as a youth he kept meticulous records.¹⁹

It would be wonderful if some new stockpile of contemporaneous materials were suddenly discovered, as happened in 1976 when a large cache of Keynes's lecture notes and letters was found at Tilton. But even contemporaneous correspondence is not an infallible guide to an author's motives. Obviously, one can write with future generations in mind; or embarrassing entries can be discreetly removed, either by the figure in question, or by his heirs, or by others seeking to protect him; or, most fundamentally, intentions can be misread. When one speculates on motivation, there are many slips 'twixt cup and lip. Additional evidence simply provides more material that must somehow be fit into the historian's story. At best, it may help to eliminate certain reconstructions as implausible.

There is a final twist to our story. In the end Hayek did, in a manner of speaking, write a review, not of *The General Theory* per se, but of the whole Keynesian approach. For it turns out that in most of his accounts of why he did not review *The General Theory*, Hayek manages to slip in an assessment of Keynes's contribution. One of these even contained an inside joke that Keynes would have appreciated, even as it smarted. At one point in criticizing Keynes Hayek quotes the opinion of the "eminent" Victorian Leslie Stephen (Hayek 1995, 249). Keynes famously despised the Victorian era, of course, but much worse, Stephen was the

18. In a letter (1975) Hayek once explained the poor state of his correspondence as follows: "I have to confess that as a result of my repeated moves what older correspondence I have is in considerable disorder. But these moves also have been the occasion of recurrent ruthless destructions of accumulated letters."

19. See, for example, item number 13 in the "Personal Papers" file in the Keynes Archives at Kings College, Cambridge, where a complete listing of all of Keynes's golf scores for the period 1898–1905 are recorded. At least there was something at which he was bad; in the first round he ever played (aged 13) he shot a 256.

straight-laced father of two of his compatriots in the Bloomsbury group, Vanessa Bell and Virginia Woolf!

F. A. Hayek's formal and reserved exterior was like the skin of an onion, hiding many layers beneath. This is a final reason why we will probably never really know why he failed to review Keynes's *General Theory*.

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