

Life Writings: On-the-Job Training with F. A. Hayek

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Don't believe a word of what you read in this essay on the childhood influences that led me to become a scientist. Don't believe a word of what you read in the other essays, either.
—Steven Pinker, "How We May Have Become What We Are" (2004)

I notice this very much nowadays, how selective my memory is increasingly becoming. . . . Another phenomenon of which I have recently become aware—I sometimes wish I could return to psychology, I have so many ideas in that field—how much memory depends on having remembered the thing before. And if you have never remembered the thing before, usually it is gone.
—F. A. Hayek, interview with W. W. Bartley III, Freiburg (10 February 1983)

New Yorker cartoon, picture of a shocked Ronald Reagan sitting in a movie theater: Ronald Reagan suddenly realizes that a cherished childhood memory was actually a scene from a movie.

Like many here, I am a historian of economic thought. This means, first, that I am concerned mainly with only one branch of life writings, namely, intellectual history, and second, that I was trained as an economist, not as a historian. I had to learn my craft on the job, as it were. My mentor, or some might say guinea pig, in learning on the job was the economist

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Friedrich A. Hayek, on whom I chose to write. It turns out that he was of great help to me, though as I show (and he would much appreciate this), his assistance was often an unintended consequence of his own purposeful human action.

In approaching Hayek as a subject, I took my task to be the standard one of explicating the context in which his ideas were formed and the process by which they developed. I suppose that different people pose different challenges for those who try to play Boswell to their ideas. In Hayek's case, one problem that I faced was to try to offer a plausible account of the many twists and turns in his intellectual development. A thumbnail sketch of his intellectual journey should indicate why this was a problem.

As a youth Hayek had a strong interest in the natural sciences, with an emphasis on biology and especially botany. At the University of Vienna, where he enrolled directly following World War I, he sat in on many different sorts of classes, but concentrated on psychology and economics. When he finally embarked on a career in economics, his first contributions were in monetary theory and the theory of the trade cycle. Though he continued work in that area throughout the interwar period, he also began writing about the economics of socialism and about what would come to be called "the knowledge problem." In World War II he switched again, penning a popular book on the political implications of planning, *The Road to Serfdom*, that would gain him worldwide fame and, in some quarters, notoriety. He immediately switched yet again, beginning work in summer 1945 on a book on the foundations of psychology, *The Sensory Order*. The book, published in 1952, drew on his early student work on psychology. In the 1950s and 1960s Hayek wrote on political philosophy and the methodology of the social sciences, in the process developing theories of cultural evolution and of spontaneously forming complex orders. Few twentieth-century social theorists have contributed in so many different areas.

Trained as an economist, I had no special expertise in any of the other fields to which he contributed. So I knew at the outset that trying to understand, much less make sense of, his peripatetic intellectual journey was going to be a major undertaking. I took some comfort from the realization that, in this age of academic specialization, virtually anyone trying to deal with a figure like Hayek would (or should) encounter similar feelings of inadequacy.

I was heartened, though, when I stumbled upon a five-hundred-odd-page transcript of interviews that Hayek had done after having won the Nobel Prize. Undertaken under the auspices of the UCLA Oral History

Project, the interviews were done in October and November 1978. They were exceedingly rich: Hayek talked in clear prose both about the development and content of his ideas and about the many people he had encountered during his long life. These interviews clearly would be of great assistance in helping me reconstruct his path. I had also learned a great deal of background information about the Austrian school from the very first of the *HOPE* conferences, organized in April 1989 to commemorate the acquisition by Duke of the Carl Menger papers. I had already read a lot of what Hayek had written, and in the early 1990s I was invited by Stephen Kresge, then the second general editor of *The Collected Works of F. A. Hayek*, to edit two of the volumes in the series. The editing job would further deepen my knowledge of Hayek's work. What I had learned at the Menger conference would allow me in my book to set the stage for Hayek's entrance, and the interviews would allow me to block out his various moves on the stage. It seemed like a perfect plan. Ah, the sweet naïveté of youth (or actually, in my case, early middle age).

On-the-Job Lesson No. 1: Hayek Reminds Me of the Unreliability of Memory

Autobiographical accounts based on memory are unreliable, period. Steven Pinker, an experimental psychologist, tells us some of the reasons why. Certain important determining factors of our experience, genes and chance, are invisible to us. But we like to tell stories, so we make up causal sequences in order to form a coherent narrative. We also may want to spare our audience: "Accurate renderings of life are famously boring—just think of home movies." And last but not least, "We all want to look good" (Pinker 2004, 84).

Hayek apparently appreciated this, too, and like Pinker, he heightened the irony by telling us so in the midst of an interview in which he was reminiscing about his past.¹ Karl Popper's biographer Malachi Hacohen discovered it when, in comparing Popper's own autobiographical writings against evidence in the Popper archives, he found multiple discrepancies. Hacohen's (2000, 14) reaction is worth repeating:

1. Hayek's own research in psychology focused in part on the role of memory. See Hayek 1952 and chapter 12 of Caldwell 2004. I recognize that the approach taken in this essay, where I recall various experiences I have had in writing about Hayek, is similarly suspect.

[Popper] produced an anachronistic account of his intellectual development, reading his later philosophy back into the interwar years. While writing my dissertation, I suspected either amnesia or willful distortion, but neither seems necessary to explain his account (and neither is likely). Autobiographical anachronism is common, and Popper's memory failure may not even be as surprising as I still occasionally find it.

There seem to be a number of things going on here. First, what we recall is always selected from the vast amount of sensory input we receive, and different people select different things. Ironically, given what his biographer experienced, Popper himself used to illustrate this point (in trying to show that science was not based on induction but was a hypothetico-deductive system), when he would ask his students to observe what they saw in his classroom and to write it down. Obviously, without further instruction, each would select different things to write down. Furthermore, in autobiographical accounts, whatever gets selected is then integrated into a larger narrative—we construct stories about our lives, just as Popper constructed a narrative of the process of his intellectual development. Finally, with repetition our construction becomes what we remember, or at least the constructions are what we return to when others ask about us. This last point was highlighted by Donald Moggridge (2003, 597) who, quoting from a book on memory (by F. C. Bartlett), noted the following of the author's conclusions: "Accuracy of reproduction, in a literal sense, is the rare exception"; "with frequent reproduction the form and items of remembered detail very quickly become strengthened and thereafter suffer little change"; "in long-distance remembering, elaboration becomes more common in some cases; and there may be increasing importation, or invention"; and "detail is outstanding when it fits in with a subject's pre-formed interests and tendencies. It is then remembered, though often transformed."

Here is how Hayek helped me understand all this. I experienced the same combination of perplexity and suspicion that Hacohen did while I was editing the *Collected Works* volume titled *Contra Keynes and Cambridge*. Hayek and John Maynard Keynes had a rather famous public spat when Hayek arrived from Vienna to teach at the London School of Economics (LSE). Hayek precipitated the tiff by publishing a heavily critical two-part review of Keynes's *Treatise on Money* (1930), and Keynes did his share by replying before the second half of Hayek's review had come out and by quickly turning from defense of his own work to a blistering

critique of Hayek's own book on the same topic, *Prices and Production* (1932). Half a decade later Keynes published *The General Theory*, the book that would both establish his own reputation and give rise to "Keynesian policies" worldwide, policies that Hayek thought wrongheaded for reasons both theoretical and political. Hayek was never one to back down from a challenge. So the question arises: given their previous history, why did Hayek not review Keynes's *General Theory*? I found this to be a particularly interesting question historiographically, because it involved explaining why something *did not happen* rather than why something did.² This involves the author in a double burden: first, to establish why it is reasonable to expect something to happen, then to show why it did not. I thought it was a question that still could be answered, because, luckily, it was an episode that Hayek had talked about frequently. Indeed, I had a number of sources where he discussed it: a previously unpublished lecture that he had given in October 1963; an article published in 1966; the 1978 interviews, where he had talked about the episode more than once; an article published in 1983; and an interview with Bill Bartley that took place sometime in the mid-1980s.

This audience will anticipate what is coming next. As I documented in my *HOPE* piece on the riddle of the review (Caldwell 1998), there were two main problems with Hayek's accounts. First, over the years he had offered at least six different reasons for why he did not do the review. (In brief: he was tired of controversy; he thought Keynes would change his mind again; he thought *The General Theory* was a tract for the times and as such not worth a review; he would have to challenge Keynes's whole macroeconomic approach, and that was too large an undertaking; he was in the midst of preparing his own alternative model in *The Pure Theory of Capital* but by the time he had it ready they were on same side, fighting against inflationary wartime policies; he was anticipating that Keynes would soon come out against his own "Keynesian" followers, but then Keynes suddenly died.) Some of Hayek's reasons made more sense than others, so I thought I could still tell a plausible story.³ But

2. This problem of figuring out why something *did not* happen might be referred to, following Weintraub 1991, 54, as a "dog that did not bark" problem, after the Sherlock Holmes story "Silver Blaze."

3. I argued in my paper for a combination of "tired of controversy" and "in the midst of preparing his own model." Based on her examination of letters that Hayek had sent to Fritz Machlup and Gottfried Haberler in 1936, Susan Howson (2001) added another reason. A. C. Pigou published a sharply critical review in the May 1936 issue of *Economica*, the journal that would have been the logical outlet for Hayek's own review. Hayek apparently did not want to

there was another problem: whenever Hayek recounted a particular reason, he would use almost exactly the same language, word for word, even in sources that were separated by as much as twenty years in time! Quite clearly, Hayek was vividly remembering the story that he had told before, rather than the episode itself.

Like Hacothen had been with Popper, I was initially shocked, but now realize I should not have been. Indeed, now that I am conscious of the point, I realize that I have also been "guilty" of the practice. I am frequently asked to talk about Hayek, and when I do so (apparently "off the cuff"), I am now aware that I return to the same tried-and-true stories, and to ways of phrasing things, that allow me to make whatever point I am making most effectively. People do not like to sound like idiots in interviews; repeating passages that have worked before is a good way to try to avoid the problem.

A Partial Solution: Make Friends with the Archival Record

An obvious check on the reconstructions we call memory is the contemporaneous archival record. Of course, for this to be possible, an archive must exist. In Hayek's case, this is not a problem—it currently weighs in at 130 boxes, and more will soon be added. Archives, though, are often only a partial solution. They *can* help one to sort out dates and a sequence of events, and this can help one to see whether a subject's account makes sense chronologically. I think that Malachi Hacothen has very effectively demonstrated the use of archives for this purpose in his reconstruction of what Popper thought when.

The Hayek archives have yielded similar tidbits. For example, in an interview Hayek reported that he began work on *The Sensory Order* in 1946, but we know from his letters that he actually began the summer before. One can trace out rather exactly the painful progress (or, maybe better, the false starts and lack of progress) that Hayek encountered in writing *The Pure Theory of Capital* from his letters in the 1930s to Fritz Machlup, who provided detailed editing advice. Hayek's letters to Machlup during the war also allow us to reconstruct his steps in shifting from

appear to be "piling on," so he told Haberler that if he was going to submit anything it would be a note to the *Economic Journal*. But he never did. At the conference Craufurd Goodwin offered his own solution to the mystery: maybe no one asked Hayek to write a review! But, of course, had he really wanted to, Hayek could still have submitted that note to the *Economic Journal*.

the Abuse of Reason project to *The Road to Serfdom*. Perhaps the best example, though, has to do with the Beveridge memo.

In an interview Hayek (1994, 102) recounted that *The Road to Serfdom* started out as a memo to William Beveridge, the director of the LSE:

A very special situation arose in England, already in 1939, that people were seriously believing that National Socialism was a capitalist reaction against socialism. It's difficult to believe now, but the main exponent whom I came across was Lord Beveridge. He was actually convinced that these National Socialists and capitalists were reacting against socialism. So I wrote a memorandum for Beveridge on this subject, then turned it into a journal article.

From the interview it appears that Hayek wrote the memo, or gave it to Beveridge, in the late 1930s. This was my presumption when I searched for the memo in the Hayek archives. I never found it, so I figured it had long been lost, a victim of one of Hayek's moves.⁴

At a conference in Cambridge in June 2004 I mentioned the missing memo, and another conference participant, Sue Howson, said brightly: "I've seen that memo in the Beveridge papers." Being Sue, she had a photocopy of the memo squirreled away in her attic in Cambridge, and she was kind enough to get it out and make a copy of it for me that very evening.

I knew I had seen the memo before, in the Hayek archives. Once I got home and compared the two, it was clear they were identical as far as the text was concerned. One difference was that the Hayek archives copy was dated "Spring 1933"; according to the memo's content, Hayek was referring to events taking place in May of that year. As a result of Sue's sleuthing, we had found the memo and could date its composition to May or June 1933. The memo is reproduced in the *Collected Works* edition of *The Road to Serfdom* (2007).

The reason I had not realized that the memo was hiding in plain sight all this time is that I had taken at his word Hayek's reminiscence that he had written it in 1939.⁵ There are two lessons here: to take reminiscences

4. In a 1975 letter Hayek once explained that his moves "have been the occasion of recurrent ruthless destructions of accumulated letters."

5. Hayek turned his memo into a magazine article published in 1938, which then became a longer public policy pamphlet published in 1939; both of these are reproduced in Hayek 1997. Hayek may have confused the date of the memo with that of the later work that grew from it.

with a grain of salt and, whenever possible, to consult *multiple* archival sources.⁶

There are some questions, though, for which archives will not be of much help. Most typically, these are questions not of timing but of motivation. Evidently, even letters that purport to explain motives need not be accurate, since such explanations are as likely to be self-serving, as our memories often are.

In addition, some questions by their nature simply resist being answered. The question I have most frequently been asked is, why did Hayek not get an appointment in the Department of Economics at Chicago instead of the Committee on Social Thought? Hayek had his own opinions about this, and so did on-the-scene contemporaries like John Nef and Milton Friedman. Multiple stories abound. But anyone who has sat in on a contentious tenure case knows that, even for people who witnessed the deliberations, multiple stories end up being constructed, complete with hypotheses about the "stated" versus "real" reasons for whatever decision gets made. In such cases documentary evidence itself, even when it exists, can be challenged. If *Wittgenstein's Poker* (Edmonds and Eidinow 2001) has taught us anything, it is that reminiscences of events that later come to be viewed as significant are likely to allow multiple reconstructions and hence, in the end, to remain ever mysterious.

On-the-Job Lesson No. 2: Hayek Forces Me toward Thick History

I prepared for writing on Hayek in the usual way. For example, to do the *Contra Keynes and Cambridge* volume, I read what Hayek and Keynes had written, then read lots of secondary sources about what they had written. I did the same thing for the volume on socialism. The literatures in each case were enormous, so I thought I was doing a good, careful academic job. But in the middle of the process I suddenly realized that if I continued on my path I would be guilty of doing thin history.

In the early 1990s (at least this was when I became aware of it) historians of economic thought began talking about thick versus thin history. (I suspect that this distinction will amuse historians.) Thin history limits

6. Of course, such sleuthing may as easily lead one to discover further inconsistencies in a story as to settle on an apparently coherent narrative. But it should at least help one to get the dates right. As Hachohen showed, it can also bring to light episodes that were not part of the person's own narrative.

itself to looking at economic texts when reconstructing an argument or episode in the history of ideas. Thick history aims at contextualizing those arguments, trying to identify the multitude of other factors—political, social, cultural, and what have you—that might have had an effect. For me, the exemplar of thick history was Peter Clarke's ([1988] 1990) book on the lead up to the Keynesian revolution. I still use his and Bradley Bateman's discussions of Keynesian historiography, published in *HOPE* in 1994, in my history of thought class (Clarke 1994; Bateman 1994).

Though I am loath for various reasons to describe myself as a thick historian (a practitioner of thick history at least sounds a little better), it was Hayek who helped me achieve whatever thickness I might claim. He did so by being such a slack, and at times inaccurate, citer of sources.

I found this out while working on the footnotes for the *Collected Works* edition of *The Road to Serfdom*. Part of what we are trying to do with the *Collected Works* is to check Hayek's quotes for accuracy. Well, for *The Road to Serfdom*, a good 50 percent of the quotes were inaccurate, mostly in niggling ways: he would drop words out, or make small changes in phrasing. There were so many of these that I had to change my initial editorial decision to put a note in for each correction, and instead only noted the major discrepancies. These were the cases, again not few, where he would get the page number, or even the name of the source, wrong. An even more maddening habit Hayek had was to directly quote someone but not say whom he was quoting or where the quote could be found. Sometimes he would provide hints: for example, he might say, "as was recently stated in a leading article in *The Economist*." It was my job to try to track all these things down.

I will, for your delectation, mention just a few of the more challenging cases. In chapter 12 he quotes extensively from nationalistic and socialist books published in Germany during and right after World War I, and in the next chapter he quotes extensively from books by the historian E. H. Carr and the natural scientist C. D. Waddington. He provides no page citations for any of the quotes, all of which I had to find myself. In the foreword to the 1956 American paperback edition of the book, he recounted an episode that surrounded a scurrilous passage that was supposed to be found in an obscure book on the book publishing industry. I could not find the passage in the copy I got through interlibrary loan, and almost gave up before discovering after looking at the University of Chicago Press archives that a threatened lawsuit caused the author to excise the passage in subsequent printings of the book. I would have had to have had the first

printing of the obscure book to have found the passage. A final example: in chapter 11 Hayek (2007, 177) poses the following challenge:

I leave it to the reader to guess whether it was in Germany or in Russia that chess-players were officially exhorted that "we must finish once and for all with the neutrality of chess. We must condemn once and for all the formula 'chess for the sake of chess' like the formula 'art for art's sake.'"

If you want to know the answer, you will find it in the *Collected Works* edition.

Anyway, in trying to do my job as the editor of Hayek's books I was forced to at least scan, and sometimes read, and truth be told, sometimes read multiple times, many long-forgotten books. Doing so helped me realize that he was responding to many different contemporary developments and arguments. Especially in *The Road to Serfdom*, many of these did not involve economists.

Working through a person's footnotes, reading those to whom he was responding, slowly but surely allows one to enter into his head and into his times. If I am not yet a thick historian, I am perhaps a little bit broader. I owe it all to Hayek's inability to get his citations right. But it is a policy that may fruitfully be followed by biographers in general, not just those who are checking footnotes for accuracy.

On-the-Job Lesson No. 3: Utilize the Expertise of Your Peers

So far I have said some pretty pedestrian things—all of them, however, have the virtue of being true.

No, I am not crazy enough to get into an extended discussion of the nature of truth. But keeping things on a pedestrian, or perhaps fundamental, level: in at least one respect, for a historian truth simply means trying to get the facts right. Working on Hayek taught me another lesson: getting the facts right is painstaking work.

When I first started out as an academic, my research was in the area of economic methodology. When I did anything that bordered on history, I realize now that what I was actually doing was much closer to a rational reconstruction, or even conjectural history, than to history. In such endeavors, offering a coherent account, or telling a good story, trumps getting the facts right. I got a vivid reminder of the historian's obligation when I

received the reader's report from the University of Chicago Press for my editor's introduction to the *Contra Keynes and Cambridge* volume. The reader was Donald Moggridge, and he provided me with four single-spaced pages of nit-picking. And I am eternally grateful for it, for he saved me from many public errors. I got similar reports from two anonymous referees on *The Road to Serfdom*. One of these actually corrected some of my *explanatory* footnotes, which in fact had offered erroneous explanations. All I can say to that particular anonymous referee is—thank you. You get the Moggridge prize.

Even if you think you have gotten the facts right, though, history is, in the end, an interpretive exercise. People can, and will, read the exact same facts differently. We are in the business of interpretation, and we always bring a point of view to our work.

I actually found this out in college when, at home over summer vacation, I was having a phone conversation with my college girlfriend, who lived many states away. I was in the early stages of trying to break up with her. (This is my memory anyway!) Afterward I heard my nosy, eavesdropping mother describe the conversation to my father, telling him how this scamp of a girlfriend was mistreating me! A mother's bias is an understandable thing, but it is not limited to mothers. Bill Bartley was going to be Hayek's biographer, and he conducted an extensive series of interviews in the 1980s to collect information on Hayek's past. Bartley was a philosopher, a student of Popper's, and was also to have been Popper's biographer. When I read the interviews, it was evident that his Popperian philosophical background shaped the questions and follow-up questions he asked. Bartley's approach, from my own biased perception, was clearly biased.

This leads me then to my second piece of advice about making use of the scientific community in which you live: once you think you have gotten the facts right, find someone to read your manuscript who knows the facts as well as you do, but who differs on their interpretation. This will at least serve as a prophylactic against *wildly* biased readings. For me this person is Phil Mirowski. We have similar interests, and often agree on the facts, but we seldom offer similar readings. Phil as prophylactic—I like the sound of that.

Finally, if your project is a really big one, like a book, it is easy to make the mistake of getting the details of each chapter right while forgetting that those chapters are supposed to feed into a larger theme. Readers, like everyone else, want coherent narratives. To the extent that my own book, *Hayek's Challenge*, has any hint of coherence, it is because Mary Morgan

did me the wonderful favor of doing an "editor's read" of my first draft. She was kind but firm in her instructions to me, a few of which I reproduce here:

At the level of chapter subsections and paragraphs, it all goes very smoothly. . . . At the level of the book as a whole, and of the shape and content of some of the chapters, I found myself left with a number of queries.

What role do you want Part I to play in the overall book? What is "the story" or explanatory strategy within Part I? . . . What is the split between Part II and III?

You should keep the history clear: [chapter] 17 should be in Part II, and 18 should be an epilogue to the whole book. But maybe it is not part of this book at all.

And most bluntly:

How is this held together?

It is never easy to read such things, but their value in helping one draw the threads of a manuscript together is inestimable.

Conclusion: Some Aphorisms to Work By

I have handed out a lot of unsolicited advice here, so I will close by reducing it all to a few on-the-job aphorisms for intellectual historians: Use oral history accounts with caution. Make friends with the archival record. Read not just the footnotes but the stuff referred to in the footnotes. And do not forget the three Ms: find your own counterparts to Moggridge, Mirowski, and Morgan. Your work will be better for it.

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Reflections of a Marshall Biographer

Peter Groenewegen

It is now more than a decade since my biography of Alfred Marshall was published by Elgar, and two decades since I commenced my preliminary work thereon at Cambridge in 1984. Its introduction showed a deliberate stance in writing biography, as concisely summarized in its final paragraph: "It is in Viner's spirit that this first full-length life of Marshall is written. It attempts to present him, insofar as that is possible, warts and all. It is fully concerned with preserving Marshall's social philosophy, linking it with his economics to prevent both being reduced to a period piece and to enable savouring them in their full flavour" (Groenewegen 1995, 16). The following reflections intend to reinterpret the methodological implications for the "scientific biographer" of this brief statement of intent. What is the Jacob Viner stance on intellectual biography with special reference to Marshall? What are the potential constraints on a "warts and all" biography? Why does economic biography need to be aware of the subject's social philosophy and, more generally, the intellectual and other background to the subject's life and work, and what special value is there for the historian of economics in this particular approach? Moreover, it is easy to argue that a biographical study in the case of Marshall assists in understanding the evolution of his economics from the 1860s to the 1920s. It also enables destruction of myths about Marshall and helps place his economics in the appropriate perspective. To address some of these issues, this essay is divided into four sections. Section 1 examines Viner's