

Postscript to the Japanese Edition (April 2022)

A False Start

Preliminary thinking for this book began in 2004. The original idea was to update our archival work during the previous fifteen years and write a sociology of the genesis of the Keynesian revolution at Cambridge by investigating the conditions under which John Maynard Keynes wrote *The General Theory of Employment, Interest, and Money* (1936). This idea, which enjoyed only a brief life, was abandoned on several grounds. Yet another book with the spotlight on Keynes? The three-volume biography by Robert Skidelsky had already been published. Although Skidelsky handled Keynes's economics with a light touch, the scholarly literature in the history of economics seemed amply supplied with studies of *The General Theory*, the motives that led Keynes to write it, and the circumstances of its composition and early reception. Could a discriminating university press be persuaded to enter a market that was already marked by vigorous competition? And even if the manuscript were published successfully, the presumed readers would confront an opportunity cost. In view of the brevity and furious pace of life, would the payoffs be sufficient to outweigh the costs of tackling yet another book that placed Keynes in the center of the arena? Even for economists who live off or for Keynes and Keynesianism, an obvious question was at stake: Would the book merit the effort? More important, our plan seemed to rest on a fallacy. Revolution-making is not book-writing, and the Keynesian revolution was not *The General Theory*. In which case, it would be a mistake to conflate the ascendancy of Keynesianism at Cambridge with the origins of *The General Theory*. The

formation of the Cambridge revolutionary party of the 1930s, an enthusiastic faction of Keynesian activists in the economics faculty, cannot be explained by any account, regardless of how thorough, of changes in Keynes's thought that led him from the premises of *A Treatise on Money* to the mode of analysis that became *The General Theory*.

Rethinking: Joan Robinson in the Foreground

Concluding that our initial plan was at a dead end, we reconsidered how the dynamics of the Keynesian revolution at Cambridge could be analyzed more intelligently. Our project required a narrative structure for elucidating doctrinal shifts in how economics should be done as well as changes in the local disciplinary culture: ethical and other axiological imperatives and social etiquettes on the basis of which collegial affairs were conducted. Between the end of World War I and the early 1930s, the economics department functioned as a small guild-like society. Three of its underpinnings seemed especially important for our purposes: the enclosed and exclusive social spaces of the medieval colleges of the university, defined by both architectural design and academic governance; a remarkably efficient intramural mail system operated by college employees that enabled a fellow of one college to send a letter to a colleague in another at breakfast and receive a reply before lunch, permitting several iterations of epistolary dialogue in the course of a single day; and a tradition of unfettered and informal sociality, in which generational differences, distinctions in academic rank, and appointment to college or university posts had little significance.

It is not too much to say that the norms and expectations of this little world experienced a systemic shock between late 1930, when Keynes and a few of his young admirers began to rethink the assumptions of the *Treatise*, and 1937-38, when the department was taking stock of

The General Theory and drawing conclusions regarding its implications for the future of economics at Cambridge. Several shocks to the system proved to be paramount. The Cambridge Marshallian tradition, both as a doctrine and as a culture of research and teaching, was destabilized. Threats to the old order created opportunities for those who saw advantages in joining an exciting heterodox movement. At the same time, devoted and uncompromising followers of Alfred Marshall, who found a host of difficulties and sources of obscurity in the Keynesian program, solidified their opposition. Alliances shifted and friendships were strained. Finally, a novel conception of economic science as revolutionary struggle radicalized and personalized pedagogical conflicts over the training of the next generation of Cambridge economists and the ensemble of skills they were expected to master. The emerging Keynesian order, it seemed, entailed a new ideal of what it meant to be an economist. On the part of both revolutionary partisans and Marshallian loyalists, there was a disposition to transpose their differences into a zero-sum game of low-intensity warfare, in which everything of significance in economics at Cambridge seemed to be in the balance.

As we reflected on these considerations, a narrative structure for our project became evident. Joan Robinson's career began just as the *Treatise* had been published, and it flourished as her partisanship on behalf of Keynesianism became increasingly zealous, truculent, and confident. As late as 1929-30, she was a faculty wife—although never a housewife. In the early 1930s, Keynes feared that she might threaten the careers of both Austin Robinson and Richard Kahn because of her liaison with the latter—Keynes's favorite student, protégé, and eventually a family friend and frequent visitor to his country estate. As Keynes's thinking became increasingly remote from the Marshallian textual hermeneutics of Dennis Robertson, with whom he had collaborated closely in the 1920s, Kahn became the colleague whose comments on his

work he esteemed above all others. Was Robinson an academic femme fatale whose entanglement with Kahn might lead to a catastrophic end for all parties? And yet by 1933, she had drafted and revised *The Economics of Imperfection Competition*, the book that established her reputation as an important new voice in microeconomic theory. It was published by Macmillan, the publisher of both Arthur Cecil Pigou and Keynes, and on Keynes's recommendation. In June 1935, Keynes asked for her comments on the galley proofs of *The General Theory*, a request that included an invitation to Joan and Austin Robinson for dinner with Keynes and his wife, followed by a concert. After *The General Theory* appeared, the relationship between author and critic seems to have been reversed. In 1936-37, Robinson wrote two books designed to advance Keynes's program: *Essays in the Theory of Employment*, which demonstrated how his analytical apparatus could be deployed to investigate technical issues of theory and policy that he had not considered, and *Introduction to the Theory of Employment*, a simplified and lucid summary of his basic assumptions and main lines of analysis—the first textbook on Keynesian economics. Keynes responded to her drafts with reservations, objections, and suggestions, and a spirited correspondence ensued. In February 1938, the senior Cambridge economists recommended her for a university lectureship, an appointment that duly ensued. Thus she became a fixture of the economics faculty, one of its leading theoreticians, Keynes's trusted lieutenant, and the chief architect of the revolutionary strategy pursued by the original Keynesians.

How did all this happen? How was Robinson able to write *The Economics of Imperfect Competition* only three years after beginning serious study of economics? What reassessment of her research priorities led her to break off work on imperfect competition and take up arms on behalf of a proto-Keynesianism that was only obscurely articulated and barely understood in the

early 1930s? How did she succeed in refashioning herself in Keynes's imagination from Austin's wife—first gaining his patronage, then becoming a commentator on whom he relied for advice, and finally achieving the status of chief expositor and propogandist of the early Keynesian revolution, the Leon Trotsky of Cambridge Keynesianism? We offer some answers to these questions by pursuing fresh analyses that position Robinson in the foreground of the dramas played out in the Cambridge of the 1930s.

Cambridge economists of Robinson's time were devotees of the personal letter, conducting their lives in exchanges of correspondence with colleagues. As a result, letters were a favored research site in which new inquiries were initiated, comments were sought and offered, and ideas were honed, clarified, revised, and occasionally discarded. Much of this correspondence remains intact in the archives of the university, an embarrassment of riches that enabled us to follow the young Robinson from 1930, when she met with several other young economists in trying to understand Keynes's *Treatise*, to 1938, when she received a university lectureship and her nemesis on the economics faculty, Dennis Robertson, left Cambridge for a professorship at London University. The wealth of the Cambridge epistolary material suggested our narrative strategy: we wrote the story of the formation of Joan Robinson's professional identity and the production of her career by reading the letters she and her colleagues exchanged, acting on the historiographic principle of following the primary sources of the 1930s. Although this is not a novel approach to the study of Cambridge economics (see Marcuzzo and Rosselli 2005), we have perhaps followed it more uncompromisingly and systematically than other scholars.

The Dardi Review: Critique and Anti-Critique

And perhaps also to excess? This is the view taken in a chiefly sympathetic and scrupulously fair review by Marco Dardi of the University of Florence. Our fundamental historiographic choice, he claims, “has the effect of magnifying the day-to-day concerns and emotions, the reactions to passing events, the self-interpretations of the moment, without the offsetting benefit of the ‘colder’ ex post rethinking.” Although Dardi admits that memory is a “distorting mirror,” the same may also hold true for “immediate perceptions,” which apparently refers to the contemporaneous evidence of their thinking produced by historical actors. In his argument, evidence of historical acts and artifacts is either contemporaneous or after-the-fact. Since both types of evidence may constitute misrepresentations, “a combination of the two kinds of evidence would seem to offer at least a chance for reducing the average distortion” (Dardi 2011, 203). We have spelled out our reasons for not supplementing the epistolary record of the 1930s with retrospective accounts by Robinson’s post-war colleagues or students or with recollections by a later version of Robinson herself. In writing the book, however, we did not consider Dardi’s more general skepticism based on his mirror-of-distortion argument. There may be some value in considering, even if briefly, its consequences.

Dardi provides no basis for the view that all historical representations may, in principle, be misrepresentations. Is this a tenable position? Consider his metaphor of historical accounts as mirrors of historical reality. How can it be established that a mirror distorts what it reflects? Only by comparing the mirror image with the unreflected object. Dardi’s argument requires two independent entities: a historical account and a historical act or artifact to which it refers and with which it can be compared. However, historical entities are accessible to us only through accounts, either contemporaneous or retrospective. In his argument, all of these accounts, including those that express the “immediate perception” of the historical actor, are potential

distortions. It follows that there is no original entity that escapes potential distortion by the mirrors of historical evidence. Or, if the historical act or actor is regarded as the original, it is also a potential distortion—a mirror image that distorts itself. This means that an indispensable condition for the comparison on which Dardi’s argument rests is not satisfied.

Without a solution to this problem, the proposal to combine contemporaneous and retrospective accounts ends not in a reduction of “average distortion” but rather a chaos of historical accounts, each of indeterminate accuracy, truth, or validity since there are no grounds on which these determinations could be made. Put rather more dramatically, Dardi’s position threatens to end in a nihilistic epistemology of history, which could hardly have been his intention. Our more modest strategy is simpler, less ambitious philosophically, and much less risky. A phenomenological social history of Robinson’s rise and ascendancy in Cambridge at the beginning of the Keynesian revolution is of course not the only legitimate approach to the territory of this book. However, the lineaments of such an analysis—built close to the ground of the intentions, objectives, rationales and motives of the actors: the meaning that they ascribed to their conduct—are presuppositions of any alternative account.¹

¹ For a general argument in support of this view of the interpretation of action, cast in rather different terms, see Quentin Skinner’s classic paper (1968).

References

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