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JOAN ROBINSON'S "SECRET DOCUMENT" A PASSAGE FROM THE AUTOBIGRAPHY OF AN ANALYTICAL ECONOMIST

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I. THE TEXT

The Modern Archives, King's College, Cambridge University contain a carbon copy of a three-page single spaced manuscript with the title "A Passage From The Autobiography of an Analytical Economist" (RFK/16/2/134-139, hereinafter "Autobiography"). Joan Robinson's initials are typed at the end of the document, which is dated October 1932.[1]

In October 1932, Heffer, the Cambridge University student bookstore, published Joan Robinson's methodological pamphlet, *Economics is a Serious Subject*, and she delivered the manuscript of *The Economics of Imperfect Competition* to Macmillan (Joan Robinson to Richard Kahn, October 30, 1932, RFK/13/90/1/19). The Autobiography was apparently drafted shortly after these two projects were completed. The typescript in Modern Archives, which seems to be the only extant copy, was not made until some months later. In a letter of March 2, 1933, Kahn suggested adding "a long section to your secret document if you can do so without spoiling it," regretting that he had not asked her for a copy (RFK/13/90/1/162-67). She replied somewhat mysteriously, alluding to a superstitious reluctance to having it typed but admitting that eventually it would have to be done (March 23, 1933, RFK/13/90/1/205-208). Since the carbon copy refers to page 275 of her book, the Autobiography was not typed until she had seen the final set of page proofs, and perhaps not until the book had appeared. The Autobiography also shows that she had drafted

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[1]See Appendix A. Unpublished writings of Joan Robinson copyright. The Provost and Scholars of King's College Cambridge 2006. With the exception of typographical corrections, this text reproduces the copy in Modern Archives. References to the Autobiography in this essay are to the number of the part (I-III) and the number of the paragraph within that part: thus I, 1 refers to the first paragraph of part I. References to the two introductory paragraphs are to "first paragraph" and "second paragraph." Students of Joan Robinson's work will not be surprised to learn that the Autobiography is written in a tendentious fashion, inviting criticism at many points. This essay is limited to an analysis of the Autobiography and its place in the development of her views on economic theory in the early 1930s.

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"The Theory of Money and the Analysis of Output," published in the October 1933 issue of the *Review of Economic Studies*. Thus there is no doubt that she revised the October 1932 text before finally handing it to a typist. Joan Robinson's correspondence with Kahn shows that she was interested in publishing the "secret document." Although her husband Austin Robinson advised against it, his reasons remain obscure (Joan Robinson to Richard Kahn, January 31, 1933, RFK/13/90/1/84-87).

Perhaps any unpublished and unremarked manuscript on economic theory by Joan Robinson would stimulate interest. However the Autobiography is an especially rich and resonant text. It offers a brief account of the genesis of *The Economics of Imperfect Competition* (Joan Robinson 1933a), the work that established her status as a bona fide member of the Marshallian guild at Cambridge and won her an international reputation. It sketches her philosophy of economic science, the metatheoretical foundation on which, she claimed, the book was written. And it comments in an idiosyncratic way on the relationship between analytical economics and economic genius.

II. THE BIRTH OF *THE ECONOMICS OF IMPERFECT COMPETITION*: TWO STORIES

The Economics of Imperfect Competition seems to have begun in a fortuitous and perhaps even serendipitous fashion. In its foreword, Joan Robinson observed that the book had its origin in her attempt to resolve several problems of economic theory by using a new analytical technique: the marginal revenue curve, a classic case of simultaneous discovery for which several economists had a legitimate claim to credit.[2] What were the circumstances that led her to apply the marginal revenue curve to problems then on the agenda of Cambridge economics?

In 1990, Austin Robinson wrote a remarkably comprehensive and detailed answer to this question, going so far as to recall the micro-social interactions and the constellation of economies of research interests and opportunities that came into play (Austin Robinson 1994). It was early 1930 and Austin was supervising Charles Gifford, a Magdalene College mathematics student who had taken up economics and would go on to win a first in Part II of the 1931 Tripos. Gifford had written a weekly tutorial essay replicating the marginal gross revenue curve in Theodore Yntema's article "The Influence of Dumping on Monopoly Price" (1928). On the day Gifford read his essay to Austin, Joan had invited Kahn to lunch with the couple. Austin appeared, as he recalled, "bubbling with excitement over the new concept" and eager to explain it to them. In his account, this conversation over lunch marked the genesis of *The Economics of Imperfect Competition*. The three began to play a game by using Yntema's curve to solve a variety of problems. As they moved from one question to another, the game became more complex and ambitious. Ultimately, it centered on a large and fundamental issue: if economic theory replaced the axiom of pure competition with the assumption of imperfect competition, what consequences would follow for the behavior of industries and firms?

[2]Drawing on Kahn's unpublished research for a lecture he gave at Harvard in February 1933, Joan Robinson identified Roy Harrod, Erich Schneider, Heinrich von Stackelberg, and Theodore Yntema as independent discoverers of the marginal revenue curve (Robinson 1933a, pp. xi-xii). See Kahn's letter to her of February 15, 1933 (RFK/13/90/1/131-138).

If Joan Robinson took the initiative in pursuing this problem, the reasons were not difficult to fathom. Kahn, Austin noted, was still a research student at work on the dissertation that would qualify him for a fellowship at King's. Austin himself had a full schedule of lecturing and supervision and would shortly move into administrative work. This left Joan. Although he did not spell out these details, her links with Cambridge were purely informal. A freelance supervisor tutoring students at her home, she had no college or university appointment and no formal connection to Cambridge economics. With domestic help, no job, and no children, her responsibilities were limited to household management as performed by an upper-middle class English matron of the time. Thus the economies of research time operated in her favor. And unlike Austin and Kahn, she had an incentive for pursuing a new and promising line of research. Without a First Class degree, a college fellowship, or a powerful patron, she could achieve her ambition of becoming a Cambridge economic theorist only by publishing an impressive original work.

Joan's more succinct account of the genesis of her book in the Autobiography, the two brief paragraphs that constitute its introduction, departs from Austin's story in several respects. The lunch with Kahn disappears. Instead, Austin introduced Joan to the curve and she was left to ponder its significance. Just as there was no lunch, there was no game the three economists played with the curve, and there was no flash of insight in which its theoretical promise instantly became apparent. Following her introduction to the curve, Joan made no immediate use of it. Nor did she do so until one day when Kahn posed a problem to her and she responded: "Let's see if Gifford's curve will do it for us. And it did. One problem led to another, and all the time Mr. Sraffa's article of 1926 kept nagging at us:—What about the supply curve in an imperfect market?" (Autobiography, second paragraph).[3] Austin claimed that Joan initiated the work and proposed the research questions. In the Autobiography, she and Kahn shared this responsibility: he raised a question and she suggested they try to answer it by using the curve. In this passage of the Autobiography it is clear that "us" refers only to Joan and Kahn, not to Austin, who had no part in this conversation. If it makes sense to say that *The Economics of Imperfect Competition* was born at a certain point, its birth was this conversation.

In the foreword to her book, Joan acknowledged Austin's contribution on several points. He named the curve, gave her illustrative empirical detail drawn from his own work *The Structure of Competitive Industry* (1931), and suggested what finally proved to be the book's title—a matter that was settled quite late in the history of the manuscript and in discussions that included Austin, Kahn, and John Maynard Keynes (Joan Robinson to Richard Kahn, January 23, 1933, RFK/13/90/1/73-77). However, with the exception of his function as messenger in delivering the news of the marginal revenue curve, Austin played no other role in its genesis. Therefore, she radically excised her husband from the origin of the book.[4]

In view of the discrepancies between these two accounts, what can be said with reasonable probability concerning the question of when Joan Robinson was introduced to the marginal revenue curve? The Autobiography begins with the observation that

[3]See Sraffa (1926).

[4]The deletion of Austin from the origin of the book conforms to Joan's letters to Kahn of the time as well as her reminisences published some forty-eight years later. See Joan Robinson (1979, pp. ix-x).

"three years ago" Austin showed Joan the diagram from Gifford's essay. Does this mean that she first learned of the curve in autumn 1929? This seems unlikely.

Kahn became acquainted with the marginal revenue curve through the Robinsons. However, he did not mention it in his fellowship dissertation, which he submitted in December 1929. In preparing the final draft, he surveyed the recent literature on his topic—the economics of the short period—and made appropriate changes (Kahn 1989, pp. 105-107). This was an exercise for which Pigou later chastised him: if Kahn insisted on repeatedly revising his text in light of current research, the dissertation would never be published (Pigou to Kahn, n.d. [circa spring 1930], RFK/2/8/18-19). If Kahn had seen the marginal revenue curve in 1929, references to some of its discoverers would have appeared in his dissertation, and he would surely have used the concept in his own analyses. Note that in Kahn's analysis of individual firms in imperfect markets, the vertical line that determines equilibrium price and output transverses the marginal cost curve. Where? Precisely at the point where it would have intersected the marginal revenue curve (see Kahn 1989, pp. 119-20). The conclusion: he and Joan had no knowledge of the curve prior to 1930.

In view of the fact that the Autobiography was not typed until spring 1933, this conclusion is supported by Joan's claim that she learned of the curve three years prior: in 1930, three years before her book was published; not autumn 1929, three years before October 1932. When in 1930? In the Autobiography, we find that her knowledge of the curve preceded Roy Harrod's paper, "Notes on Supply," published in *The Economic Journal* in June 1930 (Autobiography, second paragraph; see also Joan Robinson, 1933a, p. vi). Thus it seems that Joan Robinson first saw the marginal revenue curve after December 1929—when Kahn submitted his thesis, and before June 1930—when Harrod's article appeared.

Are there lessons here? Consider the inconsistencies between Joan's laconic account in the foreword and the Autobiography and Austin's more colorful reminiscences, which call to mind a novel by C.P. Snow on life in a Cambridge college of the 1930s. When Joan was writing, she was present at the creation. Austin was writing at a distance of six decades. Joan could hardly be expected to falsify her account. Both the Autobiography and the foreword were read by Austin and Kahn, the two men who knew most about the book's origin. If Austin's account was accurate, why did he fail to correct the Autobiography and the foreword? If his version of events was true in 1994, it was also true in 1933. These considerations are grounds for skepticism concerning Austin's narrative. At a minimum they show that it should not be elevated to the status of an authoritative chronology.[5]

III. TECHNIQUE: THE UNMOVED MOVER

The Autobiography includes some witty remarks on the philosophy of science that Joan Robinson claimed she followed in writing *The Economics of Imperfect*

[5]See Annalisa Roselli's recent illuminating essay on the correspondence of Joan Robinson and Kahn (2005), which seems to treat Austin's story as historical fact.

Competition. They draw on her methodological pamphlet of fourteen pages, a short text with a stunningly pompous title—*Economics is a Serious Subject: The Apologia of the Economist to the Mathematician, the Scientist and the Plain Man*. Did mathematicians and natural scientists need reassurance that economists were engaged in serious pursuits? And what of "the plain man," a favorite artifact of Joan Robinson's philosophical contemporaries—graduates of elite public schools who had made their way through Oxford and Cambridge to read learned essays on the epistemology and ethics of "the plain man" at meetings of the Mind Association, the Cambridge Moral Science Club, and the Aristotelian Society? In fact, these questions were quite remote from her main purpose, which was to write a crisp and convincing account of her ideas on the foundations of economic theory.

The pamphlet was a methodological exercise in the largest sense: a statement that put economists on notice as to their proper business and how they should go about it. What is the basis of the metatheoretical priority of technique or method? How are theoretical assumptions linked to technique? What is the significance of the choice between methodological "optimism" and "pessimism?" The attempt to answer these difficult and contentious questions in a short essay was obviously a presumptuous undertaking, not least for a fledgling economist who had published nothing beyond a book review.

In the 1920s, the Marshallian guild did not produce a book or even an article devoted to methodology. The pamphlet marked a significant departure from this academic culture, and in several respects. Unlike Dennis Robertson and the generations of Tripos students who followed in his footsteps, Joan Robinson was not content to treat cursory remarks by the godfathers of the guild as the last word on Cambridge metatheory, skipping through the early chapters of Alfred Marshall's *Principles of Economics* in order to arrive as effortlessly as possible at the analyses that were presumed to constitute the core of the discipline (Robertson 1952, pp. 13-14). In publishing the first purely methodological essay by an economist trained in the Tripos, she broke with the Marshallian tradition of consigning metatheoretical discussions to prefaces, introductions, appendices, and informal observations in correspondence. The pamphlet also repudiated the Marshallian doctrine on the subordinate status of economic theory as well as its relationship to substantive research. She took the mechanistic figures of speech of Marshall and A.C. Pigou dead seriously, transposing their metaphorical allusions to toolboxes, appliances, and analytical machinery into a methodological principle. As was her wont, she pushed this principle to its ultimate logical conclusion, reducing economics to its methods of research: "The subject matter of economics is neither more nor less than its own technique" (Joan Robinson 1932, p. 4).[6]

[6]Joan Robinson expected a review of the pamphlet in *The Economic Journal* and was clearly disappointed when none appeared (Joan Robinson to Richard Kahn, March 3, 1933, RFK/13/90/1/168-172). Several members of the economics faculty at Cambridge received copies, each with a pithy and sometimes cryptic dedication. Marjorie Turner (1989, p. 229) identifies some of the dedications and their recipients. Joan Robinson also sent a copy to Joseph Schumpeter at Harvard with a "touching inscription" and expected commentary in return. When none was forthcoming, she asked Kahn, then at Harvard, to speak with Schumpeter on her behalf (Joan Robinson to Richard Kahn, January 28, 1933, RFK/13/90/1/78-83 and February 3, 1933, RFK/13/90/1/88-93).

Joan Robinson maintained that the methods of her time were "only capable of giving unreal answers to unreal questions" (Joan Robinson 1932, p. 5; see also Autobiography, I, 4). The questions were unreal because they were derived from the logical possibilities of economic method, which abstracts from the properties of economic phenomena. The answers were unreal because their referents were not economic facts but artifacts of method. Thus any correspondence between the results of theoretical analysis and actual economies was purely fortuitous.

Economic analysis was defined by the position that economists take on the assumptions of their investigations. In choosing assumptions, the analytical economist employs the criterion of "tractability." In employing a given set of assumptions, is it possible to conduct an analysis by using existing technique? This is the fundamental methodological issue for the "optimistic, analytical, English economist," who regards the empirical status of assumptions as irrelevant (Joan Robinson 1932, p. 6). The "pessimistic, methodological, Continental economist," faced with a decision between assumptions that are tractable but unrealistic and alternative assumptions that are realistic but intractable, chooses the latter. Optimists are model builders committed to the project of abstract theory construction for its own sake. Pessimists believe that only facts are of intrinsic interest: the test of a good theory is not methodological tractability but conformity to facts (Autobiography, I, 5).

For the optimist, there is a close correspondence between the problems and methods of economics. A method generates the problems for which it can provide solutions. What is the basis of this supposition? The current set of theoretical problems is the work that can be done with existing technique. The possible uses of the existing array of tools determines the agenda of theoretical problems. Thus at any given point, there are as many different problems as there are unresolved tasks that existing tools can be used to perform. The optimist supposes that economic methods bear labels that identify their possible uses. In Joan Robinson's metaphor, which ascribes a voice to economic technique, economists pose questions to their methods and the methods reply, revealing the problems they can be used to investigate and the way to conduct the investigation. If an optimistic English economist fails to state his assumptions clearly, the solution is not to ask him for a more exact account of his premises but to "go behind his back and ask his technique" (Joan Robinson 1932, pp. 8-9; see also Autobiography, I, 4). This is the ultimate basis of the claim that economics is neither more nor less than its technique. On this strict instrumentalist position, economics is reduced to making and applying tools that speak when they are spoken to. Or in more banal terms, a method entails its applications, in which case the uses of a method can be derived from the method itself. The decisive consideration in economic theory is not economic phenomena and the problems they pose but economic method, which constitutes its own objects of investigation, determines how they can be analyzed, and places limits on the conclusions that can be drawn. Just as the tools at the disposal of carpenters determine the materials they can work on and what they can make with them, so the tools of economic analysis define the problems of economic theory and the constraints imposed on their solution.

Because pessimists are not of one mind concerning their doubts about the validity of analytical economics, they are divided into many tribes. Writing as if she were an anthropologist constructing kinship taxonomies of an exotic culture, Joan Robinson sketched a brief typology of pessimists. Methodological pessimists are motivated

by precision, eliminating from the analytical toolbox all techniques "which appear to involve conceptions that are not capable of measurement" (Joan Robinson 1932, p. 11). They are distinguished from fundamental pessimists, on whom she does not comment.[7] Pessimists, we learn, are not confined to the "Continent."[8] English pessimists are classified as either pure or logical. Logical English pessimists prevent the ossification of analytical economics into a methodological dogma by proposing problems that exceed the limits of current technique by a small margin, thereby challenging optimists to extend and refine their methods.[9]

The analytical response to the pessimist is essentially a confession of faith based on a vision: to exploit the limits of existing technique in a piecemeal fashion, moving from one abstract problem to another and solving each by adopting the most exacting standards of current methodology. Analytical economists work in the belief that this program will gradually evolve a method sufficiently complex to take the measure of real economies (Joan Robinson 1932, pp. 11-12; Autobiography, I, 7). In this program, technique is the unmoved mover: it answers all questions that are answerable, solves all problems that are resolvable, and produces all explanations that are possible. But there is no explanation of technique. Any putative explanation could be produced only by technique itself. Because the explanans could not be distinguished from the explanandum, any attempt to produce such an explanation would be tautological. The same holds for questions about the validity of technique. How could they be addressed? Only by technique, the validity of which is presupposed and thus lies beyond question.

The Economics of Imperfect Competition rests on this conception of method, employing assumptions that, "in all their naked unreality," are detached from the facts of real economies (Joan Robinson 1933a, p. 8). Borrowing the mountaineering metaphors favored by Marshall and Pigou, both devoted Alpinists, Joan Robinson issued a warning that the reader who follows her path "will quickly find himself in a mountainous and inhospitable territory" that is "*nur für Schwindelfreie*": heights of analysis reserved for economists who are not intimidated by the possibility of theoretical vertigo. Work at the highest levels of theoretical abstraction is essential to the progress of economic analysis, the objective of which is not knowledge of the real world but the production of tools to build a "working model" of the world (Joan Robinson 1933a, p. 1).

Truth as conformity to facts is irrelevant to this project. Faced with an inconsistency between a "charming diagram" and the facts, Joan Robinson of course refused to sacrifice the diagram. Acting on an "unscrupulous resolve," she used her technique to "trick" pessimists, convincing them that the deviation of the diagram from the facts it represents was within acceptable limits (Autobiography, I, 7). Truth in economics has a purely formal sense. An account is true when a given technique is correctly applied to the analysis of facts that are presumed as given. The relation between the presumptive facts of analytical economics and the facts of the empirical world is indeterminate.

[7]The pamphlet is dedicated "To The Fundamental Pessimist." This was Piero Sraffa.

[8]Joan Robinson recognized no differences in the national economic traditions of Austria, Germany, Italy, Sweden, and Switzerland, which all fall indifferently under the heading of "Continental" economics.

[9]Joan Robinson's exemplar of logical English pessimism was Gerald Shove—her colleague, sometime rival, and a long-time intimate of Keynes. On discovering his place in her taxonomy of pessimists (Joan Robinson, 1932, p. 12), he was irritated to find himself relegated to the status of an under-laborer who produces questions but not answers (Gerald Shove to Joan Robinson, October 19, 1932, JVR/vii/412/30-31).

Because analytical economics brackets empirical reality in order to exploit all the possibilities of technique, its conclusions are purely hypothetical. Should they happen to have empirical value is a matter of pure chance. Is this a scientific defect? The "merit" of analytical economics, Joan Robinson claimed, "lies in its charm." In language that recalls the self-referential aesthetics of Gertrude Stein: "For people who like this kind of thing, this is the kind of thing they like" (Autobiography, III, 1).[10]

IV. THE PANTHEON OF ECONOMIC GENIUS

In part I of the Autobiography, Joan Robinson defines the scope of analytical economics methodologically. In part II, she proposes a more whimsical definition. Her method, which plays on the tension between intellectual acuity and undisguised burlesque, is to demarcate the field by examining the concept of economic genius. The latter concept is in turn understood on the basis of a peculiar mode of book composition and reception.

The production of an economic genius begins when a thinker of striking originality—"a man who thinks of something to think about" (Autobiography, II, 2)—develops a new technique and writes a book about it. However, his achievement is diminished by a failure to understand the new technique and the conditions for its use. The genius conceptualizes the technique in a theoretical language that is inconsistent with its application. Because there is a sense in which he does not know what he is talking about, he mischaracterizes the technique and gives his book a misleading title. The next move is made by an unsympathetic reader, who points out the inconsistency between technique and theoretical language. As a result, the unsympathetic reader seems to demonstrate that the genius is "talking nonsense" (Autobiography, II, 3). Although the genius may be incapable of breaking this impasse, his technique is more powerful than its creator, possessing an immanent logic that speaks to a sympathetic reader. Enlightened by the critique of the unsympathetic reader, this charitable reader sees what the genius did not: the theoretical assumptions needed to understand the technique. As Joan Robinson puts this matter in her pamphlet:

[10]Lionel Robbins published *An Essay on the Nature & Significance of Economic Science* only a few months before Joan Robinson's pamphlet appeared. Did she see *Economics Is a Serious Subject* as a Cambridge alternative to the Viennese metatheory of the time, imported by Robbins to the London School of Economics and distilled in his essay? If so, the pamphlet was a peculiar response. It approximates much more closely than Robbins's essay the orthodox Viennese commitment to axiomatics and deductive methods. The basic premise of Robbins's book is a distinction between the subject matter of economics and its method. For Joan Robinson, this is not a possible distinction; the subject matter of economics is its method. Robbins disclaimed philosophical pretensions and any attempt "to elaborate, out of the void, a theory of what Economics should become" (Robbins 1932, p. ix). He made no claims to originality, insisting, surely disingenuously, that he was merely stating views that qualified as "the common property of most modern economists" (Robbins 1932, p. ix). Joan Robinson proposed to delimit the sphere of analytical economics *de novo* and instruct economists on how they should go about their work. Finally, in contrast to the methods of Robinsonian economics, which give only "unreal answers to unreal questions," Robbins stressed the empirical basis of the postulates of abstract economic theory. Read in the light of Robbins's essay, Joan Robinson's pamphlet is a more radical departure from the methodological traditions of Cambridge economics.

> The account that the economist gives of the assumptions required for his technique is very often misleading. "The formulae are wiser than the man who thought of them" and the technique knows a great deal more about the assumptions that it requires than the economist who is expounding it (1932, p. 8).

When the sympathetic reader grasps the innate wisdom of the technique and its theoretical imperatives, it becomes clear that the genius, after all, "was perfectly right" (Autobiography, II, 3). The economic genius may be compared to an explorer who discovers a new continent but is mistaken in identifying it. The unsympathetic reader points this out. The sympathetic reader, following clues in the explorer's map, redrafts the cartography of the new methodological world.

Joan Robinson claimed that in her time there were three men of economic genius, the three Cambridge economists who wrote the books that proved decisive for her theoretical apprenticeship: Marshall, Pigou, and Keynes.

Marshall understood the subject matter of his *Principles* as the theory of value.[11] "The Fundamental Pessimist," Sraffa, demonstrated that Marshall had committed radical and self-destructive logical errors. The sympathetic reader—Joan Robinson (1933a, p. 16; 1933b, p. 26)—set matters aright by following the logic of Marshall's technique buried in his footnotes. When Marshallian value theory is re-conceptualized as the analysis of the output of a single commodity, the place of the new Marshallian continent in the universe of analytical economics is clear.

Pigou wrote a book about resource allocation and committed the colossal error of calling it *The Economics of Welfare.* Precisely because of the magnitude of this error, Cambridge supervisors, baffled by his concept of human welfare, advised their students not to worry over this matter and concentrate on the real subject of the book, the allocation of resources. In the Autobiography, Joan Robinson seems to have little enthusiasm for applying the concept of economic genius to Pigou. Who were the unsympathetic and sympathetic readers? And what was the inner logic of Pigou's book that would enable the sympathetic reader to see that, like Marshall, he was not writing nonsense? These questions are left untouched.

When she turns to Keynes, who mistitled his book on the analysis of output as a whole *Treatise on Money*, her interest in economic genius is revived. The first unsympathetic reader of the *Treatise*, she suggests, was perhaps the author himself in his "cutting epigrammes about people who write books on money" (Autobiography, II, 7). Although the theoretical implications of his technique may not have been immediately apparent to Keynes, they were quickly grasped by "two or three hardy, self-conceited young colleagues"—members of the fabled Cambridge Circus. Keynes's sympathetic readers listened to his technique and drew the proper inferences. Since the Circus was formed immediately following publication of the *A Treatise on Money*, confusion between the author's conception of the book and its actual analysis was quickly cleared up.

In "The Theory of Money and Analysis of Output," Joan Robinson noted that Keynes failed to see that he was investigating the analysis of output and not money. This is the sense in which he did not understand that he had produced "a revolution":

[11]Note that at this point Joan Robinson shifts her criterion for genius from a mistakenly titled and misconceived book to a misnamed and misunderstood theory.

a novel theory of the long-period analysis of output (1933b, pp. 24-25). In redrawing the theoretical map of the *Treatise*, she compared Keynes's misconceptions to Marshall's errors, which in her view she had also corrected (1933b, pp. 25-26). Thus in two of her three cases, she reserved for herself the role of the sympathetic reader who listens to the technique of the confused and misunderstood genius and, following its voice, places his analysis on a sound theoretical footing.[12]

As Joan Robinson understands economic genius, the originality of the genius and the profundity of his errors are commensurate. Deep misconceptions require many more years to clarify than less recondite and fundamental mistakes. The project of asking questions of Marshall's technique, learning from its answers, and finally reconfiguring the *Principles* on a new set of assumptions required some fifty years. In the case of the *Treatise*, this project was executed in roughly "a week." As Joan Robinson surveyed the pantheon of economic genius in 1932-33, Marshall stood in first place, with Pigou a distant second. The "Fundamental Equations" of the *Treatise*, however, were "to be congratulated on Mr Keynes' progress" (Autobiography, II, 8).

APPENDIX

A PASSAGE FROM THE AUTOBIOGRAPHY OF AN ANALYTICAL ECONOMIST.

October, 1932.

Three years ago a promising pupil of my husband's showed up an essay in which he used the marginal revenue curve.[13] The question on which it was written was only

[12]A persuasive case can be made that Joan Robinson, unsurprisingly, regarded J. H. Clapham as Pigou's unsympathetic reader and saw herself and Kahn as his sympathetic interpreters. Limitations of space allow only a brief presentation of this case here. Kahn's article "Some Notes on Ideal Output," an examination of resource allocation based on Part II of *The Economics of Welfare*, attempts to dispose of Clapham's criticisms of Pigou (Kahn, 1935, p. 13). In the Cambridge summer vacation of 1934, Joan and Kahn were working on a draft of this article. She answered some of his questions, suggested ideas, and urged him not to struggle with the more recalcitrant issues until they met again in Cambridge: "don't go and solve all the difficulties without me" (September 4, 1934, RFK/13/90/2/76-83). Clapham had argued that Pigou's analysis of the laws of returns is incoherent because there are no criteria for deciding whether specific industries operate under conditions of increasing or diminishing returns (Clapham 1922). Kahn claimed that it was not difficult to identify industries that produced increasing returns. Public utilities and railroads, which have substantial fixed costs, are obvious cases. Economists agreed that subsidies for such industries were advantageous since they increased the output of industries that benefit from lower utility and transportation costs. Because public utilities and railways generally succeed in eliminating their competition, economists also agreed on the advantages of compelling them to increase their output. Thus the issue that Clapham regarded as an irresolvable conceptual problem was reduced to "an exercise in the control of public utilities" (Kahn 1935, p. 13). Kahn concluded that "Dr. Clapham's economic boxes were full all the time, but the economists were looking on the wrong shelf" (Kahn 1935, p. 13). Where were economists looking? On the shelf where they found increasing and diminishing returns industries. What was the right shelf? Presumably the shelf where they would find public utilities and railways. Where does Kahn's article take the analysis of the allocation of resources? Back to chapter 21 of Pigou's *Economics of Welfare:* "Public Control of Monopoly," his argument for state regulation of private monopolies in order to ensure that their output approximates the socially optimal level. The genius was right after all.

[13]To "show up" an essay is to submit it to a teacher or examiner for evaluation.

some trivial class room point, and the pupil attached no importance to it. But my husband showed me the diagrams, saying: I wonder none of us thought of that before. I felt an instinctive sympathy with the marginal revenue curve, and, though I did not touch it, I felt pleased when Mr. Harrod, another of its numerous independent discoverers, got it safely into print.

Then one day Mr. Kahn put some problem to me, and I said: Let's see if Gifford's curve will do it for us. And it did. One problem led to another, and all the time Mr. Sraffa's article of 1926 kept nagging at us:—What about the supply curve in an imperfect market? In two years I had accumulated matter for a book.

I.

Intellectual self-consciousness—the trick of watching the mind at work—is bad for the geniuses. But lesser minds can do the world a service by showing how it all happens. It is with this purpose that I am writing the autobiography of an analytical economist.

When I had all the chapters of my book written, I said to myself:—The modern cult of intellectual honesty demands that I should put in a chapter on Assumptions. Now what shall I assume?

It is necessary to explain why this cart-before-the-horse method, which would so much shock a mathematician, does not lead to disaster in economics.

The economist knows, first, the bit of technique—drawing a supply curve, or setting out a Quantity Equation—which he was taught when he was an undergraduate. He knows roughly what that technique can do, and he picks out of the real world some appropriate commonsense concepts such as a firm, a pair of boots, or a man-hour of labour. He sets his technique to work on these and gets some interesting results. Then he has to ask his technique (for he has no one else to ask) what aspects of those conceptions are too indigestible for his technique to swallow. If he is using a two-dimensional technique he must choose only one independent variable out of all the variables mixed up in the real-world situation. For most problems he chooses price as the independent variable. Then he must set about to cut all the other variables out of the real-world picture by arbitrary assumptions. Of these the most formidable is Time. He makes time stand still by talking only about equilibrium situations.

Just then a colleague points out that in the real world time is far more important than price. If he is of a pessimistic temperament he tears up his manuscript and turns to what he feels is less amusing but more admirable—constructing equations with $\underline{n}$ variables, which he knows that he will never be able to solve. His only pleasure now is to exercise ingenuity in spreading his own pessimism by pointing out such facts as that if the foreman gets a set of false teeth the workers' nerves are upset and output declines. But if he is a convinced optimist he just slips into his chapter an assumption: I assume that Time stands still, and goes on with his two-dimensional problems.

An autobiographical illustration will show this process at work. I had been solving some problems which seemed both pretty and new with the aid of a supply curve of a factor of production to an industry. When it came to writing up the assumptions I looked up the current views on the question of the supply of factors. First there was some old fashioned stuff about the *fine gifts of nature* which did not cause much

trouble. But there were some very respectable theories which said that *capital* was a factor, and some still more respectable theories that said it was not. There was a new theory just getting about that *dirt* was a factor of production, and an old unsolved dispute as to whether *risk-bearing* was a factor or not.

I was utterly bewildered. But whatever my elders might say I was not going to sacrifice that charming diagram on page 275. With this unscrupulous resolve I asked my technique what I had better to do. Don't be silly—it replied—you and I don't have to worry about all that stuff. That all belongs to the analysis of Output as a Whole. My relief was immense, and I set about to vindicate my technique by drawing a supply curve that would shock the pessimists to only a reasonable degree. I looked up the works of a great English pessimist to get a pre-digested account of factors in the real world. With some not too arbitrary assumptions I picked out the case of a falling and the case of a rising supply curve of a factor to an industry which were congenial to the technique and raised no fundamental difficulty. And then by means of a trick I forced all the other cases into the same mould. This sounds an unscrupulous line of conduct, but a little well placed unscrupulousness—I comforted myself—has done us no harm in the past. And I placated my professional conscience by putting into the footnotes some objections to my trick which it would have taken any reader, approaching the subject *de novo*, a very long time to state correctly for himself.

When the book was written no one would have known that it was not the horse that was drawing the cart.

II.

This autobiographical fragment refers to the analysis of value or, more properly, the analysis of the output of a single commodity. There are only three subjects at the present time in pure economic analysis. If any one thinks of three more I refute him by a circular argument. What I choose to call pure economic analysis are the three subjects:—the analysis of the output of a single commodity, the analysis of the distribution of resources between different uses, and the analysis of output as a whole.

Now why are there no more subjects? Because there have only been in recent times three economic geniuses, Dr. Marshall, Professor Pigou, and Mr. Keynes. Once more the argument is circular. In my vocabulary genius is not a word with which to flatter men of whom I have a high opinion. A *genius* in economics is a man who thinks of something to think about.

There are two marks of a genius in economics. First, he calls his book by the wrong name. Second, an unsympathetic reader points out that he is talking nonsense and then a sympathetic reader notices that, be that as it may, he was perfectly right.

On these tests Dr. Marshall was the greatest genius of the three. He called his subject the Theory of Value, and said he was describing how price is determined. The Fundamental Pessimist pointed out that this account of the matter was quite nonsensical by the simplest possible proof. Just think, he said, of a commodity produced under constant returns, and what becomes of the pair of scissors, and the balls in the bowl? The price of a commodity is determined by its cost of production. It did not take the sympathetic reader long to notice that by merely removing the name Theory of

Value, and substituting the words The Analysis of the Output of a Particular Commodity that difficulty could be removed. But the whole process of finding out that Marshall was talking nonsense and of asking his technique why he was right after all has taken nearly fifty years. The errors and the genius were on a commensurate scale.

Professor Pigou was not such a great man. On the first test he beat even Marshall. He thought he was talking about some notion of human welfare which has far less bearing on the analysis of the distribution of resources than the analysis of price has on the analysis of output. But the magnitude of his error made it harmless. No one could ever make out what his idea of human welfare was, and they all advised their pupils to skip Chapter I and get down to the analysis.

On the other tests he makes a poor show. After two or three editions there are only very few passages where the technique is still to be heard protesting: Just don't believe that little bit, and I will see you through.

Mr. Keynes was not quite on the same scale. To call the analysis of Output as a whole a Treatise on Money was in the right tradition. But it was Mr. Keynes himself who first began (with the candour which endears him to his friends and alienates his enemies) to make cutting epigrammes about people who write books on Money.

On the second test he does not do as well as the others, for the whole process was too rapid to be really impressive. Mr. Keynes' technique took the wise precaution of dropping a hint to two or three hardy, self-conceited young colleagues before it began the job of telling its own author what it was talking about, and there was only a period of about a week in which the Treatise was taken at its face value in the way that the Principles and the Economics of Welfare were taken for years. At the present date the Fundamental Equations are to be congratulated on Mr. Keynes' progress.

III.

It was these events which led me to reflect that the time had gone by when Professor Pigou need reiterate in the prefaces to the successive editions of the Economics of Welfare: This subject is dull, difficult, but useful. The day has dawned when we can say: You may have your own opinion about whether this subject is ever going to be useful. For us its merit lies in its charm. For people who like this kind of thing, this is the kind of thing they like.

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